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Biological Contributions to the Development of Psychology

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INTRODUCTION

This chapter concerns several important influences that biology has had on the development of psychology as a science. Specifically, we attempt to account for an apparent paradox: In the nineteenth century, rapid advances were made in relating biology in general and brain function in particular to the phenomena of mind. Throughout much of the first half of the twentieth century, however, these same relationships were all but ignored and the foundations for a scientific psychology were sought in the environment.

The biological aspects of psychology, perhaps more than other special branches of the discipline, resist historical compression. Psychobiology, as we shall call the subject,[1] is deeply rooted in both philosophy and

[1] There is still no universally accepted criterion for distinguishing among the terms physiological psychology, psychobiology, neuropsychology, and biopsychology. A growing convention would reserve the term neuropsychology to theory about the human nervous system based on research involving complex cognitive processes, often in settings in which clinical findings are directly relevant. Physiological psychology strikes many as too restricted, for much current work falls under headings such as biophysics, computer science, or microanatomy that are synonymous with physiology. Thus, psychobiology is used here to refer to the broadest range of correlative studies in which biobehavioral investigations are undertaken and referenced to phenomenal experience.

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biological science and was the subject of speculative and experimental psychology 2 centuries before Wundt christened the Leipzig laboratory (1874). Thus, while it is generally agreed that instrumental conditioning received its first great impetus from the work of Bechterev (1911) and Thorndike (1943, 1965) and classical conditioning from that of Pavlov (1927), it would be hazardous to date psychobiology from these beginnings. The subject's history (see Robinson, 1981) could plausibly commence in Greece with Aristotle's *Historia Animalium* or *De Anima*, would surely include the works of Democritus, Leucippus, and Epicurus, and then proceed to a host of figures from Galen in the second century to Descartes in the seventeenth.

There has been one or another form of biologically rooted psychology for as long as there has been serious psychological discourse. No narrow range of time can be taken as introducing its modern expression. Owing to its dependence on the biological sciences, it has tended to spurt ahead with many of biology's significant advances, but the history of the specialty has been something of an ensemble of variations on a more or less constant theme. That theme, of course, is the uniqueness of the human mind as an expression of the uniqueness of the brain, which is itself a metaphysical rather than a scientific view: "In the middle ages thinkers were trying to discover proofs for the existence of God. Today we seem to look for proof for the existence of man" (Heschel, 1965, p. 26).

With these qualifications noted, it is still necessary to begin somewhere. The chapter will be developed according to the following outline: First we describe at some length the most influential nineteenth-century neurobiological contributions to psychology and trace their development to the present. We then examine the emergence of a twentieth-century psychology that eschews the brain in favor of evolution, forfeiting the already maturing psychobiology bequeathed by the nineteenth century. We pause to assay possible reasons why nineteenth- and twentieth-century psychologies are so different from each other, and we conclude by noting that toward the end of the twentieth century some rapprochment between nineteenth- and twentieth-century biological influences has appeared in the form of a prosperous cognitive neuropsychology again prepared to wrestle with the problem of relating mind to brain.

Taking the uniqueness of humans as the theme against which controversy and experiment alike were, in a deep sense, ultimately projected during these two centuries, it becomes easier to distinguish genuine fathers of our ideas from godfathers, grandfathers, and mere custodians. We begin with Franz Joseph Gall and the problem of localizing functions in the brain.

BRAIN SYSTEMS AS ORGANS OF MIND

Alexander Bain (1861) observed that "phrenology is no longer a subject of party heat or violent altercation. Men can support or impugn it with the composure becoming a purely scientific controversy" (p. 14). So many accounts of phrenology—the first discipline to study brain as the organ of the mind—are mere caricatures, and so few of Gall's voluminous contributions have been available, that the modern student may still be confused as to just what this "purely scientific controversy" was all about. It involved at once a thesis, a method, and a set of implications. The thesis was expressed most economically by Gall himself (1822–1825/1835, vol. 1):

> If . . . man has faculties which essentially distinguish him from the animal, and which give to him the peculiar character of humanity, he also offers in his brain . . . parts which animals have not; and the difference of effects is thus found to be explained by the difference of causes. (p. 103)

Certainly these are not the words of some misguided popularizer (though popular they may have become) to be relegated to the dusty attic of history. Rather, they evoke the timeless issue of just what it is that gives rise to our humanity. Their contemporary ring is reflected in passages culled from today's concern with this very issue:

> To begin with, let us assume that it makes sense to say, as we normally do, that each person knows his or her language—that you and I know English, for example—that this knowledge is in part shared among us and represented somehow in our minds, ultimately in our brains, in structures that we can hope to characterize abstractly, and in principle quite concretely, in terms of physical mechanisms. When I use terms such as "mind," "mental representation," "mental computation," and the like, I am keeping to the level of abstract characterization of the properties of certain physical mechanisms, as yet almost entirely unknown. There is no further ontological import to such references to mind or mental representations and acts. In the same way, a theory of human vision might be formulated abstractly in terms of certain modes of representation (say, images or stick-figure principles) that determine the nature of such representations and rules, and so on. In the latter case the inquiry belongs to the study of mind in the terminology that I will adopt, though it need in no sense imply the existence of entities removed from the physical world. (Chomsky, 1980, p. 5)

Gall was thus not the last, nor of course the first, to advance the bold scientific claim that the brain is responsible for humanity. Julien de la Meetrie says as much as *L'homme Machine* (1748), and Pierre Gassendi (1644/1962) challenged Descartes' dualism on the same grounds in the seventeenth century. But Gall was the first to present such a thesis with an immense background of data drawn from anatomy, pathology, and clinical neurology. Indeed, when compared with this background, his much (and properly) maligned "bumpology" is seen to form but a small part of his overall program addressing the relationship between faculties of mind and anatomically separated portions of the brain. In defense of his thesis, Gall undertook brilliant and numerous studies not only of the cadaverous adult human brain and cranium, but of fetal brains, the brains of a wide variety of species, and brains and crania representative of a broad range of developmental stages. Thus his contributions to comparative and ontogenetic neuroanatomy were as great as those he made to psychology: "If, at some future day, naturalists should become better acquainted with the structure of the brains of animals, they may perhaps find in the brain the surest principle for the division into genera" (Gall, 1822–1825/1835, vol. 2, p. 113).

For his data, Gall relied on a combination of clinical, naturalistic, and post mortem observations. He never tired of examining the heads of prominent men of his own time, often going out of his way to meet someone whose achievements aroused interest in his skull! What was controversial about Gall's methods was not only what seemed to be their vulnerability to observational bias, but also their aloofness toward an experimental approach that was already becoming "official." Even more than this, Gall's methods were identified with a thesis as objectionable to the scientific community as it was to the still-dominant religious traditions jealously guarded by watchful clerics. Learning from his own observations that no two brains are identical, and that great individual differences are apparent even at the fetal stage of development, Gall insisted that no degree of environmental homogeneity would eradicate the innately established differences among humans, or for that matter among all animals of any degree of complexity. Coming on the heels of the 18th-century Enlightenment's vaunted egalitarianism, this was a thesis bound to excite enmity. Moreover, by insisting that each fundamental faculty (aptitude), intellectual or moral, is conditioned by a specific "organ" of the brain, Gall seemed to be rupturing the integrity of the ineffable self whose oneness was its signal feature.

By what mechanism might the faculties be coordinated and integrated into the self that is phenomenally experienced? Actually, it was mainly because of the potential dismemberment of the self that Pierre Flourens undertook to refute Gall's theory experimentally. The modern habit of crediting Flourens with toppling phrenology fails to convey the fact that in the Gall-Flourens dispute it was Gall, not Flourens, who upheld a rigorously scientific and antimetaphysical perspective. Flourens, proceeding from the premise that the self is indivisible, insisted that the cerebrum functions as a whole. So wed was Flourens to this metaphysical position that Gall was led to protest in desperation:

> It is in vain, that we demonstrate to the adversaries of the plurality of organs, that, from the lowest species . . . up to man, the cerebrum becomes more and more complicated. . . . Obstinately bent on explaining the simplicity of the *moi*, they see in all these incontestable facts nothing but a diminution of the simple cerebral mass. (Gall, 1822–1825/1835, vol. 6, p. 87)

Even a brief account of Gall's neuropsychology must make mention of his bumpology. It is important to appreciate that Gall did not claim that the cranial surface perfectly registers the morphological nuances of the cerebral mantle. In fact, it was Gall's own research that proved a less-than-perfect match. Nor did he claim that some specific region of the brain controlled intellect: "There are . . . as many different kinds of intellect as there are distinct qualities and faculties. . . . [A]n organ of intellect or understanding, is as entirely inadmissable, as an organ of instinct" (Gall, 1822–1825/1835, vol. 6, pp. 265–266). Accordingly, his theory was not, and could not be, overturned by any experiment purporting to find the survival of global functions following the ablation of specific structures. General problem-solving, for example, involves any number of more basic propensities, according to Gall, and will not be eradicated by the destruction of this or that small area of brain. In an almost ironic manner, therefore, Gall's system of phrenology is as much a "mass action" system as that proposed by Flourens and later by Lashley, but for fundamentally different reasons. Gall would defend a principle of mass action not in terms of most of the brain being devoted to a given faculty, but rather in terms of a given function arising from a multiplicity of faculties, each one of which depends on a specific organ of the brain.

