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Nineteenth-century ideas on hemisphere differences and "duality of mind"

Anne Harrington
Wellcome Unit for the History of Medicine, Oxford University, Oxford OX2 6PE, England

Abstract: It is widely felt that the sorts of ideas current in modern laterality and split-brain research are largely without precedent in the behavioral and brain sciences. This paper not only challenges that view, but makes a first attempt to define the relevance of older concepts and data to present research programs.

In the 19th century, there was a body of literature that held that many mental pathologies could be explained by supposing that each individual potentially had two conscious brains. Madness resulted when these begin to interfere with each other or otherwise functioned independently. The left-sided localization of language by Broca in the 1860s complicated matters by showing that the two brain halves functioned differently. Broca argued that functional asymmetry was a reflection of man's capacity to "perfect" himself; soon, the left hemisphere was transformed into the superior, uniquely human side of the brain. Considerable effort then went into seeing how far the functions of the right hemisphere complemented those of the left. The resulting dichotomies of mind and brain interacted – and sometimes also conflicted – with "duality of mind" theories. In the 1880s, the Paris school of neurology helped bring about a revival of interest in these theories with its startling metalloscopy and hemihypnosis experiments.

A section of this target article is devoted to the views of Hughlings Jackson. Jackson's physiological/philosophical writings on hemisphere specialization and mental duality largely set him outside of the rest of the 19th-century tradition. The article concludes that at least some of the data gathered in the 19th century might prove useful or interesting to certain investigators today. More important, it asks how far an awareness of the "time-bound" nature of 19th-century concepts should change the way in which one surveys the laterality scene today.

Keywords: aphasia; Broca; cerebral dominance; consciousness; history of neurology; hypnosis; Jackson; laterality; mental duality; split brain

For the past 15 years or so, the behavioral and brain sciences have been fascinated with the duality and asymmetrical functioning of the human brain. Does severing the corpus callosum result in two "conscious entities" within a single individual (Sperry 1968)? Does the age-old tendency for societies to dichotomize human experience reflect the fact that, in the human species, "there are two types of thinking generated in the same cerebrum" (Bogen 1969)? Has Western society, with its emphasis on logical thinking and language skills, overdeveloped its left hemisphere at the expense of the intuitive, holistically oriented right hemisphere, whose functions are claimed to be far more developed in the cultures of the Orient (Ornstein 1972)? Does the new research cast light on the neurology underlying certain dynamic unconscious processes described by Freud (Galin 1974)? What are its implications for attempts by philosophers to reconcile a personal, mentalist view of human consciousness with an understanding of human beings as biological systems (Nagel 1979; Puccetti 1981)? What does it mean for our understanding of personality differences (Bakan 1969), or differences in the cognitive capacities of the sexes (McGlone 1980; Witelson 1976)? Can we find in this research a clue to the problem of the origins of consciousness (Jaynes 1976)? Does it tell us anything about the biology of mental illness (Marin & Tucker 1981), the means by which the soul interacts with the body (Eccles 1976), or the original source of neurosis in the phylogeny of the human race (Janov & Holden 1975)?

Underlying much of the present excitement is a widespread feeling that we are pushing back frontiers that earlier generations had never suspected existed, that the sorts of ideas being propounded today are largely without precedent in the behavioral and brain sciences. Benton (1972:8) tells us, for example, that the 19th century essentially ignored the right side of the brain, believing it to have "no distinctive functions," but to be "merely a weaker version of the left." Puccetti (1981) affirms that the history of speculation about mental duality in relation to man's double brain "is very brief indeed," with Wigan (1844) being the first to propose that each man might in fact have two conscious brains in his skull, and Bogen (1969) being the second; this hiatus, Puccetti believes, gives "some idea of the popularity" of the concept.

Here and there in the modern laterality literature one does find some partial correctives, some indications that the last word has not been said on 19th-century ideas about the double brain, and that there is a gap in the historical record (see, e.g., Zangwill 1974; Oppenheimer 1976; and esp. Harris 1980). To date, however, there has

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been no attempt to come to terms in a comprehensive manner with the relevant literature, and no systematic effort to define the precise relevance of that literature for researchers working in the field of laterality today. This essay offers an initial attempt to meet both objectives.

We begin with the basic conviction that the history of science is more than "a kind of paperchase of ideas back through the ages" – that authors and their linguistic vocabulary must be set in their true historical setting, and that the historian must constantly be aware of how the meanings of words and ideas change their shape and color over time, so as to adapt to new circumstances and new needs (Stone 1981:86–87). Before one can grapple with the question of the 19th century's relevance, then, one needs to understand it on its own terms. The chief interest of the work to be outlined here lies not in the fact that it somehow harbored "premonitions" of this or that aspect of 20th-century thought on brain duality, or that it prophetically anticipated one or another modern finding about hemisphere differences. The point is a rather more subtle one concerning how a historical perspective on modern laterality research might be important to scientists interested in the place of their own work in the flow of human ideas.

1. The early case for "duality of mind" (pre-Broca period)

Since the earliest days of physiology, feeling had been strong that the seat of the soul's operations would have to be some unitary organ that could be regarded as corresponding to the unity of the soul's experienced consciousness. As the view gradually gained ground in the 17th and 18th century that the workings of the soul were intimately correlated with the workings of the brain, the almost perfect bilateral symmetry of the latter structure severely limited the number of appropriate spots. Thus, René Descartes (1596–1650) was forced to choose the tiny but centrally located pineal gland as the site of the soul's actions; François Gigot de la Peyronie (1678–1747) granted the honor to the corpus callosum; still others opted for the septum lucidum or the central ventricle.

Little by little, however, it became clear that these medial organs had little or no immediate connection with consciousness. The phrenologists, led by the Austrian anatomist, Franz Joseph Gall, were among the first early in the 19th century to take the growing body of facts as they found them and boldly map human consciousness onto the convolutions of the two cerebral hemispheres. Gall taught that each of the phrenological "faculties" existed in duplicate – one in each hemisphere – so that each side of the brain could in the end serve as a complete organ of the mind, just as a single eye can serve as a complete organ of sight. As the phrenologically oriented French neurologist, Jean Baptiste Bouillaud, put it in his 1825 *Traité*: "We have a double intelligence: an intelligence on the right and an intelligence on the left" (Bouillaud 1825:264).

Although there was no absolute agreement on this issue, most phrenologists were inclined to suppose that any disorder that upset the perfect symmetry and presumed simultaneous functioning of the two hemispheres would have the effect of disordering the faculties involved. Here the phrenologists were joined in their opinion by certain others who, while rejecting Gall's mosaic model of the mind, were nevertheless prepared to accept the brain as the organ of the mind. Thus one finds the idea beginning to be entertained that certain forms of insanity may result from incongruous or independent action of the two hemispheres.

As early as 1747, the radical French materialist, Julien Offrey de la Mettrie (1709–1751), described the unilateral madness of Pascal, for whom "madness and wisdom each had its compartment or its lobe, the two sides separated by a fissure" (la Mettrie 1747:120). In the beginning of the 19th century, America's first psychiatrist, Benjamin Rush (1745–1813), wondered whether cases of somnambulism where patients seem "to depend upon *two* minds" could be ascribed "to all the mind being, according to Dr. Gall, like vision, a double organ, occupying the two opposite hemispheres of the brain" (Rush 1981:670). The Edinburgh phrenologist, Hewitt Watson, in his 1836 "What Is the Use of the Double Brain?" also felt that "two-fold personality," as well as "many cases of insanity" might be explained by assuming pathological dissociation between the two hemispheres. In 1838, the pioneering French alienist, Jean Esquirol (1782–1840), spoke of a form of madness in which a man becomes "the *homo duplex* of St. Paul and Buffon, impelled to evil by one motive, and restrained by the other." He attributed this "lesion of the will" to "the duplicity of the brain, whose two halves, not being equally excited, do not act simultaneously" (Esquirol 1838:363). In 1840, Sir Henry Holland (1788–1873) – the fashionable London doctor who became physician to Queen Victoria – similarly suggested in his "On the brain as a double organ" that "some of the aberrations of mind, which come under the name of insanity" – especially sinning against knowledge and desire – might be due to "incongruous action" of the brain's two hemispheres. (Holland 1840:184–86).

Of all the advocates for a relationship between madness and disordered action between the two hemispheres, however, none was more fervent or far-reaching in his claims than the early-19th-century British doctor from Brighton, Dr. Arthur Ladbroke Wigan. Wigan's masterwork, *A New View of Insanity: Duality of Mind,* was published in 1844 near the end of the author's life. In it, he set out to prove that "each cerebrum [hemisphere] is a distinct and perfect whole," capable of independent thought and independent volition. In the healthy brain, one of the two hemispheres is almost always superior in power, and exercises control over the volitions of its fellow. In cases of disease, however, where "one cerebrum becomes sufficiently aggravated to defy the control of the other," insanity can set in as the two hemispheres pursue independent courses, their separate wills struggling against each other, their separate thoughts jumbling together. The only way to prevent or control distressing cases such as this is through "a well-managed education" which serves to "establish and confirm the power of concentrating the energies of both brains on the same subject at the same time" (Wigan 1844:26–30).

Wigan was enormously proud of both the profound originality and the profound importance of his "duality of mind" thesis, likening it to Harvey's discovery of the circulation of the blood, and contemplating consequences

following its establishment more conducive to the happiness of mankind even than those apt to follow from vaccination and the electric telegraph. In essence, he was convinced that he was laying the groundwork for a "moral physiology" (p. 102) which, for the first time in human history, would allow the existence of "moral evil in the world" to be understood, and steps taken for its scientific control and prevention. In his hands, brain physiology became inextricably interwoven with the ethical values of early Victorian society.

Wigan's book was reviewed by the British phrenologist/mesmerist, John Elliotson, in *Zoist* (1847). Elliotson attacked the author savagely for his unwarranted claims of absolute originality, his dismissal of phrenological doctrine, and his self-aggrandisement. He also pointed out that if, as Wigan argued, a person requires two brains to do two things at once, "he ought to require several brains when he does several things at once, and a countryman walking the streets of London, using his stick, talking, hearing, and staring as he proceeds, could not dispense with fewer than five" (p. 233). An anonymous reviewer in the *Journal of Psychological Medicine* ([Review of] "New View of Insanity," 1848) called Wigan's inquiry "novel" but as yet "*sub judice*" (p. 30). Forbes Winslow, whose own writings would later make a strong impression on such men as John Hughlings Jackson, was convinced that Wigan was a genius who had discovered "great psychological truths." "Generations may roll away ere a just appreciation will be made of the suggestions contained in his celebrated treatise on the 'Duality of the Mind'" (Winslow 1849:497).

2. The discovery of asymmetry

2.1. Challenge to the "laws of symmetry"

The early advocates of duality of mind all took it for granted, of course, that the two hemispheres of the brain were functionally identical. The overturning of this view, with the discovery that the "faculty of articulate language" exists only on the left side of the brain, occupies a peculiar place in the early history of neurology. Initially, the fact that lesions causing speech disorders almost always had their seat in the left hemisphere only was seen as an unexpected – and thoroughly unwelcome – complication to the effort being made by Paul Broca in the early 1860s to localize speech in the frontal lobes. Nevertheless, by the end of the 1860s, the asymmetry problem had forced a transformation in the way neurologists regarded higher mental functioning in the human brain and in how they sought to measure man's worth in the scheme of things.

As is well known, the association between speech disorders and damage to the left cerebral hemisphere actually seems to have been noticed first by an obscure French country doctor, Marc Dax, in 1836. In more than 40 cases involving speech loss without paralysis of the articulatory organs, Dax had noticed left-brain damage, but he had been unable to find a single case that involved damage to the right side of the brain alone (Dax 1836). Dax's work was not published during his own lifetime, but it was brought to the attention of the Académie de Médecine in 1863 by his son, Gustav Dax. It was in that same year that Broca – having already determined that articulate language seemed to have its chief seat in the foot of the third frontal convolution of the brain (Broca 1861a; 1861b) – also first began to struggle with the problem of asymmetry (this coincidence of dates has led some historians to speculate that Broca might have known of Dax's work, although he himself always denied it; see Souques 1928; Joynt & Benton 1964). On April 2, Broca reported on eight new cases supporting a localization of speech in the third frontal convolution, and he then went on to note how remarkable it was that, in all these cases, the lesion had been on the *left* side. "I dare draw no conclusions," he said, "and I await new facts" (Broca 1863:202).

But the peculiarity of the coincidence continued to trouble him, and he urged his colleagues to search for a "counter-proof"; that is, some case of aphasia involving the *right* frontal lobe rather than the left. By July 1863, however, when no such case had been unearthed, he was forced to concede that it might soon be "necessary to admit that the faculty of articulate language is localized in the left hemisphere" ("Atrophie" 1863:380–81).

Broca was painfully aware that an asymmetrical localization of language would represent a virtual "subversion" of French physiological teaching of the time, still strongly guided by the work of Marie Francois Xavier Bichat (1771–1802). Bichat, a brilliant young physician whose vitalistic orientation profoundly influenced French physiology up to the time of Claude Bernard, had also established what came to be called the "laws of symmetry." These laws were derived from Bichat's distinction between organs that serve "organic life" and the passions (organs of digestion, respiration, circulation, generation), and organs that serve the life of "external relations" and understanding (the cerebral hemispheres, sensory organs, arms and legs). The latter organs, Bichat taught, are found in symmetrical pairs because the organism must be able to relate to the external world with both sides of its body. The two parts of each of these pairs necessarily function identically and in unison (Bichat 1805).

2.2. Toward a new paradigm of brain functioning

The challenge to Bichat's "laws of symmetry" crystallized finally in 1865, in the wake of a report on Dax's work by L. F. Lélut, one of the members of a commission assigned to evaluate the old doctor's ideas in 1863. Judgment had been swift and harsh: Not only was the asymmetry hypothesis ridiculous, but the idea that mental faculties could be localized *anywhere* in the brain was phrenological hogwash, and not worthy of serious scientific attention. Bouillaud – who had been defending the idea of cerebral localization, and especially Gall's frontal localization of language for some 35 years – was naturally incensed. His reply to Lelut in the spring of 1865 touched off a passionate debate on the "faculty of articulate language" that would last for more than three months and can be considered – as Riese (1947:326) has argued – the climax of the early history of aphasia. Although discussion was far from limited to the asymmetry problem (and indeed, Broca's more recent work attempting to localize speech in the third posterior convolution received far more attention than Dax's paper), when the question of

asymmetry was raised, tempers grew hot. There was a widespread feeling that, not only did the idea contradict all physiological teaching, but it was patently absurd, an insult to the wisdom of Nature. As Paul Briquet indignantly put it,

> Would it be possible for the right eye to see only blue, black, and red, while the left eye would be made for seeing only green, yellow, and blue? Could the right ear hear in music nothing but *do, re, mi, fa,* and the left ear *sol, la, ti*? Would one make the right nostril for smelling pleasant odors, and the left for the nasty ones? – and for the tongue, similarly, have it taste sweet flavors on the right side only, and acid ones on the left side? ("Discussion" 1864–65:714. Cf. Bateman 1890a:408; Schiller 1979:192)

Then, on April 4, 1865, Bouillaud ("Discussion" 1864–65:543) suggested to the Académie de Médecine that perhaps an asymmetrical localization of language would not be so absurd as everyone was saying, for it was not without precedent in human physiology. Were there not certain acts for which we normally – even exclusively – employ our right hand in preference to our left? Would it be absolutely impossible, then, that for certain mental functions, such as speech, we similarly favor our left hemisphere? Riese (1947:331) has said that Bouillaud's words here mark the "birthday of the doctrine of left cerebral dominance," in that the link between right-handedness and left-brain speech was made for the first time. In fact, there is nothing in Bouillaud's remarks to suggest that he was advocating a causal link between handedness and speech asymmetry; he seems, rather, to have simply been drawing an analogy. Indeed, Bouillaud tended to think any unilateral theory of language localization highly improbable (Soury 1899:592). There is good reason to believe that, at the debates, he was in some sense exploiting Dax's work for his own purposes – using it as an excuse to promote his strongly held views on frontal lobe language localization.

Nevertheless, there is no doubt that a causal connection had been made by the time Broca stood up before the Société d'Anthropologie in 1865 and declared that "the majority of men are naturally left-brained [for speech]; and that exceptionally some people, those we call left-handers, are on the contrary right-brained" (Broca 1865:383). It is crucial to realize, however, that these words were not intended to represent a radical subversion of Bichat's "laws of symmetry." Broca was forced in the end to account for the clinical data, but he proved unwilling to undermine the foundations of French physiology.

He argued, then, that Bichat had been essentially right: There were no innate functional differences between our two hemispheres. The asymmetry of language localization was simply due to the fact that – as the anatomist, Pierre Gratiolet, had shown (Gratiolet and Leuret 1839–57, II:241–42) – the left frontal lobe of the cerebrum grows a bit in advance of the right. Functionally, the two lobes begin identically, but physically (perhaps, as would be later argued, because of unequal blood flow), the left is the more precocious side. In childhood, then, when we must master the complex intellectual and motor skills that characterize civilized life – articulate speech being preeminent among them – we tend to rely upon our slightly more robust frontal lobe, and thus we educate it to the exclusion of the other (Broca 1865:393).

This proposal of Broca's that functional asymmetry is due to the impact of education and civilization upon the human mind is remarkable for the way it discovers a virtue in necessity. From being a physiological absurdity, functional asymmetry was suddenly declared "one of the principal traits of the human brain" (Broca 1869:392), a reflection of man's capacity to lift himself by his own efforts beyond mere animal existence into a civilized, human state (see Bérillon 1884:10; Ball 1884:35; Bastian 1880:400; Delaunay 1874). Because the effects of education upon the brain were believed inheritable (see le Bon 1879), it was also argued that the more motivated races – those capable of what Broca (1860) called "perfectibility" or continuing self-improvement – would tend to develop brains that were more and more asymmetrical as time went on. It is not surprising, therefore, that by 1869 Broca "had been able to assure" himself that asymmetry was less pronounced in the brains of Negroes than in those of whites (1869:393). Similarly, the French biologist, Gaëtan Delaunay, argued that women's brains were less asymmetrical than those of men, resembling in that respect the brains of savages and young, uneducated children (Delaunay, 1874). This idea was still being echoed as late as 1903 by the influential Italian criminologist, Cesare Lombroso, who proposed as well that "born criminals" lack the asymmetrical nervous system that marks a man of morality and civilization (see also Marro & Lombroso 1883). In short, under Broca, asymmetry not only became a distinguishing mark of humanity in general, but a means of distinguishing the "better" vintage segments of mankind from the substandard (cf. Gould 1981).

It is important to realize that the anatomical evidence for the claim that language asymmetry arises from developmental differences between the brain's two frontal lobes was inconclusive at best. Broca (1875), attempting to fortify Gratiolet's work, reported having found in a series of studies that the left frontal lobe in man weighs an average of 4 grams more than the right. An earlier, extensive study made in England by Robert Boyd (1861) had found the left hemisphere in general heavier than the right by ⅛ of an ounce. In contrast, John Thurman (1866) found a tendency for the right hemisphere to outweigh the left, and in Germany, Ecker (1868), concentrating on the developmental aspect of the problem, was able to find no significant asymmetry at all. Part of the disparity between many of these findings can probably be blamed on poorly controlled measurements – as one researcher (Thurman 1866:4) admitted, "in dividing [the hemispheres] . . . it is difficult to cut always in the exact median."

2.3. From asymmetry to left-brain superiority

Broca never ceased to insist that these so-called nutritional differences between the two hemispheres in no way implied that the left hemisphere is congenitally endowed with the capacity to learn language in some special way denied to the right hemisphere. This is why he could propose that, in certain cases of injury or disease to the left side of the brain, the right side could step in and shoulder at least some of the duties previously handled by

its heartier twin. This came to be known as the doctrine of cerebral substitution, and it went a long way toward making Broca's third frontal convolution resistant to criticism on clinical grounds. It was now possible for a patient injured in that region to suffer little speech defect, or to recover more or less completely from any defect originally experienced – all without calling the fundamental correctness of Broca's localization work into question. In such a situation, the potential for dogmatism was very real.

Nevertheless, as the century progressed, the original interpretation of asymmetry taught by the Broca school would undergo a subtle but significant mutation. Increasingly, the injunction that asymmetry pure and simple was what counted – rather than the *left*-sidedness of the asymmetry – was forgotten. This is understandable; it must have been easy to lose sight of the details of Broca's argument, and to focus simply on the remarkable fact that left-brain damage produces speech loss, while right-brain damage does not. At the same time, the strong Lamarckian element in 19th-century French evolutionary thought would rapidly have tended to blur any initial distinction between left-brain superiority acquired by individual cultivation and left-brain superiority as part of man's unique native endowment.

New developments in the history of cerebral localization doubtless also played an important role in encouraging a tendency to see the left-sidedness of cerebral asymmetry as an essential factor in the equation. In 1870, German physicians Gustav Fritsch and Eduard Hitzig applied galvanic currents to the cortex of laboratory dogs and demonstrated the existence of "motor centers" in a part of the brain traditionally held to be inexcitable. The superiority of their electrical method of localizing over the old use of experimental lesions attracted almost as much attention as their actual findings. Other neurologists, notably David Ferrier (1876) in England, hailed the Fritsch and Hitzig method as a technological breakthrough and adopted it to continue the search for both motor and sensory centers in the brains of various laboratory animals. It soon became clear that all these centers were *bilaterally* represented. Simultaneously, new clinical studies were increasingly reinforcing the idea that most or all of the higher "intellectual" functions associated with human beings had their seat in the left hemisphere alone.

This well-documented phase in the history of aphasia – which will occupy us only briefly here – opened with the work of the German anatomist, Karl Wernicke, who, in 1874, published his classic paper arguing for a relationship between "sensory aphasia" (loss of receptive speech capacity) and damage to the left temporal lobe. "Word deafness" as such had been described by Charlton Bastian in England as early as 1869, but it had not previously been linked to a specific anatomical locus. In the wake of Wernicke's work, the number of discrete symbolic disorders associated with discrete lesions of the left hemisphere proliferated. Eventually, some neurologists took the further step of combining all the new findings into a single theoretical framework, based upon principles borrowed from the old "associationist" psychology (these were the "diagram makers" later contemptuously described by Head [1926]). Hugo Liepmann's (1900) description of apraxia perhaps represents a crowning piece of evidence for a view of the left hemisphere as the superior, uniquely *human* side of the brain. From a variety of evidence, Liepmann had been led to conclude that the left hemisphere predominates in voluntary, purposeful acts (although not quite to the same degree as existed in language); he later came to believe that the activity of the right hemisphere was under the guiding influence of the left. It is hard to avoid the conclusion that some metaphysical will was here being localized in the left side of the brain. One is not surprised to learn, then, that Liepmann "saw in left cerebral dominance the major part of man's superiority when compared with the animal" (Riese 1947:334).

The belief in the special development of the left hemisphere's frontal lobe also contributed to the tendency to exalt that brain half, for the frontal lobes were widely regarded at this time as the site of human reason and intelligence. Gall had argued that all the intellectual faculties unique to man were located in the anterior part of the brain, and that a man was intelligent in proportion as the frontal part of his brain was more developed than the parietal and occipital parts (Soury 1899:514). According to Soury (p. 599), Gratiolet had called the frontal lobes the "flower of the brain" and believed them to be especially developed in the white European races; only thus could one account for the "sovereignty of mind" that had enabled these races to build a world empire. Broca, Magnan, Ferrier, Wundt, Bianchi, Schule, and Mingazzini all in one fashion or another regarded the frontal lobes as the chief source of understanding, will, attention, and consciousness (Soury 1899:515, 519, 917). It seems, then, that – for those who accepted Gratiolet's claims – the left hemisphere may have gained part of its good reputation by association.

3. Dichotomies of mind and brain

Although it is quite true that the left hemisphere was increasingly seen as the superior side of the brain – predominating in most or all higher mental and moral activities – it is *not* true that the "other side of the brain" (to use Bogen's term) was ignored. Indeed, a considerable amount of effort went into trying to see how the functions of the right hemisphere might be seen as complementing those of the left. (See also Table 1.)

