Irreducible Mind

TOWARD A PSYCHOLOGY FOR THE 21st CENTURY

EDWARD F. KELLY,
EMILY WILLIAMS KELLY,
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Preface and Acknowledgments

This book originated from a seminar directed to theoretical foundations of scientific psychology, initiated in 1998 by Michael Murphy under the auspices of the Center for Theory and Research of Esalen Institute. By the year 2000 our discussions had advanced to the point where we believed we could demonstrate, empirically, that the materialistic consensus which undergirds practically all of current mainstream psychology, neuroscience, and philosophy of mind is fundamentally flawed. We therefore committed ourselves to developing a book-length presentation which would systematically articulate and defend this point of view.

Our general strategy was to assess the overall state of psychology, as it exists here at the beginning of the 21st century, from a perspective that deliberately but selectively takes into account its first hundred-plus years of organized scientific effort. The essential driving idea was to step backward, the better to jump forward—"*reculer pour mieux sauter.*" The tactical opportunity for this exercise was to be provided by the centennial of the publication in 1903 of an extraordinary book by a largely forgotten genius, F. W. H. Myers, titled *Human Personality.* Deeply admired by William James and other leading scholars of that period, this two-volume work is unquestionably a great but neglected classic of our science. It advances an elaborate but empirically supported theory of the constitution and functioning of human beings, one that in many ways is sharply at odds with current mainstream thinking, but one that we believe penetrates far closer to the empirical truths of the matter. By framing the relevant issues in the context of Myers's work, we thought, we would be able to justify and to some extent foreshadow what we anticipate will become a major and vitally necessary reworking of central parts of scientific psychology.

The basic plan of the book was to be threefold. First, we would provide an exposition of Myers's theoretical and empirical contributions. Second, we would systematically and critically examine subsequent research on a variety of empirical topics that were central to the theoretical position he developed. Finally, we would attempt to assess, in light of this review, where things now stand in psychology and where we need to go. The goal throughout would be not simply to celebrate Myers's project as he himself left it, but

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to carry it forward in the context of relevant substantive and methodological achievements of the intervening century.

The large book you hold in your hands realizes these intentions, to the extent permitted by our collective capacities and knowledge. We missed our original deadline, which seemed at first to lie in a far-distant future, by a full three years. This was due not to lack of effort on our part but to the dimensions of the task, which we seriously underestimated. The book could easily have become larger still. The subjects we discuss are individually complex and deeply intertwined, with ramifications that proliferate endlessly in interesting directions. Most chapters and even parts of some chapters could easily become books in themselves, and some probably will. Many chapters also deal with issues that lie at or beyond the currently recognized boundaries of "accepted" science, and therefore pose special challenges for responsible presentation. Despite their intrinsic difficulties, however, these diverse materials combine to produce what we think is a compelling demonstration that current mainstream opinion in psychology *must* change, and in directions that are both theoretically fundamental and humanly momentous. In a nutshell, we are arguing for abandonment of the current materialistic synthesis, and for the restoration of causally efficacious conscious mental life to its proper place at the center of our science. We hope to catalyze the emergence of an enlarged and reunified mainstream psychology, one that does not systematically ignore—as the present-day mainstream does—many large bodies of evidence deeply relevant to our most central and abiding human concerns.

In the interest of effectively promoting this sea-change we have deliberately crafted our book for a primary audience consisting of advanced undergraduate and early-stage graduate students, particularly students in disciplines such as psychology, neuroscience, and philosophy. These are the future leaders of our field, and we want to reach them before they suffer the "hardening of the categories" that all too often accompanies entry into these highly specialized professions. To do so required that our material be presented with a level of currency, detail, and rigor commensurate with that of the other professional materials such persons are exposed to on a daily basis, and we have attempted to meet that standard.

This has necessarily involved some difficult tradeoffs, however, for we also wanted our book to be accessible to anyone of good general education and intelligence who is seriously interested in its subject matter and willing to make the necessary effort. We have tried to ease the burden for such persons in various ways—for example by defining obscure or "jargon" terms, providing interim summaries and abstracts, and relegating many points of more technical or scholarly interest to parentheses and footnotes. However, there is no escaping the fact that some parts of the argument, especially parts of Chapters 1, 4, 7, and 9, are likely to present difficulties, particularly on first encounter. We implore such readers to be understanding and patient with us and persistent in their own efforts, skipping over any particularly

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challenging sections at first and returning to them later with a better sense of how everything fits into the overall scheme.