This is not the occasion for either elegy or eulogy where Gall and his thesis are concerned. It is enough to note that he put the problem of the localization of function on the map of neuropsychology and cast it as a scientific problem to be settled by observation and experiment, a problem requiring careful study of the psychology of lower and developing organisms. Additionally, he did much to promote *characterology*—the study of personality—as a proper subject for a scientific psychology, or *bio*psychology.

Gall was one of the major pre-Darwin architects of the naturalistic and evolutionary perspective that would come to dominate psychology by the end of the nineteenth century. He rebuked those "who still love to believe that animals are only machines, automatons" (Gall, 1822–1825/1835, vol. 6, p. 118). Indeed, "the real detractors of the human species are those who think they must deny the intelligence of animals to maintain the dignity of man" (vol. 1, p. 94). Perhaps the Darwinian perspective is most clearly given in Gall's defense of his theory against no less a critic than Napoleon. On inquiring into the nature of Gall's science, Napoleon had been told that Gall "attributes to certain bumps [on the skull], [those] dispositions and crimes which [I, Napoleon, believe to be] not in the nature [of man] but [to] arise [solely] from the conventional arrangements of society" (vol. 6, p. 243). To this, Gall replies:

> In regard to my doctrines, the ideas and prejudices of Napoleon differ in no respect from those of the vulgar. What would become of the bump for thieving, if there were no property? Of that of ambition, if there were no society? What would become of the eye if there were no light?—but light exists . . . In the same way, property and society exist in nature . . . [N]either Napoleon nor his advisers has penetrated sufficiently far into the nature of things, to perceive that the organization of man and animals, is calculated for and adapted to the existences of the external world. (vol. 6, p. 243)

As with any major successful theory, there is danger that popular distortions and misunderstandings may develop, that sublime work and thought may spawn ridiculous consequences. This was the fate of Gall's contributions when they were popularized as the cult of phrenology. Soon, people were feeling each other's skulls in order to gauge their characters. In reaction to this vulgarization continued careful scientific work in the same tradition as that pioneered by Gall disavowed "localization," even when such localization of function was in fact found. Thus, Flourens has come down to us as an antilocalizationist, when in fact his major experimental contribution was to separate motor control from intellectual (problem-solving) abilities.

Specifically, Flourens showed that cerebellar resections impair motor functions, leaving problem-solving ability otherwise intact, while cerebral resections fail to interfere universally with motor function but leave the animal stupid: "The ablation of the cerebrum which abolishes intelligence has absolutely no effect on the control of reflexes and movements. The ablation of the cerebellum which abolishes all regularity of movements, does not in the least affect intelligence. This opposition [of effect] is remarkable" (Flourens, 1858, pp. 48–49).

BRAIN, REFLEX, AND WILL

By the end of the eighteenth century, the time of Gall's earliest contributions to psychobiology, a number of scientists had already made seminal contributions to an understanding of neural functions. Of all the mechanisms examined, the one that would come to serve psychology most significantly was the reflex. It was implicit in the mechanistic part of Descartes' philosophy, and although there was then no firm scientific support for the view, the possibility of a materialistic monism was envisioned. Descartes declared against this possibility, proclaiming instead his famous dualism based on *cogito ergo sum*. But nineteenth-century psychology, rooted in empiricism rather than in rationalism, was not to be convinced so easily.

The earliest truly systematic investigations into the organization of spinal reflexes were completed by Robert Whytt and appeared in the edition of his works published by his son in 1768. By studying the involuntary movements of decapitated frogs, Whytt clearly established that mechanical stimulation of the nerves was the causal antecedent of these movments. Comparing the time course of paralyses induced by vascular ligatures with those resulting from a sectioning of the nerves, he showed unequivocally that paralysis is not a correlate of reduced blood flow but a consequence of denervation. Further, he left no room for doubt regarding the power of the nerves to overcome "acts of will." In the natural state, "VOLUNTARY contraction is owing to the stronger action of the nervous influence upon any muscle, excited by the power of the will . . . In proportion as the *stimulus* is more or less gentle, so (*ceteris paribus*) is the contraction of the muscle to which it is applied (Whytt, 1768, pp. 9–10).

Georg Prochaska, a German physician, was perhaps the first to use the term "reflex" (or *reflexion*) in the modern psychological sense. His research was a conscious extension of Whytt's efforts though, like Whytt, Prochaska had no idea of the means by which nervous "power" is translated into muscular contractions:

> The reflexion of sensorial into motor impressions, which takes place in the sensorium commune, is not performed according to mere physical laws, where the angle of reflexion is equal to the angle of incidence, and where the reaction is equal to the action; but that reflexion follows according to certain laws, writ, as it were, by nature on the medullary pulp of the sensorium, which laws we are able to know from their effects only, and in nowise to find out by our reason. The general law, however, by which the sensorium commune reflects sensorial into motor impressions, is the preservation of the individual; so that certain motor impressions follow certain external impressions calculated to injure our body, and give rise to movements having this object, namely, that the annoying cause be averted and removed from our body; and *vice versa*, internal or motor impressions follow external or sensorial impressions beneficial to us, giving rise to motions tending to the end that the agreeable condition shall be maintained. . . . [T]his reflexion may take place, either with consciousness or without consciousness. (Prochaska, 1784/1851, pp. 431–432)

Prochaska's "general law" is an early formulation of the law of effect, couched in the language of sensory–motor integration. It was not until early in the nineteenth century, however, that Charles Bell (1811) discovered the anatomical basis of the reflex—that is, the division of sensory and motor functions of the spinal cord, a discovery later made independently by François Magendie (1822) and now generally known as the Bell–Magendie law. As the eighteenth century ended, Luigi Galvani published his observations of the electrical foundations of neuromuscular processes (1644/1954), finally providing an explanation of the power involved in translating sensations into movements. Galvani's work was controversial and did not succeed in narrowing the range of theoretical possibilities. Indeed, it was not until the middle of the 19th century that talk of "aethers" and "dephlogisticated air" gave way to the language of electrophysiology in discussions of neural mechanisms.

Between 1850 and 1880, largely through the achievements of Hermann von Helmholtz (1856–1869/1924) and Emile du Bois-Reymond (1848–1884), the electrical theory became official. Du Bois-Reymond advanced a polarization theory of neural excitation conceptually akin to late twentieth-century findings on graded potential changes (see Pribram, 1960, 1971; Schmitt, Dev, & Smith, 1976), especially as these occur in axonless "local circuit neurons" of the brain (Rakic, 1976). Thus, in the century beginning with the research of Whytt (circa. 1750) and culminating in Helmholtz's studies of neural propagation in the 1850s, the anatomy, gross physiology, and theoretical significance of the reflex were uncovered. The foundations for a mechanistic, materialistic integration of the human being had been laid.

Initial attempts at stimulation of neural tissue culminated in the mapping of the motor cortex of the brains of wounded soldiers during the Franco-Prussian war by Gustav Fritsch and Edward Hitzig (1870/1969). These investigators also carried out careful experiments that located the parts of the dog's cortex responsive to electrical stimulation:

> The starting point of the present investigations [was] observations one of us had the opportunity to make on human subjects, which concerned the first movements of voluntary muscles produced and observed after direct stimulation of the central organs.
>
> In the first *experiments* the animals (dogs) were not narcotized, but later the skull was trephined under narcosis, on an as far as possible plane area. The whole half of the calvarium, or only the part of it covering the anterior lobe of the brain, was then removed by means of cutting bone with forceps with rounded tips.
>
> Part of the convexity of the cerebrum in the dog is of motor character (this expression is used in Schiff's sense) and another part of the non-motor character.
>
> Generally speaking, the motor part is situated more in the anterior and the non-motor part in the posterior regions. Electrical stimulation of the motor part can produce combined muscle contractions in the contralateral half of the body.
>
> If very weak currents are used, these muscular contractions can be localized to certain narrowly defined groups of muscles.
>
> Using very weak currents, the possibility of exciting a well-defined group of muscles is limited to very small spots which may be called centres for the sake of brevity. A very slight shift in the position of the electrode still causes movement

> in the same extremity, but if initially the stimulus caused extention, for example, now, after the change of position, flexure or rotation would be evoked. (Fritsch & Hitzig, 1870/1969, pp. 353–355)

In both the laboratory and the clinic, these observations were extended over the next century. Careful mappings showed that the arrangement of cortical "centers" reflects the arrangement of the body musculature (but with distortions such that muscles serving finer and more complex operations are represented in a proportionally larger area). This arrangement—a "homunculus" when the human cortex is involved—accounted for the observation of Hughlings Jackson (1873) that grand mal epileptic seizures progressed predictably from one muscle group to an adjacent one. The thesis that the brain is a representational system was thus further established.