3.1. Anterior versus posterior functioning

Comparisons of the two sides of the brain took their point of origin from the fact that Gratiolet not only had argued that the *left anterior* lobe grew in advance of the right; he had equally made the case that the *right posterior* (occipital) lobe grew in advance of the left, so that in the end a kind of balance was achieved between the two sides of the brain. Broca, having seized upon the first part of this passage from Gratiolet to account for language asymmetry, did not neglect the second part either. In 1866, he reported on the work of a German osteologist, Hans Carl Barkow, who had found a tendency in a number of human skulls for the frontal region to be slightly more pronounced on the left side, and the posterior region slightly more pronounced on the right side (Barkow 1864). Praising the exactness of Barkow's results, Broca

Table 1. *Dichotomies*

19th century dichotomies (with selected references)

Left hemisphere	Right hemisphere	References
Anterior	Posterior	(Broca 1866; Roques 1869)
Humanness	Animality	(Bourru & Burot 1886–87; Broca 1869; 1877; Myers 1886)
Motor Activity	Sensory activity	(de Fleury 1872; Exner 1881)
Intelligence	Emotion/sensibility	(Brown-Séquard 1874a; Luys 1881b)
"Life of relations"	The "organic life"	(Brown-Séquard 1870; 1871; 1874a)
Reason	Madness	(Luys 1879; 1881a; Montyel 1884)
Male	Female	(Delaunay 1874; Klippel 1898)
White superiority	Nonwhite inferiority	(Delaunay 1874; 1878–79)
Waking self	"Subliminal" self	(Manaceïne 1894; 1897; Myers 1885)
Objective	Subjective	(Crichton-Browne 1895; Jackson 1880–81; Verity 1870)

20th-century dichotomies (after Springer & Deutsch, 1981)

Left hemisphere	Right hemisphere
Verbal	Nonverbal/visuospatial
Temporal	Simultaneous
Digital	Analogic
Rational	Intuitive
Western thought	Eastern thought
Abstract	Concrete
Objective	Subjective
Realistic	Impulsive
Intellectual	Sensuous

then mentioned that he had had the opportunity to confirm them himself on two series of twenty brains, male and female respectively. All this new evidence made him increasingly confident that Gratiolet had been right, and that there is "a sort of compensation between the weight of the two frontal lobes and the two occipital lobes" (Broca 1866:196). Three years later, these alleged frontal/posterior differences between the hemispheres were given an explicitly *functional* cast, with the reported finding that the left frontal lobe was more abundant in gray matter than the right (gray matter being seen as the "stuff" of intellect), while the right occipital lobe was more abundant in gray matter than the left (Roques 1869:728).

Perhaps surprisingly, there was little or no consideration of the idea that a more developed right occipital lobe could mean particular right-sided involvement in certain visual tasks. It is true that John Hughlings Jackson would argue, beginning as early as 1864, for the existence of special "visuo-perceptive" functions in the right hemisphere, but (as will be seen) his claims were not based specifically on a view of the occipital area as "visual cortex." Berlin physiologist Hermann Munk in fact did not localize his "visual center" in the occipital lobes until 1879 (correcting David Ferrier's earlier localization in the supramarginal and angular gyrus). In a paper published in 1895, an American physician from Philadelphia, Th. Dunn, also suggested that there might exist "a centre (which may, for convenience, be named the geographical centre) on the right side of the brain for the record of optical images of locality, analogous to the region of Broca for that of speech on the left side in right-handed persons" (Dunn 1895:54). Dunn's views, however, were similarly not based on a consideration of any possible anatomical differences between the two occipital lobes but were derived from clinical observation.

The fact that the possibility of special right-brain visual capacities was mostly overlooked – again – does not mean that the right side of the brain was ignored altogether. If the anterior lobes were identified with human intelligence and reason, the posterior lobes were equally seen as the site of the passions and instincts. Even if Broca had not already cast suspicion on the right hemisphere, then, by implying that it is permitted to remain in an uneducated, half-savage state, the reputation of that side of the brain would doubtless have suffered from its association with posterior-lobe functioning. Thus it was argued that the right hemisphere plays a predominant role in sensibility, emotion (passion, criminal impulsiveness), and activities related to vegetative, instinctual life – in this sense, neatly complementing the intellectual activities of the left hemisphere.

3.2. *Intelligence versus sensibility and passion*

It would be wrong, though, to make it seem as if preconceived notions about posterior- versus frontal-lobe functioning were alone responsible for such a lateralized view of brain functioning. It became known, beginning in the 1870s, that there were cortical sensory centers located predominantly in the posterior regions of the brain, and motor centers in the frontal regions. The Viennese physiologist, Sigmund Exner (who would later serve as Freud's university instructor in physiology), compared motor and tactile representation in the human brain for various parts of the body and came to the conclusion that motor representation was both more intensive and extensive on the left side, while sensory representation was more intensive and extensive on the right side (Exner 1881:64–65). In France, Armand de Fleury (1872:840) had similarly concluded that, all other things being equal, left-brain damage tended to cause disorders of movement, while right-brain damage was much more frequently associated with disorders of the sensibility.

In 1881, Jules Bernard Luys – a neuroanatomist who had done important work on the thalamus, and a physician at the Salpêtrière and Charité in Paris – published a paper arguing for an "emotion" center in the right hemi-

sphere that would complement the "intellectual" centers already established in the left hemisphere. He had been struck by the fact that there seemed to be definite and consistent personality differences between his patients suffering from right hemiplegia and those suffering from left hemiplegia. Whereas the former, he said, were "more or less apathetic, more or less silent, passive and stricken with hebetude," the latter were peculiarly emotional (they sobbed, but their tears seemed unmotivated and rang hollow) and suffered from manic-like symptoms and delusions of persecution. Luys (1881b) felt that these affective abnormalities might be explained on the hypothesis that some normal inhibiting center for emotion in the right hemisphere had been destroyed by a lesion supplementary to that responsible for the left hemiplegia – a lesion which he had tentatively localized in the temporal area of the brain.

The case for a special right-hemisphere role in sensibility and emotion was also given an important boost by repeated observations that hysterical disorders – above all, the characteristic anaesthesias that Charcot had christened the "stigmata" – tended to manifest their symptoms *unilaterally* on the *left* side of the body (implying, according to French theory at the time, a "functional" lesion of the right hemisphere). Paul Briquet's (1859) *Traité clinique et thérapeutique de l'hystérie* – a book which served as a point of departure for Charcot's view of the disease – noted that hysterical hemianaesthesia had been observed three times more frequently on the left side than the right, and hysterical hemiparalysis was found by later researchers to follow a similar pattern (Richer 1881:530, 552). Brown-Séquard (1874a:14) examined 121 cases of hysterical hemiplegia and found a ratio of 4:1 in favor of the left. Richer went so far as to call hysteria's predilection for the left side of the body "Charcot's rule," although he warned that there were several exceptions. That the rule was widely accepted into the early 20th century can be seen from the number of authors cited by the British psychoanalyst, Ernest Jones, in a 1908 article that claimed to refute the old belief that hysteria favors the left side of the body.

3.3. "Life of relations" versus the "organic life"

Brown-Séquard (1874a:10) argued that, whereas the left hemisphere was primarily concerned with communication and intellectual activity, the right hemisphere served chiefly in "the emotional manifestations, hysterical manifestations included," *and* in "the needs of the nutrition of the body in various parts." He proposed that the two sides of the brain might thus be characterized according to Bichat's old division of animal existence into "*the life of relations*" on the one hand and the "*organic life*" on the other. His belief in the right-brain's special "organic" or "nutritional" activities was primarily based on comparisons of symptoms produced by organic lesions of the left and right side of the brain. He claimed that various "troubles of nutrition" – bedsores, oedema, pulmonary congestion, involuntary evacuation of faeces and urine – more frequently accompanied right-sided lesions than left, and that physical symptoms from right-brain lesions generally were more severe and apt to result in death than left-sided ones (Brown-Séquard 1870, 1871; but see the contradictory conclusions by de Fleury 1872). Relevant to his argument, perhaps, was the curious claim made by the German physiologist, Budge, that the "cerebral centre for the movements of the stomach" was on the right side of the brain; irritation of this side causing the stomach to move, while irritation of the corresponding parts on the left side of the brain produced no effect whatever (Brunton 1874:18).

It should be pointed out that, although he studied the problem of hemisphere functional differences, Brown-Séquard did not believe these differences to be innate, but rather a consequence of improper childhood training. "If we have two brains," he demanded before his American audiences in 1874, "why not educate both of them?" (Brown-Séquard 1874a:1; see also 1874b). Brown-Séquard even went so far as to deny that one side of the cortex controls sensation and movement on the opposite side of the body; each hemisphere, he felt, is a complete brain, not only for intellectual tasks as the other "duality of mind" advocates believed, but for full (bilateral) motor and sensory functioning as well. He pointed to cases in which there had been loss of an entire brain half without lasting interference to sensation and movement, and he presented experimental evidence to show that disabilities following unilateral lesions were merely due to "inhibitory action" from other parts of the brain. Although he made some converts, his radical antilocalization views were not generally well received, and they brought him into conflict with such leading neurologists as Charcot and Ferrier. Nevertheless, like a prophet scorned but true to his vision, he continued to argue his case well into his old age (see, e.g., Brown-Séquard 1887, 1890).

3.4. Reason versus madness

The post-Broca years also saw a rise of the idea that the right side of the brain was a natural breeding ground for madness. If madness is a loss of all civilized standards of reason and morality, a reversion to prehuman, brutish behavior (as late-19th-century alienists were inclined to believe); and if only the left half of our brain is properly civilized, then it becomes possible to envision the "brute brain within the man's" (the phrase is Maudsley's) as lying on the right side of the skull. In 1879, Jules Bernard Luys became the first to argue implicitly along those lines with his declaration that, in the insane, the natural disparity in weight between the hemispheres is increased to pathological proportions – *and* "completely reversed." Instead of the left lobe slightly outweighing the right as (he believed) in the sane, "nutrition" is guided in the opposite direction so as to favor the right hemisphere. (Luys 1879:554).

Two years later, Luys (1881a) published the results of an examination he had carried out on 55 brains of persons judged insane at death; the right lobe was said to outweigh the left in 71% of them. Montyel (1884), whose later sample of 89 brains excluded cases of general paralysis (which he felt were an exception to the rule), pushed the figure up to 81%. Corroborating data on 400 brains had been published across the Channel by Crichton-Browne (1878), but was contradicted by the earlier data of Boyd (1861), which were still considered relatively authoritative. The question of a link between the right brain and madness continued to be debated throughout the century (see, e.g., Lyon 1895); indeed, in 1887, Montyel

would go so far as to declare (rather grandly) that, ever since Luys had published his 1881 statistics, it "had not ceased to be the order of the day."

3.5. Male versus female

The belief in the right hemisphere's evolutionary inferiority that had led naturally to its associations with madness was also the fundamental inspiration behind certain arguments linking it to the female mind, and the left hemisphere to the male mind. In 1874, Gaëtan Delaunay, a French "comparative biologist," published what was to become quite an influential medical dissertation entitled *Biologie comparée du côté droit et du côté gauche*. He argued – and his dissertation cites a number of authors who apparently concurred – that the differences between the two sides of the brain were analogous to, and *responsible for*, the differences presumed to exist between male and female brains (not to mention the brains of nonwhites and small children). That Delaunay was no mere aberration but spoke for a recurring dimension of 19th-century thought on the problem of hemisphere differences is suggested by the fact that, as late as 1898, a French physician could still write: "The terms 'male hemisphere' and 'female hemisphere' should render rather well the differences in the nature of the two brains, of which one, more intellectual, is more stable, and of which the other, more exciteable, is also more rapidly exhausted" (Klippel 1898:56–7). It is interesting that, once one has given the two hemispheres sexual identities, the idea of cerebral dominance becomes a rather apt metaphor for the social and economic domination of men over women in 19th-century Europe.

3.6. Biological superiority versus biological inferiority

In the early 1880s, Delaunay was busy at the Salpêtrière carrying out a series of studies (all published in the reputable *Lancette Française*) designed to show that the right hemisphere did indeed predominate in inferior individuals, and the left in superior ones. He claimed to have found, for example, that "evolutionarily advanced" individuals (men, whites, the educated classes, and generally all people of French origin) tended to direct themselves toward the right in walking, owing to their highly developed left frontal lobes. Individuals at an "inferior" level of evolution, on the other hand (women, nonwhites, children, the lower classes, aged persons, and sometimes – in these politically sensitive years after the Franco-Prussian War – Germans), tended to direct themselves toward the left. Similarly, superior individuals tended to rotate toward the right ("In France, in all our national dances, we turn to the right"), and to cross their right legs over their left, thus sitting predominantly on their left buttocks. Inferior individuals, in contrast, tended to rotate toward the left, and usually crossed their left legs over their right (Delaunay 1879, 1883, 1884).

One of Delaunay's most interesting and best-known studies along these lines involved an attempt to relate the alleged different evolutionary levels of the two hemispheres to alleged differences between dreams produced on the left side of the brain and dreams produced on the right side. Unilateral dreaming was solicited by having the subject sleep on his left or right side, so as to alter blood flow to one or the other side of the brain. Delaunay claimed that the right hemisphere's dreams were generally illogical, richly sensorial (owing, presumably, to the right brain's more developed posterior area), often concerned with distant memories, and apt to be nightmarish. Left-brain dreams, on the other hand, were considerably less absurd and more intelligent, were concerned with recent events rather than reminiscences, and often contained a considerable amount of conversation – not surprisingly, because (of course) the faculty of articulate language was on the left (Delaunay 1882).

3.7. Waking life versus sleep and dreaming

Late in the century, Marie de Manaceïne, a (woman!) psychologist from St. Petersburg, performed some experiments that she took to mean that the *left* hemisphere was primarily concerned with waking existence, while the *right* was especially involved in sleep and dreaming (what this meant for Delaunay's left- versus right-brain dreams is not clear). In a series of experiments performed on 52 subjects, Manaceïne found (1897:140–41; also 1894) that tickling a sleeper's face on the right side of the median line always caused him to brush at himself with his *left* hand, even when he was lying on his left side and the action of that hand was impeded. Significantly, left-handed people (of which she had a small sample) always brushed at their faces with the *right* hand, even when the left was lying free. "These facts," Manaceïne believed, "may be explained on the hypothesis that the most active cerebral hemisphere is resting during the hours of deep sleep." This same conviction that the left side of the brain is the side of waking life, with the right being more involved with the subconscious mental processes of sleep, would ultimately lead Frederick Myers to conclude that the "subliminal self" was probably primarily focused in the nondominant hemisphere (Myers 1885; see also Section 4).

3.8. Left versus right hysteria

Pierre Janet, educated in the doctrines of the Charcot school (though keenly aware of its failings), was familiar with the statistics indicating a prevalence of left over right hysteria and saw no reason to question the truth of this physiological "rule." At the same time, he did not ignore the fact that at least $\frac{1}{3}$ of all cases of unilateral hysteria were *right*-sided, and in his 1898 *Névroses et idées fixes*, he even stated his belief that hysterical mutism and aphasia tended to occur in conjunction with right-sided anaesthesias and paralyses, but rarely with left. It seemed to him that this association (which, he noted, others had also remarked upon) had relevance for any attempt to determine the relationship between hysterical ailments and organic ones. In 1899, then, he undertook a systematic study of the case records of 388 hysterical patients in an effort to see whether there was any consistent relationship between the side of the body afflicted with permanent anaesthesias and paralyses and the other sorts of ailments suffered by the patients.

His statistics revealed a few interesting trends. As he had predicted, there was a significant tendency for

language-related disorders to occur in conjunction with right-sided hysteria. Somnambulism, fugue, and attacks of pathological sleep were found to be significantly more frequent with left hysteria (lending credence to the right hemisphere's image as inward-looking and hypersensitive); difficulties with mobility occurred most frequently in conjunction with right hysteria (in conformity with the idea that the left hemisphere was more intensely concerned with motor functioning than the right). The statistics offered no support to the widespread view that the left hemisphere was more or less exclusively involved with intellectual functioning (a fact Janet himself pointed out). Nor did "nutritional" disorders appear to be differentially lateralized, notwithstanding the views of Brown-Séquard. In the end, Janet himself was most personally struck by the finding that respiratory disorders were 10 times more frequently associated with right hysteria than with left. He ventured to suggest that there might exist a certain rapport between the more voluntary levels of respiration and articulate speech, making for a more pronounced cortical representation of the former on the left side of the brain (Janet 1899 [the name F. Raymond – Janet's supervisor – appears as coauthor of this study, but such use of joint signatures was only a formality; see Ellenberger 1970:341]).

4. Revival of interest in "duality of mind" (post-Broca period)

The historian's attempt to make sense of the late-19th-century effort to compare and characterize the two hemispheres of the brain is complicated by the fact that this same period saw a dramatic revival of interest in the argument that the two hemispheres might function independently. The fact of asymmetry, and the perceived structural and functional differences between the two brains – particularly the left-sided localization of language – were widely seized upon as evidence in favor of the duality of mind and brain. As Brown-Séquard (1874a:5) put it in 1874, "the very fact that the loss of speech depends on a disease of the left side of the brain . . . is extremely important in showing that the two sides of the brain may act independently of each other."

The new wave of interest in duality of mind actually began just before the discovery of asymmetry, with the publication in 1860 of Gustav Theodor Fechner's (1801–1887) *Elemente der Psychophysik*. The Swiss naturalist, Charles Bonnet (1720–1793), had demonstrated that, when segmented worms were cut into two or more sections along the line of segmentation, each piece continued to maintain an independent existence, with the full range of behavioral responses appropriate to its species. This proved, Fechner felt, that unity of mind was dependent upon the anatomical integrity of the nervous system. If one could split a *person* down the center, and maintain the psychophysical activity of both halves, one would in the same way see the "doubling of a human being." Each of our hemispheres, then, was capable of supporting a mind on its own; nevertheless, it was easier when the burden was shared between two hemispheres equally. "The two brain hemispheres can be compared to two horses pulling one wagon . . . If one [horse] is released, the wagon proceeds with more difficulty, or if it still goes at the same rate because the one horse pulls more strongly, it endures a shorter distance" Fechner 1860:536).

Later, the German pessimistic philosopher, Eduard von Hartmann (1842–1906), would betray the influence of Fechner with his argument (1869:118) that, "because *separate material parts give separate consciousness*," the human brain – with its two distinct halves – produced two separate streams of conscious mental activity. Our sense of unified identity was illusory, he argued, and simply due to the fact that the commissures connecting our two hemispheres mingled the two streams of consciousness into one. If one could join the *brains* of two men by a similar "bridge of conduction," they too "would no longer know themselves as two Egos, but only as one Ego, as my two cerebral hemispheres also only know themselves as one Ego."

When one moves from philosophy to psychiatry, speculation comes thick and fast and is too vast to permit individual analysis. Taken as a group, the psychiatric duality-of-mind literature is best understood as a part of a much larger reaction by a self-consciously naturalistic, materialistic medical community to what Ellenberger (1970) has called "the discovery of the unconscious." These were years that saw a fascination with a whole range of phenomena that, in one fashion or another, were seeming to point to the presence of "two minds" operating within a single individual. The rise of spiritualism – with its "spirits" speaking through entranced mediums – had led the way. Then, new hypnosis studies had begun to demonstrate how it was possible to create new (and often more intelligent or interesting) personalities in the trance state of which the waking self retained no memory. By 1880, divided or double personality had become one of the most widely discussed disorders of the 19th century; two of the most famous instances – Mary Reynolds from the United States and Dr. Azam's Fèlida from France – would be copied from book to book and circulated for decades. Meanwhile, a view of man as a chronically divided soul was finding wide currency in the literature of the time (Stevenson's *Dr. Jekyll and Mr. Hyde* being only the most famous example).

Medical men living in this environment began to see two minds struggling against each other in all sorts of mental pathologies which an earlier age might have seen quite differently: cases, for example, in which patients suffered violent and motiveless swings of mood; in which voices carried on a dialogue with a patient, or echoed all his thoughts back at him; in which the patient was haunted by some hallucinated "other person"; in which an extravagant delusion was maintained on one subject, while the patient continued to exhibit perfectly sound reason and judgment on all other subjects; in which the patient was actually aware that he was suffering from a delusion or a pathological impulse and struggled against his affliction (partial insanity, or *la folie lucide*). The double brain was an appealing way for medical men to recast all these supposed examples of mental duplicity into materialistic (and thus, by the logic of time, scientific) terms. For some, the metaphor was so compelling, the isomorphic mapping of "selves" onto brain halves so neat, that the relative paucity of hard physiological evidence in

individual cases often failed to deter them (for representative works, see Huppert 1869, 1872; Luys 1879 [repub. 1888]; Descourtis 1882; Ball 1884; Bérillon 1884; Maudsley 1889; Ireland 1891).

4.1. Two brains/two opposing personalities?

As has been mentioned, Broca's asymmetrical localization of language was seen as important new evidence for Wigan's old argument that the brain's two hemispheres may function as independent organs of thought. At the same time, Broca's work should have suggested that an important revision of the old argument was in order. So long as the two sides of the brain were believed to function identically, physicians were free in accounting for cases, say, of double personality indifferently to delegate a "self" to each hemisphere. Now, however, it seems that if one still wished to claim that morbid independent action of a man's two hemispheres could turn him into some sort of Dr. Jekyll and Mr. Hyde, then one would have to argue further that Jekyll would tend to express himself from the educated, civilized, left side of the brain, and Hyde from the uneducated, passionate, right side. Luys came close to taking this position in his proposal for "reversed dominance" in the insane, but it is not clear whether he ever firmly integrated his views on right-brain madness with his views on brain duality and duality of mind. Can one, then, find evidence for the existence of a truly "post-Broca era" view of brain duality and double personality?

In 1885, Frederick Myers, one of the founders of the British Society for Psychical Research, published what William James (1890, I:400) called a "highly important" article on automatic writing, in which he argued (1885:43) that the secondary or "subliminal" self (which he believed to exist in each of us) tends to "appropriate the energies" of the right side of the brain for its activities, while the left hemisphere tends to be "more immediately at the disposal of the waking mind." Myers pointed out (pp. 78–79, 57) that the writing produced automatically by subjects in a trance state or using a planchette (the forerunner of today's ouija board) was often filled with errors similar to those produced by aphasic, right-hemiplegic patients presumed to be relying on their right hemispheres to write. In both cases, words were often distorted, abbreviated, and produced in mirror image or backward. The fact – as the embarrassed Spiritualists were forced to admit – that "Planchette . . . is sadly given to *swear*" was seen by Myers (pp. 44–45) as additional evidence for special right-lobe involvement in that activity; and he turned here to Hughlings Jackson's demonstration that aphasic patients often retain an ability to curse as a means of "automatic" emotional release, an action which Jackson believed to be mediated by the right hemisphere.

In the extremely complex case of the multiple personality Louis V., about whom the French alienists Bourru and Burot wrote (1888), it was found that the patient's hysterical hemiparalysis and hemianaesthesia could be "transferred" (a procedure which will be discussed further on) from the left to the right side of the body and vice versa. Each transfer was accompanied by dramatic changes of personality, memory, and education. When afflicted on his left side (which was presumed to inhibit the normal action of his right hemisphere), the patient was gentle, almost timid, and his language was correct and polite. Turned into a right-sided hemiplegic (thus inhibiting his left hemisphere), he became violent, arrogant, obscene, and spoke with difficulty "like a child who is learning to speak," although he was able to read. Bourru and Burot (1888:127) concluded that "Louis V. directed by the right hemisphere is a different individual than the Louis V. who corresponds to the left hemisphere. The right-sided paralysis only allows the violent and brutal aspects of his character to appear; the left-sided paralysis transforms him into a peaceful boy." They were thus inclined to accept the conclusions that Frederick Myers had drawn from their case; namely, that "the personality of man is double like his brain; the personality of the left hemisphere is the good one, whereas that of the *right hemisphere is the evil, the brutal and the savage*" (Bourru & Burot 1886–87:265; see also A. Myers 1885–86; F. Myers, 1886).

Toward the end of the century, the Scottish physician, Lewis Bruce (1895), would revive lagging interest in the argument for a link between double personality and morbid independent hemisphere action with his "Welsh case." He had under his observation a patient who alternated between periods in which he spoke English and was right-handed, and periods in which he spoke a rather incoherent form of Welsh and only used his left hand. In the English stage, when "presumably using the left cerebral hemisphere," he was the "subject of chronic mania," restless and destructive, yet exhibiting "a fair amount of intelligence." During the Welsh stage, "presumably using the right cerebral hemisphere," he became the "subject of dementia" or advanced "melancholia," was shy and suspicious, sat doubled up in a chair for hours, and spoke in an almost unintelligible manner. "I have got Welsh patients to act as interpreters; they tell me that they cannot understand much of what he says, but what they do understand is spoken in the Welsh language." Bruce concluded from these observations that "in this case the cerebral hemispheres are capable of individual mental action, and that the mentally active cerebrum has a preponderating influence over the control of the motor functions, the patient living two separate existences during the two stages through which he passes; the mental impressions received during each of these separate existences being recorded in one cerebral hemisphere only." (See also Bruce 1897.)

4.2. Objections to later double-brain theories of double personality

It must be admitted, however, that the above arguments for a "two brains/two minds" theory of double personality are somewhat exceptional in the way they take into account the post-Broca data on differences between the two hemispheres. A great many late-19th-century authors paid surprisingly little attention to the complicating factor of functional asymmetry when implicating the double brain in double personality. Curiously, the fact that both of their "selves" could generally speak without difficulty does not appear to have troubled many people.