Our book has the outward form of an edited volume but is atypical of that genre. It is united throughout by a single theme, our collective drive toward a broadly correct, though necessarily incomplete, scientific picture of the mind as it relates to brain activity. This generalist impulse contrasts sharply with the extreme specialization that characterizes the sciences and other modern professions, and that is often especially pronounced in edited books. Edited volumes in science often address narrow topics and consist of pieces authored in hermetic isolation for small groups of specialists interested mainly in talking to each other. That is emphatically not the case with the present work. The book as a whole and its chapters individually take on big issues and seek to engage large numbers of readers. Several chapters include two or more of us as authors, and all were generated not in isolation but in conformity with an overall plan that emerged through group discussions spanning a period of years.

Our collective professional experience covers a wide range in terms of education, research, and teaching in psychology, psychiatry, neuroscience, and philosophy, and all of us have had the opportunity to read and critique every part of the book, usually in multiple versions. In addition to pooling our own professional expertise in this way, we have sought critical feedback on chapter drafts from outside volunteer readers including both professional colleagues from various disciplines and more general readers representing a diversity of backgrounds. Their efforts and suggestions have led to numerous improvements throughout the book, for which we are grateful. We are acutely aware that many gaps and imperfections remain, and we take full responsibility for these. One of our main points is that what is most urgently needed for further theoretical progress is more and better data of certain critical and specified kinds; this, the greater good, seemed better served by getting the book out now in reasonably finished form than by obsessing further over potentially endless refinements.

Finally, we wish to acknowledge contributions from individuals who have supported this project in various special ways. Among our "test-drivers" we give particular thanks to Carlos S. Alvarado, William Barnard, Frank Benford, Lori Derr, Ross Dunseath, Lorin Hollander, Fritz Klein, Jeff Kripal, James Lenz, Cory Maxwell, Francis McGlone, Michael Murphy, Margaret Pertzoff, Michael Schaffer, Ben Snyder, Ian Stevenson, Pim van Lommel, and Ray Westphal. Seminar participants who have contributed vigorously to the interdisciplinary conversations that helped shape the book include Richard Baker, John and Alyce Faye Cleese, David Fontana, Owen Flanagan, Arthur Hastings, Sean Kelly, Antonia Mills, Michael Murphy, Gary Owens, Frank Poletti, Dean Radin, William Roll, Bob Rosenberg, Marilyn Schlitz, Charles Tart, Jim Tucker, and Eric Weiss. Frank Poletti efficiently managed the logistics of our meetings, and Bob Rosenberg skillfully oversaw production of our digital version of *Human Personality* (see p. xxx of our Introduction). Robert F. Cook provided translations of the

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many French and Italian passages in *Human Personality* as well as the translation of Théodore Flournoy's review, for the digital version of *Human Personality*. John Cleese rescued us from periodic despondency and financially supported the mechanics of book production. Faye Joseph and Gary Owens provided additional financial support for book production, and the Institute for Noetic Sciences provided support for our meetings. Nancy L. Zingrone generated our camera-ready copy, including the index. Lori Derr, Dawn Hunt, and Martha Stockhausen provided invaluable help in tracking down references. We thank our associates from Rowman & Littlefield, especially Stanley Plotnick, Jon Sisk, and our editor Art Pomponio, for taking strong interest in this project and then sticking with it despite the many subsequent changes in book content and organization that delayed its completion. Above all we thank Michael Murphy for initially conceiving this project, for bringing us together in the spectacularly stimulating environment of Esalen, and for his apparently limitless reserves of comradeship, wit, and wisdom.

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Introduction

Edward F. Kelly

The central subject of this book is the problem of relations between the inherently private, subjective, "first-person" world of human mental life and the publicly observable, objective, "third-person" world of physiological events and processes in the body and brain.

Scientific psychology has been struggling to reconcile these most-basic dimensions of its subject matter ever since it emerged from philosophy near the end of the 19th century. Both were fully present in William James's monumental *Principles of Psychology* (1890b), the earliest English-language survey of the new academic discipline that is still widely cited today. James explicitly acknowledged the normally intimate association between the mental and the physical, and he systematically and sympathetically rehearsed what little was then known or surmised about the brain. Unlike many of his scientific contemporaries, however, James resisted premature and facile attempts at neural reductionism. When he recognized limitations on the physiological side, he was content to record his psychological observations and await further progress in neurophysiology. The bulk of the *Principles* therefore consists of masterful expositions, relying heavily on sophisticated observation of his own inner workings, of central properties of mental life such as attention, imagination, the stream of consciousness, volition, and—at the heart of everything—the self (Leary, 1990).