The great respect that neurologists and neurophysiologists attained for functions of the cerebral cortex in the nineteenth century made them suspect that these motor regions of the cerebral mantle were in fact the seat of the will (i.e., of volitional action). But it was not until the mid-twentieth century that experimental tests of this possibility proved feasible. By then it was possible to record electrical activity of the brain and relate it to behavior. Changes were shown to occur in recordings from the motor cortex not only when movement occurred, but also prior to those movements, and even in their absence when patients simply willed an act but did not express their will (Kornhuber, 1974).

Meanwhile, indications of a number of mechanistic and materialistic theories began to surface in the second half of the nineteenth century, although it would remain for Bechterev and Pavlov to contribute the necessary experimental procedures and findings to flesh out such theories. For example, Alexander Bain (discussed at length in Chapter 4, this volume), published two important volumes devoted to a biologically based psychology—*The Senses and the Intellect* (1855) and *The Emotions and the Will* (1859)—both of which remained authoritative in the English-speaking world for some thirty years.

The process of association was the linchpin of Bain's psychology, a process governed by Bain's version of Hume's laws of continuity, repetition, and resemblance. Since the time of Hume, British empiricist psychologists had generally defended the associational theory of ideas, as can be seen in the psychological essays of Thomas Brown, James Mill, and John Stuart Mill (one of Bain's close friends). By both diffusion and invention, the continental world of science had arrived at much the same place under the influence of Charles Bonnet, Johann Herbart, and others. The fact that David Hartley (1749/1970) had been obliged to rely on Newtonian "vibratiuncles," whereas Bain could speak in the more modern idiom of "nerve currents" should not be overestimated, however, for Bain avoided an uncritical materialism and reductionism such as that of August Comte, who would have all mental phenomena finally absorbed by the science of physiology. Instead, Bain took a dualistic position, and if he had any doubts, Mill's *A System of Logic* (1843/1874) would have removed them.[2] Compte's kind of radical reductionism was, at best, a metaphysical position, and at worst mere sloganeering. Bain (1861) puts it well in his book on character:

> It is not only incompetent, but wholly unphilosophical even in attempt, to resolve mind into brain, nerve and muscle; the things are radically distinct in their nature, as heat is different from gravity, or light from solidity; the true aim of the inquirer is to find the laws of their relationship. (p. 17)

Bain's assessment of phrenology was grounded in the judgment that Gall had been on the right track but had gone beyond the facts and rushed too quickly into the outer reaches of theory. What he found most commendable in Gall's efforts was just this habit of the inquirer: the search for lawful dependencies between psychological phenomena and brain processes. He also wished that the phrenologists had given more weight to the causal agencies of education and society, but Gall was actually less fatalistic than one would gather from Bain's critique. In any case, Bain's system of physiological psychology was entirely sensitive to the effect of environmental nuances, appreciating that the brain, too, was a malleable organ shaped by a history of sensations and nurtured by exercise, nutrition, and both formal and general education.

Herbert Spencer also sanctioned the biological approach to psychological issues in his *Principles of Psychology* (1855/1897), and men such as Théodule Ribot in France, Wilhelm Griesinger in Germany, and Henry Maudsley in England were now bringing psychopathology under the same explanatory scheme. Thus, the historical and conceptual lines from Whytt through Bain and his contemporaries and then to Pavlov were remarkably direct. A materialistic reflexology, a mechanistic monism, could perhaps account for the integrative function of the nervous system—the transmutation of sensations into action. But around each corner, and after each experimental result had been interpreted, the question of man's consciousness of this integrative activity, his awareness of awareness, remained to be answered.

[2] It is in Book 6, Chapter 4, sections 1 and 2, that Mill demonstrates the survival of a science of mind, no matter what the relationship between neural and mental events may prove to be. It may be noted that, in his preface to the 8th edition of *A System of Logic* (1874), Mill acknowledges several debts to Bain's *Logic* (1870). On the whole, Mill's expectations regarding a thoroughly biological science of the mind were tolerant but lacking in conviction.

BRAIN AND CONSCIOUSNESS

Turning to the world of the senses, we again find at the center the issue of the uniqueness of man with his subjective consciousness. Once again the approach is neurophysiological, but this time it is framed in an elegant and sophisticated psychophysics based on correlations between physics and reports of experience. One has merely to list names to call up the image of prodigious progress in this field of inquiry; for example, Johannes Müller, Ernst Weber, Gustav Fechner, Charles Sherrington, Hermann von Helmholtz, Ewald Hering, and Ernst Mach. To Müller (1833–1840/1852) we owe the notion that sensations are kept separated in neural processing, while to Weber (1851) and Fechner (1859) we owe the foundations of psychophysics and their famous law that the felt intensity of a stimulation is a logarithmic function of the physical intensity of the stimulus. Sherrington's work on the visual mechanism is less well known than his studies on the interactions among spinal reflexes, but in *The Integrative Action of the Nervous System* (1911/1947) he reviews his experiments on sensual fusion as examples of interactions among sensory events that, to some extent, parallel those of reflex events.

In his monumental *Sensations of Tone* (1863/1954) and the *Treatise on Physiological Optics* (1856–1869/1924), Helmholtz brought sensory psychophysics and physiology to a peak still unsurpassed in sophistication, thoroughness, soundness, and comprehensiveness. Modifications in detail, however, have engaged scientists since Hering's *Outlines of a Theory of the Light Sense* (1905/1964) challenged Helmholtz's trichromatic color theory with a four-color opponens–process mechanism. As DeValois has since shown, using microelectrode recordings from cells in the visual pathways (DeValois, 1960; DeValois & Jacobs, 1968), both Helmholtz and Hering were correct—Helmholtz at the initial processing level, Hering at somewhat later stages.

In his five editions of *The Analysis of Sensations*(1886/1959), Mach brings to bear his own and some earlier work on broader issues, as the title of one of his chapters indicates ("The Influence of the preceding Investigations on our Conception of Physics"—see also Ratliff, 1965). This joining of experimental results with the larger intellectual scene laid the foundation for logical positivism. It was then reflected back into psychology via the philosophers of the Vienna Circle as operational behaviorism, a scientific discipline firmly rooted in objective observations. It thus became possible for scientific psychology to attempt to banish introspective analysis to philosophy (for another view of this issue, see Chapter 13, this volume). The issue is once again one of mind and brain, and of pitting consciousness against behavior. As Mach (1886/1959) states:

> We may thus establish a guiding principle for the investigation of the sensations. This may be termed the *principle of the complete parallelism of the psychical and physical.* According to our fundamental conception, which recognizes no gulf between the two provinces (the psychical and the physical), this principle is almost a matter of course; but we may also enunciate it, as I did years ago, without the help of this fundamental conception, as a heuristic principle of research.
>
> The principle of which I am here making use goes further than the widespread general belief that a physical entity corresponds to every psychical entity and *vice versa;* it is much more specialized. . . . At the same time the view here advocated is different from Fechner's conception of the physical and psychical as two different aspects of one and the same reality . . . . When I see a green leaf (an event which is conditioned by certain brain-processes) the leaf is of course different in its form and color from the forms, colors, etc., which I discover in investigating a brain, although all forms, colors, etc., are of like nature in themselves, being in themselves neither psychical nor physical. The leaf which I see, considered as dependent on the brain-process, is something psychical, while this brain-process itself represents, in the connection of *its* elements, something physical. And the principle of parallelism holds good for the dependence of the former immediately given group of elements on the latter group, which is only ascertained by means of a physical investigation which may be extremely complicated.
>
> This principle has, moreover, always been more or less consciously, more or less consistently, followed.
>
> For example, when Helmholtz assumes for every tone-sensation a special nerve-fibre (with its appurtenant nerve-process), when he resolves clangs, or compound sounds, into tone-sensations, when he reduces the affinity of compound tones to the presence of like tone-sensations (and nerve-processes), we have in this method of procedure a practical illustration of our principle. (pp. 60–65)

The orientation of the nineteenth-century experimentalists was primarily biological, and it has been twentieth-century biologically oriented investigators such as Keffer Hartline, George von Bekesy, and Russel DeValois who have carried on this tradition. Hartline and Bekesy showed that Mach's differential equations accounting for perceptual contrast were a function of lateral inhibition in vision, audition, and somesthesis (see Ratliff, 1965). DeValois (1960) relates Hering's four primary opponens–process color theory to electrical recordings from cells in the lateral geniculate nucleus, the half-way station between retina and cortex in the visual system, thus relegating Helmholtz's trichromatic mechanism to the retinal receptors. The issue of Mach's mind-brain parallelism versus Fechner's multiple-aspect monism was left for the Vienna Circle to resolve, which they did in favor of Fechner (see Feigl, 1960). Not until the end of the twentieth century did Mach's dualism see a revival by one of his Vienna students, the philosopher Karl Popper, working with the neurophysiologist John Eccles (1976). This time, however, the dualism became a trialism involving the "mind world" of cultural artifact that can mediate interaction. In this way interaction, rather than parallelity, became the theme.