For contemporaries, an important objection to the double-brain theory of double personality arose out of an increasing awareness that one is "not bound to the number two in considering the mass of conscious,

subconscious, and unconscious states that may succeed one another in our body" (Rosse 1892:187). While it was admitted that the double brain might be able to account for some morbid phenomena, it could not stand as an all-inclusive theory – in cases of *triple* personality, it was asked, where is one supposed to locate the third self? (cf. Ribot 1891:110–11)

The brain-duality hypothesis was also criticized for the crudeness with which it was often applied. Ribot (1891:109–13) disdainfully spoke of some authors who believed that "cerebral dualism suffices to explain every discrepancy in the mind, from simple hesitation between two resolves, to the complete duplication of personality." Though ultimately endorsing a limited theory of hemisphere-independent action in pathology, he pointed out that "contradictions in the ego . . . are not oppositions in space (from one hemisphere to the other), but oppositions *in time*. They are – to use a favorite expression of Lewes – successive 'attitudes of the ego.'" In Germany, Wilhelm Griesinger had not been "inclined to attribute a high value" to the double-brain hypothesis (cited in Ribot 1891:109), and in England Charlton Bastian (1880:49) also spoke of the "very doubtful nature" of much of the evidence offered. Even Bourru and Burot (1888) recognized the limitations of the brain-duality hypothesis in explaining cases of alternation like that of Fèlida, and the artificial provoking of new personalities in hypnosis.

Finally, the double-brain hypothesis of double personality was undermined for some because of uncertainty regarding the functions of the corpus callosum, the great median tract of fibers joining the two sides of the brain. Most advocates of "duality of mind" assumed that the healthy person experiences a sense of unified identity in spite of the functional independence of his two hemispheres because the corpus callosum serves as a bridge of conduction between the two psychic realms. Meynert and Broadbent both believed (on rather shaky anatomical grounds) that similar convolutions in the two hemispheres were united by these fibers, in this way uniting sensation and thought (Bastian 1880:483–85). Hamilton (1884–85) had also argued for the probable psychic functions of the corpus callosum in 1885, although he had challenged the Meynert/Broadbent schema.

Nevertheless, there were a disturbing number of cases on record in which the corpus callosum had been "found to be entirely wanting, without any mental derangement or deficiency of intellect . . . and without any manifestation of a double personality" (Ireland 1886:318; see also Knox 1875; A. Bruce 1889). Descourtis (1890) tried to explain the apparent absence of mental "doubling" in cases involving loss of the corpus callosum by appealing to the fact that the brain's two hemispheres – with their almost identical life experiences and hereditary dispositions – would naturally tend to feel and react as a single personality, even though functionally independent; in this respect, they resembled the Siamese twin brothers (this was a common analogy in late-19th-century discussions of the double brain). For William McDougall (1911:117, 295–96) early in the 20th century, the fact that absence of the corpus callosum failed to result in any observable double consciousness seemed cause to rejoice. To his mind, the materialistic belief that the unity of personal consciousness is strictly dependent upon the unity of the material connections of the brain was thereby refuted – thus chalking up a clear victory for "animism," or the belief that mind (or soul) is an entity distinct from the body.

Liepmann and Maas's work suggesting that normal communication between the two hemispheres could be interrupted by damage to the corpus callosum had been published in 1908 but may not have been known to McDougall. These German clinicians reported on a right-handed patient with a callosal lesion who was unable to follow verbal instructions to write using the left hand. Nonetheless, their finding had relatively minimal impact at the time, and it by no means put an end to the controversy surrounding the corpus callosum and its functions. Even if it had been widely accepted, it is not clear that it posed any grave threat to the Cartesian soul McDougall was so concerned to save. There is no evidence that Liepmann's patient experienced himself as "two selves," or suffered any break in his subjective sense of unified identity – any more than do the split-brain patients of today.

4.3. The "experimental" evidence

4.3.1. Phase one: metalloscopy. Although, as said before, physiological evidence for "duality of mind" was often wanting in individual cases, there did exist a body of literature that purported to offer solid experimental evidence (and quite dramatic evidence at that) demonstrating in a general way that the brain's two hemispheres can function independently. This literature represents one very important reason why duality-of-mind theories did as well as they did in the late-19th-century (particularly in France).

The story behind this development began in 1876 when an elderly doctor, Victor Burq, wrote to Claude Bernard in his capacity as president of the prestigious Société de Biologie of Paris. Burq explained that for the past 35 years he had been successfully curing women suffering from hysterical hemianaesthesia by applying metallic discs to the afflicted side of their bodies. He asked Bernard whether the Société would be willing to investigate and establish the validity of this phenomenon, which he called metallotherapy (later the term "metalloscopy" would be used to denote the production of phenomena using metals, but without therapeutic intent).

Bernard, with commendable open-mindedness, agreed; he appointed a commission to study Burq's claims, which consisted of Charcot, Jules Bernard Luys, and Amédee Dumontpallier. The commission's report a year later (Dumontpallier, Charcot & Luys 1877) was enthusiastic: Not only had the genuineness of the metallic effects been confirmed (there was more doubt about the long-term therapeutic benefits), but a new finding had been made in the course of investigation. It seemed that when sensation was restored to a region on one half of the body using metals (and soon electric currents and magnets), symmetrical regions on the healthy side lost normal sensibility. This implied, according to French theory, a dynamic transference of the responsible "functional" lesion from one hemisphere of the brain to the other. It was Dumontpallier who named this phenomenon (with a banking analogy in mind) the "law of transfer."

Variations on the original discovery followed rapidly. It was reported that one could bring about a series of

"consecutive oscillations" in which sensibility would transfer repeatedly from one to the other side of the body (Richer 1881:531, 539–40). One of Binet and Féré's (1885:20) patients subjected to these oscillation experiments complained of a pain first in the right, then the left side of her head, and she declared: "It's as if there were doors banging between the two sides of my brain." Matters were further complicated when doctors began to declare themselves able to transfer not only hemianaesthesia and (by this time) hemiparalysis, but voluntary behavior and intellectual activity as well. This was called psychic transfer (Binet & Féré 1885; 1887:16–17). Binet and Féré (1887:297–98) reported, for example, how one subject was made to write down figures in an ordinary way with her right hand while a magnet was concealed near her left elbow. She was able to write only as far as the number 12 with her right hand before she felt compelled to stop and switch the pen to her left hand. The figures she then set down were written in mirror image, and she had become agraphic with her right hand.

The experimenters themselves tried to take a strictly biophysical view of all these events, and they stressed that there was nothing mysterious involved. Burq's hypothesis, subsequently adopted in one form or another by most of the others, was that the metals or magnets acted somewhat like a solenoid, producing a slight electrical current when placed in contact with the skin, which then acted on the nervous system (see Adler 1880–81). Brown-Séquard (1887) would later show that the "transfer" of hysterical symptoms had an apparent analogue in certain experiments he had performed involving "transfer" of *organically* based disorders from one to the other side of the body. In spite of the effort to keep explanation at a no-nonsense, physiological level, however, there was the inevitable minority opinion that attempted to account for transfer by a more mystical theory of magnetic polarity in the human body, reminiscent of that proposed by Mesmer almost a century before (see, e.g., Chazarain & Dècle 1887–88). Jules Bernard Luys would ultimately advocate an extreme version of this theory, extended to include a belief in neo-Mesmeric "emanations" that could pass between individuals. (Joseph Babinski [1886] also apparently believed in an emanations theory for a time and performed some startling experiments to demonstrate it. He later recanted, however.)

Beginning about 1885, Charcot's rival at Nancy, Hippolyte Bernheim, began a campaign to undermine the metalloscopy research, arguing (1885) that Charcot and his workers had unwittingly trained their subjects to transfer symptoms and produce other phenomena and had led them to expect that such and such an effect would occur. At Nancy, where his patients knew nothing of metalloscopy, he had been unable to produce transfer. All the metalloscopy findings, he concluded, could be accounted for in terms of "suggestion." Bernheim's concept of suggestion has often been criticized as a rather facile explanation, because it is an unanalyzed term and does not explain *how* "expectation" or unconscious "training" could have brought about the wide range of physiological alterations and cures to which the Paris workers were witness. Nevertheless, as Janet (1925:II, 797) put it, "the effect of [Bernheim's] . . . criticisms was that of a thunderbolt," leading to dissent within Charcot's ranks, and a widespread feeling among outside observers that the whole experimental enterprise had somehow been a cheat. Janet believed that "the metallotherapists could and should have defended themselves. They ought to have asked for a more precise definition of the term suggestion; and when an agreement had been reached, . . . they should have been ready to abandon the phenomena which obviously belong to the field of suggestion thus defined, while going on to enquire whether there was a residual group of phenomena which . . . were still worthy of examination. But before they could have done this, they would have had to enter the domain of psychological science, which seemed to them difficult and repugnant. They preferred to throw up the sponge."

4.3.2. *Phase two: Hypnotizing the double-brain.* Before the decision was made to "throw up the sponge," however, Burq's metallotherapy had managed to play a critical role in arousing Charcot's interest in hypnosis, and it had contributed importantly to the development of the Salpêtrière view of hypnosis as a physiological phenomenon, an artificially induced state of hysteria consisting of three distinct stages – catalepsy, lethargy, and somnambulism (Charcot 1882). Most important, for the purposes of this study, one also finds that the interest in the possibility of functional hemispheric independence aroused by the phenomenon of transfer was now carried over into this new hypnosis research.

Charcot and Richer (1878) recorded how Gabriel Descourtis, then an *élève du service* at the Salpêtrière, was responsible for the original idea of trying to hypnotize the brain's two hemispheres separately. This turned out to be a rather simple procedure. A patient could be plunged into the first stage of hypnosis – catalepsy – by the normal physical means (a bright light, a sudden noise, etc.). As it was held that shutting the eyes stimulated the second phase of the trance – lethargy – one here varied the procedure and only shut *one* eye, in this way presumably affecting only the hemisphere on the contralateral side (though of course the anatomical reasoning here was faulty). The experimenters declared that the patient immediately became lethargic, but only on the side of the body that corresponded to the shut eye. The patient remained in a state of catalapsy on the other side and was thus hemicataleptic and hemilethargic at the same time (p. 970).

In Germany, Rudolf Heidenhain (1880), Grutzner, and O. Berger (1880) had also – and in apparent ignorance of the French work – begun to experiment with a form of hemihypnosis (Hall 1881), but no one else came close to matching the virtuosity of the French in this endeavor. Richer (1881:371) reported how hemilethargy and hemicatalepsy were induced in a patient and then transferred from one side of the body to the other. One could also induce hypnosis in a patient, it was said, breathe on one half of his forehead while sheltering the other half with a screen, and cause only half his brain to awaken; Luys (1890:141) remarked on the "cold-blooded" manner with which patients then noticed that they were "cut in two." It was even claimed that one could command a hypnotized patient to read aloud or write, and then induce a sudden onset of aphasia by altering the trance state of the left hemisphere. Because similar right-lobe manipulations were said not to disturb speech, this last was offered

as new evidence – *in vivo* – of Broca's left-sided localization of language (Ballet 1880) and attracted a considerable amount of attention in France and abroad (for a critical appraisal, see Bateman 1890a:335–42).

According to Dessoir (1887:544), however, it was Dumontpallier who "was the first to show in a decisive manner that the duality of the cerebral system was proved by these hypnotic phenomena." Dumontpallier's experiments (1882, [& Magnan] 1883, [& Bérillon] 1884), in which he induced independent and different hallucinations simultaneously in his patients' two hemispheres, caused a considerable stir in the scientific community, attracting the admiring notice of such illustrious French scientists as Pasteur, Chevreul, Milne-Edwards, Brown-Séquard, Henry Bouley, and Paul Bert (Bérillon 1899:11). In one study, for example, a subject's face was said to express horror on one side and contentment on the other as one hemisphere hallucinated an attack by dogs, and the other enjoyed a country fête (Dumontpallier & Magnan 1883). Paul Bert, recently elected president of the Société de Biologie after Bernard's death, declared before a session of the Société in June 1884 that Dumontpallier's demonstration that hypnosis can divide a man in two and create a double individual was the only true innovation to have emerged from the new studies of hypnosis, the only fact the old magnetizers had never suspected (Bérillon 1899:13, 1884:185–86).

Jules Bernard Luys (1890b:126–27, 129–30) at the Charité hospital believed himself to have produced not only a doubling of the intellectual faculties as Dumontpallier had, but a doubling and polarization of emotional experience as well. Using magnets, he said, joyful feelings could be made to migrate to one side of the brain and feelings of despair to the other, while in the exact middle there would appear a line of experimentally induced indifference. He also managed to elicit contrary emotions in his subjects by pressing certain substances sealed in glass tubes to first the left, then the right side of their neck. This work, it must be said, was considered suspect and eccentric, even by contemporaries, and in the end it helped rob him of a good part of a scientific reputation it had taken him some 40 years to acquire.

All the double-brain experiments, however – even the less extravagant ones of Charcot, Dumontpallier, Bérillon, and others – ultimately fell into disrepute, one of the victims of the wide-ranging Paris/Nancy dispute over the nature of hypnosis and hysteria, from which the latter was to emerge more or less triumphant. Doubt was also thrown upon the studies for anatomical reasons. Because the French hemihypnosis relied upon a method of presumed cerebral manipulation via the retina, the assumption had to be that all the fibers of one eye's optic tract pass from the retina to the opposite hemisphere. Physiology in the 1880s was still not clear on this point. Although it was correctly believed by most that the fibers cross partially at the chiasma and that was all (as Wallaston had argued long before), Charcot had suggested (1875) that there is an additional crossing of the fibers in the corpora quadrigemina that makes in the end for full decussation. This view was criticized by Ferrier, Michael Foster, and others in England. Some German and French physiologists, on the other hand (including Brown-Séquard), took a third view and held that actually complete decussation of the human optic nerve takes place at the chiasma, in the same manner as had been shown for a variety of other, lower species (Bateman 1890a:339–40, 1890b:292–94; Olmsted 1946:147–49). By the time the issue was settled in favor of Wallaston and the English school, however, the double-brain hypnosis vogue had already more or less died a natural death.

4.4. Miscellaneous applications of the brain-duality theory

While the argument for hemisphere functional independence was used most extensively in theories of mental pathology, especially double personality and partial insanity, one also finds it being invoked to account for a variety of normal mental phenomena. In particular, it was believed that some brain-duality hypothesis was required to explain certain aspects of dreaming – those cases, for example, where a dreamer is carrying on a long conversation with another person and two distinct trains of thought are being evolved (Bastian 1880:492); or where a dreamer finds himself baffled by twists of plot in what is nevertheless a drama of his own creation (Greenwood 1892); or where a dreamer "puts and solves his own riddle" (Maudsley 1895:220).

Maudsley (1889:187) felt that paramnesia – the "singular feeling which almost everybody has had . . . of having been before in exactly the same circumstances and having had exactly the same experience" – might be "due to the instantaneously successive consciousness of the separately acting hemispheres," a view which had also been advocated by Jensen (1868) and Huppert (1869, 1872) in Germany and had been long before proposed by Wigan. In the United States, a physician from Georgia, R. C. Word (1888), appealed to the double brain to account for "certain mysterious phenomena . . . which have hitherto baffled every effort at explanation": mind-reading and slate-writing. He felt that "under certain peculiar circumstances one side of the brain may converse with the other side." In England, the philosopher Robert Verity argued (1870:44) that the duality of consciousness into subject and object, thinker and thing thought of, corresponded exactly with the duality of the human brain, "each duplicate hemisphere being carefully separated from the other . . . and yet intercommunicating with the other through central organs of unity."

Finally, one finds the argument for hemisphere functional independence being pressed into service as the basis of a movement early in the 20th century to cultivate universal ambidexterity. John Jackson, a grammar school educator from Belfast, was one of the founders of the British Ambidextral Culture Society in 1903, whose aims he glowingly described in his book, *Ambidexterity or Two-Handedness and Two-Brainedness: An Argument for Natural Development and Rational Education* (1905). Although admitting some innate basis for man's "dextral pre-eminence," Jackson attacked the hard-line nativist view of functional asymmetry and cited Wigan's claim that each hemisphere may serve as a "distinct and perfect" organ of thought. Given proper training, then, it was possible to foresee a brave new world in which "each hand shall be absolutely independent of the other in the production of ANY KIND OF WORK whatever; . . . if required, one hand shall be writing an original letter, and

the other shall be playing the piano . . . with no diminution in the power of concentration" (p. 225).

In the United States, Jackson and his followers were violently attacked by Gould (1907), who called them "cranks" and "sillies" and accused them of endangering the health of schoolchildren everywhere. In England, Sir Crichton-Browne (whom Jackson dubbed "the English Goliath of lopsidedness") put the ambidexters in the same class as those who "are addicted to vegetarianism, hatlessness, or anti-vaccination and other forms of aberrant belief," and he hinted darkly that they were all destined for "the already over-crowded lunatic asylums" (cited in Jackson 1909:8). The shrillness of these attacks is quite striking, and it says something about the extent to which the post-Broca interpretation of asymmetry as a sign of superiority and health had gotten under neurology's skin.

5. The Hughlings Jackson perspective

John Hughlings Jackson (1835–1911) is one of the best-known names in this study (along with perhaps Broca, Wernicke, and Charcot) and is the only one whose reputation today may be even greater than it was in his own time. He is also the maverick of this study; his views defy attempts at assimilation that would not risk misleading or confusing the reader. This also allows a considerable amount of space to be devoted to Jackson's thought on hemisphere differences and brain duality. He had much to say on these issues and – in spite of the frequent references to his work in the modern laterality literature – much of what he had to say has been misunderstood or only partially appreciated. There is a particular need today to go beyond both panegyrics and secondhand reports and begin to take a fresh look at what Jackson's views on the double brain actually were.

5.1. Dissent from the French "faculty" school

In the 1860s, Jackson was one of the very few important aphasiologists to begin to have serious doubts about the reality of Broca's speech faculty. Historians (e.g., Greenblatt 1970, 1977; Engelhardt 1975; Young 1970) have pointed out that the "faculty" approach to cerebral localization was essentially foreign to Jackson, who had been reared in the British philosophical tradition of associationism. As a student at York, he had been strongly impressed by the teachings of Thomas Laycock (1812–1876), who had pioneered the idea that one could account for all nervous functioning, including that of the brain, in terms of a reflex principle operating according to associationist laws. Jackson had also imbibed the writings of such associationist psychologists as Alexander Bain, George Henry Lewes, and especially Herbert Spencer. We may conclude that he ultimately found it impossible to accept the essentially dynamic view of mental processes implicit in associationist theory, and at the same time to go along with an approach that broke up aspects of mind and spatially localized them as static entities.

Jackson's dissent from the French faculty approach to language, however, can be even more decisively linked to his growing puzzlement over the "'strangeness of this association' of 'the loss of a purely mental faculty' [i.e., speech] with a decidedly physical deficit such as hemiplegia" (Greenblatt 1977:424). It soon became clear to him that he was dealing with an issue that struck straight at the heart of the mind–body problem. How could one localize a *mental* entity within a *physical* space? The answer was, quite simply, that one could not. Some 70 years earlier, the philosopher Immanuel Kant had denounced the doctrine of the seat of the soul, pointing out (in Riese and Hoff's summary, 1950:59, 61) that the soul "perceives itself only by the internal sense; thus it cannot assign to itself any place in the body." *Motion and sensation alone,* Kant went on to say, admitted "of a spatial relationship with the cerebral organs."

This was essentially Jackson's conclusion as well. The only logically possible field of inquiry for physiology was that of sensorimotor relations. There could be no "physiology of mind," Jackson declared, any more than there could be a "psychology of the nervous system." Thus, the explanation for the "strange association" of the loss of a "mental faculty" like speech with a "physical" motor defect like hemiplegia lay in realizing that – so far as the physiologist was concerned – hemiplegia and aphasia were both motor disorders, to be understood within a common framework. Jackson thus took the crucial step of rejecting the traditional view of the cortex as the *terra incognita* of the mind. As Laycock had done before him, but more speculatively, he proposed instead to extend the principle of sensorimotor reflex action right up into the highest cortical centers. This resulted in his developing a distinctive perspective on the problem of the double brain, which will be considered in the sections that follow.

5.2. Unilateral disorders and "sparing"

In an 1878–79 paper on affections of speech, one finds Jackson paying tribute to the work of a younger English neurologist, William Broadbent, and acknowledging to him a "particular indebtedness." "Broadbent's hypothesis – a verified hypothesis – is, I think, essential to the methodological investigation of affections of speech" (p. 155).

Few today are familiar with the "essential" hypothesis to which Jackson is referring here, but its effect on Jackson's thought was profound. In 1866, Broadbent had proposed an explanation for the rather strange fact that in hemiplegia – regardless of the extent of unilateral brain damage – there are always some muscles that are more severely paralyzed than others. Limbs (especially the arms and hands) generally suffer the most, whereas the muscles of the trunk and upper face escape with little or no weakness. Jackson himself in 1863 had made the interesting observation that the muscles that tend to be most severely afflicted in hemiplegia are also those "most under the control of the will" (cited in Greenblatt 1977:417), though he had been unable to offer a theory as to why that might be.

Broadbent, however, had noticed something else. He pointed out in his 1866 article that the parts of the body that suffer most in hemiplegia are those that are moved "independently of the corresponding parts of the opposite side" – that is, they tend to be muscles that are *unilaterally* innervated by the contralateral side of the brain. The parts of the body that are spared in hemiplegia, on the other hand, are those that are generally only capable of being moved "in concert with the corre-

sponding muscles of the opposite side" – that is, they are parts of the body that are innervated only *bilaterally*, or by both sides of the brain together. The reason, according to Broadbent (1866), that these latter muscles escape damage in hemiplegia is that, though they are indeed normally innervated by the two sides of the brain in tandem, they are "*capable of being excited by either singly*" (italics orig.). Thus, when an individual suffers unilateral brain damage, the muscles of bilateral use on the paralyzed side of the body can continue to function because the undamaged ipsilateral brain half can continue to call them into service.

Before Jackson could make use of Broadbent's hypothesis, it was necessary that he come to the important conclusion that it is not in fact muscles that are paralyzed in cases of brain damage, but movements that are lost (Jackson 1873). Once that step was taken, Jackson could propose that, within the hierarchy of the nervous system, it is the "highest," most "voluntary" movements that are the first to be lost in cases of trauma; functionally "lower," more "automatic" and less specialized movements tend to be preserved longer. (In Jackson's later conception, the terms "voluntary" and "automatic" had nothing to do with some metaphysical "will," but simply referred to the relative levels in the nervous system of physiological complexity, flexibility, and independence.)

This done, the importance of Broadbent's observations for explaining the intriguing relationship between muscles paralyzed in hemiplegia and muscles "under the control of the will" became clear. Broadbent's observations on hemiplegia showed that the hierarchy of the nervous system is complicated on the level of the brain: Movements here tend to be represented *unilaterally* in proportion as they are voluntary; they tend to be represented *bilaterally* in proportion as they are involuntary or automatic (cf. Gowers 1885:51–53). This is why, in a unilateral disorder such as hemiplegia, only the most voluntary movements are lost. Furthermore, because Jackson believed that the entire nervous system must be conceived in sensorimotor terms – that one could not distinguish between "physical" and "mental" functions in the brain – he saw no reason why the insights gleaned from hemiplegia should not be valid for another unilateral motor disorder, aphasia.

5.3. Propositional and emotional language

Clinical observation, Jackson felt, strongly suggested that like the hemiplegic, the aphasic loses his most voluntary (speech) movements but is spared the loss of his more automatic movements. That is to say, he is generally severely deficient in the highly voluntary and specialized capacity to form "propositions" or string together units of speech to express ideas, but he may often manage to burst forth with samples of involuntary "emotional speech" – swearwords, exclamations, and certain types of gestures. Following Broadbent, this implies that automatic or emotional speech is essentially a *bilateral* activity and thus, in the left-brain-damaged aphasic, may be set into motion by the undamaged right side of the brain. In contrast, the highly "special" and "educated" movements of propositional speech are represented only unilaterally in the brain, like the special voluntary movements of the arm and hand; thus, these movements are lost when half the brain is damaged (see Jackson 1868b:358, 1868d:275, 1871:642, 1873:84, 1874a:99; Gowers 1885:126).

Aphasia, of course, differs from hemiplegia in one crucial respect: the latter can result indifferently from damage to either side of the brain, whereas the former almost always results from left-sided damage only. Jackson (1866:661, also 1868a:209) accepted the French anatomical explanation for this curious fact and spoke of Gratiolet's "statement to the effect that the frontal convolutions on the left side are in advance of those on the right in their development. Hence, if this is so, the left side is sooner ready for learning. It is the elder brother." Like Broca, then, Jackson did not believe that language asymmetry was a consequence of innate functional differences between the two hemispheres. Both sides of the brain, he said, lay down representations for *words*, but it is "the left [that] begins to act" first, and therefore it is the left that becomes proficient in the voluntary language movements that constitute *speech*.

The automatic (bilateral or either-sided) functions of language, however, were not restricted by Jackson to emotional outbursts but also included the very important process by which we passively "receive the propositions" of another; by which we understand language. This was, Jackson argued, perhaps the most involuntary language function of all. The process by which words take on meaning is a strictly deterministic one operating according to associative laws. Every healthy English-speaking person, Jackson (1868c) said, is "*compelled*" to understand the meaning of, say, the word "horse" each and every time it is spoken; he is helpless to prevent some image of the animal from springing to mind. Thus, both sides of the brain, having the capacity for the automatic functions "in which words serve," can "understand" language, though only the left can speak.