James's person-centered and synoptic approach was soon largely abandoned, however, in favor of a much narrower conception of scientific psychology. Deeply rooted in earlier 19th-century thought, this approach advocated deliberate emulation of the presuppositions and methods—and thus, it was hoped, the stunning success—of the "hard" sciences, especially physics. James was barely in his grave when J. B. Watson (1913) published the founding manifesto of radical behaviorism, the logical culmination of this tradition. Psychology was no longer to be the science of mental life, as James had defined it. Rather it was to be the science of behavior, "a purely

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These techniques have yielded a torrent of new information about the brain. Scientists and philosophers confronting the mind-body problem even as recently as a century ago knew only in a relatively global and undifferentiated fashion that the brain is the organ of mind. Today we know a great deal more, although our knowledge undoubtedly remains in many respects extremely primitive relative to the brain's unimaginable complexity. We know a lot about the structure and operation of neurons and even lower-level constituents. We also know a lot about the *structural* organization of the brain, its wiring diagram, and thanks mainly to the new imaging technologies we have begun to learn a fair amount about its *functional* organization, the manner in which complex patterns of neural activity are mobilized and coordinated across spatially separated regions of the brain in conjunction with ongoing experience and behavior.

The empirical connection between mind and brain seems to most observers to be growing ever tighter and more detailed as our scientific understanding of the brain advances. In light of the successes already in hand, it may not seem unreasonable to assume as a working hypothesis that this process can continue indefinitely without encountering any insuperable obstacles, and that properties of minds will ultimately be fully explained by those of brains. For most contemporary scientists, however, this useful working hypothesis has become something more like an established fact, or even an unquestionable axiom. At the concluding ceremonies of the 1990s "Decade of the Brain," for example, Antonio Damasio (1999) encapsulated the prevailing view:

> In an effort that continues to gain momentum, virtually all the functions studied in traditional psychology—perception, learning and memory, language, emotion, decision-making, creativity—are being understood in terms of their brain underpinnings. The mysteries behind many of these functions are being solved, one by one, and it is now apparent that even consciousness, the towering problem in the field, is likely to be elucidated before too long.[1]

That an enormous amount of methodological and substantive progress has been made by scientific psychology in its first century can hardly be denied, and I do not mean to deny it. But what sort of root conception of human mind and personality has so far emerged from all this effort? There are many rapidly shifting cross-currents and variations of detail amid the welter of current views, but to the extent that any provisional consensus has been achieved by contemporary mainstream scientists, psychologists and neuroscientists in particular, it is decidedly hostile to traditional and commonsense notions and runs instead along roughly the following lines: We human beings are nothing but extremely complicated biological machines. Everything we are and do is in principle causally explainable from the bot-

1. This quotation and others in this book that do not list a page number were taken from sources published on the internet without specific pagination.

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tom up in terms of our biology, chemistry, and physics—ultimately, that is, in terms of local contact interactions among bits of matter moving in strict accordance with mechanical laws under the influence of fields of force.[2] Some of what we know, and the substrate of our general capacities to learn additional things, are built-in genetically as complex resultants of biological evolution. Everything else comes to us directly or indirectly by way of our sensory systems, through energetic exchanges with the environment of types already largely understood. Mind and consciousness are entirely generated by—or perhaps in some mysterious way identical with—neurophysiological events and processes in the brain. Mental causation, volition, and the "self" do not really exist; they are mere illusions, by-products of the grinding of our neural machinery. And of course because one's mind and personality are entirely products of the bodily machinery, they will necessarily be extinguished, totally and finally, by the demise and dissolution of that body.

Views of this sort unquestionably hold sway over the vast majority of contemporary scientists, and by now they have also percolated widely through the public at large.[3] They appear to be supported by mountains of evidence. But are they correct?