BRAIN AND FEELINGS

Brain processes do not operate in isolation from the rest of the organism. In today's computer language, the brain is a "central processing unit" ("CPU") connected to a set of "peripherals" that include input from receptors and output to effectors. Effectors are muscles and glands, and the muscles can be further classified as those moving the organism in its environment (usually striped muscles) and those involved in regulating the internal organs (viscera) of the body (usually smooth muscles). The receptors are of three kinds: (1) those excited by energies originating at a distance, which constitute our sense organs; (2) skin and muscle receptors, which signal what is happening at or near the body surface; and (3) receptors, which derive their input from inside the body—from the viscera, from chemical secretions circulating in the blood stream, and from various interactions among these sources of internal stimulation which, as we shall see, are the basis of our feelings.

The *Milieu Intérieur* and *la Vie Végétative*

Claude Bernard in the 1830s and 1840s performed a series of experiments showing that the brain is critically involved in the regulation of interactions among sources of internal stimulation: the milieu interieur. Previously, feelings had been attributed to the circulation of humors, or chemicals secreted by various visceral organs. In his *Leçons sur la physiologie et la pathologie du systeme nerveux* (1858), Bernard, a physiologist working with the body's metabolic functions and seeking to bring the experimental method into the teaching of medicine, reviews the work of François Magendie. It was the Magendie who distinguished sensory from motor functions of peripheral nerves by sectioning their dorsal and ventral roots as they connect with the spinal cord. On the basis of his own experiments, Bernard further distinguishes the *nerf grand sympathique*—the sympathetic nervous system—from both the sensory and motor systems. He goes on to discuss his famous "picure" experiments, in which he sectioned, or injured by a needle point, various structures in the cervical spinal cord and brain stem. These experiments showed that the sympathetic (and parasympathetic), autonomic, vegetative functions of the organism were under control of the central nervous system:

> The section of the spinal cord between the cervical and the brachial plexus does not simply produce this appearance of organic movements. One must ask, in

addition, whether this section produces only an excitation of the neural and motor systems in "une vie animale" but does not also produce the inverse in "la vie végétative." In actuality, one finds, after this operation, a diminution in the abdominal circulation and the energy of the functions of the abdominal organs becomes corroded; there is a diminution in the blood pressure, the urinary output and in secretory activities.

This operation modifies profoundly certain chemical and organic phenomena to such a point that, when that animal is also starved for several hours one finds his liver completely emptied of sugar, but filled, however, with glycogen.

It is good to note that it is not necessary to section the entire spinal cord: one can limit the cut somewhat to a fairly large prick: the effects are of the same nature. (Bernard, 1858, vol. 1, pp. 379–380)

The distinction that Bernard makes between *une vie animale* and *la vie végétative* has persisted in French physiology and psychology. Animal life is animated; that is, characterized by movement-in-the-world, whereas vegetative functions tend toward the maintenance of a *milieu intérieur*, or internal environment. Animation is oriented outward, vegetative processes are inwardly directed. In Bernard's view, man's vegetative life had to be thoroughly understood before his uniqueness in disease or in health could be assessed.

Bernard's pioneering work was carried forward by the Viennese scientists Johann Karplus and Alois Kreidl (1909), who applied their findings more directly to the human condition. They demonstrated hypothalamic as well as lower brain stem controls over vegetative functions, as well as distinguishing further between sympathetic and parasympathetic portions of the vegetative system and its central controls. They also emphasized the reciprocal catabolic–anabolic nature (metabolic use, metabolic buildup) of the functions of these systems. Together with the Viennese medical community, they applied their findings to psychopathology through a classification of the normally balanced psychic functions dependent on autonomic reactivity.

In the English-speaking community, the peripheral autonomic system was carefully charted by Langley (1900), who is responsible for naming it:

The sympathetic system, as we have seen, supplies nerve fibres to certain structures in all parts of the body. In some parts of the body these structures receive nerve fibres from other sources than the sympathetic. It is, in consequence, convenient to have some term to include the whole nervous supply. The words "organic," "vegetative," "ganglionic," and "involuntary" have all been used, but they have also been used in senses other than we require. The term "visceral nervous system" has been employed by Gaskell and others, including myself. The word "visceral," however, is obviously inapplicable to some of the structures brought under it, such as the nerve fibres which run to the skin. I propose, then, following a suggestion of Professor Jebb, to use the word "autonomic," including under that term the contractile cells, unstriated muscle, cardiac muscle, and gland cells of the body, together with the nerve cells and fibres in connection with them. (pp. 659–660)

The controls of the central brain stem (including hypothalamus) over the autonomic nervous system were studied by Walter Cannon (1929). Cannon enlarged on Bernard's conception of the maintenance of the *milieu intérieur* by developing the concept of homeostasis. In the hands of his student and colleague, Norbert Wiener (1948), homeostasis was given engineering precision by treating it as the mechanism of an error-correcting negative feedback, the basis of cybernetics (i.e., the theory of control systems).

Cannon also addressed the psychological import of his physiological work. Carl Lange (1885/1887) of Copenhagen had taken variations of vegetative functions of the organism to be the basis of emotion defined as the maintenance of stability or its disruption, and William James further developed Lange's ideas into an influential theory: Emotions were felt whenever bodily, and especially visceral, activity was initiated by a sensory input. Testing this idea, Cannon showed that cutting the nervous innervation to and from the viscera does not alter emotional reactivity, whereas electrical excitation of the hypothalamus does. Brain processes, not vegetative functions, are therefore responsible for emotional experience and expression:

Since visceral processes are fortunately not a considerable source of sensation, since even extreme disturbances in them yield no noteworthy emotional experience, we can further understand now why these disturbances cannot serve as a means for discriminating between such pronounced emotions as fear and rage, why chilliness, asphyxia, hyperglycemia and fever, though attended by these disturbances are not attended by emotion, and also why total exclusion of visceral factors from emotional expression makes no difference in emotional behavior. It is because the returns from the thoracic and abdominal "sounding board," to use James' words, are very faint indeed, that they play such a minor role in the affective complex. The processes going on in the thoracic and abdominal organs in consequence of sympathetic activity are truly remarkable and various; their value to the organism is not to add richness and flavor to experience, but rather to adapt the internal economy so that in spite of shifts of outer circumstance the even tenor of the inner life will not be profoundly disturbed. (Cannon, 1929, p. 358)

We note, however, that the part of the brain that Cannon found to be involved in emotion was, after all, the same part that Karplus and Kreidl (1909) and others had found to control vegetative functions. Of course, William James had never suggested that the peripheral visceral mechanism per se was working in isolation. Rather, he had made it clear that a report to the brain of visceral activity was critical:

> If the neural process underlying emotional consciousness be what I have now sought to prove it, the physiology of the brain becomes a simpler matter than has been hitherto supposed. Supposing the cortex to contain parts, liable to be excited by changes in each special sense-organ, in each portion of the skin, in each muscle, each joint, and each viscus, and to contain absolutely nothing else, we still have a scheme capable of representing the process of the emotions. An object falls on a sense-organ, affects a cortical part, and is perceived; or else the latter, excited inwardly, gives rise to an idea of the same object. Quick as a flash, the reflex currents pass down through their preordained channels, alter the condition of muscle, skin, and viscus; and these alterations, perceived, like the original object, in as many portions of the cortex, combine with it in consciousness and transform it from an object-simply-apprehended into an object-emotionally-felt. No new principles have to be invoked, nothing postulated beyond the ordinary reflex circuits, and the local centres admitted in one shape or another by all to exist. (James, 1890/1950, vol. 2, pp. 472–474)

Thus the James–Lange theory continued to dominate conceptualizations of the biology of emotions well into the middle of the 20th century.

La vie animale

At this point, new data enlarged the scope of theorizing. Lindsley (1951), working with Magoun and Moruzzi, had shown that destruction of the reticular formation in the core of the brain stem left the organism with only vegetative functions. On the basis of this observation, he proposed that the reticular system produced an activation of the brain, a proposal confirmed by evidence that electrical stimulation of the reticular formation led to a desynchronization of the brain electroencephalogram (EEG). Such desynchronization ordinarily accompanies alertness. These results led Lindsley to an activation theory of emotion in which *une vie animale* (animated, activated movement-in-the-world) rather than *une vie végétative* plays the central role. The following quotation defines emotion for Lindsley:

> As far as it may be considered a theory, the conception to be described here may be labeled an "activation theory." It is based largely upon recent findings concerning the electroencephalogram and particularly the interaction of the cerebral cortex and subcortical structures. The activation theory is not solely an explanatory concept for emotional behavior but relates also to the phenomena of sleep–wakefulness, to EEG manifestations of cortical activity, and to certain types of abnormal behavior revealed in neurologic and psychiatric syndromes.
>
> The theory rests mainly upon the following points, which are supported by experimental evidence:

1. The electroencephalogram in emotion presents an "activation pattern," characterized by reduction or abolition of synchronized (alpha) rhythms and the induction of low-amplitude fast activity.
2. The activation pattern in the EEG can be reproduced by electrical

stimulation of the brain-stem reticular formation extended forward into the basal diencephalon through which its influence projects to the thalamus and cortex.