Nevertheless (and this point is crucial), although both sides of the brain can automatically "understand" speech, Jackson (1876c:174) held that only the left can go one step further and become conscious of this understanding; only the left can ever become "conscious in words." This aspect of Jackson's thought is explained more fully in the section on the duality of mental operations. For the moment, it will suffice to say that the justification for this claim is bound up with Jackson's belief that only the left side of the brain is capable of voluntary speech or "propositioning." One will recall that Jackson's use of the term "voluntary" had nothing to do with true free will, but simply described the physiological functioning of the highest (but still lawfully determined) levels of the nervous system. From a *psychological* perspective, then, the only difference between a voluntary and an involuntary act was that the former was always initially "preconceived" or "*represented in consciousness*" (Jackson, quoting Spencer 1868b:359, italics orig.). By definition, in other words, actions could be deemed voluntary if they were accompanied by consciousness. Conversely, "the more operations are automatic, the less we are conscious of them" (1874b:141). This is why Jackson could argue that the speechless man whose intact right brain could still (automatically) understand speech – and could therefore instantly hand him a brick on command – had no "memory" of the word "brick," was not "conscious of the *word itself*. He has no consciousness of it, but of the thing it is symbol of – a very different thing" (1874b:140–41).

5.4. The functions of the right hemisphere

If the right side of the brain lacks the capacity to be "conscious in words" (since it lacks voluntary speech), this is not to say that it is unconscious altogether. The speechless man has lost his capacity for verbal consciousness, but, Jackson argued, he remains fully conscious of things. For Jackson, this suggested that, alongside the sensorimotor processes underlying speech and verbal thought, there is a second series of "sensorimotor processes concerned in the *recognition* of objects (not in *seeing* objects), and in putting images of things in 'propositional order,' so to speak" (1872:514). Partly because this "other" series of operations seemed unaffected by left-brain damage, Jackson proposed that it must be mediated in its most voluntary aspect by the right side of the brain.

Jackson's direct clinical evidence for his localization of "object recognition" and imagistic thought in the right hemisphere was chiefly drawn from only two cases. In 1872, he described a man suffering from a relatively rare form of left hemiplegia in which the leg was more affected than the arm. The striking thing about the case was that this man seemed to have lost his ability "to recognise places and persons. At one time he did not know his wife, he gave his watch away, and having wandered from home, was unable to find his way back" (p. 513). When Jackson came to describe a second, similar case in 1876, he called this inability to recognize objects, persons, and places "imperception," and he argued that it was a defect "as special as aphasia." It occurred, he believed, as a consequence of insult to the right-posterior lobe, that lobe being the "leading" side for object-recognition, and the left "more automatic." "This is analogous," he wrote, "to the difference I make as regards use of words, the right is the automatic side for words, and left the side for that use of words which is speech" (1876a:148).

Two points must now be made. The first is that Jackson himself (1872:514) admitted that the clinical evidence for his conception of "imperception" was "slender." It was anatomy, rather, that he saw as providing the truly "strong" support for his views. What was the anatomical evidence then? It may surprise some to learn that here Jackson pointed to the claims that had been made in France for asymmetrical but complementary (left-anterior/right-posterior) development in the human brain. The uncertain merits of these claims were discussed earlier.

Why, though, would a more developed occipital area on the right side of the brain (assuming it really exists) have been seen by Jackson as strong evidence for his belief in the special visual-perceptive functions of the right hemisphere? As noted before, the answer has nothing to do with Munk's "visual center" in the occipital lobe, and takes us to our second point: Jackson's theories on the complementary activities of the two hemispheres seem to have been significantly shaped by certain a priori convictions, which partly biased the way he interpreted later data. In the hierarchy of the nervous system, Jackson felt that speech represented the pinnacle – the highest and most voluntary – of all possible *movements*. Visual perception, in a similar way, was seen by him as the highest or most voluntary of all possible *sensations*. As early as 1864, then – before he had any conclusive clinical evidence – he suggested that if it turned out that Expression had its chief seat in the left side of the brain, there was "no absurdity" in supposing – on theoretical and, one is tempted to say, even aesthetic grounds – that its "corresponding opposite," Perception, might turn out to have its chief seat in the right side. Jackson's discovery several years later of the French anatomical argument for asymmetrical development between the two brain halves appeared as a valuable confirmation of the correctness of his original proposal. One is justified in asking, then, to what extent Jackson's views on right-brain functioning might be seen as a somewhat idiosyncratic interpretation of certain (highly inconclusive) anatomical data.

Taking a broad view of what has been summarized so far, one finds that Jackson's conception of left- and right-brain differential functioning implies that, in the course of increasing sensorimotor specialization ("evolution") in the human nervous system, the motor and sensory aspects of voluntary mentation have divided themselves up, so to speak, between the two hemispheres. At the highest cerebral levels, one no longer finds a strictly vertical hierarchy of functions, but the beginnings of evolution along the horizontal axis. The left-anterior (motor) lobe has become specialized for speech and verbal thought (the most voluntary of movements), and the right-posterior (sensory) lobe has become specialized for visual perception and imagistic thought (the most voluntary of sensations).

5.5. The duality of mental operations

The right anterior and left posterior lobes of the brain, then, were held by Jackson to be at a functionally lower level of "evolution" than their "elder brothers." This is not to say, however, that these two corners of the brain were seen as playing no role at all in the sensorimotor activities underlying speech and perception. Jackson (1878–79:167) felt that the "energizing" of these lower, automatic nervous arrangements, "although unattended by any sort of consciousness, is essential for, and leads to" the energizing of those higher, more voluntary nervous arrangements in the left-anterior and right-posterior parts of the brain, which are attended by consciousness.

In short, *mentation is a dual process.* During the first involuntary stage, words and images are revived in various ways according to associative laws and are arranged in "propositional order." The end of this automatic, preparatory phase represents the beginning of thought in its voluntary aspect, as words and objects now made into significant entities or perceived in a meaningful way "well up" – as if from nowhere – into consciousness (see Jackson 1868b, 1868c, 1874a, 1876c:73–76, 1878–79, 1880). "It is, I think," Jackson observed (1878–79:168), "because speech and perception are preceded by an unconscious or subconscious reproduction of words and images, that we seem to have 'faculties' of speech and of perception, as it were, above and independent from the rest of ourselves . . . We seem to ourselves to Perceive, as also to Will and to Remember, without prior stages, because these prior stages are unconscious or subconscious."

It has been seen that, for Jackson, the speechless man

continues to understand language "automatically" using his right hemisphere. It can now be understood that this is because he has retained the first, automatic, and unconscious half of his "verbalising" series, which is bilaterally represented in his brain, and has lost the second (unilaterally represented) voluntary/conscious half that is speech (while the duality of his imagistic series remains intact). In principle, the "imperception" patient should be in the converse situation, though it is not clear that the clinical evidence supported Jackson here.

Sometimes Jackson referred to the first, automatic half of thought as "subject consciousness," and the second, voluntary half as "object consciousness": "The substrata of consciousness is double, as we might infer from the physical duality and separateness of the highest nerve centers [i.e., the cortex]. The more correct expression is that there are two extremes. At one extreme, the substrata serve in Subject-consciousness, at the other extreme in Object-consciousness . . . Subject-consciousness is not commonly spoken of as consciousness; it is sub-consciousness bordering in unconsciousness . . . Object consciousness . . . [is] consciousness commonly so-called" (1876c:174–76).

Although the terms "subject" and "object" consciousness may seem to introduce an unnecessary complication into Jackson's "mental duality" theory, Jackson in fact hoped that they would serve as both a clarification and elaboration of the simple automatic/voluntary dichotomy. The term "subject consciousness" was meant to describe the psychological (subconscious or unconscious) processes that accompany the energizings of that level of the brain we call our "*self*." Physiologically, the *self* is to be understood as the "summation" of all the lower sensorimotor processes in the nervous system, the "re-representation" of all the parts of the body as a whole; it has its anatomical locus in the highest nervous centers possible that do not yet involve consciousness.

When these centers are activated as a result of some impression from the environment (or a lower-level internal stimulus), we as "selves" are naturally altered; physiologically we are not the same. Nevertheless, *we do not know that yet*. Jackson held that we can become conscious of changes in our "selves" (in our highest nerve centers) only indirectly, only in symbolic form. In order to *know* (be conscious of) some thought or perception that has been aroused in us, we must "object-ify" it using words or images arranged in propositional form; and then we must "project" that now-symbolized thought outside our "self" so it can be contemplated by that same "self."

Consciousness was thus envisioned by Jackson as a sort of inner perceiving organ; words and images were the "stuff" that cloaked otherwise unknowable or invisible thoughts and let them be "perceived" (Sigmund Freud would later develop very similar views). Mercier (1901:503), one of Jackson's followers, wrote: "While sensations are objects of contemplation by the subject, yet as they are being received, in the moment of their reception, they are rather modifications of the subject than objects of contemplation by it . . . It is only by a fiction [i.e., by the symbolizing process] that we can bring them out into the field of object-consciousness for examination." As Jackson (1887:96) put it: "We may say that what is commonly called consciousness (object consciousness) is a 'revealing of self.'"

Jackson applied his theory of mental duality or subject/object consciousness to a great many (apparently) widely differing pathologies. Aphasic "recurrent utterances" and "paraphasias" among normal people were explained in terms of faulty processing from the subjective (right-brain) to the objective (left-brain) stage of verbalizing. Delirium was seen as a "dissolution" to what was normally the first, subjective stage of thought, in which words and images were revived involuntarily in the brain following the associative "law of resemblances."

The epileptic "dreamy state" – in which the patient may experience a sense of déjà vu, a vivid dreamlike revival of memories of former surroundings, or a terrifying sense of loss of personal identity – was defined by Jackson as a heightening of subject consciousness accompanied by a diminishing of object consciousness. The tendency for patients to describe their experiences as a feeling of "double consciousness" was, Jackson (1876b:702) felt, quite proper, because the "dreamy state" in fact represented a "revelation of the normal duality of all healthy mental action"; the person being dimly aware of both the objective and the subjective stages of his thought processes. Here Jackson was not too far in his thinking from such men as Wigan and Maudsley, who argued that the feeling of déjà vu might be due to the "instantaneously successive consciousness" of two independently functioning brain halves. Later observations by Jackson and at least two younger colleagues suggested that the *right* hemisphere tends to play a particular role in the production of "dreamy states," which was somewhat at variance with Jackson's original schema of double-brain functioning. Thus, in his Cavendish lecture on "dreamy mental states," Crichton-Browne (1895:21) was led to call the right hemisphere "the more subjective of the two hemispheres of the brain" (Jackson 1880–81; Pick 1903).

6. Conclusion

Were this essay appearing in a history of science journal, it would require no special justification. For the historian, the interest of the material surveyed here would be self-evident. It is not simply in virtue of being a much-neglected aspect of neuropsychiatric thought that 19th-century perspectives on the asymmetry and duality of the brain are of interest historically. The literature, rather, has much to tell historians about certain tensions and paradoxes underlying the overall 19th-century attempt to come to terms with mind and madness in a post-Darwin world of "atoms and ether" (as William Kingdon Clifford once put it) with "no room for ghosts." In so doing, it exposes some of the broad philosophical difficulties inherent in any attempt to lay the shifting boundaries of human personality out upon the static lobes and convolutions of the brain, and it sheds light on the way scientific theory may be molded by sociocultural influences.

This is all very well for the historians, but what has the subject to offer scientists working in the field of laterality today? Some modern-day laterality researchers might be inclined to feel that the opinions of 19th-century writers on brain duality and hemisphere differences, while

perhaps interesting, have little or no immediate relevance to present research programs – particularly since it is so clear that much of that older literature is weakened by sociocultural influences, philosophical a priori assumptions, and methodological defects.

There are two replies to be made to this sort of argument. First: It is hardly likely that all the older work would be judged empirically vacuous by modern standards, and it is possible that some reports of findings might prove useful or interesting to certain investigators today. Would not Galin, Diamond, and Braff (1977) and Stern (1977), for example, studying the possibility that conversion reactions (hysteria) are more frequent on the left side of the body, want to know that data were gathered on this question throughout the second half of the 19th century? Is it possible that some of the quite large body of literature claiming that the right hemisphere tends to outweigh the left in the insane might be of interest to psychiatric researchers looking at schizophrenia as a left-brain "dysfunction" (e.g., Gruzelier & Hammond 1976)? Might not Bakan (1969), arguing for a relationship between hypnotizability and right hemisphericity, possibly be interested in Janet's 1899 report suggesting a tendency for left-sided hysteria to be more frequently complicated by periods of fugue and somnambulism than right-sided hysteria?

Should scientists such as Dimond (1979), interested in the psychiatric implications of the split-brain research, care about the case of Louis V., Bruce's "Welsh" case, Myers' argument linking automatic writing to action of the right hemisphere? What is the meaning of Manaceïne's work on hemisphere "substitution" during sleep – might it be at all relevant, say, to the work of Goldstein, Stoltefus, and Garduck (1972) on changes in interhemispheric amplitude relationships in the EEG during sleep? Might there be something in the opinion of Brown-Séquard that certain visceral disorders are more frequently associated with right-hemisphere lesions than with left? What of the clinical data reported by Sigmund Exner and Armand de Fleury suggesting that sensory disorders are more frequent with right-brain lesions, and motor disorders with left-brain lesions? Or the observations by Hughlings Jackson, Pick, and Crichton-Browne pointing to a link between epilepsy originating in the right hemisphere and "dreamy" mental states? These are just a few examples that come readily to mind, but they will do to make the general point.

At the same time, it is believed that the chief contribution the 19th century is in a position to offer modern-day scientists lies at another level – a *historical* level rather than an empirical one. This is the second – and more important – reply to those scientists who fail to see the relevance of studies such as this one to their own work. At the historical level of understanding, the "mistakes" of the 19th century become no less significant than the scattered observations that happen to fit well into modern-day perspectives.

We would draw, then, the following moral from our history: One of the most striking things about the 19th-century literature on the double brain is how time-bound it is; how plainly in its broad theoretical contours it can be shown to reflect the larger sociocultural ethos within which the writers were immersed. In many instances, certain methodologies or interpretations of data that seem patently defective or dubious to us today, become comprehensible and even inevitable when reinserted in their original historical context. For the scientists and medical men themselves, however, all these limiting forces on their work were essentially invisible; there was no question in their minds about the precise and objective nature of the research they were carrying out.

Yet by no stretch of the imagination can the men about whom we are speaking be considered fools, or cranks, or even second-rate scientists. On the contrary, they often represented some of the leading lights of the medical and scientific establishment, and their work was published in the most authoritative professional journals of the time.

Knowing all this should change the way in which one surveys the modern laterality scene. It will not do to say that what is true for science in the 19th century has no implications for our understanding of science in the 20th; that modern laterality research is different from that of a century ago, because it is so much more methodologically sophisticated and is grounded in a far sturdier foundation of experimental and clinical data. Neither of these latter claims is particularly disputed. But even sophisticated methodologies are conceived within a guiding theoretical framework, and no data are gathered in a cultural vacuum. How much does an appreciation of the development of science through history force us to consider our own science as time-bound in ways we cannot even suspect? To what extent has the double brain, both in the 19th century and in our own time, served as a projecting screen for collective social preoccupations? More generally, what social and philosophical forces are acting when scientists are satisfied that, by dichotomizing human cognition and identifying the dichotomies with the two sides of the brain, they have come closer to understanding cognition itself? And how is one to explain their preoccupation with dichotomies in the first place?

Certainly, a tendency to perceive experience in stark polarities and left/right janus-faced reversions seems to be a deeply imbedded cognitive habit of our species (Needham 1973), and Corballis (1980) has recently pointed out that much current laterality thinking is couched in dichotomous constructions that find parallels in mythological systems around the world. Does the fact that scientists were no less prone to these sorts of constructions in the 19th century (Table 1) suggest that science's capacity to come to terms with the bilateral brain may not only be limited by historical contingencies, but may also be distorted to an unknown degree by the deeper rhythms of the scientists' own minds and brains?

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Reinventing hemisphere differences

John L. Bradshaw

Department of Psychology, Monash University, Clayton, Victoria 3168, Australia

Harrington reminds us that a modern preoccupation with human laterality has long historical antecedents. Many of us believed that this current interest began mainly in the middle of the present century. We now learn of an enormous groundswell throughout the last century. Was this earlier interest maintained throughout the first half of this century, or was there a hiatus? If the latter, then the 19th-century preoccupation with glotogenesis, which twice culminated in a virtual ban on discussions of the origins of language by the Société de Linguistique de Paris (see, e.g., Dingwall, 1979), and its recent resurgence (De Grolier 1983) provide an intriguing and probably related parallel.

Why this fascination? Ask educated laymen what are the basic questions of existence and a hierarchy of three will probably emerge:

1. The origin, evolution, and future of physical matter, energy, and the universe, including, perhaps, a possible place for a deity in the scheme of things.
2. The origin and evolution of life.
3. The nature of human conscious awareness, individuality, volition.

The third question comes close to explaining a fascination with hemisphere differences and mental duality.

A second, more mundane reason is that the issue promises (but often fails to deliver) the grossest examples known of localization of function, to provide yet another handle on the mind/body problem, the interface between structure and function, which takes us back of course to the third question. Thus our two most essentially human attributes, language and nonverbal cognition, are asymmetrically represented. We are interested in how we differ from other species, and speech loss or apraxia after left-hemisphere trauma are socially catastrophic, even though many might be unaware of diminished minor-hemisphere function. Would there have been similar interest if hemisphere duality had instead consisted of, say, sensory versus motor function (indeed, see de Fleury, 1872), or long- versus short-term memory, episodic versus semantic memory, classical versus operant conditioning, hunger plus sex versus thirst plus aggression (or any other such arrangement), vision versus audition, tactile, and kinesthetic sensibility? Of course we dichotomize, even in the presence of triads or tetrads, not because that is just another manifestation of our own duplex nature, but because, as indeed information theory indicates, in this way we can at least try to play twenty questions with nature and win (though cf. Newell, 1973). The botanists' dichotomous key is not bothered by a multiplicity of variables. Of course other cerebral dichotomies are possible – cortical/subcortical, anterior/posterior. Only the latter comes close to being "interesting," matching the sensory/motor dichotomy as a very crude and incomplete approximation. With the current view that perception involves at least an implicit response, interest in the sensory/motor distinction has revived, probably because it appears to be becoming less sure, more subtle – and that is another reason for our interest in cerebral asymmetry, the essential slipperiness of any distinctions.

Another reason is that it embodies a microcosm of psychology: All the sensory modalities and motor processes are involved, together with sex differences, developmental aspects, psycholinguistics, genetics and inheritance, clinical aspects, and the effects of brain damage at birth and in adulthood. The boundaries of psychology, physiology, psychopharmacology, neurology, and anatomy are happily crossed, often without regard to the problems of reductionism, and other disciplines like art, music, and education are enthusiastically invited. Above all, there is at least the possibility of examining two independently functioning personae within a single body – something better even than a clone, as most experiences will have largely been shared by both individuals throughout life, and far better than identical twins.

Looking at the historical antecedents to a line of scientific thought, we experience two competing tendencies: (1) to say how clever our predecessors were to get it so nearly right, with so small a data base, for example, the "atomists" of classical antiquity like Lucretius; (2) to smugly assert from our superior vantage point how wrong they were. Both approaches are of course on their own equally incomplete and unhelpful. Indeed, how can one judge oneself from the viewpoint of posterity? We may wonder at the parallels between 19th- and 20th-century thought on the duality of the mind, but of course the two traditions are far less independent than were even the contemporaneous though mutually isolated Darwin and Wallace on the theory of evolution, with a long informal tradition and prior Zeitgeist for biological change and adaptation. We would really have to look at two totally independent cultures, for example, American Indian and European in the 15th century, to see the extent to which the dual constraints of external reality and the modelling propensities of the human mind lead to true parallels of theory.

Nevertheless, a paper chase through the ages does reveal some intriguing parallels between 19th- and 20th-century approaches to mental duality. Harrington has already mentioned some recurring hypotheses in her conclusion, such as the relationship between lateral asymmetries and conversion reactions, hysteria, schizophrenia, hypnotism, and sleep. I could add the following recurrent ideas: that there is a maturational precocity of the left hemisphere (Corballis & Morgan 1978); that language functions of the left hemisphere are not so much innate to it but rather a consequence of its earlier maturation (Waber 1979); that females (McGlone 1980), illiterate peoples (Vocate 1984), and criminals (Andrew 1978) have reduced or reversed asymmetries; that language has preempted visual–perceptual processing space on the left so that the superior abilities on the right occur as if by default or as a corresponding opposite (cf. Corballis 1983); that expressive propositional speech is a prerogative of the left hemisphere, whereas the right can only cope with receptive comprehensional aspects (cf. Coltheart, Patterson & Marshall 1980); that the right hemisphere predominates for imagery and imagistic thought (for review, see Farah, 1984); that it functions as a largely unconscious automation (Eccles 1973); that it subserves emotion (Ley & Bryden 1981) and dreaming (Kerr & Foulkes 1981); that its writing is distorted, abbreviated, and often mirror reversed (Gazzaniga & LeDoux 1978); that it deserves a better deal from educationalists (Bogen 1975; 1977); that the hemispheres differ in abundance of grey matter (see, e.g., Goldberg & Costa 1981, for reviews); that the corpus callosum may be the best instrument yet available for plugging into a sentient brain (a remark which in its current form has been attributed to Bogen); and that there is an overlap between the concepts of distal/proximal musculature, voluntary/involuntary functions, and impaired/preserved motor capacities after a unilateral stroke. How many times has the wheel been reinvented?

Hemisphere asymmetry: Old views in new light

Jozef Černáček

Department of Neurology, Comenius University Medical School, 811 03 Bratislava, Czechoslovakia

Harrington's target article is evidence that science is part of the ongoing cultural milieu. New ideas and even new discoveries are rooted in the current state of scientific "truth." If they bypass the current state too radically, their acceptance has to wait for evolution and adapt to new methodologies and to changing philosophical opinions.

Thus Boyd's finding of a heavier left hemisphere cited by Harrington has been supplemented or replaced by histological studies (Galaburda, Sanides & Geschwind 1978) and by computed tomography (Chang & Damasio 1980). But controversies remain: according to Harrington, in 1868 Ecker found no reliable signs of hemispheric asymmetry, and neither did Henderson, Naeser, Weiner, Pieniadz, and Chui in 1984 using computed tomography.

Likewise Delaunay in 1874, as quoted by Harrington, speculated about the biological differences between the male and female brain. In 1982 Geschwind and Behan pointed to developmental and immunological differences in the male and female brain, and Inglis and Lawson (1981) found sex differences in the effect of brain damage.

One hundred years ago Delaunay classified people according to their tendency to walk to the right or left side. Glick, Crane, Jerussi, Fleischer, and Green in 1975 wrote about biochemical differences in the brains of rats according to their right- or left-side turning behavior.

These few examples complementing those mentioned by Harrington may show that (1) it is useful to go far into the past when reviewing a problem one is working on, (2) it is useful to compare recent research findings with ancient ones and not to view them only in the light of their contemporary level of science. One thus comes to the opinion that in many fields the spiral of growth of human knowledge is a rather low one.

Right and left as symbols

M. C. Corballis

Department of Psychology, University of Auckland, Auckland, New Zealand

People throughout recorded history and in all cultures have interpreted their environment and experience in terms of oppositions, or dichotomies, such as day and night, light and dark, good and evil, health and sickness, male and female, reason and intuition. Right and left constitute another such opposition, but one that seems to have a special, overriding status. Right–left symbolism permeates the customs, myths, and mores of virtually all cultures, ancient and modern, in surprisingly similar ways (Needham 1973). In his classic essay on the subject, Hertz (1909) pointed out that the symbolic potency of the right–left dichotomy owes much to human handedness – the "pre-eminence of the right hand." Since the dramatic discovery of cerebral asymmetry in the 1860s, however, dichotomies prevailing in human thought have been grafted onto the cerebral hemispheres; dichotomania has gone to our heads.

The question arises as to whether the right–left distinction is somehow reponsible for our tendency to dichotomize. Hertz thought not, and he argued in fact that the responsibility was round the other way. He maintained that dualistic thinking was "inherent in primitive thought," and that handedness emerged in order to resolve the conflicts between oppositions:

> For centuries the systematic paralyzation of the left arm has, like other mutilations, expressed the will animating man to make the sacred predominate over the profane, to sacrifice the desires and interests of the individual to the demands felt by the collective unconscious, and to spiritualize the body itself by marking upon it the opposition of values and the violent contrasts of the world of morality. (Hertz 1909, translated by Needham 1973, p. 21)

Hertz was an enthusiastic advocate of ambidexterity, as a means to overcome the one-sidedness imposed by the primitive dichotomization of thought and to "develop the energies dormant in our left side and in our right cerebral hemisphere" (Needham 1973, p. 22).