The authors of this book are united in the conviction that they are *not* correct—that in fundamental respects they are at best incomplete, and at certain critical points demonstrably false, empirically. These are strong statements, but our book will systematically elaborate and defend them. Our doubts regarding current psychological orthodoxy, I hasten to add, are at least in part shared by others. There seems to be a growing unease in many quarters, a sense that the narrowly physicalist contemporary approach to the analysis of mind has deflected psychology as a whole from

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2. Newton's law of universal gravitation, insofar as it implies instantaneous action at a distance, appears to conflict with this characterization of physical causation, and indeed this feature greatly troubled Newton himself. The idea that matter could influence other matter without mutual contact was to him "so great an Absurdity that I believe no Man who has in philosophical matters a competent Faculty of thinking, can ever fall into it" (Newton, 1687/1964, p. 634). Newton himself presumed that this difficulty could eventually be removed—as indeed it was, more than two centuries later, with the appearance of Einstein's theory of relativity.

3. Just as this introduction was being drafted, a lengthy cover story on "mind/body medicine" appeared in the September 27, 2004, edition of *Newsweek*. This article exemplifies throughout the attitudes I have just described, and it culminates in a full-page editorial by psychologist Steven Pinker, author of *How the Mind Works* (1997), decrying what he terms "the disconnect between our common sense and our best science." Pinker further advises *Newsweek*'s massive readership that contrary to their everyday beliefs "modern neuroscience has shown that there is no user [of the brain]. 'The soul' is, in fact, the information-processing activity of the brain. New imaging techniques have tied every thought and emotion to neural activity." These statements grossly exaggerate what neuroscience has actually accomplished, as this book will demonstrate.

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what should be its most central concerns, and furthermore that mainstream computationalist/physicalist theories themselves are encountering fundamental limitations and have nearly exhausted their explanatory resources. The recent resurgence of scientific and philosophic interest in consciousness and altered states of consciousness, and in the deep problems which these topics inherently involve, is just one prominent symptom, among many others, of these trends.

Even former leaders of the "cognitive revolution" such as Jerome Bruner, Noam Chomsky, George Miller, and Ulric Neisser have publicly voiced disappointment in its results. Chomsky in particular has railed repeatedly and at length against premature and misguided attempts to "reduce" the mind to currently understood neurophysiology. Chomsky (1993), for example, pointed out that empirical regularities known to 19th-century chemistry could not be explained by the physics of the day, but did not simply disappear on that account; rather, physics eventually had to expand in order to accommodate the facts of chemistry. Similarly, he argued, we should not settle for specious "reduction" of an inadequate psychology to present-day neurophysiology, but should instead seek "unification" of an independently justified level of psychological description and theory with an adequately complete and clear conception of the relevant physical properties of the body and brain—but only if and when we get such a conception. For in Chomsky's view, shared by many modern physicists, advances in physics from Newton's discovery of universal gravitation to 20th-century developments in quantum mechanics and relativity theory have undermined the classical and commonsense conceptions of *matter* to such an extent that reducibility of mind to matter is anything but straightforward, and hardly a foregone conclusion.

Several contemporary state-of-the-art surveys in psychology—for example, Koch and Leary (1985), Solso (1997), and Solso and Massaro (1995)—provide considerable further evidence of dissatisfaction with the theoretical state of things in psychology and of a widely felt need to regain the breadth of vision of its founders, such as William James. Solso and Massaro (1995) remark in their summing-up that "central to the science of the mind in the twenty-first century will be the question of how the mind is related to the body" (p. 306) and that "the self remains a riddle" (p. 311). David Leary's (1990) essay on the evolution of James's thinking about the self begins by documenting the remarkable degree to which the *Principles* had already anticipated most of the substance of subsequent psychological investigations of the self. He then goes on, however, to emphasize that later developments in James's own thought—developments completely unknown to the vast majority of contemporary psychologists—contain the seeds of an enlarged and deepened conception of the self that can potentially secure its location where James himself firmly believed it belongs, at the very center of an empirically adequate scientific psychology. From still another direction, Henri Ellenberger (1970) ends his landmark work on the discovery of the unconscious with a plea for reunification of the experimental and clini-

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cal wings of psychology: "We might then hope to reach a higher synthesis and devise a conceptual framework that would do justice to the rigorous demands of experimental psychology and to the psychic realities experienced by the explorers of the unconscious" (p. 897).

As will become apparent, our book wholeheartedly endorses this historically conscious, ecumenical, and reintegrative spirit. Before proceeding with its very unusual substance, however, we must set forth certain methodological principles that have guided us throughout, and that we strongly encourage our readers to adopt as well.