3. Destruction of the basal diencephalon, i.e., the rostral end of the brain-stem activating mechanism, abolishes activation of the EEG and permits restoration of synchronized rhythmic discharges in thalamus and cortex.
4. The behavioral picture associated with point 3 is the antithesis of emotional excitement or arousal, namely, apathy, lethargy, somnolence, catalepsy, hypokinesis, etc.
5. The mechanism of the basal diencephalon and lower brain-stem reticular formation, which discharges to motor outflows and causes the objective features of emotional expression, is either identical with or overlaps the EEG activating mechanism, described under point 2, which arouses the cortex. (Lindsley, 1951, pp. 504–505)

As a counterpoint to Lindsley, let us note that common observation, as well as introspection, caution that something may be missing when emotion is considered simply in terms of activation. For example, weeping is not just more laughing, and fear is not just more love, although there is some truth to the notion of quantitative continuity in these processes. The suggestion thus arises that activation theory, while part of the story, is not in itself the whole story.

Emotion and Motivation

At about the time that Lindsley was developing his activation theory, a group of investigators in John Fulton's laboratory at Yale were demonstrating that both the limbic forebrain and the cerebral cortex were capable of regulating vegetative functions (see Pribram, 1961). These results shifted the locus of control from the brain stem (including hypothalamus) to the forebrain. Following James W. Papez (1937), Paul MacLean (1949) focused on the limbic systems (a ring of forebrain structures at the internal border—the limbus—of the cerebral hemispheres) and their connections with the hypothalamus as a "visceral brain" responsible for vegetatively based emotions. The Papez–MacLean theory thus followed the Bernard–James–Cannon tradition by bringing in ever higher order brain circuitry of control over vegetative functions. The trend, then, was increasingly centralistic and less peripheralistic.

Further experimental observations made possible a more comprehensive theory of feelings (see Pribram, 1984, and Young, 1943/1973) that included the humoral, visceral, and activation themes. This theory distinguishes emotion based on *une vie végétative* (visceroautonomic) and motivation based on *une vie animale* (activating) influences. It derived from a host of research results obtained and reviewed by Pribram (1981).

William James was in part vindicated by Pribram's comprehensive view. What it added was that a brain representation based in part on humoral and vegetative activity, rather than the humoral and vegetative activity per se, must be involved for emotion and motivation to be manifest. Pribram's theory also takes activation into account but differs from Lindsley's in that emotion, as in the classical theories, is conceived to be vegetatively based, while activation, (*une vie animale*) relates to motivation to action (or as James put it, "to enter into *practical* relations with the environment"). In keeping with Lindsley's views, however, since both emotions and motivations can be felt (i.e., experienced), it becomes important to identify feelings as encompassing both motivations and emotions. Feelings of zest for work, love for another person, and so on are as frequently experienced as are those of rapture in listening to music or falling in love. Thus, an overall theory of feelings rooted in neurohumors came to encompass an activation, *vie animale* theory of motivated actions *and* an arousal, *vie végétative* theory of emotional passions.

Finally, the work of Paul Ekman (1973) and Sylvan Tomkins (1962) on the expression of emotions elaborated a direction of research begun by Charles Darwin (1859/1964). In this tradition the variety and subtlety of feelings is delineated, a subtlety in humans that can have no other origin than the participation of the cerebral cortex. Neurophysiological studies have shown that the brain cortex participates in the regulation of visceroautonomic activities (Bucy & Pribram, 1942; Kaada, Pribram & Epstein, 1949; Wall & Pribram, 1950), and that decortication decidedly impoverishes expressions of appetites (e.g., hunger) and passions (e.g., sexual responsivity) in rats, cats, and rabbits (D. Oakley, 1981). Once again, William James's view is in large part corroborated. This time, it is his suggestion that emotions and motivations share in neural systems involved in other aspects of experience and behavior. The road from experiments on the regulation of vegetative functions to those producing some understanding of the feelings of humans took a little over a century. Not a bad yield in such difficult terrain.

BRAIN MODELS OF MIND

Toward the end of the nineteenth century, the popular fad for phrenology gradually faded, and attempts at the localization of function became respectable once more. A series of experimental results and sophisticated models sought to relate brain organization to relevant psychological processes. David Ferrier, Edward Schäfer, and Victor Horsley in Britain, and Friedrich Goltz and Hermann Munk in Germany, resected certain portions of animal brains and observed the effects on behavior. Their publications soon became common knowledge among biomedical scientists.

Models based on these data did not simply equate a brain locus with a psychological faculty; this particular error would once more appear in later popularizations. Rather, the argument followed the lines set forth by Gall: Mental phenomena (psychological processes) depend on the brain, much as respiration depends on the lungs.[3] Of course, no physiologist equates respiration with pulmonary anatomy, or even with pulmonary physiology. The function of respiration depends as well on red blood corpuscles, the hemoglobin they contain, and respiratory enzymes that facilitate the exchange of oxygen and CO_2 across membranes. Similarly, no physiologist, then or now, would identify a psychological process with a brain locus or even the functions of that locus.

It is true that the occipital lobes of the primate brain are centrally involved in visual processing (just as the lungs are centrally involved in respiration), and that other parts of the brain are only tangentially involved (just as the pancreas is only tangentially involved in respiration). However, this fact does not locate the psychological "in" the physiological process. Rather, it identifies and separates the structures involved in a process and specifies their function in the total system. Thus Paul Broca (1861) showed that language is ordinarily dependent on the left, not the right, cerebral hemisphere, and Carl Wernicke (1874) established that a relatively restricted region of the hemisphere is involved. In another classical study, Freud (1953) warns against the popular phrenological error of identifying locus and process. He presents a sophisticated model that accounts for the evidence of language impairment (aphasia) by lesions of the brain.

Freud (1895/1950) also undertook a much more ambitious task that he variously called a "Project for a Scientific Psychology" or a "Psychology for Neurologists." It was meant to be as complete and detailed a statement of the relationship between normal and abnormal mental processes and their brain substrates as evidence at the turn of the century would allow. Freud did not publish his model, but his teacher, Meynert (1890), and his colleague, Exner (1894), did publish models that were similar, if not as brilliantly conceived (see Pribram & Gill, 1976).

[3] Gall never tired of distinguishing between a cause and a condition. For him, brain physiology did not cause mind but served as its necessary condition.

These models are remarkably similar to those developed in later years. For example, the regulation of emotional and motivational activity is attributed to physiological drive stimuli impinging on core brain receptors. Such regulation is abetted by neurosecretions (from "key" neurons, in Freud's project) stated to be akin to adrenalin. Memory storage is due to the development of facilitated pathways in the brain through a lowering of synaptic resistance by use (Freud's law of association by contiguity). The cerebral cortex is identified as necessary for self-reflective consciousness in a manner not very different from that proposed by Lawrence Weiskrantz and Elizabeth Warrington (see Weiskrantz, Warrington, Sanders, & Marshall, 1974) on the basis of their "blind-sight" findings on patients with occipital cortex removals, or the proposals of Benjamin Libet (1966) based on his electrical stimulations of the postcentral cortex of man. Patients with blind-sight can identify the location and form of large objects in the part of the field (contralateral of the lesion) that by ordinary test (perimetry) and verbal report of their introspection is totally inaccessible to consciousness. They say they cannot see, that they are totally blind and guessing, even though their performance is 80–90% correct. The Freud-Exner and the Weiskrantz–Warrington theories, a data-filled century apart, are quite similar in essence.