Although a few would agree with this account of the origin of handedness and cerebral lateralization, Hertz anticipated later developments in calling for an end to the oppression of the left hand and right cerebral hemisphere. This was a common theme in the 1960s and 1970s; one modern educator, for example, notes "the tragic lack of effort to develop our children's right brain strengths. That potential – a source of . . . creative, artistic, and intellectual capacity – is at present largely unawakened in our schools" (Garrett 1976, p. 244). This romanticization of so-called right-hemisphere values seems to me to represent one difference between the 19th- and 20th-century dichotomies listed in Harrington's Table 1. The 19th-century dichotomies reflect the inferior status bestowed on women, nonwhites, animals, and the insane, while right-hemisphere values in the 20th-century list owe at least something to black power, women's liberation, the protest against the Vietnam War and the military–industrial complex, and the rise in popularity of Eastern religious cults. As in more primitive times, the left–right dichotomy continues to carry the burden of contemporary social, political, and philosophical issues.

What explains the symbolic potency of left and right? Elsewhere (Corballis 1980) I have suggested two related factors. One is the paradox of human handedness: The hands are structurally almost identical, yet they are functionally very different, as a right hander can easily verify by trying to write or throw with the left hand. Again I cannot resist Hertz: "What resemblance more perfect than that between our hands! And yet what a striking inequality there is!" (Needham 1973, p. 3). Functional asymmetry in a structurally symmetrical pair seems to imply a noncorporeal ingredient, bestowed perhaps by some divine creator, hence the common belief that the right side is sacred and the left profane. The same paradox seems to apply to the cerebral hemispheres, whose functional asymmetry belies their anatomical symmetry, and in Eccles's (1965; 1981) notion that human self-consciousness is exclusively left-hemispheric, we may perhaps discern the modern equivalent of the primitive idea that the right side of the body is touched by the divine.

The second source of left–right potency is that right-handedness and left-cerebral specialization for language are exclusively human phenomena, and so differentiate us from other species – and especially from other primates. Again this invites the Cartesian interpretation that some noncorporeal property has been bestowed uniquely on the human brain, and more especially on the left hemisphere. As Harrington makes clear, this idea is featured prominently in 19th-century thinking, as in the views of Hugo Liepmann; it may have provided the consoling thought, in the post-Darwinian trauma, that we may be different from the apes after all.

To some extent these two aspects of left and right are yielding to more exacting investigation. It is becoming clear that there are anatomical (e.g., Galaburda, LeMay, Kemper & Geschwind 1978) and hormonal (Geschwind & Behan 1982) differences between the hemispheres that may at least partly account for functional asymmetries, and there is increasing evidence for functional asymmetries in nonhuman species that in some respects resemble those in humans (Denenberg 1981).

It should not be denied that there is at least some scientific basis for the dichotomies tabulated by Harrington. The very commonality between 19th- and 20th-century dichotomies suggests some basis in truth, although it is scarcely reassuring that the 19th-century ideas, once prevalent, should have sunk so completely into obscurity. It is to Harrington's credit that most of us are aware of them at all. The commonality, moreover,

might reflect the enduring nature of human social, political, and philosophical concerns rather than any underlying neurological truth. Primitive associations with the left and right hands or sides of the body show considerable uniformity across diverse cultures, separated in time and geography, yet they clearly have little basis in fact.

Another dangerous dichotomy lurks, however: that between science and myth. The two surely lie at the extremes of a continuum; no healthy science is without a dose of myth, just as all myths convey a measure of truth. I have no doubt that conceptions of human laterality will continue to evolve both as a result of careful scientific evaluation and in response to broader human concerns.

Laterality as a means and laterality as an end

Paul Eling

Max-Planck-Institut für Psycholinguistik, 6522 BC Nijmegen, The Netherlands

Harrington describes, in considerable detail, how eagerly 19th-century scientists accepted theories about particular psychiatric disorders that were based on the notion of the independence of the two cerebral hemispheres. In her review, she attempts to make two points. The first one is that many hypotheses discussed in current papers on laterality were also discussed and accepted – at least by some authors in France – in the prior century, even though the empirical basis was inadequate. I suppose that this is not a great shock to most researchers working in the area of laterality. On the contrary, the Wigan–Broca–Wernicke–Jackson type of review can be found in many papers and textbooks. In this sense, Harrington challenges a belief that is probably not held by most researchers.

However, as Harrington indicates, there is a second, more subtle argument to be made. Although I had some difficulties detecting this argument, I expect she has in mind the moral that can be drawn from history, as discussed in her conclusion. In principle, I am inclined to support this second point. My experience reading the 19th-century literature on brain–behavior relationships suggests that it is illuminating to keep in mind how theories and ideas described in these papers are related to ideas developed in other areas of science (e.g. physics, Russelman 1984) and to the larger sociocultural ethos. And the question that follows is, of course, what about us?

Although I am clearly in favor of Harrington's approach, I would like to make two critical remarks. The intention of these remarks is positive: to allow a clearer elucidation of the argument.

It first seems important to correct a potential misunderstanding that results from throwing all laterality work into one heap. This is sometimes done by those who are skeptical about certain claims made with respect to the functioning of the two hemispheres. It is my impression that Harrington's paper also presents a caricature of what laterality studies are about in general.

Since dichotomies are commonplace in this area, it will do no harm to introduce one more. This has to do with two ways of doing laterality studies. The first line of research can be characterized as "laterality as an end." Researchers doing this type of work consider the two hemispheres to work independently. The purpose of their work is to demonstrate that the left and right hemispheres are specialized for particular functions. Their ultimate goal is to describe the underlying principle that is shared by all the functions represented in a single hemisphere. The basic assumption these researchers make, although usually it is implicit, is that each hemisphere can function on its own.

The second line of research is not as widely used as the first one. In this approach, phenomena of asymmetrical functioning of the brain are used to study – at the neurological or psychological level – the working of the brain as an integrated system. A good example would be Broca himself. Although it is widely stated that Broca is the founder of laterality, in the first sense described above, he was much more careful in his statements. As Harrington describes, he was forced to accept the notion of left-sided representation of speech. She correctly quotes Broca when claiming that the majority of people are "left-brained" and that by exception only some of those we call left handers are "right-brained" (Broca 1865, p. 383). Although Broca talks about the two hemispheres and hand preference here, he does not claim that there is a clear relationship. Harrington fails to take into account Broca's disclaimer of this relationship, which appears two pages later (Broca 1865, p. 385; Eling 1984).

Apart from having difficulty accepting asymmetrical representation, Broca also carefully restricts his claim to only a small portion of the faculty of language. He believes that this faculty consists of three different layers with several parallel components in each. This system enables one to communicate in different modalities. He also does not believe that the left hemisphere is the exclusive place for the general faculty of language or for the special faculty of articulated language (which is, according to Broca, also used for speech *perception*). Only the speech production aspect of this last faculty is asymmetrically localized by Broca. Thus, he was far from stating that there is something like "cerebral dominance," even for language.

What Broca has done in his analysis is to demonstrate that "language" is represented by many subsystems, of which the faculty of coordinating speech movements is only one that can be isolated by a localized lesion. Broca does not claim that the left hemisphere is verbal and the right nonverbal. He is arguing that there is such a thing as a speech coordination function, which is apparently not doubly represented.

From history we can learn that this approach using laterality effects as a means to study the functioning of the brain has been used by others. However, it was clearly not as popular as the laterality studies that contain (implausible and therefore interesting?) claims such as predicting that each hemisphere is enough to function as a human being or to have a full personality in itself. Nevertheless, I consider this approach to provide a more serious possibility of learning something about how the brain works. Moreover, I wonder whether this approach is affected to the same degree by the more subtle argument Harrington wants to make in her conclusions.

This brings me to the two critical remarks mentioned in the beginning. In her review of the studies Harrington restricts herself mainly to the 19th-century papers on dual brain, dual mind. On the basis of this evidence alone it is difficult to see how these papers reflect the larger sociocultural ethos. What I miss in her argumentation, then, is the link with notions and beliefs that were held concurrently in other (scientific, religious, political) circles.

Indeed, it is remarkable to see how quickly some theories were accepted at a time when the empirical basis was clearly deficient, but when the general Zeitgeist was apparently right. This should change the way in which one surveys the modern laterality scene (why not other areas of brain research?). The interesting question of how this should change our view of our contemporary work is not addressed by Harrington. I think that the answer to this question will contribute significantly to our evaluation of the different interpretations of laterality studies that can be found in the current literature.

Brain theory and the uses of history

Samuel H. Greenblatt

Division of Neurological Surgery, Medical College of Ohio, Toledo, Ohio 43699

At the beginning of her target article, Harrington proposes that a historical perspective on her subject may be of importance to

contemporary workers in laterality research. After a thorough and thoughtful review of the subject she concludes that her results should have two uses for the working scientist. First, there is the seemingly self-evident point that some 19th-century data are still valid and useful. Second and more interesting is the importance of recognizing historical relativism. In Harrington's words, it is now easy to see that the views and disputations of the 19th century were severely "time-bound." Though she does not actually say it, she implies, with good reason, that we are as much time-bound as the people of any other era. In other words, we are both beneficiaries and victims of the intellectual milieus in which we work.

This last point can be usefully illustrated by expanding upon a historical event that Harrington mentions only briefly. In what we now call the neurosciences, the intellectual milieu of the 1880s was really very different from that of the 1860s. The ostensible difference was the existence and acceptance of a viable theory of cortical localization, but localization had come into its own because a more fundamental change had occurred simultaneously. During these few decades, the earlier view of the cerebral convolutions as the "common sensorium" – the "*terra incognita* of the mind" – was replaced by the modern idea that the convolutions are subject to the same physiological laws and analyses as the rest of the neuraxis (Greenblatt 1977:428; 1984:250). This realization followed from the successful extension of the sensorimotor paradigm of associationist psychology to the cerebral cortices. Hughlings Jackson was the leading theoretician in the advocacy of evolutionary associationism in this context, but the same theoretical foundation underlies the work of the French and German localizationists of the 1870s and 1880s (Buckingham 1984).

Though it generally goes unrecognized, 19th-century associationism is still very much with us (Buckingham 1984; Young 1970). To put it more critically, I would claim that our theories of how the brain works have not advanced beyond the fundamental assumptions of evolutionary associationism. From this point of view, then, there is a third and potentially powerful use of historical analyses such as the one Harrington has presented. If carried to sufficient depth, such studies should lead to conscious recognition of the assumptions that bind us to our own times. In other words, we may be able to break out of our own limiting assumptions by finding out what they are. Where is our *terra incognita* of the mind?

I think our own territories of the unknown will be found at the points in associationism where the theory becomes ambiguous, that is, relatively powerless to predict or explain new findings. One of the best examples of this ambiguity is how associationism deals with laterality. There is nothing in the theory that predicts dominance and precious little that explains it, but neither is it contradicted. Hughlings Jackson eventually dealt with the undeniable data about dominance by viewing the phenomenon as part of a developmental hierarchy. This approach is somewhat satisfying as a broad perspective, and it can lead to a certain amount of further investigation, but so far it has not led to a clear understanding of laterality in biological or physiological terms. What fundamental advantage did dominance convey and how is a lateralized function integrated into the total brain mechanism for cognitive function? Even more broadly, how are we to conceive of a total brain mechanism for a cognitive function in associationist theory? To put it in Jacksonian terms (1874b:132), the reflex-based, sensorimotor paradigm of associationism does not explain "propositionising."

None of these comments is meant to be historically pejorative. Our 19th-century forebears adopted associationism because it worked for them in their circumstances. It *predicted* the presence of motor processes in the cerebral cortices, and the existence of those processes was confirmed. We do not need to discard our legacy. Rather, by understanding it more fully we should be able to build upon it more successfully.

Nineteenth-century views on madness and hypnosis: A 1985 perspective

J. Gruzelier

Department of Psychiatry, University of London, Charing Cross and Westminster Medical School, London W6 8RP, England

Harrington has provided a welcome antidote to the ahistorical leanings of contemporary neuroscience in her target article. In concluding she questions whether knowledge of the 19th-century view of madness as a right-hemisphere function would have influenced contemporary investigations of schizophrenia as a left-sided disorder. This is best regarded as a tongue-in-cheek remark for it does little justice to the richness of thought of either century. To consider the 19th century, the evidence reported in support of right-hemisphere involvement in psychosis was flimsy, such as Maudsley's reference to psychosis as "the brute brain within the man's" and involved bland conceptual juxtapositions such as the alignment of reason with the left hemisphere and instinct with the right. This ignores the influential and seminal insights of Emil Kraepelin (1855–1926) and Eugen Bleuler (1857–1939), who documented psychotic phenomenology in great detail. Both believed that impaired linguistic functions were cardinal features of schizophrenia. The requisite localisation of such functions to the left hemisphere was provided by Kahlbaum (1874) "the vicinity of the sylvian fissure and of the second and third frontal gyri, i.e., the areas which on the basis of findings in aphasia are regarded as the site of mental speech formation, is very noteworthy in regard to the clinical symptoms related to speech (mutism and verbigeration), which are very prominent in catatonia" (Zilboorg 1967, p. 448).

Notwithstanding the weight of contemporary evidence in favour of left-hemisphere involvement in schizophrenia (e.g., Gruzelier 1981; Newlin, Carpenter & Golden 1982), recent evidence suggests that different types of syndrome constellations in schizophrenia may reflect opposite states of hemispheric balance. The withdrawn, retarded syndrome often found in chronic, nonremitting schizophrenia, which is accompanied by a reduction in linguistic functions, reflects a loss of left-hemisphere activation. There is a corresponding increase in right-hemisphere activation, which contributes to depressive affect. In contrast, the reactive and linguistically florid syndrome, often found in acute schizophrenia, reflects the opposite state of hemispheric imbalance (Gruzelier 1983; 1984).

An impairment of interhemispheric pathways may be one of the sources of hemispheric imbalance in schizophrenia. Here views on hemispheric disconnection as long ago as the mid-18th century were particularly insightful, as was their subsequent expression in the mid-19th century in the writings of Wigan (1844). Their inspiration to contemporary research on callosal disconnection in schizophrenia has been acknowledged (Green, Glass & O'Callaghan 1979).

The attempt to attribute to madness a reversal in asymmetries of the weight of the hemispheres (Luys 1879) has a counterpart in recent attempts to compare schizophrenic and control brains with computed tomography for asymmetries in the width of the frontal and occipital lobes. While a consensus has yet to be reached, a proportion of schizophrenic brains do show cranial reversals, and this appears to be a higher percentage than in the normal population (Luchins 1983).

Schizophrenia aside, the affective psychoses may well have a right-sided origin (Flor-Henry 1979) in keeping with 19th-century views on psychosis. Nevertheless, the manifestation of mania and depression is thought to reflect left- and right-hemisphere functions respectively (Flor-Henry 1979). There is also consistency between the 19th- and 20th-century views on lateralisation in the control of emotion (see Gur, 1983, for a review on the latter).

Hemispheric interactions are also a current focus of interest in the neuropsychology of hypnosis. Contemporary research

began by observing parallels between the hypnotic experience and right-hemisphere functions, functions compatible with the theories of Janet and Hughlings Jackson: somnambulism, the dream-like quality of hypnosis, primary process thinking, automatic memory, childlike emotions, visual imaging, and the suspension of a critical attitude. Those who were hypnotically susceptible were assumed to possess a predisposition to right-hemisphere processing (Bakan 1969). Recent research suggests that the inhibition of left-hemispheric processing is a prerequisite for a successful hypnotic induction, and this occurs subsequent to the focusing of attention and the engagement of the left hemisphere (Gruzelier, Brow, Perry, Rhonder & Thomas 1984). A preexisting right-hemisphere bias when not coupled with a flexibility to shift to left-hemisphere processing may in fact be counterproductive to the induction of hypnosis. In fact, a flexibility in the allocation of cognitive resources appears to contribute to hypnotic susceptibility (MacLeod-Morgan & Lack 1982). Here an analogy may be drawn with the virtuosity of the French School of the 19th century in producing consecutive oscillations in sensitivity from left to right and in hypnotising the hemispheres alternately.

Perhaps most intriguing of all were the 19th-century views of the dynamic interplay between hemispheric functions, processes which continue up to the present day to receive scattered attention. The bulk of research in lateralisation then and now has been concerned with fairly circumscribed functions and structural impairment, and with some notable exceptions (e.g., Denenberg 1983; Kinsbourne 1975) it has been left to psychopathologists to unravel the lateralisation of dynamic brain functions.

The ambidextral culture society and the "duality of mind"

Lauren Julius Harris

Department of Psychology and Neuroscience Program, Michigan State University, East Lansing, Michigan 48824

The invitation to comment on Harrington's splendid essay posed a problem of choice, as the author has set a table laden for me with too many tantalizing dishes. Finally, though, I was drawn to an item placed well away from the main courses, among "miscellaneous applications of the brain-duality theory." This is the story of the ambidextral culture movement – salad to Harrington, meat and potatoes to me (see Harris, 1985).

Harrington has wisely noted that the men about whom we are speaking cannot be considered fools, or cranks, or even second-rate scientists. This was no less true for the principal figures in the ambidextral culture movement. As even one of its severest critics, Sir James Crichton-Browne, had to admit, the movement had the support of "a large number of highly educated, intelligent and reasonable people, and of some men of light and leading" (Crichton-Browne 1907, p. 624). But what attracted them to the movement? There were several reasons, all of which are relatable to more general issues in the development of ideas about brain duality.

In speaking of the origins of this educational movement, Harrington has noted only that John Jackson, its founder, had promoted Wigan's (1844) view that each hemisphere may serve as a distinct and perfect organ of thought. The implication for Jackson was that with proper training, the hands could become, as Jackson put it with his characteristic restraint, "absolutely independent . . . in . . . ANY KIND OF WORK whatever" (1905, p. 225). In urging ambidextral training, however, Jackson had promised richer rewards than merely independence of the hands (and, by implication, of the cerebral hemispheres). Ambidextral training also would equally train the two hemispheres and thereby enhance – even double – mental power. Here, still another figure enters the story – Brown-Séquard. As Harrington mentioned, but not in connection with the ambidextral culture movement, Brown-Séquard also had called for equal training of the two hemispheres.

Wigan, of course, had written in the era before Broca; Brown-Séquard, afterward. So, for Brown-Séquard, the new evidence linking speech (as well as intelligence and will, as Brown-Séquard supposed) predominantly to the left hemisphere carried a startling implication: "If we make use of only one brain for most of our actions, we leave inactive one-half of the total mass of brain matter, and, therefore, we leave quite useless one-half of the most important of our organs as regards manifestations of intelligence, will, and perception or sensation." Therefore, "we ought to give education to the two sides of the brain, or, rather, to the two brains" (Brown-Séquard 1874a, p. 2). Because Brown-Séquard assumed that brain and mental development were connected to the "leading movements of the body" (1874a, p. 20), the means by which he proposed to accomplish this two-brain education were predominantly motor. Thus every child should be made to "exercise the two sides of the body equally – to make use of them alternately . . . In this way it would be very easy indeed to obtain a great deal, if not all the undeveloped power possible to the individual" (1874a, p. 20).

Jackson's promise that equal-hand training would enhance mental capacity might have been a potent enough inducement for the teacher-members of the Ambidextral Culture Society. The medical men may have been more impressed with another possibility. By Jackson's time, aphasia had become a prominent medical problem. Jackson even thought it was increasing in incidence: "This disease of Aphasia, together with its related 'Agraphia,' is becoming more prevalent every day, a circumstance calculated to raise serious apprehensions as to the future, if nothing can be done to arrest its advances" (1905, p. 124). Jackson's answer was that equal training of the hands would not only enhance mental power, it also would build language centers in the right as well as in the left hemisphere. As such, ambidextral training would help to prevent or to ameliorate the aphasias and hemiplegias resulting from unilateral brain injury.

The ambidextral culturists thus promised remarkable psychological and medical benefits. Could they substantiate any of their claims? To show that two different tasks could be carried out at the same time, Jackson drew largely from anecdote and, frankly, parlor tricks, for example, a photograph in his book of two different letters written concurrently by a 17-year-old girl – after 8 months of practice (1905, p. 193). Another girl, Jackson's own daughter, appears to have been particularly accomplished in such feats, judging from an account in 1903 by the president of the Ambidextral Culture Society, the distinguished orthopedic surgeon, E. Noble Smith, F.R.C.S., Edinburgh. (Smith himself had become an advocate of ambidextral training in the conviction that it would reduce scoliosis.)

To support his bold assertion that ambidextral training could aid victims of aphasia, Jackson cited medical opinion. For example, Sir James Sawyer, consulting physician to the Queen's Hospital, Birmingham, and a vice-president of the Ambidextral Culture Society, proposed that ambidexterity, by tending to the more equal use of the two sides of the brain, "might prevent, or help in the cure of, some cases of hemicrania. Perhaps it might prevent some cases of hemiplegia" (1900, p. 1303).

Jackson was also able to invoke the great name of Sir David Ferrier, for although Ferrier had acknowledged the evidence that the speech center was in the majority of instances in the left hemisphere, he also said that there was no reason, "beyond education and heredity, why this should necessarily be so" (1886, pp. 450–51).

> It is quite conceivable that the articulating centres of the right hemisphere should be educated in a similar manner. A person who has lost the use of his right hand may by education and practice acquire with his left all the cunning of his right. In such a case the

> manual motor centres of the right hemisphere become the centres of motor acquisitions similar to those of the left. As regards the articulating centres, the rule seems to be that they are educated, and become the organic seat of volitional acquisitions on the same side as the manual centres. Hence, as most people are right-handed, the education of the centres of volitional movements takes place in the left hemisphere. (p. 451)

Ferrier also suggested that, although the left articulatory center was the one commonly and specially educated in speech, it was conceivable that "a person who has become aphasic by reason of total and permanent destruction of the left speech centre may re-acquire the faculty of speech by education of the right articulatory centres" (p. 451).

As the quoted passages above show, Ferrier had not, in fact, specifically said that this education could be brought about by hand training, although one can see why Jackson might have felt justified in drawing such an inference. A later writer, however, was explicit on this point. Varia Kapiani (1913), a staunch advocate of ambidextral training, mentioned cases by a Dr. Manfred Fraenkel in which speech that was lost after a left-sided stroke with right-sided paralysis reportedly had been regained by systematic writing with the left hand. Even after a second left-sided stroke, speech remained, proving clearly, according to Fraenkel, that left-hand writing had caused the power of speech to transfer to the right brain. Evidently unknown to the ambidextral culturists was C. T. Buzzard, physician to the (British) National Hospital for the Paralyzed and Epileptic, who had developed a system of therapy based on this assumption (1882, p. 441).

In further support of his views, Jackson and other advocates of ambidextral training cited reports that left-hemisphere injury was less likely to result in permanent speech loss in children than in adults. As we know, Broca (1865) himself had noted this relationship and had suggested, in explanation, that "someone whose third frontal convolution, ordinarily the seat of articulate language, would be atrophied from birth, would learn to speak and would speak with the *right* hemisphere" (pp. 386–87; emphasis in original, my trans.). (This, then, was the circumstance for which Broca invoked what became known as the doctrine of cerebral substitution, mentioned by Harrington.) Similar views were expressed by Brown-Séquard (1874a) and by the neurologists Howell T. Pershing (1897, p. 787) and Sir William Gowers (1896–1902; quoted in Jackson, 1905, p. 123).[1]

Broca (1865), whose opinions on this matter were not, in fact, mentioned by Jackson, was cautious in his interpretation of the age difference. He noted, for example, that the adult aphasic rarely received the intense language training ordinarily provided children (p. 390). The neurologist Bernard Sachs, however, concluded that in young children, hemispheric differentiation for speech "is not nearly so complete as in the adult" (1897, p. 323); and Gowers inferred that there must be "a capacity for the acquisition of voluntary speech processes on the right side of the young which there is not in the adult" (quoted in Jackson, 1905, p. 123). Consequently, "The exclusive relation of voluntary speech to the left brain is due to the disuse for speech of the right brain: it seems to occur in the transition from childhood to youth, and it is related to the use of the right hand" (Gowers, quoted in Jackson, 1905, p. 123). The reports thus gave "clear proof," as Brown-Séquard put it, "that the right side of the brain can be educated to become a leader in mental faculties as well as the left side of the brain" (1874a, p. 18).

Given the state of knowledge at the time, the ambidextral culturists' interpretation of the available clinical and other evidence cannot be said to have been purely fanciful. Consider, for example, the proposition that training the left hand might succeed in building new language centers in the right hemisphere, thereby facilitating recovery from aphasia. Jackson, as we have seen, could invoke the opinions of eminent physicians and scientists, even if, as in Ferrier's case, such a view was at most only implicit. The fact that neuropsychologists today reject any such proposition does not mean that its early advocates were foolish to believe in it. Consider also the ambidextral culturists' application of the principle that in the young brain the right hemisphere has a capacity for the acquisition of language that is lacking in the adult brain. Not only was this in Jackson's time a perfectly reasonable interpretation of the clinical evidence such as it was, but neuropsychologists in our own time have drawn a similar conclusion on the basis of new studies (e.g., Chase 1974; Krashen 1973; Lenneberg 1967). Indeed, as St. James-Roberts has observed, the principle has become a "part of neuropsychological dogma" (1981, p. 32). Only lately, on closer inspection of the evidence, has the dogma begun to be questioned (St. James-Roberts 1979; 1981).