Perhaps even more remarkable is the detailing in Freud's project of the cortical mechanism involved in conscious perception. Freud distinguishes the quantitative, intensive properties of sensory inputs from the qualitative properties that beget consciousness. These qualitative properties are a function of the patterns of periodicity of receptor discharge—that is, patterns reflecting the physical patterns of energy to which the receptors are sensitive. Goldscheider (1906) developed a similar model in some detail:

> The simplest conditions are found in the case of association within the same sensory domain, e.g., the visual domain. Let there be a simple visual object, e.g., a circular line.
>
> Hereby a certain number of ganglion cells of the visual center are excited from the periphery simultaneously or in immediate succession. From each one of these cells the excitation will propagate in the various directions which are indicated by the anatomical conduction pathways emanating from the cell. Each one of these receptive elements of the sensory domain (ganglion cells) can thus be considered as a center which radiates the excitations arriving from the periphery in the various directions like a bundle of force lines. The great majority will lose themselves without effect in the chaos of the fiber network, perhaps only stimulating it trophically. *Only where the force lines meet will they produce a special effect,* namely, as was elaborated above, *produce those unstable chemical agents.* The locations at *which the force lines meet are to be viewed as the resultants of the pulses of different intensities some of which may have originated simultaneously and some in short temporal sequence.*

> These locations will form a connected system of lines which can be viewed as a spatially connected bundle. I will call it a *node line* or a *force line* resultant. (p. 146)

The works of Fergus Campbell and John Robson (1968) and of DeValois, Albrecht, & Thorell (1978), to name a few of many contributors, have established that cells in the visual cortex are indeed "tuned" to frequencies (the inverse of period), but that these frequencies are patterns over space rather than (or in addition to) over time. (Physicists know such spatial patterns, when they are described as waves, by the term "wave numbers.") These findings have been augmented by the work of Georg von Bekesy (1957) in audition and somesthesis, and by Nicholas Bernstein (1967), M. T. Turvey (1977), and Pribram A. R. Sharafat and Beekman (1984) in the area of motor functions. Collectively, this work stands in remarkable agreement with David Hartley's theory of neural resonances, and with more refined versions of this theory promulgated in the latter half of the nineteenth century.

A further problem faced in Freud's project is that such resonance is reinforced in some instances yet fails to reach threshold in others. This problem is addressed by the proposal that the sensory input must be matched to a preexisting pattern in order to attain threshold. Once again, twentieth-century psychologists with a biological orientation (e.g., Bruner, 1958; Jasper, 1958; Sokolov, 1960) have developed similar models on the basis of their experiments. Finally, Freud notes that such a patterned lowering of threshold involves reflex circuitry—feedback, in today's terminology—a proposal that has been endorsed by twentieth-century theorists such as MacKay (1966), Teuber (1960), and Holst and Mittelstaedt (1950), and confirmed experimentally (see Spinelli & Pribram, 1966; Lassonde, Ptito, & Pribram, 1981).

Freud's model is, of course, not unique in its prescience. Whether one is reading Schäfer's *Textbook of Physiology* (1898–1900) or Alfred Binet's philosophical treatise on *Mind and the Brain* (1907), the impression remains that the end of the nineteenth century was not very different from the end of the twentieth century in its treatment of the relationship between mind and brain. Mind is dependent on the intricacies of sensory, motor, and brain processing.

These models were, of course, made possible by the accumulation of evidence, the greatest amount of which accrued from shrewd clinical observation coupled with post-mortem pathoanatomical dissections of the brains of persons who had shown a psychological disturbance. These clinical data were supplemented by experimental neurosurgery on animals, where brain extirpations could be carefully controlled. In the hands of David Ferrier (1878), Edward Schäfer (1898–1900), Friedrick Goltz (1892), and Hermann Munk (1881), these attempts at experimental verification of clinical observations reached only a modest state of sophistication, for the experimenters lacked precise quantitative behavioral measures of performance. Thus only obvious changes were observed.

In the clinic and laboratory, knowledge gained about brain–mind relationships during the 19th century was prodigious, and the resulting models sophisticated. Humans were found to be unique in brain and therefore in mind. We must ask, then, why so promising a line of investigation nearly came to a halt during the early decades of our own century. When the body of twentieth-century experimental psychology is reviewed, a very different impression of the human place in the world is obtained. Such a review and some reasons for this difference make up the following section.

COUNTERPOINT: A BRAINLESS AND MINDLESS EVOLUTIONARY PSYCHOLOGY

Psychology, seen solely as the science of behavior (rather than brain function), becomes a platitude toward the middle of the twentieth century. There were, of course, a few throwbacks to the 19th-century notion of psychology as the science of mind (e.g., Miller, 1962), but contrary to their predecessors, they did not reflect a neurobiologically rooted conception of mind. Rather, these writers and their brethren heralded the triumphs of a roguish, adolescent independence from mother philosophy, aunt education, and whatever other family ties might still bind. The stated aim was to mathematize, to develop laws in the image of the mechanistic physics of Newton.

What led to this turn of events? Why, a century after Wundt's achievement of a broad, experimentally based biological and social psychology, did psychological inquiry suddenly espouse only the environmental and social branches and deny its neurobiological roots?

> The behaviorist asks: Why don't we make what we can *observe* the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws concerning only those things. Now, what can we observe? We can observe *behavior—what the organism does or says*. And let us point out at once: that *saying* is doing—that is, *behaving*. Speaking overtly or to ourselves (thinking) is just as objective a type of behavior as baseball.
>
> The rule, or measuring rod, which the behaviorist puts in front of him always is: Can I describe this bit of behavior I see in terms of "stimulus and response"? By stimulus we mean any object in the general environment or any change in the tissues themselves due to the physiological condition of the animal, such as the change we get when we keep an animal from sex activity, when we keep it from feeding, when we keep it from building a nest. By response we mean anything

> the animal does—such as turning toward or away from a light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books, and the like. (Watson, 1925/1959, pp. 6–7)

The behaviorist approach initiated by Watson was continued by Clark Hull (1951), Kenneth Spence (1960), and Edward Tolman (1932). In their hands, functional relationships between stimuli and the organism's response were mapped mathematically. Physiological variables were not measured directly but inferred to intervene between stimulus and response. Watson had been interested in physiological measurement—behavior for him meant movement; now, though, at best, physiological constructs replaced physiological observations.

The trend toward environmentalism was taken to its logical conclusion by B. F. Skinner. Behavior became the environmental consequence of the movement, the action that produced a paper record that "could be taken home at night and studied." Environmental consequences, not the physiology of man, became the substance and tool of the behaviorist.

These steps toward environmentalism were embodied in the view of methodological behaviorism (i.e., behavior is indeed a potent measure of man). In testing this potency, it is not altogether surprising that the measure for a while became its own end. Of more importance historically is the shift from physiology to environment. Watson's initial proposal was that peripheral physiological recordings of patterns of muscle contraction would reflect the ongoing *neural* patterns that are coordinate with psychological processes. Skinner took exception to this:

> The important advance . . . that is made by turning to the nervous system as a controlling entity has unfortunately had a similar effect [similar to that of resorting to mental explanations] in discouraging a direct descriptive attack upon behavior. The change is an advance because the new entity beyond behavior to which appeal is made has a definite physical status of its own and is susceptible to scientific investigation. Its chief function with regard to a science of behavior, however, is again to divert attention away from behavior as a subject matter . . . . (I am not attempting to discount the importance of a science of neurology but am referring simply to the primitive use of the nervous system as an explanatory principle in avoiding a direct description of behavior.) (Skinner, 1938, p. 4)

Thus peripheral physiological measures gradually gave way to recording the behaviors of the entire organism, which entailed a subtle shift from behavior as movement to behavior as the environmental consequence of that movement. Skinner could thus declare that behavior is the cumulative record of lever depressions. While Watson's psychology (1925/1959) was still physiologically rooted, his message was that behavior should take its own measure, fly free, and leave mind behind in the home nest of philosophy. In the hands of Hull (1951), Spence (1960), Tolman (1932), and Skinner (1938), behavioral science did just that, and successfully—so successfully, in fact, that the question can now be raised as to just what might be the relationship of a science of behavior to a psychology conceived as the study of the psyche (i.e., mental processes; see, e.g., Pribram, 1979).

Focus on the technical excellence of method thus contributed to the growing pains of a psychology that was the successful young offspring of the nineteenth century. Technical achievement can also account for much that has happened during the twentieth century to loosen psychology from its earlier roots in mind and brain. But technical achievement had to operate in a context, and the question arises as to just what context, aside from the very general technical thrust of this century, operated to disengage psychology from its neuobiological moorings.

An answer to this question comes from the fact that in addition to encouraging a brain-based psychology, the 19th century spawned the theory of biological evolution (see Chapter 4, this volume). In a deep sense, Darwinian evolution is as much an environmentalist as a biological theory, something clearly recognized by scholars such as Julian Huxley (1942/1974) and anticipated by Spencer (1852). Though environmentalist, the theory differed[4] from Lamarckian conceptions in that Darwin (1859/1964) suggested a more acceptable process by which adaptation to the environment could occur: The apparent relatedness among the diverse creatures of the earth could be explained on the basis of biological variation coupled with a principle of selection. Selection, the Darwinists noted, is due to environmental contingencies, both physical (Huxley) and social (Spencer).

Nineteenth-century biological psychology had failed to provide any overarching theoretical frame for understanding psychological problems. True, the brain had become identified as the anatomical basis of mental life, but the actual processes and mechanisms, while modeled in general, remained for another age to discover. By contrast, Darwin's biological principles of evolution did provide a universally applicable mechanism by which large parts of the psychological as well as the biological order might be explained.