In short, although some of the ambidextral culturists undoubtedly were – in George Gould's (1907) dismissive terms – "cranks" and "sillies," they were, for the most part, serious people struggling with serious ideas. Indeed, it seems to me that their counterparts today – the educators and others who call for "two-brain education" in the schools – do not, in many respects, measure up to their predecessors (see review in Harris, 1985).

ACKNOWLEDGMENT

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NOTE

1. Jackson was citing a paper by Gowers entitled "The use of words, &c.," dated 1896–1902. I have not seen this paper, but the statements quoted are essentially identical to those published in Gowers's major work (1885).

The case for applied history of medicine, and the place of Wigan

H. Isler and M. Regard

Neurology Department, University Hospital, 8091 Zürich, Switzerland

History is only viable if it is not boring. Harrington's historical review is stimulating and lively enough to avoid this common plight, and her sensitivity for important developments is remarkable. She makes a strong case for applied history of medicine; we can only support this, trying to add some corroborating evidence.

There is not much room for mental duality in our thinking, which is still moulded upon the doctrines of Descartes who held that the soul, or spirit, or subject, must be "one and indivisible." In this context we have to stress the importance of A. L. Wigan, who attempted to overcome this barrier. He incurred the displeasure of his contemporaries (Isler & Regard 1984) and does not appear in general histories of medicine and of lateralization (Young 1970; Hécaen & Dubois 1969; Hécaen 1978; Schiller 1979). He has been mentioned several times since 1963 (Hunter & MacAlpine), but the bulk of his achievements have hardly been appreciated so far, including his precise ideas of higher cerebral activity as well as his dynamic balance model of hemispheric function. Wigan succeeded in establishing new patterns of discovery, which enabled him (see Isler, 1968) to accommodate a surprising amount of information on the brain's "behavior." He created the concept of hemispheric dominance and gave a perfect prediction of both the importance of left-hemisphere dominance and the qualifications of its future protagonist (who later turned out to be Broca) (Wigan 1844, p. 313). He recognized prosopagnosia as unilateral (p. 170). Describing the split-brain condition (p. 52), he found that it did not prejudice everyday life. He thought that the healthy left hemisphere, "having thus the possession of reason" (p. 98), could balance a diseased mode of mind in the right hemisphere. "It appears almost certain that sensation and perception are not performed by the same organs which exercise the purely reasoning faculties" (pp. 320; 99). "There was only one organ of speech,

and it was compelled to obey the most tyrannic of the two discordant influences" in a case of paragraphy (pp. 144–241) (see also "as a phrenologist would say . . . organs of the mind," (p. 269). The dominance of the healthy hemisphere over the diseased one is destroyed by anger, passion, or drink (p. 52f). Dominance is exchangeable and varies individually in healthy people (pp. 157, 337). "I suspect that in the case of left-handed persons there is a transposition of the relative power of the two brains" (p. 337).

As we see in Harrington's target article, Brown-Séquard made himself unpopular by insisting on a set of ideas that are indistinguishable from Wigan's own: each hemisphere is a complete brain; one can be lost without loss of sensation and movement; disabilities from unilateral lesions can be due to inhibitory action from other parts of the brain; functional hemispheric differences may be due to training. He actually quoted Wigan in 1877 (Bogen 1973). He was a great friend of Broca's, and it is rather likely that he acquainted Broca with Wigan's work, through which he was to be given his route to lasting fame.

Harrington's dramatic history of the aberrations of the double-brain concept – metalloscopy and transfer of hysterical hemisyndromes to the other side – is a classical example of applied history of medicine. Only after reading about the bursting of this intellectual South Sea bubble of the 1870s can one fully understand the violent fear of all things subjective and emotional that plagues brain researchers up to this day. This catastrophe was clearly the result of degeneration of ideas closely resembling those of Wigan. How much has the suppression and neglect of his work had to do with this development?

Wigan's patterns of discovery appear as a closer approximation to nature than many popular doctrines and models of our time. Their heuristic potential should be applied.

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Scientific amnesia

David E. Leary

Department of Psychology, University of New Hampshire, Durham, New Hampshire 03824

Since Nietzsche, according to one recent commentator, the question of truth has been transformed: "It is no longer, 'What is the surest path to Truth?', but, 'What is the hazardous career that Truth has followed?' " (Foucault 1980, p. 66). After reading Anne Harrington's analytic review of nineteenth-century neuropsychological theory and research, I cannot help wondering whether science, at least in one of its aspects, has become one of the hazards in the career of Truth.

My ruminations upon this point revolve around a single question: Why have we forgotten so much of the scientific episode – and so much of the scientific material – that Harrington reviews? This was no minor drama in the history of neuropsychology; its central characters were not bit players; its temporal and spatial setting was not far removed from our own; and most of its literature is readily accessible in our university libraries. Yet Harrington's assertion that few contemporary scientists have referred to, much less utilized, significant portions of this literature is but slightly exaggerated.

Why do scientists forget previous lines of research, even major lines of research? It is, I suggest, at least partially due to one of the norms of science itself. The norm goes back to the 17th century, but its full, and I would say detrimental, actualization is a product of our own times.

It was Sir Francis Bacon, of course, who issued the most notorious call to arms – the call to study nature instead of books. René Descartes' similar rejection of the study of "letters" and his espousal of the principle of systematic doubt corroborated Bacon's stricture against reliance upon prescientific and quasi-scientific texts. So too did John Locke's less strident recommendations about the empirical pursuit of knowledge. All three thinkers, along with their contemporaries, considered the distrust of tradition – especially of the scholastic tradition passed down in written form – to be one of the common, enabling virtues of "the new mechanical philosophers" who constructed early modern science. As we know, that distrust has paid remarkable dividends.

Unfortunately, however, distrust breeds distrust, and what began as an appropriate concern about reliance upon previous nonscientific texts has become a thoroughgoing skepticism about all "outdated" texts, scientific ones included. Whatever can be said in defense of this bibliophobia, it has led to what might be called the functional illiteracy of many contemporary scientists. By functional illiteracy I mean, of course, not the inability to read, but the selective limitation of reading, with marked preference accorded to the most recent material on any given subject. In either form, this functional illiteracy accounts for another functional disorder suffered by many contemporary scientists, a disorder we might call scientific amnesia.

Should I call this form of short-term memory a disorder? Obviously, I feel that it is a disorder for scientists not to remember the work of the past, but as I have suggested, such amnesia is legitimized by the norms of their discipline. Especially over the past century, as the amateur pursuit of "natural philosophy" has been transmogrified into the professional business of contemporary "science," distrust and hence disregard of dated research have been raised to a fine art. As a result, scientists are constantly losing sight of those parts of their discipline's history upon which they are no longer actively building. Their blindness to such apparently dead-end, tangential, and stalled historical developments has helped sustain the illusion of linear progress in science, but ironically it has at the same time reduced the likelihood of true progress, that is, the likelihood that these developments will be recalled and built upon when opportune moments occur. Instead it condemns scientists to occasional, unwitting reinvention of past successes and failures.

The illusion of steadily cumulative, linear progress in science is also sustained by the cult of the scientific method. I readily admit that we have received many good things in exchange for our methodolatry, and I recommend continued obeisance at the methodists' altar, though of a less drastic, less true-believing sort than we have witnessed through much of this century (see Toulmin & Leary 1985). But we should recognize the price we have paid for our faith in the Cartesian dogma of the prerogative of method: we have forfeited confidence in, and thus use of knowledge gained by methods other than those we currently prefer to use. So even those scientists who might otherwise grant some value to dated knowledge are likely to assume that such knowledge is incommensurate with our current knowledge because of its tainted methodological pedigree. The implicit conclusion for many is: If earlier investigations, as in 19th-century neuropsychology, did not utilize currently popular research methods, they cannot have much to offer.

On the contrary, I believe with Harrington that we have much to learn, not only from the particular historical episode that she has reviewed, but from many others as well. *What* we have to learn, from *which* episodes, cannot be predicted in advance. It will depend on what it is we want to know, and what it is we think we know. But at minimum, greater literacy – less amnesia – should help to sensitize us to the historicity of our own topical interests, conceptual frameworks, methodological approaches, and theoretical convictions. This should have salutary, liberating effects. (I do not believe that the history of science should be X-rated. See Brush 1974.) It should help us to become aware of our own blind spots, even if it cannot produce perfect vision.

Picking up on what Harrington has already suggested, I

would argue that the separation of science and scholarship, upon which modern science has been based, seems to have reached a point of precipitately diminishing returns. We need to reinstate scholarship, especially historical scholarship, into our scientific disciplines – into undergraduate and graduate training, into our scientific endeavors, and into our presentations and publications. If we can do so, at least one obstacle in the path to Truth – the hazard of scientific amnesia – will be removed. Other obstacles will surely remain, but perhaps they will then be seen more clearly for what they are. As it is now, it is often difficult to know which obstacles deserve our fresh, naive attention and which have already been dealt with by our predecessors.

Hemisphere differences before 1800

Gert-Jan C. Lokhorst

Department of Philosophy, Erasmus University, Rotterdam, The Netherlands

Harrington's target article is a fine survey of 19th-century ideas on the cerebral hemispheres, but the history of her subject is longer than she thinks.

In the first place, there is at least one ancient Greek theory of hemispheric specialization. This is how a 12th-century Latin codex describes the views of Diocles of Carystus, a famous Athenian physician from the 4th century B.C.:

> He says that phrenitis (madness, frenzy) arises from an inflammation of the heart and an obstruction of the blood or innate heat, from which the brain derives sentience and intellect. The latter is that with which we understand, the former that with which we sense. Accordingly, there are two brains in the head, one which gives us our intellect, and another one which provides sentience. That is to say, the one which is lying on the right side is the one that senses; with the left one, however, we understand. (See Lokhorst 1982a:35)

Diocles's theory is astonishingly modern: the terms he uses, *intellectus* on the left side and *sensus* on the right, are even identical to the last entries in Harrington's table. Where the theory comes from is not clear. Before Diocles, Alcmaeon and Empedocles had already distinguished between sentience and intellect; moreover, it may already have been surmised in his time that the hemispheres are different in that each one only controls the opposite half of the body. There is, however, no evidence of an earlier combination of these two ideas. How the theory fared after Diocles's times is clearer: It fell into complete oblivion, the reason for this probably being the emphasis on the unitary system of the cerebral ventricles in later accounts of brain function. Nevertheless, if the early 19th-century brain scientists had shown a greater regard for history, they easily could have known about Diocles's theory: There are no less than three 16th-century editions of the codex (Horatianus 1532; cf. Lokhorst 1982b).

In the second place, there is more to be found in the 18th-century literature than Harrington suggests. I am thinking not only of the discussions about the function of the corpus callosum and the crude split-brain experiments from this period (see Neuburger 1897), but especially of Meinard Simon Du Pui, whose *De homine dextro et sinistro* (1780) clearly anticipates some of Henry Holland's and Wigan's ideas. Thus, he wrote that man is from a medical point of view a "*homo duplex*, a right man and a left one" (p. 108); "man's nervous system is just as bipartite as the rest of his body, with the result that one half of it may become affected while the other half continues to carry out its proper functions" (pp. 184–85). Broca knew of these views but considered them too extreme: "The thought is far from me to divide man into two distinct beings, like Du Pui has done" (Broca 1865:393).

Harrington's 19th-century material gives a good picture of the period. However, one should not underestimate the anatomical knowledge available by the end of the century concerning hemispheric differences. For example, the recently discovered fact that the right hemisphere contains relatively more white matter (i.e., fibers) than the left one (Gur, Packer, Hungerbuhler, Reivich, Obrist, Amarnek & Sackeim 1980) was already known to Van Biervliet (1899, p. 296). Furthermore, mention should be made of a highly curious book, *Le duplicisme humain* (1906), in which Camille Sabatier, a retired French politician, puts all previous research into one grand, unified perspective.

Finally, what about the use of history for present-day research? I agree that the older literature may yet have a stimulating role to play. However, I fail to see any broader significance of historical investigation for contemporary research. For, suppose a scientist has learned from history how to recognize a time-bound and socioculturally colored theory when he sees one (as Harrington exhorts him to do). Does this help him in his capacity as scientist? No; for no matter how clearly a theory may reflect its cultural or psychological origins, it can still turn out to be either true or false.

The many-mind problem: Neuroscience or neurotheology?

John C. Marshall

Neuropsychology Unit, University Department of Clinical Neurology, The Radcliffe Infirmary, Oxford OX2 6HE, England

No one in any of his or her right minds could now seriously deny that the hemispheres of human (and many animal) brains exhibit "complementary specialization." The aphasias (Lecours, Lhermitte & Bryans 1983) and high-level apraxias (Freeman 1984) are the best documented examples of deficit consequent upon left-hemisphere lesion, and where comparable deficits are not typically seen after right-hemisphere lesion; contrariwise, performance on a number of visuoperceptual and spatial tasks is maximally impaired after right-hemisphere damage (Ratcliff 1979). There are other disorders – the amnesias (Squire 1982) and the prosopagnosias (Damasio, Damasio & Van Hoesen 1982) are the clearest representatives – where it would seem that the full-blown clinical syndrome only emerges consequent upon bilateral damage. (This does not preclude the possibility that the two hemispheres make a differential contribution, either qualitative or quantitative, to autobiographical memory or to the recognition of familiar faces.)

It is possible to argue that the normal brain is functionally symmetric, and that the appearance of asymmetric capacities is an artifact of overemphasis on the performance of lesioned brains. However, the fact that many of the asymmetries found after brain damage can also be shown in radiological and electrophysiological investigations of the normal brain (and in behavioral studies of lateralized stimulus presentation) makes such an interpretation seem, for the moment at least, somewhat perverse. Our best bet is that the human brain is indeed left-right asymmetric in many aspects of its normal functioning; and the discovery of these asymmetries can be seen as an (almost) unqualified success story for the progress of cognitive neuropsychology as a normal science in the century or so that has followed Paul Broca's very considerable amazement on finding that his aphasic patients had left-brain damage.

What morals, then, should we draw from Harrington's acute, amusing, and instructive analysis of some of the highways and byways that 19th-century neuroscientists explored en route to our current, perhaps fragile, consensus? The first moral is obvious, but important: Do not believe the third-hand accounts of the past with which textbook writers attempt to convince us that we are so much more clever and wise than our predecessors! It really will not do simply to acknowledge that Hughlings Jackson wrote a paper on the daulity of the brain. Harrington has performed an invaluable labour of love, digging around in the primary literature and presenting 19th-century views on the

duplex brain within a comprehensible intellectual framework. The past is not another country and, besides, the wench is not dead.

From this follows the second moral: Those who know no history are destined to repeat it. What is particularly interesting, however, is which bits are repeated. As a good historian, Harrington argues that discoveries are made in a particular ideological context and interpreted in the light (or darkness) of prevailing sociological conditions. I am both more and less convinced of this than is Harrington.

First, less. It is, I think, a considerable tribute to the self-correcting nature of scientific argumentation that so many seemingly good ideas were so rapidly shown to be nonsense; Burq's metalloscopy, Charcot's hemihypnosis, and, of course, Gall's craniology are compelling examples. Anyone can make a mistake (David Ferrier once localized primary visual functions in the angular gyrus), but errors of this nature are quickly sorted out and do not tend to recur.

On the other hand, speculations about how many minds, selves, or souls a single brain can support do recur. These arguments have, at best, a wildly tangential relationship to the data that purportedly sustain them; it is thus hardly surprising to find that sociological constraints determine, in large part, the metaphorical interpretation of results. Localizing a fairly well-defined psychological function, color perception, for example (Zeki 1980), is one thing; localizing the yin and the yang is something else.

Contemplate the following three statements:

> Different parts of the brain have different functions; these parts can be differentially active in the normal brain and they can become radically disconnected from each other after brain lesion.
>
> "Each mental organ", says one of our cleverest phrenologists, "speaks its own language and understands the language which it speaks itself." (Lange 1925, p. 121)
>
> The new idea that emerges from these data, is that, quite literally, there are several selves to man, and what I want to argue is that they are not necessarily conversant with each other internally. (Gazzaniga 1978, p. 233)

In either the 19th or the 20th century, moving from the first to the third of these remarks is an enterprise fraught with more than scientific considerations.

Two hemispheres do not make a dichotomy

A. David Milner

MRC Cognitive Neuroscience Research Group, Psychological Laboratory, University of St. Andrews, Fife KY16 9JU, Scotland

Harrington's essay is fascinating as a summary of 19th-century writings on the topic of cerebral hemisphere asymmetry and interaction, but, more important, it highlights instructively how prevailing attitudes influenced and moulded the ideas that were put forward. If we can see this from our 20th-century vantage point, I have no doubt that our own excesses in this area will be equally evident to a 21st-century historian of science. Of course, scientific theories are never conceived in a cultural vacuum, and indeed in an important sense theories can only be successfully explanatory if they provide suitably familiar metaphors or analogies and are consistent with an existing conceptual framework. Likewise, neuropsychology is not unique in science in its love of the simple dichotomy. Not only is it didactically convenient to contrast nature versus nurture or waves versus particles, but there is evidently a profound human need to attempt to impose an order on nature. It is difficult, however, to resist the feeling that the topic of hemisphere specialisation is unusually prone to excesses of both these kinds at the present time, just as it was a hundred years ago.

The main impetus to such theorising in recent times was provided by the behavioural studies of "split-brain" animals, and subsequently humans, initiated by Sperry and his coworkers in the 1950s and 1960s. Yet very little knowledge about human cerebral asymmetry emerged from this work that was not already known from studies of unilaterally brain-damaged individuals. One can only infer from this that the excitement created by the "split-brain" work derived instead largely from the separate (though related) evidence that the disconnected cerebral hemispheres, though not alike, could function independently. This conclusion has led almost imperceptibly and perhaps inevitably to the totally unjustified inferences that nondisconnected cerebral hemispheres might be able to function independently, and that different individuals might rely on one hemisphere rather than the other in their cognitive "style" (see Beaumont, Young & McManus 1984). (In parallel, discussions as to whether the "split-brain" patients could be said to be in possession of two "minds" led in some quarters back to speculations that intact individuals might have, and be capable of separately activating, two "minds"). One result was that the late 1960s' antiscientific ethos and fashion for meditation and mysticism became incorporated into a 1970s' pseudoscientific identification of realism and logicality with the left hemisphere, and imaginative and intuitive thought with the right. Such myths continue to pervade our culture in the 1980s. (The glorification of irrational thought has been built into some of these modern distinctions; such thought was relegated to the right hemisphere in the 19th century too, by some theorists, but evidently it was at least disapproved of.)

These wilder flights of fancy (many of which are still taken seriously outside mainstream academic psychology) have been accompanied by a host of "respectable" hypothesised distinctions between the hemispheres, so that information-processing metaphors like "digital vs. analogue" and "serial vs. parallel" can be listed along with "yin vs. yang" and the others. Harrington revealingly compares such a modern list with its 19th-century equivalent. Our society has thankfully changed in some ways, such that, for example, racial stereotypes are no longer incorporated into our neuropsychological theories (although sexual stereotypes there still may be in some cases, albeit in a more subtle form). But despite the differences, it is striking that the search for a simple characterisation of the brain's functional asymmetry has been powerfully rehabilitated after a long hiatus during the first half of this century. As in the last century it is once again tacitly assumed that the characterisation should be along "psychological" lines, that is, that the hemispheres differ in ways similar to the ways in which people differ or in which an individual differs from one occasion to another.

There is actually no unequivocal evidential support for any of the proposed functional distinctions between the hemispheres though many do have a genuine basis in fact. The most secure is that most closely tied to the classical neurological data, namely, verbal versus nonverbal/visuospatial (the first on Springer and Deutsch's modern list as presented by Harrington). Yet as a recent analysis of lesion data (Bryden, Hecaen & De Agostini 1983) shows, even this pattern is far from universal. Contrary to widespread belief, the evidence indicates that only 72% of right-handed males, 46% of right-handed females, 26% of familially sinistral left-handers, and 35% of nonfamilial left-handers show the classical pattern of left-hemisphere specialisation for language and right-hemisphere specialisation for spatial ability. This leaves almost half of the general population unaccounted for. Although a few people appear to show the opposite pattern, verbal/spatial complementarity in either lateral direction occurred in an estimated total of only 73% of right-handed males, 55% of right-handed females, 29% of familially sinistral left-handers, and 42% of nonfamilially sinistral left-handers. It clearly follows that a large number of people must share control of these capacities within one hemisphere. Most important, Bryden et al.'s analysis also revealed that hemisphere specialisation for language was statistically independent of hemisphere specialisation for visuospatial ability; that is, these two tenden-

cies in the population cannot be causally interdependent. (Actually one cannot exclude the possibility that the two might be partially and separately cocaused by common underlying determinants.)

These sobering calculations suggest that the search for an explanatory dichotomy for the cerebral hemispheres is totally misguided. There may be several real "gradients" of specialisation between the hemispheres (e.g., for linguistic comprehension, for spatial analysis, for gestalt perception, for emotional arousal, for complex motor control), each of which will favour one or the other hemisphere (c.f. LeDoux 1983), but each of which is determined independently of the others. Of course some such gradients will tilt in a given direction for nearly all people (e.g., the gradient for expressive speech), others will do so for most people (e.g., spatial ability), while yet others may tilt with a roughly equal incidence in either direction. These last might be invisible in neurological group studies, but could be detectable in other kinds of research. Such a series of gradients could result, through their interactions, in a wide variety of different cognitive types. Ultimately they should be open to characterisation in processing rather than psychological terms.

Once one begins to entertain the possibility of such independent gradients, it becomes increasingly apparent how narrowly restrictive are the persistent attempts to construct dichotomies between the hemispheres. It also becomes unsurprising that a variety of studies have found only weak relationships between tests of laterality and tests of inferred hemispheric preference (Beaumont et al. 1984). In the light of these considerations, it might be appropriate not only to abandon the search for *the* hemispheric dichotomy, but indeed to abandon the widespread tendency even to talk of hemispheric dichotomies at all. Our knowledge is only of a certain number of capacities for which in a given individual there may be a quantitative *difference* between the hemispheres. It adds nothing to the scientific status of such findings to pair some of them together as apparent opposites.

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Lateralization and sex

Ursula Mittwoch

Department of Genetics and Biometry, University College London, London NW1 2HE, England

Harrington attributes to Delaunay the idea that the two sides of the brain are analogous to, and responsible for, the presumed thought processes of males and females. The author also suggests that once the two hemispheres have been given sexual identities, the idea of male cerebral dominance becomes an apt metaphor for the social and economic relationship between the sexes in the 19th century. Lest it be thought that male chauvinism is an invention of the 19th century, attention should be drawn to the fact that an association of the supposedly superior, dominant side of the body with maleness and of the inferior, weaker side with femaleness predates the discovery of the lateralization of the cerebral hemispheres by two and a half millennia. Two new aspects of the association between body side and sex that developed in the 19th century would seem to be the extension of the concept of lateralization to the brain and the inversion of the relationship between right and left.

Before Broca's (1865) finding of the major role played by the left cerebral hemisphere in the production of speech, the right was not only regarded as the superior side, but was also associatd with maleness; whereas femaleness went with the left, inferior side. The *Pythagorean Table of Opposites* lists two adjacent pairs – right and left, male and female (Lesky 1951; Lloyd 1973). According to Aristotle, the larger claw of lobsters and crabs is situated on the right. Although this statement proved to be erroneous (Thompson 1919), Aristotle concluded that the right side is naturally superior to the left. That this notion has been well and truly integrated into our own sense of values is evident by the meanings of words like "right," "righteous," "dextrous," and "adroit," on the one hand, and of "sinister," "gauche," or "left-handed," on the other (Mittwoch 1977).

Just as the right side is superior to the left, so the male is superior to the female. Aristotle believed that the male is characterized by an abundance of the superior element, fire, and the qualities "hot" and "dry," while the female has an abundance of water and is therefore rather cold and wet (Gordon 1979; Aristotle 1979).

Given the relationships between right and left and between male and female, only one further assumption was required to arrive at a consistent theory of sex determination. If we add the additional postulate that the right side is hotter than the left, the conclusion follows that males are associated with the right side and females with the left (Lesky 1950; Lloyd 1973; Mittwoch 1985). This theory was open to some variation. Parmenides (5th century B.C.) seems to have thought that the sex of a child is determined by its position in the womb, males being on the right and females on the left, whereas his contemporary Anaxagoras thought that the father was responsible for the sex of his offspring, semen from the right side becoming male and from the left side female. Aristotle (4th century B.C.) criticized both theories, since he had evidence from dissections that fetuses of different sex could be found on the same side of the uterus and also that males with only one testis could father children of either sex. However, such factual details did not seriously disturb the right–left theory of sex determination, which in fact persisted until the discovery of sex chromosomes, that is, the beginning of the 20th century (Meisenheimer 1930; Mittwoch 1985).