An illustration of this shift from biological brain mechanisms to evolu-

[4] Darwin himself never fully acceded to his own innovation, for he became increasingly Lamarckian the more he thought about the human species. But it was his development of the principles of selection that captured the imagination of many and continues to dominate not only biology and psychology but social and physical (including chemical) theory as well (see, e.g., McGuinness, 1986).

tion as a means of explanation is seen in what happened to Freud after he attempted his comprehensive neuropsychological project. In the project iself, when he was unable to specify a brain mechanism because of a paucity of facts, Freud resorted to a "Darwinian explanation" that he felt must suffice until a mechanistic explanation became available (Pribram & Gill, 1976). Sulloway (1979) carefully documents the argument that Freud's later writings leaned heavily on Darwinism—so heavily, indeed, that Sulloway calls Freud a "biologist of the mind." But as Freud said so often, this resort to Darwinism and his repeated disavowals of mechanism were not disavowals in principle, but only for the interim—the time was not yet ripe for mechanistic explanations (see Pribram & Gill, 1976, pp. 162–168).

More important, the theory of evolution displaced the individual organism, including human individuals, from the center of scientific concern and replaced it with the species en masse. Psychological individuality, like biological individuality, was simply one of the entries in the large table of natural variations from which the pressures of the environment would select winners and losers. Skinner (1971) called for a society in which the freedom and dignity of man were recognized as anachronistic feelings of no utility in the process of survival, but the argument was not convincing to many who also sought to go *Beyond the Punitive Society* (Wheeler, 1973).

In a more subtle way, evolutionary theory emphasized function to so great an extent as to reduce in importance a historical subject of psychophysiological interest—namely, the structure of mental processes such as ideas and their putative sensory foundation. Even instincts (those behaviors shared by species as diverse as birds and bees and humans) became suspect, as Frank Beach notes in "The Descent of Instinct" (1955). Evolutionary theory, with its utilitarian slant, tended to direct the energies of psychological inquiry toward hedonistic variables and their effects on "representative" organisms. To read Darwin is to anticipate Skinner, but not Pavlov. It is to anticipate both comparative and genetic psychology, but not physiological psychology as it would emerge in the twentieth century. This is not to say that there was anything in evolutionary theory that was hostile to such developments, only that the theory was largely indifferent to them. In fact, this indifference was quietly shared even by those working within the framework of physiological psychology, a framework that could not easily assimilate the Darwinian perspective, although paying homage to it.

During the long session of Darwinized psychology, it was often the biologists, neurophysiologists, and neurosurgeons (e.g., Sherrington, Sperry, Eccles, Penfield, and Pribram) who preserved psychology as the study of (brain-related) mind. It is in this latter sense that contemporary psychobiology is descended from Gall and not Darwin, and this is also one of the reasons behind that odd historical tendency that finds the neuropsychological perspective waning when the behaviorist one waxes.

Is it any wonder, then, that the Zeitgeist of twentieth-century psychology, when at all determined by biology, was the Zeitgeist of evolution, not of the brain? For example, the influential ethological studies of animal behavior offered an understanding of human psychology that studies of brain and behavior never seemed to attain. The significant words in the previous sentence are "seemed" and "influential." The point is not whether more understanding might actually have been achieved by the brain–behavior studies. They simply did not attain status as readily, if at all, as the often more remote (e.g., birds versus people) studies in ethology.

This complex point can be illustrated further in the difference between Nobel prizes awarded in the brain–mind and the evolutionary–psychology areas. When Egas Moniz received the prize in 1949, it was in part for intervening in brain tissue (frontal leukotomy) in order to influence the deranged minds of humans. The data base upon which this intervention was inaugurated consisted of two chimpanzees and half a dozen monkeys rather poorly observed, with few control procedures. Still, the human patients were obviously changed by the surgical intervention, so that a definite relationship between brain and mental processes could be clearly discerned from the results. What was missing were the details: clear descriptions and conceptualizations of the mental processes to be influenced, a clear understanding of the functions of the part of the brain being invaded, and an established relationship between these two sets of details.

In contrast, when Konrad Lorenz, Niko Tinbergen, and Ernst von Frisch received their prize in 1973—also in the category of physiology and medicine—a detailed body of data concerning animal behavior had been initiated and developed, as had a body of observational evidence on human behavior. What was completely lacking was evidence of a necessary relationship between the behaviors of birds and bees and those of man. When similarities were observed, were they only analogous, or did they entail some deeper homology? The overriding acceptance of the theory of evolution made such questions seem unnecessary. One has only to think back to another age, one concerned with the uniqueness of human mental and spiritual capacities, to see the sharp contrast between it and the faith of the twentieth century in an evolutionary behavioral science. Some current studies in human ethology (e.g., Eibl-Eibesfeldt, 1979; Reynolds, 1981) bring the techniques of ethology to bear on anthropological observations. They are a recognition of the failures as well as the successes of earlier work and thus address this fundamental issue.

Ethological investigations are only one instance of the influence of nineteenth-century evolutionary doctrine on twentieth-century psychology. Another is seen in Skinner's operant behaviorism (1969), where environmental contingencies, appropriately scheduled, control the behavior of organisms. (The behaviors themselves are biologically diverse initially.) Shaping procedures select by rewarding certain behaviors (an environmental procedure), thus ensuring their recurrence at the expense of others. The behaviors that fit the contingencies survive—a clear instance of survival of the fittest.

Within the regnant spirit of Darwinian evolution, the principles of operant reinforcement were derived from the study of rats and pigeons. In turn, these same principles were brought to bear ipso facto to explain the origin and evolution of language. The success of Chomsky's famous critique (1972) was perhaps due not so much to his detailed analysis of whether a limited aspect of linguistic behavior might be subject to the rules of operant conditioning as to his questioning the legitimacy of wholesaling the then unexamined Darwinian assumptions underlying the operant framework. Skinner (1974) addresses this issue himself, clearly stating his indebtedness to the doctrine of evolution. Only later, however, was it shown that, when applied to man, operant behaviorism must at a minimum take cognizance of man's unique capacities of cognition (Bandura, 1969).

The apparent success of behaviorism, apart from its broad (if not deep) database, should be understood in terms of its ability to retain the Darwinian message while liberating psychology from the hereditarianism that the twentieth-century intellectual community found so objectionable. The post-Watsonian behaviorists (Hull, Skinner, Spence) supplied an experimental psychology devoted to organismic adaptations—a psychology able to assess the manner in which environmental variations come to sample the behavioral potentialities of a species; a psychology (apparently) able to disregard nativistic theories of individual differences by (allegedly) showing them to be grounded in the purely historical details of an organism's development; and a psychology able to get along quite well without aid from the biologist, the clinical neurologist, or the philosopher of science. What was promised was an objective science of behavior based on controlled observation and measurement of the environmental determinants of conduct. "Mentalism" was put on notice, and psychobiology was taken to be virtually beside the point. On the behavioristic account, it made no more sense to look inside the organism for the neural correlates of behavior than to look inside for its mental causes.

CODA: BRAIN AS MACHINERY OF MIND

Had behaviorism offered no more than rhetoric and promises, it surely would not have captured so large a share of modern psychology and held it for decades. But it also offered data, mountains of it. The separation between Thorndike's "puzzle box" and today's computerized operant laboratory is about eight decades. In that time, behavioristic psychology revolutionized techniques, not only in experimental psychology but also in pharmacology, education, psychotherapy, rehabilitation, and other areas. Skinner's pioneering studies of partial reinforcement deepened our understanding of how certain forms of behavior become persistent, and studies of avoidance conditioning clarified the principles governing fear-induced behavior. The behavioristic epoch in modern psychology transformed our perspective and introduced changes in the discipline that will survive long after behaviorism itself, as an *ism,* is merely a historical entry.

The question raised and left unanswered by the plethora of behavioral data was what relevance they might have to the persistent problems of psychology—problems such as the organization of memory or the use of representations in thought, attention, and the deployment of skills. It remained for those working in the latter part of the century to address the problems of psychology with the behaviorists' tools. By 1960 many psychologists, now turned "radical" (Skinner, 1969) or "subjective" (Miller, Galanter, & Pribram, 1960) behaviorists, attended once again to these persistent problems. The latter group now sought to gather data related to the organization of experiential determinants of behavior, under the umbrella of "cognitive science," and to impose the same rigor on them that behaviorism had demanded in the treatment of a far narrower class of phenomena. The problems themselves were those which had been identified earlier as the essence of mind.

In the transition from the old to the new, a certain innocence was lost. The modern cognitive sciences did not adopt the nineteenth century model of the actual biological brain. Instead, they turned to mechanical "brains"—to computers, TV scanners, and other hardware—for their inspiration (see Pribram, 1980). This decision was dictated in large measure by the precise knowledge that can be attained with these engineered devices. The strategy here is based on the thesis that we are concerned with processes, not mechanisms, and that the former can be understood according to certain "design features" that are utterly indifferent to the composition of the actual parts. We understand radar, for example, by understanding the terms—the transfer functions—embodied in the radar equation. Thus we can specify the performance of a given radar system without ever inquiring into its location or its material composition.