The theory put forward by Broca in the second half of the 19th century that the superiority of the right hand had its origin in the left cerebral hemisphere threatened to invert the right–left theory. This new development paved the way for the seemingly innovative idea put forward by Delaunay and other authors cited by Harrington that the superior thought processes of the male had their origin in the left, superior hemisphere, and vice versa that female thinking is associated with the right, inferior hemisphere. It would seem, however, that this break with the past was of interest mainly to specialists and did not make much impact on the general public. After all, right is still right.

During the course of the last century, the battle against sexism has met with considerable success so that the social and economic domination of men over women has been markedly reduced. By contrast, lateralism still reigns supreme. Even though the stigma of left-handedness may be diminished, we are a long way from putting into practice the recommendation by Plato that children should be taught to use both hands equally, just as they learn to use both feet. "Through the folly of nurses and mothers we have all become lame, so to speak, in our hands" (Plato, in *Laws*, quoted by Lloyd, 1973, p. 185).

Irrespective of the exact relationship between lateralization of the brain and handedness, there can be little doubt that, if we wished, we could be ambidextrous.

What textbooks between 1887 and 1911 said about hemisphere differences

David J. Murray

Department of Psychology, Queen's University, Kingston, Ontario, Canada K7L 3N6

My first thought on putting down Harrington's fascinating target article was "Why is most of this new to me?" I have admittedly made no attempt to specialize in the history of physiological

psychology, but even in the reasonably extensive reading I did for a paper on experimentation in memory in the late 19th century (Murray 1976), I never ran across much of this work. It occurred to me that one reason I was ignorant of much of the research on laterality was that, as I acquired knowledge in psychology over the years, it was by a fairly consistent route: consult a textbook, then go to the primary literature. Works that did not get into the textbooks were simply unexplored except insofar as the primary literature (journal articles, etc.) led to them. Accordingly, I did an informal survey of five of the most widely read texts on physiological psychology from the period 1887 to 1911.

I originally began by listing Harrington's 19th-century dichotomies as shown in Table 1, in the hope of finding out what each text said about each dichotomy, but the exercise was fruitless: I could find no mention of such ascriptions as intelligence/emotion, male/female, or reason/madness to the left/right hemispheres respectively. In fact the texts all had in common a relatively complete eschewal of wild speculations about hemispheric functioning and focused fairly narrowly on the fact that speech seemed to be more localized in the left rather than the right hemisphere. The details are worth spelling out, however, for they reveal the slow process by which original data became accepted into the corpus of knowledge thought respectable enough for the consumption of textbook readers.

Wundt (1887) gave a diagram of the left hemisphere in which he distinguished between a motor speech area in the frontal lobe just anterior to the Sylvian fissure and a sensory speech area in the upper temporal lobe. One of his sources for believing that speech was located in the left hemisphere was evidence that suggested that aphasia was more often associated with right than with left hemiplegia. I could find no other suggestions about the distinctive roles of the two hemispheres, but Wundt was quite speculative about the frontal lobes, assigning to them a center for "apperception."

Ladd's text (1887) was the English-language equivalent of Wundt's and widely read in its day. Part 2, Chapter 2, is on the localization of cerebral function. We read there how the new research inspired by Fritsch and Hitzig had led, in the previous 15 years, to an extensive search for localization of function in the cerebrum. Much space is devoted to the evidence for motor areas in the cerebral cortex, but Ladd chiefly relied on the monograph of Exner (1881), also referred to by Harrington. This monograph was very detailed on motor dysfunction following damage to the hemispheres and incorporated evidence that motor dysfunction could be associated with sensory dysfunction. Ladd writes, however, that

> Exner feels warranted in affirming that "the tactile cortical fields for the different divisions of the body coincide in general with their motor cortical fields." It is to be noted, moreover, that the percentage of the cases of disturbance of tactile sensations occurring on the right hemisphere is more than twice as large as that of the left. *Sensibility seems, then, to be the predominating function of the right hemisphere as motion is of the left.* (p. 284, italics Ladd's)

Later Ladd reviews the evidence that speech is localized in the left hemisphere but does not integrate his discussion with the passage just quoted.

James (1890) gives a picture of the left hemisphere similar to that of Wundt but with Wundt's "motor" and "sensory" areas now renamed after Broca and Wernicke, respectively. James disagreed with Wundt's speculations on the apperception center, and, although his recently reconstructed lectures on *Exceptional Mental States* (Taylor 1982) show his wide acquaintance with the fringe literature of psychology, he does not indulge in speculation about the two hemispheres other than to discuss their role in language mediation.

Ebbinghaus's text (1905) was less forbidding than Wundt's and was read widely by German psychologists at the turn of the century. He said less about the brain than any of the others, and his main concern in the first volume, chapter 2 (p. 11) was to establish in his readers a sense of caution about ascribing any function to any particular brain part – the difficulties of the topic are elucidated in a discussion of the functioning of the occipital lobes. There is little on the separate roles of the hemispheres.

Ladd and Woodworth's text (1911) updated that of Ladd (1887). There is much more extended discussion of aphasia, with a diagram of the left hemisphere showing Broca's and Wernicke's areas as well as an area in the middle frontal gyrus labelled "Writing?" The discussion of the motor areas is less cluttered than in the 1887 text, and the detailed discussion of Exner's findings is no longer there, nor is the passage quoted above. After an extended discussion of other areas of the hemispheres, Ladd and Woodworth stress the predominance of the left hemisphere in right-handed persons and then write:

> It would almost seem, from the evidence obtained, that the left hemisphere so completely takes charge of acts of skill, and of the intellectual processes concerned in them, as to leave nothing for the great bulk of the right hemisphere to do. Such a conclusion is, of course, in itself extremely improbable, especially in view of the equal size and inner development of the two hemispheres, but it must be admitted that the role of the right hemisphere, aside from the simplest sensory and motor functions, is not at all clearly made out. (p. 264)

Thus the ultracautious note sounded by 1911 in the textbooks. Even Hughlings Jackson was hardly mentioned in the books listed – only James acknowledged the possible truth of his "levels" theory of nervous functioning, and none of the texts referred to Jackson's theories of laterality. Whether the texts omitted speculative material because it was uncertain and likely to mislead the reader, or simply because it was difficult and made for a less black-and-white picture than the teacher desired, or both, is not clear. Even now the most subtle reward one can gain from an experiment in psychology is to have it mentioned in a widely read text. Textbooks are a sort of battlefield in which scientific contributions compete for space. If the contributions lack solidity or are overcomplex they are left out, and a generation of learners does not know about them until historians such as Harrington exhume them.

Continuity of thought on duality of brain and mind?

Jane M. Oppenheimer

Department of Biology, Bryn Mawr College, Bryn Mawr, Pa. 19010

Harrington has presented a more comprehensive enumeration and analysis than has hitherto been available of selected 19th-century ideas on functional asymmetry of the brain and its relation to the mind. She has concentrated mainly on work published in English and in French. Should this be reflected in the title? References to work in German are sparser, and one wonders why. Did interest in this area lack in Germany, where progress in physiology, including that of the brain, was burgeoning in the latter 19th century? An explanation is needed, or at least desirable.

It is far more difficult to comment on an article that concentrates on history than on one dealing with experimental findings. In both, facts are assembled and interpretations of them are attempted. But it is harder to judge interpretations of ideas than it is to evaluate conclusions from empirical or quantitative data, even when contemporary work is at issue. The problem becomes more acute for historical data, since both terminology and the connotations of particular words change with time.

I have no challenge to the facts that Harrington presents. My chief concern is that I wish that her discussion of actual influence of one thinker's ideas on those of others had been further extended. Let us consider Hughlings Jackson, for example. She correctly devotes much space to his ideas and says of him that his "reputation today may be even greater than it was in his own time." Retrospectively contemplated, one of the most fascinat-

ing aspects of the work of Hughlings Jackson was that it was inadequately appreciated, not only in his own time, but for some time after; if I remember correctly, it began to draw deserved attention only in the 1930s. Yet it did have its influences earlier. It is usually considered undesirable for authors who publish in specialized learned periodicals to refer to articles in encyclopedias, but the best recent evaluation of the work of Hughlings Jackson is found in the article about him in the *Dictionary of Scientific Biography* (Clarke 1973). Clarke points out far more clearly than does Harrington what Hughlings Jackson's ideas meant to others who worked nearer to him in time.

Harrington, nevertheless, is at her strongest when she describes and analyzes the thought of those she chooses to discuss. Her weakness is in the presentation – or rather nonpresentation – of her analyses as part of ongoing history. The first sentence of her abstract begins by saying that "This paper challenges the belief that lateralization has no history." Many of us know that *the study of* brain lateralization has a history; a few of us are even named in the third paragraph of the article, and we are only a small sample. The sentence in the abstract continues by saying that the paper "attempts to define the relevance of older ideas to present research." But that is exactly what it does not do, except in the most abstract terms in the very beginning of the paper and in the final conclusion. In fact, influences on late-20th-century research are mostly never mentioned. Late-20th-century research itself is barely mentioned, Bogen and Sperry permitted only a single reference each, both from the 1960s. Where is Akelaitis (1942, 1944)? Gazzanig (1972)? Geschwind (1965)? Jerre Levy (1971)?

The conclusion says that if the essay were in a history of science journal, it would not require justification. Indeed, it requires it even less in a journal concentrating on empirical evidence. The writing of generalities on the meaning of history is superfluous in such a paper as this. The uses of history would better speak for themselves if history were permitted to state explicitly which 19th-century ideas were specifically effective in stimulating what 20th-century thought, *and which were not*. Such a contrast might at least arouse speculation about reasons for continuities and discontinuities in history and science, and even about the origin of ideas.

Experiencing two selves: The history of a mistake

Roland Puccetti

Department of Philosophy, Dalhousie University, Halifax, Nova Scotia, Canada B3H 3J5

Embedded within the history of the concept of mental duality, about which Harrington has given us the fullest account so far, there lies a further notion that rests, I believe, on a simple mistake (see section 4.2). The mistake was shared, in fact, by members of the opposing camps on the question of dual mentality.

For McDougall (1911) the absence of signs of double consciousness where the corpus callosum was missing proved the unity of the mind was not dependent upon the integrity of the central nervous system. Descourtis (1890) thought that unifying factors provided by the two cerebral hemispheres' having shared a common body simply disguised dual consciousness in the disconnected state. Liepmann (1900), however, showed that under careful observation the loss of communication between the half brains could manifest itself, for example, when his right-handed patient with a callosal lesion was unable to write with the left hand upon verbal instructions to do so. Yet even this patient, Harrington notes gave no evidence that he "experienced himself as 'two selves,' or suffered any break in his subjective sense of unified identity – any more than split-brain patients of today."

In philosophy it is a pretty safe bet that that if something is inconceivable it will do no conceptual work in any direction. But such is surely the case with requiring of mental duality that it be *experienced* by someone without a corpus callosum (better, without any forebrain commissures, as in complete cerebral commissurotomy). To see why this is so, let us start with the assumptions of "folk psychology," namely, that each normal human being is an organism endowed with one brain that is the biological substrate of a single conscious mind. Given these assumptions, could I feel a sensation of yours, or you one of mine? Surely any sensation I feel is mine, independently of what you feel at or near the same time; and what you feel is your sensation quite apart from what I may be feeling just then. Consider: Even if our brains were electronically interconnected such that when someone (even I!) steps on your right foot, not only do you feel this, but the relevant neural events in your left somatosensory strip trigger mimicking events in the homologous area of my brain milliseconds later; still I do not feel your sensation, for there were two sensations of a foot being stepped on in quick succession to each other: yours and mine. Indeed, even if that primary receptive area of neocortex in your brain were anesthetized, so that there was only one sensation, mine, it would still *be* mine, though it was your right foot being stepped on that initiated the train of afferent impulses terminating in the illusory sensation I would have of *my* right foot being stepped on (in this indirect way I would be giving myself the sensation of my foot being stepped on by stepping on yours!). So it looks clear not only that sensations cannot be ownerless (Strawson 1959) but also that they are nontransferable between distinct conscious subjects, minds, or selves.

But then it is equally clear that both proponents and opponents of mental duality were mistaken in looking for evidence of it in experience: for no one could experience such a condition. Take the model proposed by Wigan (1844) and revived by Bogen (1969). If I have two cerebral hemispheres and each is the organic basis for a mind, then I, one and the same person, have two distinct minds. But as we saw in the folk psychology example, minds cannot introspect each other. It makes no difference if you internalize the two minds to my body. If there is another mind in my head besides the one now thinking what to say next in this commentary, I cannot have introspective access to its conscious contents with this same mind (based, no doubt, in the left cerebral hemisphere). If I could, they would not be two minds, but one. And if there *are* two such minds, then the one that is not writing this commentary cannot be *my* mind, but must be someone else's mind. What mental duality really implies is dual personhood in the same body (Puccetti 1973; 1977; 1981).

But to say that mental duality cannot be experienced is not to say that it cannot be evidenced in other ways. Take Liepmann's right-handed patient with a callosal lesion who was unable to follow verbal instructions to write using the left hand. What explains this inability? First, ipsilateral pathways are insufficient to allow the speaking left hemisphere to guide the left hand in writing. Second, while we normal right handers can (though clumsily) write using the left hand, we utilize the commissural relay system (left hemisphere–right hemisphere–left hand) to do so, and these pathways had been disrupted by the callosal lesion. Third, the only neurological resource remaining for Liepmann's patient in confronting this task was the mute, aphasic, and agraphic right hemisphere, which of course had never learned to write with either hand!

Now suppose someone asks of Liepmann's patient this simple question: Can he write? On the model provided by folk psychology, a single mind has been divided (or the cerebral hemispheres have been rendered functionally independent of each other) by virtue of the callosal lesion. So the answer has to be: Yes *and* no; he can write a letter with the right hand, but not

even a sentence with the left hand (though it is not paralyzed at all). This is, obviously, a disturbing response, but we are stuck with it so long as we persist in construing consciousness as spanning both hemispheres of a normal human brain.

Consider now the Wigan–Bogen model confronting the same question. On that view, the patient can write with "mind-left" but not with "mind-right." Since both minds are *his* minds, the answer has to be Yes, leaving unexplained why he can't do so using the left hand (for Wigan this is especially provoking, since he thought both hemispheres capable of ratiocination, presumably including putting words on paper). At the very least, a neurologist would find such a model unhelpful in explaining unilateral agraphia subsequent to callosal lesion.

Why not, then, reject both these models and say that lesion to the corpus callosum in this patient cut off cross input between the two cerebral hemispheres, each of which served as the biogical substrate of a distinct person? Thus the mute person on the right side of the head could not direct the left hand in writing, while the speaking and writing person based on the left hemisphere was unable, in the absence of cross–recross pathways between the two hemispheres, to direct the left hand in writing.

To speak this way is admittedly awkward and even misleading, for it suggests that persons are just human cerebral hemispheres. They are not, for – and this is why Harrington in the target article was right to say that McDougall need not have feared that mental duality threatened belief in a mind distinct from the body – brain processes, unlike mental acts, have no intentionality. They are not *about* anything, do not take objects or themselves have semantic content. It is this observation, known as Brentano's thesis to philosophers but increasingly overlooked by them for over 100 years, which guarantees distinctness of mind and brain. If mental duality is true, each human organism is the basis, normally, for two ongoing conscious lives rather than one, though selective pressure in all twin-brained species (all vertebrates) conceals this from us, by introducing an inhibitory mechanism that prevents each half brain from having introspective access to the conscious content of the other (to prevent, for example, a doubling of the subjective visual field), and in reflective species like Homo sapiens sapiens by giving us such a powerful drive to believe we talkers and writers are all alone in our heads that to date not a single split-brain subject has acknowledged the true state of affairs. It may be some consolation to recognize that our mute cerebral companion is probably aware of the truth, even if nonverbally, since from an early age he must have known he was not doing the talking or writing performed by that body. But of one thing we can be sure, namely, that the mute half brain did not learn this by experiencing two selves, because that is an inconceivable expectation.

Do we have one brain or two? Babylon revisited?

Aaron Smith

Neuropsychological Laboratory, University of Michigan, Ann Arbor, Mich. 48109

Since the Egyptian Surgical Papyrus over 3,500 years ago, the tortuous history of efforts to define the nature of human brain functions and the specific neuroanatomical substrata that underlie them reflects the gradual development of diverse, unrelated disciplines; the emergence, disappearance, and occasional reemergence or "rediscovery" of unique or specific human brain structure–function relationships; and changing Zeitgeists, social forces, and philosophic approaches. Until the revisions of Galenic dogmas and long-established teachings on human neuroanatomy by Vesalius (1543), various functions were localized in the cerebral ventricles. With the development of modern neuroanatomy, the cerebral mantle was arbitrarily and increasingly differentiated into lobes from two (anterior and posterior) to three (anterior, middle, and posterior) to, currently, four (frontal, temporal, parietal, and occipital).

Beginning with the frontal lobes, each of these variously defined gross structures became the successive focus of attention for varying periods. The current intense efforts to lateralize almost all human cerebral functions derives directly from recent studies of patients undergoing a revised surgical procedure – commissurotomy – for relief of intractable seizures. Although the efficacy of commissurotomy for eliminating seizures has received little attention, the findings in studies of the behavioral sequelae in a small number of epileptic patients have given rise to remarkable and extravagant generalizations (in addition to those cited by Harrington, see Smith, 1984, p. 310). Educators, commentators, psychologists, psychiatrists, music and art teachers, and laymen currently refer to the holistic, artistic, intuitive, nonhuman "right brain" and the intellectual, analytic, "human" "left brain." Unaware of the history reviewed by Harrington, authors increasingly focus on efforts to lateralize almost all human cerebral functions exclusively to either the left or right hemispheres in a manner suggestive of a dichotomania from which no specific human function may be safe.

Harrington's superb and scholarly review of the considerable preoccupation with hemispheric differences and "duality of mind" in the 19th century is therefore a salutary and timely contribution that will afford new as well as old, but broader, perspectives and contexts for evaluating the remarkable claims and extraordinary interpretations based on limited studies of a small number of "split-brain" patients. It is hard to imagine that the recent authors of such claims were aware of the striking similarities to their extravagant generalizations based on clinical findings in patients without commissurotomy in the last century. As Smith (1981; 1984) has pointed out, many of the conclusions on the exclusive localization of specific functions in either the left or right hemisphere are clearly incompatible with the results of studies of effects of left and right hemispherectomy for infantile epileptogenic lesions as well as for malignant tumors in adults. Moreover, comparisons of preoperative and long-term follow-up studies of patients with complete or partial commissurotomy have demonstrated that the nature and severity of deficits both *before and after commissurotomy* reflect the effects of *extra-callosal lesions* that were present before or may have been incurred during or after surgery. In view of the small number of patients and the numerous sources of ambiguity inherent in studies of this clinical material (Campbell, Bogen, & Smith 1981), Harrington's penetrating and careful review of the interpretations of findings in 19th-century studies will be of special interest to investigators planning studies of effects of commissurotomy.

The broader implications of Harrington's essay, however, have even greater relevance for students of brain functions. As she points out, the history of concepts of human brain structure–function relationships reflects the influence of sociocultural and technical advances in related disciplines that provide more accurate definitions and precise measurements of brain structure and function. In view of the significant advances in neurosurgery, neuropathology, neuroanatomy, and the emergence and development of neuropsychology and other related disciplines since the last century, how can we explain the recent recurrence of the preoccupation with dichotomization of human cognitive processes at this time? Is the "double brain" simply a manifestation of collective social preoccupations? Or is it possibly a recurrence of the same tendency to prefer a neat, simplified, and organized static mosaic model of the brain rather than the complexities of a dynamic model that was reflected in the victory of Broca over Jackson in their 1868 debate in Greenwich on the cerebral mechanisms underlying speech and the nature of the various functions involved?

Unlike all other paired and single organs, human brain cells do not regenerate. The human brain also differs from other single and paired organs in the vastly greater range of functions. It also apparently differs from other organs in its remarkable versatility and plasticity in compensating for loss of function resulting from brain injuries in adults as well as children (Smith 1984). Current descriptions of the two cerebral hemispheres as "twin brains" or a "double brain" are largely based on observations of different deficits resulting from commissurotomy and from seemingly similar differences in effects of left- vs. right-sided brain lesions. However, numerous reports have also described similarly unique differences between effects of frontal and posterior lateralized lesions and between cortical and subcortical lesions. Might not one be able to cite such differences as evidence that we have not only two but four, eight, or more brains?

Harrington's superb review of 19th-century studies of hemisphere differences is more than a timely and scholarly tour de force that should temper the current scientific and public enthusiasm and uncritical acceptance of the extravagant claims based on recent studies of a few "split-brain" patients. In devoting a considerable portion of her review to the brilliant studies of Hughlings Jackson, she calls attention to the limitations of current graduate training of students and teachers in brain and behavioral sciences. While she notes that Jackson's works have been "misunderstood or only partially appreciated," the simple fact is that the overwhelming majority of students and teachers in the brain and behavioral sciences have never read Jackson's prolific writings. Jackson's writing style is ponderous and lugubrious. One hopes, however, that the rediscovery of Jackson's work, especially his focus on principles underlying the organization or evolution of brain function, its disruption by brain injuries at various stages of cerebral maturation, and its compensation or restitution, will stimulate experimentalists as well as clinicians in the behavioral and brain sciences to follow the example of David Ferrier and integrate clinical and experimental approaches for further definitions of such principles.

Author's Response

Historical and scientific issues en route from Wigan to Sperry

Anne Harrington

Wellcome Unit for the History of Medicine, Oxford University, Oxford OX2 6PE, England

Ever since I began this research, I have felt that it would be exciting to engage in a dialogue with modern-day neuroscientists, and I am grateful to the unique *BBS* peer commentary format for making such a dialogue possible. I was stimulated and, by and large, gratified by the response my target article elicited. I have organized my reply into a seven-part structure, which I hope will prove to be broadly integrative, yet permit sufficient attention to individual remarks.

1. "Great Men" in the history of laterality

I am concerned here with questions and comments on specific individuals or groups of individuals, and I begin with **Lokhorst,** who tells me that my story stretches back longer than I think, and in particular that I have omitted a key figure in the history of laterality: the 4th-century B.C. Athenian physician, Diocles of Carystus. Having read both of Lokhorst's publications on Diocles's writings (Lokhorst 1982a, 1982b), I think he has called attention to a remarkable document, and I am glad to believe that this ancient physician may have been the first to entertain the idea of hemisphere functional specialization as a concept. To that extent, he does indeed deserve a place in the history of this area. I am undecided, however, about the further claim that Diocles's views on the nature of hemisphere specialization are directly comparable with the sorts of distinctions chiefly developed in the last 20 years. Because we do not know how Diocles came to be "right," there is no way of telling whether we are dealing here with an accidental linguistic correspondence, or a true conceptual one. Did Diocles choose to put "intellectus" on the left and "sensus" on the right for empirical reasons, or is it possible that the localization could just as readily have been the other way around? If the latter, might not the duality of the brain have served simply as a neat means of expressing physically the metaphysical distinction between sentience and intellect, which Lokhorst traces back to Alcmaeon and Empedocles?

Eling has focused attention on a considerably better known "Great Man" in the history of laterality: Paul Broca. He lays stress on Broca's cautious and meticulous treatment of the problem of speech localization and asymmetry, and distinguishes between Broca's "laterality as a means" approach to brain functioning, and the cruder "laterality as an end" approach favored by so many of his followers. I like this distinction, but wonder if it is true, as Eling suggests, that Broca's approach was therefore also less time-bound; less molded by metaphysical assumptions, or by the larger sociocultural ethos? On going back to the Broca passage Eling cites in his commentary, I was struck by the reason Broca (1865:336) gave for disclaiming a clear relationship between speech asymmetry and handedness. He saw no grounds for assuming, he said, that the "motor" and the "intellectual" part of the two hemispheres must (in left-handers, at least) be developmentally bound up with each other.

This distinction between "motor" and "intellectual" parts of the brain was derived from the reigning doctrine of Broca's day – mentioned by **Greenblatt** – that the cerebral convolutions were the *terra incognita* of the mind, and not analyzable in terms of sensory-motor reflex functioning. For Broca, "articulate speech" was a *mental* faculty (1861b:335), and its localization in the third frontal convolution entailed no belief that parts of the cortex might serve motor functioning. In the years immediately following Fritsch and Hitzig (and the rise of the distinction between "motor" and "sensory" aphasia), would Broca's caveat for caution – which seems so perspicacious in light of modern findings – have seemed wise, or merely outdated?

It may also be relevant, in considering the problem of how far Broca himself may or may not have been influenced by the wider sociocultural ethos, to note the extent to which Broca imbued his views on laterality (or functional asymmetry) with *moral* significance. In the target article, I spoke about his decision to conflate asymmetry with the capacity for "perfectibility" and to make it one of the "chief characteristics" of the human brain (Broca,

1869:393; 1877:527–28). Although I cannot develop the argument here, I believe that this aspect of Broca's scientific thought needs to be understood against a background of wider cultural concern at the time about the implications of evolutionary theory (Lamarckian, Spencerian, Darwinian). In a postevolutionary universe, what still gave substance to mankind's old claims to pride of place in the natural world?

Oppenheimer is concerned about the fact that there are fewer references to works by German figures in the target article than to English and especially French scientists. I have been less successful tracking down German sources, but is this fact due to a weakness of my historiography or could it reflect differences in national research interests? According to Oppenheimer (1976) the 2d series of the Surgeon General's Index Catalogue lists 22 late-19th-century articles about the double brain, written in French, English, and German. In fact, only *one* of the listed articles is German (Meynert 1891), and, ironically, this one turns out to have been incorrectly indexed. It is an interesting paper, but it is not about the double brain.

In the early 1860s, Wilhelm Griesinger – one of the most influential German psychiatrists of the nineteenth century – summarily dismissed the theories of Wigan and Holland as lacking in sufficient proof (Griesinger 1861:25–26). Could this have had an inhibiting influence on the rise of later German "duality of mind" theories? In contrast, could the greater French interest in brain duality reflect the fact that the French were particularly preoccupied with the pathologies for which the double-brain theories were intended to account (Binet 1889:8–9)? Similarly, could the fact that late century German aphasiology was dominated by the "geometric" orientation of Karl Wernicke have discouraged a tendency in that country toward the kind of broad dichotomizing one finds in Broca-influenced France? After all, with his Gratiolet-inspired explanation for language asymmetry, Broca had focused attention on hemisphere differences from the very beginning.

While I believe that there is a clear case to be made for different national foci of interest in this area, I certainly do not rule out the possibility that during the isolated, chauvinistic years after the Franco-Prussian war there was more German interest in the double brain than my research methods so far have managed to unearth. Quite by serendipity, I recently came upon a remarkable case of apparent "unilateral delirium" reported at the turn of the century by Eugen Bleuler (1902). It was not referenced in any of my primary sources, and it was also apparently unknown to the late Norman Geschwind (1965), who was well read in the German literature and interested in finding examples of "disconnection" syndrome. The patient Bleuler observed had lain in his bed, perfectly quiet and relaxed on the left side of his body, but in a state of violent delirium on the right side. The right hand grasped at ropes, chopped things with an axe, sowed seeds, and slung away invisible objects with great effort. Sometimes it would seize hold of the blankets and try to yank them away, and once it upset the patient's dinner. When this happened, the left hand readjusted the bedclothes, wiped the patient's mouth, and gave every appearance of remaining in contact with reality. The consciousness associated with the delirious right-sided activities had full command of language; the rational consciousness associated with the left hand was occasionally able to speak but was more limited in this respect. Bleuler interpreted this case in terms of independent hemisphere action, and he felt it might instructively be compared with Liepmann's recently published (1900) study of unilateral apraxia.

A number of the commentators spoke of the work of Hughlings Jackson. **Oppenheimer** wishes I had extended my analysis of Jackson to include a discussion of his influence, both on contemporaries and on later 20th-century figures. I make no apologies for having failed to address the vast and disputed question of Jackson's general influence on other figures (in addition to Clarke 1973, see Dewhurst 1982, and the bibliography in Ey 1975). What, however, about the fate of Jackson's views on the double brain? They received no mention in Head's (1926) very influential interpretation of Jackson's thought, chiefly (I believe) because they belied the image Head was trying to create of Jackson as a brave and lonely "holist." Head did discuss Jackson's views on mental duality and imperception, but he completely dissociated them from their anatomical framework, and hopelessly misrepresented them to boot. What of the influence of this aspect of Jackson's thought on his immediate contemporaries? With one possible exception, I think it was minimal, except for isolated ideas (e.g., that the right hemisphere serves "emotional" speech). The possible exception is Sigmund Freud. There is evidence – which I hope to lay out fully in a later publication – that certain aspects of Jackson's views on mental duality (especially the idea that thoughts must be "objectified") had a significant impact on Freud's concept of consciousness, as first enunciated in the *Project*.

Notwithstanding Jackson's undoubted importance, I would strongly resist any attempt to idealize him or his work. While I readily agree with **Smith** that there might be much to be gained from greater attention to some of Jackson's heuristic principles, I also believe that, interwoven with the exhilarating flashes of genius, there are ambiguities, paradoxes, and blind spots. This may be especially true for Jackson's idiosyncratic views on the double-brain. **Greenblatt** has called attention to some of the problems raised by Jackson's attempt to apply associationist principles to the problem of laterality. I will raise just one more. Because Alexander Bain's associationist psychology convinced Jackson that words were stored in the brain in the form of articulatory movements, he never wavered in his view that speech, both on its expressive and on its receptive side, was a motor function. Consequently, his theoretical system took no account whatsoever of the work of Karl Wernicke.

Finally, I come to Wigan, whose "patterns of discovery" have been eloquently defended by **Isler & Regard.** Although they raise a number of interesting issues, let me simply try to balance their portrait of Wigan as a scientist by pointing out that he was no less a moralist. In arguing that the cause of insanity is to be found in pathological brain functioning – specifically, in the incongruous action of the two hemispheres – Wigan was self-consciously opposing himself to the "monstrous" views of such men as Johann Heinroth (1773–1843), who argued, in contrast, that the main cause of mental illness was sin (Winslow 1848:504–5). The paradox of *Duality of Mind,* however, is

that even as it proclaimed reassuringly that insanity was caused by physiological malfunctioning and carried no moral taint, it affirmed that every individual had a *moral duty* to cultivate sufficient power over his two brains – to make each act as a "sentinel and security for the other" – in order never to succumb to the ravages of madness himself (with insanity, then, becoming an implicit failure to carry out this duty properly). Significantly, Wigan tells us that although he had been mulling over the problem of man's double brain for some 25 years he only felt compelled to "put pen to paper" after reading the Reverend John Barlow's 1843 popular tract, *Man's Power Over Himself to Prevent or Control Insanity.*

2. What is it like to experience two selves?

It is the belief of **Puccetti** that confusion over the true nature of mental duality has plagued most of the history of this subject. Both proponents and opponents of mental duality were mistaken in looking for evidence of it in experience, for no one could experience such a condition. Two minds cannot introspect each other, because the moment they did, they would not be two minds, but one. Consequently, what mental duality really implies is dual *personhood* in the same body.

I have no difficulty accepting the logical validity of **Puccetti**'s argument, but I wonder if he has correctly assessed the historical record. To begin, there is the rather banal objection that von Hartmann in 1869 seems to have made much the same argument as Puccetti has done, declaring that if one could join the brains of two men, they would no longer experience themselves as two selves, but as one (see section 4 of the target article). A more interesting sort of objection to Puccetti's historical verdict, however, lies implicit in the clinical material 19th-century clinicians used to support their belief in hemisphere functional independence. Take, for example, the case of "D," first recorded in the 1850s by a Dr. Jaffé. D always spoke of himself in the first person plural because, as he explained, there was some "other" person inside him – hidden in the left side of his body, while he occupied the right side. This "left D" was forever doing things against the will of the "real" D, on the right side. Autopsy revealed profound asymmetries between the two brain halves (Descourtis 1882:38; Luys 1888:524; Ribot 1891:127–28; Lyon 1895:108).

Then there was the case, reported in 1884 by Benjamin Ball, of a young man who found himself haunted by a stranger, a "Mr. Gabbage," whom he could see, but only from the bust upwards. Gabbage began to torment the young man with incessant questions and forced him to commit senseless and violent crimes. One day, while Ball was talking with his patient about his condition, the latter said:

> You are not up to date in science. You don't seem to know that one often has two brains in one's head. This is precisely what I have. Gabbage has the left brain, and I possess the right brain. Unfortunately, it is always the left side which gets the better of me, and that is why I cannot resist the advice of this man who seems to be an evil spirit or at least a malevolent person" (Ball 1884:37).

Judging from the sort of clinical material they used, I would wager that most of the 19th-century clinicians in question – had they thought about it – would have been glad to agree with **Puccetti** that mental duality really means dual personhood. I suspect, however, that they would have been more skeptical of Puccetti's further argument, that one can have no subjective experience of this phenomenon. The testimony of their patients would have seemed flatly to contradict this view. It is not really relevant whether (1) it is a sloppy use of language to call the cases cited above a doubling of the *self* (it is, perhaps, really an invasion of the "self" by some "other") or (2) neurologists today would agree that this sort of "mental duality" has anything to do with the duality of the brain. I wish to suggest only that 19th-century neurologists and alienists were not so incoherent in their thinking as Puccetti thinks; they were simply operating within a different conceptual and empirical framework than he.

3. Ambidexterity versus asymmetry

The review of **Harris** of ambidextral culture grounds that educational movement in its contemporary scientific and clinical base far more thoroughly than I was able to do. I should only like to add that, although John Jackson was the founder of ambidextral culture in Britain, I have recently discovered that he had an American predecessor. James Liberty Tadd, director of the Philadelphia Public School of Industrial Art, promoted ambidextral training as part of his effort to introduce comprehensive "real manual training" into the public school system. "I am firmly convinced," he wrote in his large and impressive manual (the cover of the first edition shows a young girl demonstrating "ambidextrous coordination in 4 directions"), "that the better and firmer the union of each hand with its proper hemisphere of the brain, and the more facility we have of working each together and also independently, the better the brain and mind and the better the thought, the reason and the imagination will be" (Tadd 1899:48). By 1900, according to E. N. Smith (1900:581), some 2,000 schoolchildren were regularly undergoing training and were said to have become "relatively sharper and more intelligent than others."

Harris has stressed that advocates of ambidexterity "were serious people struggling with serious ideas." Given this – and considering that they "promised remarkable psychological and medical benefits" – why, then, did they always remain on the margins of mainstream medicine and pedagogy? Perhaps at least a partial answer may be found in the fact – which I mentioned in my earlier remarks on Broca, and which **Corballis** has also pointed out – that there was and is a strong inclination to regard the functional asymmetry of the hands and brain as a sign of human uniqueness, something which sets man apart (and above) the rest of animal creation. The issue of human uniqueness is in turn closely tied up with the idea raised by **Bradshaw**, that both past and present fascination with the double brain may be partly a manifestation of a deeper existential urge to come to terms with human consciousness and its status in the physical universe.

In the 19th century, then, for every Belfast principal with a vision of ambidexterity as progress and liberation,

there was a neurologist who, on some barely articulated level, perceived it as a loss of civilized standards of existence, a return to animal status. Interestingly, in our own time, it seems that the earlier tension between asymmetry and bilateral training has been resolved, and this is why I do not believe that the analogy that has been drawn between the 19th-century ambidexterity campaign and the "two-brain" education movement of today is an exact one. People today are no longer seeking to promote functional *identity* between the two hemispheres. Instead, believing that the human brain is uniquely wired up to serve two fundamentally incompatible forms of cognition, they plead for a fair and equitable balance of power, based on mutual respect.

4. Our multiple left and right brains: Coping with dichotomies

The "love of the simple dichotomy" (as **Milner** has put it) was the focus of a number of the commentaries. In her supplement to my discussion of "male" and "female" hemispheres, **Mittwoch** explains the way in which masculinity became associated with the right side of the body in ancient Greece through a two-stage argument: (1) males are dominated by the superior humoral qualities "hot" and "dry"; and (2) the right side of the body is hotter than the left. I suspect Delaunay's (1874) identification of the left brain with masculine thought processes does not represent so great a break with the past as Mittwoch suggests, for it did not replace but simply complemented a belief in the masculinity of the right side of the body.

I do not know what the implications of this might be, but perhaps it is worth noting – in light of **Mittwoch's** comments – that Delaunay's interest in the "comparative biology" of right and left was initially aroused when, as an ambulance attendant in the Franco-Prussian war, he began to be struck by the fact that the left foot of soldiers suffering from frostbite was almost always more severely afflicted than the right. He relayed this curious fact to Brown-Séquard, who assured him that, according to certain researches of German origin, the right side of the body did tend to be hotter than the left.

It is no doubt true, as **Bradshaw** argues, that dichotomies serve a heuristic function in science; they are a way of playing "Twenty Questions with Nature." How far, however, does the "dichotomania" that has characterized the last 100 years of neurology represent a disinterested interpretation of data according to classical inductive methods, and how far does it represent something of quite a different order – something, as **Corballis** eloquently urges, in the realm of myth and metaphor? According to Needham (1979), simple relations of opposition serve as one of the most basic resources in the articulation of symbolic or mythical categories. Linking categories by antithetical pairs is not merely a way of imposing order onto experience. As a rule, linked pairs are perceived not only as opposites, but as unequal in rank or worth. Consequently, dual opposition systems are also a way societies enshrine certain moral and social discriminations, particularly between things judged sacred and things judged profane.

During the second half of the 19th century, the rise of evolutionary theory had made it possible to assign subhuman ontological status to the less desirable members of one's society (women, nonwhites, madmen, etc.); and to set up an opposition between this (right hemisphere/profane) category of subhuman individuals, and the (left hemisphere/sacred) category within which one placed one's self. Even as one tried to affirm one's superiority and distinctiveness, however, there lingered an uneasy awareness that one's human status was by no means immutable. The (left-hemisphere) processes of civilization and progress could be reversed; the (right-hemisphere) beast from whom one was descended still lurked within. Do we have here at least a partial explanation for the potency of the 19th century's vision of madness as a struggle for control between the two brains?

Corballis has identified some of the ways in the which the left/right dichotomy continues in our own time to carry the burden of social, political, and philosophical concerns. He also points out, however, that just because perceptions of the two brain halves seem to be influenced by broader human concerns, this does not mean that they do not also have some basis in neurological truth. Along these lines, **Marshall** suggests that the laterality theories most apt to reflect wider sociological constraints are those that have only a broadly tangential relationship to the data that purportedly sustain them. Although he readily admits that a great deal of nonsense has been said in recent years about localizing the yin and the yang, he affirms that cerebral complementary specialization is a fact – and that its discovery has been one of the (almost) unqualified success stories of cognitive neuropsychology. **Milner** disagrees. He finds no unequivocal evidence for any of the current functional distinctions between the two hemispheres (including the classic verbal versus nonverbal/visuospatial dichotomy), and he suggests that "the search for an explanatory dichotomy for the cerebral hemispheres is totally misguided." **Smith** is less emphatic but equally skeptical. He feels that static, dichotomous models of the brain gloss over the complexities inherent in the phenomenon of brain plasticity, and – following in the tradition not only of Hughlings Jackson but of such early 20th-century neurologists as Carl von Monakow and Kurt Goldstein – he makes a plea for a return to a more dynamic, evolutionary approach to higher brain functioning. It is intriguing to me, as an historian of science, that these experts, presumably responding to the same pool of data, can disagree so widely, not only over what they know, but over the very principles of their enterprise.

5. Building bridges between the past and present

There are two basic ways in which past scientific ventures may be related to apparently similar ones in modern times. I will call the first (with apologies to Barbara Tuchman) the "distant-mirror" approach. This approach aims to juxtapose a piece of the past against a piece of the present, and it hopes in this way to change and deepen the latter's perception of itself. The second – and more difficult – I will call the bridge-building approach. This approach tries to come to terms with the dynamics of scientific change – to show how the past turned into the

present. It lays stress on drawing a clear distinction between ideas that are similar because of a direct line of influence and ideas that, like Old and New World monkeys, have many important features in common but nevertheless seem to have evolved independently of each other.

Clearly, bridge building is an important part of the historian's task. Nevertheless, I cannot accept **Oppenheimer's** claim that, because I took the distant-mirror approach in the target article, I defeated my own aim of "atempt[ing] to define the relevance of older ideas to present research." One cannot speak intelligently about continuities and discontinuities between a past and present research tradition before one has struggled to come to terms with the literature and intellectual climate associated with the former. *Contra* **Eling,** I do not believe a cursory "Wigan–Broca–Wernicke–Jackson" review at the beginning of a research paper comes close to conveying an accurate sense of this earlier tradition. My aim in the target article was therefore to present a broader, more synthetic, and, one hopes, more contextually sensitive picture of the 19th-century literature than I believe has hitherto been available.

I agree with **Eling** that the goal of contextualization might have been met more successfully if had managed to make more links with the wider sociocultural ethos (although surely I made some). Yet – given the fact that the article was already well over the suggested maximum length – I wonder how I could have managed to do so convincingly and still have covered the same range of material. As things are, Eling criticizes me for having thrown too much work into a single heap, thereby presenting a "caricature of what laterality studies are about in general." I presume he means that I was not critical enough in separating what he would consider the sheep from the goats. But is this properly the historian's task? In general, my aim was to be neither more nor less critical than the 19th century itself, and to avoid hindsight judgments that – as **Bradshaw** has put it – tend unhelpfully either to applaud our predecessors for being so clever or to point out smugly from our superior vantage point how wrong they were.

Returning to the goal of contextualization, obviously in a full-scale study, much more could have been done. I could have spoken, for example, of the way in which a neurologist's stand on language localization in the mid-19th century served as a litmus test of his politics and ethics (cf. Hécaen & Lanteri-Laura 1977:54); of the implications of the fact that Broca's Société d'Anthropologie was known in French science as a focus for left-wing, anticlerical activity in French science (Hammond 1980); of the way in which ideas in physiology about division of labor and specialization of function were influenced by ideas current in political economy at the time (Schweber 1980:250–56); of the sociological and institutional context of Charcot's dramatic metalloscopy and hemihypnosis experiments (cf. Owen 1971); of scientific naturalism, evolutionary theory, the Franco-Prussian war, the literary tradition on man as a soul "divided against himself," and so on.

In a full-scale study, I could also have attempted some bridge building. **Bradshaw** asks whether interest in 19th-century ideas was maintained throughout the first half of the 20th century, or whether there was a hiatus. **Oppenheimer** wants to know which 19th-century ideas were effective in stimulating 20th-century concepts and which were not. My impression is that there was a hiatus and that much of the older literature fell into neglect or disrepute. Consequently (with certain qualifications), it was not effective in stimulating much of the 20th-century literature. The latter seems quite clearly to have taken its chief point of origin from certain clinical literature of the 1940s and 1950s, and especially from the animal "split-brain" and human commissurotomy work of the late 1950s and 1960s. In attempting to understand how this discontinuity between the two research traditions might have happened, I have experimented with a number of historiographic approaches (Kuhnian paradigm shifts; sociocultural levels of explanation; the changing fortunes of key historical figures such as Freud and Charcot[1]). I have paid particular attention to two key developments: (1) a growing conceptual and institutional rift between the clinical world of brain malfunction (neurology) and the clinical world of personality disorder (psychiatry), which led to the resurrection of an at least heuristic Cartesianism within medicine; and (2) a strong, if not absolute, swing in neurology away from the localizationist views of the 19th century toward a more unitary, equipotential view of cerebral functioning.

Leary and **Murray** have suggested some other approaches to explaining the discontinuity between the 19th-century literature and its 20th-century analogue. Leary, most intriguingly, finds an answer in the norms of science itself – in the Baconian clarion call to study nature instead of books. Because functional illiteracy in science is tolerated (and perhaps even encouraged), scientists are constantly losing sight of those parts of their discipline's history upon which they are no longer building. I can only concur with Leary's conclusion that this has the effect of sustaining the illusion of linear progress in science while at the same time probably reducing the rate of true progress.

Murray's argument is complementary to **Leary's.** He focuses on the people who play a key role in determining how much of a discipline's past will be remembered and which parts: the textbook writers. I am generally convinced by his argument, but I am just a little puzzled about James. It is not true that James failed to indulge in speculation about the two hemispheres other than to discuss their role in language mediation. In fact, he cautiously put forward the view that, in certain forms of personality dissociation, "the systems thrown out of gear with each other are contained one in the right and the other in the left hemisphere" (1890, I:399–400). In floating this idea, he laid particular stress on Myers's (1885) views on automatic writing, but referred as well to Maudsley's "instructive" (1889) essay on the double brain, and Luys's (1888) article in *Encéphale.* Wigan's book had been cited earlier (p. 390). Why did this passage make so little impression?

6. Comparisons between old views and new

Responding gamely to my distant-mirror presentation of the 19th-century literature on hemisphere differences and brain duality, **Černáček, Bradshaw,** and **Gruzelier** all attempt in various ways to compare old views and new,

or to look at the latter in the light of the former. Bradshaw compiles an illuminating list of recurrent themes but warns that we are really in no position to say how far such parallelisms might be due to similarities in the 19th- and 20th-century Zeitgeist, the constraints of external reality, or the modelling propensities of the human mind. Černáček is inclined to conclude gloomily that, because we are still today pursuing some of the same sorts of studies and disagreeing over many of the same sorts of conceptual issues, the "spiral of growth of human knowledge" must, after all, be "a rather low one."

Perhaps **Černáček** is right, but in all fairness I do not believe one can disregard the impact of genuine methodological advance upon the spiral of human knowledge, even within an ahistorical science. Improved methodology can define the borders of disagreement more sharply than may have been possible in the past, and it can help insure that what may have been a largely unsuccessful venture in one era might prove to have more lasting results in another. Only time will tell. The fact, though, that debates over hemisphere anatomical asymmetries (for example) now center on data derived from computer topography rather than from craniology or the weighing of brain halves at least offers the hope that modern studies may be less prone to the sort of unconscious bias (Gould 1981) that plagued a fair portion of the 19th-century work.

In addition, as **Gruzelier** notes, simply pointing out similarities between research interests may obscure differences in the conceptual framework within which scientists interpret their results. The intellectual environment has changed considerably in the century between Luys's first (1879) suggestion that the right hemisphere predominates in the mad, and Gruzelier and Flor-Henry's (1979) collection of studies on hemisphere asymmetries in psychopathology. Gruzelier particularly stresses how much effort was devoted in the early 20th century to elucidating the place of impaired linguistic functioning in the phenomenology of schizophrenia. The symptomatic parallels between schizophrenia and the aphasias then made it reasonable to ask whether the former disorder might be dependent upon left-hemisphere dysfunctioning. In the target artcle I never intended to belittle the conceptual differences between the 19th and 20th centuries – quite the contrary. In the passage to which Gruzelier refers, I was only asking whether any of the older data might be interesting to researchers today.

7. Reflections on the uses of history

If it is clear from history that positive knowledge-claims in science interact with the wider sociocultural milieu, how should this change one's view of contemporary work? **Eling** suggests that an answer to this question would represent a significant contribution to science's ability to evaluate the different interpretations of laterality studies that can be found in the current literature. Both **Greenblatt** and **Leary** have, I think, made a start toward providing one. The possibility is raised of a self-critical science that consciously struggles to recognize the extrascientific assumptions that constrain it. It may not be able wholly to escape them, but at least it can acknowledge that they exist, and can temper its knowledge-claims accordingly.

Of the commentators, I have the impression that **Lokhorst** and possibly **Marshall** would be most vocal in questioning the genuine need for such a science. Marshall concedes that certain highly speculative forms of theorizing may recur throughout history, but he speaks of the "self-correcting nature of scientific argumentation" and affirms that errors such as Gall's craniology, Burq's metalloscopy, and Charcot's hemihypnosis "are quickly sorted out and do not tend to recur." He does not say, but perhaps implies, that given this inherent progressiveness of science, there is no practical need for scientists to remember and study such errors. I am less sure. Quite apart from the problematic status of the traditional idea of scientific progress in current philosophical debates, history itself makes me wonder whether it is true that, once errors in science are sorted out, they do not tend to recur (for an early repudiated version of metalloscopy, see Perkins, 1798; for hemihypnosis, see Braid, 1843:131–32, 138; for a later variation on Gall's craniology employed on hysterical patients, see Sollier, 1900).

Lokhorst takes a considerably stronger stand than Marshall. He declares that he fails to see how historical studies have any broader significance for contemporary research because at the end of the day, scientists are still not absolved of their responsibility of adjudicating between different knowledge-claims. Quite so; I am far from suggesting that questions about the truth-value of scientific ideas can or should be summarily reduced to questions about their relation to the wider sociocultural context. Suppose, though, that a scientist knows from history that he is working in a field that is prone to certain sorts of easy generalizations, philosophical pitfalls, and influence from extrascientific quarters. Could he not use this knowledge to bring into focus certain contemporary issues and problems that would otherwise be more difficult to see? If so, there seems every reason to suppose that a knowledge of history must, in the final analysis, enhance his critical skills as a *scientist.*

NOTE

1. I share **Isler & Regard's** suspicion that there is a link between the late-19th-century reaction against the excesses of the Paris school of hypnosis and the early-20th-century aversion to Wigan-style mental duality theories.

References

Editorial Note

The following bibliographic note, entitled "Problems of hemisphere differences and 'duality of mind' in the Russian literature" was submitted by S. M. Blinkov, Institute of Neurosurgery, Moscow 125047, USSR. I would point out the book by N. N. Bragina and T. A. Dobrocotowa, *Funktionelle Asymmetrien des Menschen,* Leipzig, VEB G. Thime, 1984, p. 330, Übersetzung aus dem Russischen L. Pickenhausen, though containing only two important references published in the 19th century (Baldwin, I. M. (1890), "Origin of right and left handedness," *Science* 16:242, and Baldwin, I. M. (1895), *Mental Development in Child and the Race,* New York,

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