In the new technology we have a positive reason for the cognitive sciences to turn away from the biological brain. A negative reason is that the immense accumulation of neurophysiological information compiled during psychology's behavioristic hiatus proved to be inaccessible to those who had not participated directly. Neurobiology, after all, had not stood still waiting to be rediscovered by psychology. Indeed the new neurobiology had remained somewhat aloof as psychology celebrated its utterly independent status. There were, of course, occasional forays into psychological territories: the work of David Hubel and Torsten Wiesel on "feature" selection by neurons in the visual cortex (Hubel & Wiesel, 1959); the discovery that the nondominant hemisphere is specialized for nonlinguistic functions (reviewed in Mountcastle, 1962); and the finding that aspects of mind can be split if the functions of the cerebral hemispheres are separated by severing the corpus callosum (Sperry, 1974). For the most part, however, studies of brain function failed in any immediate sense to suggest answers to the more complex questions again arising within psychology—questions regarding the origins of conscious awareness, memory and retrieval, symbolic coding, relationships between language and thought, and principles of cognitive development.

In *The Self and Its Brain* (Popper & Eccles, 1977), a celebrated philosopher of science and a Nobel laureate in neurophysiology teamed up to address this issue by suggesting, as had Franz Brentano (1874/1924-1925), Freud's illustrious professor of philosophy, that self-consciousness (in contrast with consciousness) is a uniquely human attribute. When it comes to mechanism, however, Popper and Eccles have little to say, even about animal consciousness. Near the end of *The Self and Its Brain,* Popper confides:

> I think that with respect to consciousness, we have to assume that animal consciousness has developed out of non-consciousness—we don't know more about it. At some stage this incredible invention was made. . . . But in saying this I know very well that I am saying very little . . . It is not an explanation, and it must not be taken as an explanation. (p. 560)

It was just this sort of reasoned frustration that turned psychology away from both the biological sciences and philosophy and toward the reassurances of behaviorism. Yet despite these very real differences, in the years since 1960 there occurred, by virtue of the cognitive "revolution" in psychology, a serious resumption of investigations of the brain as the organ of cognition, and therefore of mind.

It is not accidental that, of all specialists within modern psychology, it is the psychobiologist who has the most regular contact with issues ordinarily taken to be philosophical. In view of the historical contacts between philosophy and those branches of biological science devoted to neurology and neurophysiology, such contact is to be expected. In our own century alone, this connection has been amply illustrated in the works of such scientific luminaries as Pavlov, Sherrington, Penfield, Sperry, and Eccles. Each of these men (all but Penfield recipients of the Nobel Prize), first distinguished himself by making fundamental contributions to what we now call the "neural sciences." But each also reserved significant space in his published works to address the larger philosophical, metaphysical, and psychological implications of his scientific discoveries.

Meanwhile, methodological and radical behaviorism developed something of a philosophy of science, an ontology, even something of a system of social ethics. Understood in these terms, behaviorism found much to reprove in both the distant and more recent history of neuropsychology, for in the latter discipline there has been a willingness, even a felt necessity, to accept verbal reports of subjectively experienced cognitive, ideational, conscious, affective, volitional, and motivational aspects of human psychology (see Pribram, 1962, 1971) as determiners of behavior. Radical behaviorism took an ontological stand against a causal role for any subjectively labeled central states or representations in the organization of behavior. It insisted that if they exist at all, it is only as physically specifiable neural or endocrine states, or as epiphenomena of observable behavior.

The issue is important and can perhaps be brought into focus by the following analogy: By observing the properties of hydrogen and oxygen atoms, physicists find lawful relations among interactions, as when two hydrogen atoms combine with one oxygen atom in a certain way to make up a molecule of H_2O. However, H_2O has peculiar properties not shared by either element while separate. For example, it liquifies at ordinary earth temperatures and solidifies when the temperature drops just a bit. And when it solidifies, it floats on its liquid base, something most other compounds don't do. The following issues may be raised by the scientists who made these observations: Some want to label the H_2O combination "water" because common language calls it that, but others state that such labeling is unscientific. The question is then raised as to whether or not water as such is in any way causally related to hydrogen and oxygen. Certainly the H_2O formula places constraints on the distribution of hydrogen and oxygen, and on the uses to which these elements can be put. But water also makes life as we know it possible. The chemical and biological consequences of combining hydrogen and oxygen are far-reaching, but are they therefore any less scientific? In studying the effects of combining elements, is the "downward causation" of their distribution to be ignored? Are chemists and biologists "soft" in their approach to science when they discuss the properties of water?

If we substitute the brain, or more accurately, the body (organism) for hydrogen and the environment for oxygen, behaviorally effective interactions (i.e., combinations) produce a new level of organization. Now the question is whether or not it is acceptable to label some of the combinations vision, others attention, others love, and still others dignity and freedom, just as H_20 is labeled water. Could there be a "causal" relationship between freedom and the distribution of brains and organisms in the world? What might be wrong with a psychology that affirms that freedom makes spiritual life possible, just as the wetness of water makes biological life possible? These were questions addressed by scientists such as Sherrington (1955), Sperry (1976), Penfield (1975), Pribram (1970; 1985) and Eccles (1976) in response to earlier, more classically behavioristic stances such as Gilbert Ryle's "ghost-in-the-machine" (1949).

Radical behaviorism attempted to model itself on Newtonian mechanics. A search had been instituted for lawful relationships between the antecedents (causes) and consequences (effects) in behavior.[5] In this way, behaviorism was a kind of functionalism (see Chapter 6, this volume). By contrast, early 19th-century psychology had been structural in its biological orientation (i.e., it was interested in principles of organization). It was a discipline seeking to define the organizational properties, the faculties of mind, and their biological underpinnings. When, toward the end of the century, the winds of change began to blow, new insights were derived during the development of functionalism. Some were embodied in Freud's psychoanalytic metapsychology, others in Helmholtz's and Mach's physicalistic sensory psychology, and still

[5] At the end of the twentieth century, as at the beginning of the nineteenth, the issue of cause versus condition has been raised. When we speak of conditions, we are more apt to use the term reason than cause, but this is not universally the case. Aristotle's distinction between "proximate efficient" and "final" causes is relevant here. Are conditions final causes in the sense that they determine the constraints toward which systems tend? Biologists such as Waddington (1957) suggest that evolutionary doctrine describes such constraints.

others in the development of approaches to problems of psychological assessment, as in Binet's (1907) measures of mental ages.

While a functionalist behaviorism (and its European counter trend, a functional phenomenology) came to hold sway in early and mid-twentieth-century psychology, a new structuralism developed in anthropology and linguistics. All but unknown among psychologists, this structuralism searched not so much for anatomical organs of mental faculties as for the structures of process. "Structure" in this new sense meant stable organizations, identifiable orders in ongoing functional relationships. This time, change came from an unreconstructed functional behaviorism and a functional phenomenology (see, for example, Miller, et al., 1960; Merleau-Ponty, 1963).

At the time (i.e., in the mid-twentieth century) when these developments were taking place in the body of experimental psychology, a growing conservatism characterized physiological psychology. The trend in this subdiscipline was toward a reductionism that if continued, would have seen physiological psychology absorbed by neurophysiology, at the expense of physiological psychology as a psychological discipline (Pribram, 1970). Simultaneously, however, there transpired a courtship of what was previously a branch of physiological psychology, namely neuropsychology, by cognitively oriented psychologists, and this courtship produced a number of results that led in the opposite direction. Not the least of these was the reanimation of psychobiology by such issues and phenomena as attention, problem-solving, complex perceptions, and the contextual determinants of information processing, artificial intelligence, and the like.

Perhaps the most telling change in this nonbehavioristic direction occurred when neuropsychologists faced the clinic and its concomitant facts of human brain function and the correlated phenomena of psychological disturbance and debility. Clinical neuropsychology blossomed in its relationship with the cognitive resurgence to the point that a separate division of "clinical neuropsychology" was established within the American Psychological Association. It has always been the clinical residual of complexities and exceptions that has steered a neurobiologically rooted psychology away from the easy reductionism and metaphysical certainty that often captured other branches of the discipline.

In the latter decades of the twentieth century, biological influences in psychology have indeed reached a frontier. This frontier was established by contributions of the 19th century showing that the mind of man is rooted in a unique brain and, equally, by later contributions viewing behavior universally as a measure of, and often a substitute for, universal mind. The challenge was (and is) to resolve evolutionary radical behaviorism with a brain-structured, human-centered mentalism. When and if such a resolution occurs, we may see strides in understanding the spirit of mankind that will rival the technical advances of the twentieth century.

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