First and perhaps foremost is an attitude of humility in relation to the present state of scientific knowledge. Although we humans indisputably have learned a great deal through systematic application of our scientific methods, and are learning more at an accelerating rate, we undoubtedly still have a long way to go. There is surely a great deal about the physical world in general, let alone brains, minds, and consciousness, that we do not yet understand. Furthermore, our intimate *familiarity* with the basic facts of mental life—including, for example, our ability to direct our thoughts to states of affairs in the external world, and indeed the fundamental fact of consciousness itself—should not be confused with *understanding,* or blind us to the deeply puzzling and mysterious character of these phenomena. The self-assurance, even arrogance, of much contemporary writing on these subjects seems to us wholly unjustified and inappropriate. From this point of view many old scientific books and papers that purport to explain features of mental life in terms of hypothetical brain processes make fascinating reading, because of the many ultra-confident pronouncements they contain which in hindsight we know to be false. Future readers of many present-day books and papers about brain, mind, and consciousness, we believe, are likely to experience similar reactions.

Second, we emphasize that science consists at bottom of certain attitudes and procedures, rather than any fixed set of beliefs. The most basic attitude is that facts have primacy over theories and that beliefs should therefore always remain modifiable in response to new empirical data. In the forceful words of Francis Bacon (1620/1960), from the beginning of the scientific era: "The world is not to be narrowed till it will go into the understanding...but the understanding to be expanded and opened till it can take in the image of the world as it is in fact" (p. 276).

Although all scientists presumably endorse this idea in principle, there are complications and subtleties in practice, because "facts" and theories are strongly interdependent. As remarked long ago by philosopher F. C. S. Schiller (1905), "for the facts to be 'discovered' there is needed *the eye to see them*" (p. 60). Many of the issues discussed in this book revolve around well-documented empirical phenomena—facts, we will insist—that have been systematically ignored or rejected by mainstream scientists who find them too discordant with prevailing views to take seriously.

This is a tricky and delicate business, however; for when current scientific opinion hardens into dogma it becomes scientism, which is essentially

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a type of fundamentalism, a secular theology, and no longer science. As William James (1896) remarked, "science means, first of all, a certain dispassionate method. To suppose that it means a certain set of results that one should pin one's faith upon and hug forever is sadly to mistake its genius, and degrades the scientific body to the status of a sect" (p. 6).[4] Although this may seem uncontroversial, even trite, it is easily and often forgotten. The history of science is therefore replete with the sad spectacle of scientists—sometimes even very prominent scientists talking about their own scientific specialties—issuing what later prove to be profoundly erroneous judgments. For example, Badash (1972) studied the last third of the 19th century, when a "malaise of completeness" pervaded the physical sciences. James Clerk Maxwell commented in 1871 that "the opinion seems to have got abroad, that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry on these measurements to another place of decimals" (p. 50). In 1894 his American counterpart A. A. Michelson declared that "it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice" (p. 52). But the next year brought the discovery of X-rays, and within the decade radioactivity, the electron, quantum theory, and relativity would shake the foundations of physical knowledge. Misjudgments of this magnitude—and many further examples could easily be adduced—should certainly give pause to anyone tempted to presume that today's science defines the limits of the possible.

Although facts have primacy, not all facts are of equal importance. The ones that should count the most, relative to a given problem, are obviously those that can contribute most to its solution. A useful principle that provides orientation and helps guide the search for such facts was stated as follows by Wind (1967): "It seems to be a lesson of history that the commonplace may be understood as a reduction of the exceptional, but the exceptional cannot be understood as an amplification of the commonplace" (p. 238). This lesson has not penetrated contemporary cognitive science, which deals almost exclusively with the commonplace and yet presumes—extrapolating vastly beyond what in reality are very limited successes—that we are progressing inexorably toward a comprehensive understanding of mind and brain based on classical physicalist principles. This serene confidence seems to us unwarranted. It is now evident, for example, that chess-playing computer programs represent progress toward real intelligence in roughly the same

4. The degradation feared by James is exemplified by media figure Michael Shermer, editor of *Skeptic* magazine, in an opinion piece appearing in *Scientific American* (June, 2002). There Shermer not only overtly embraces scientism, apparently unaware of the generally derogatory connotations of this term, but goes on to characterize leading contemporary scientists in quasi-religious terms: "This being the Age of Science, it is scientism's shamans who command our veneration."

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market or in the flood of sensationalized drivel invading our TV and movie screens. The public credulity that enables this industry to thrive is deplorable, and we ourselves deplore it, but this has no bearing on the underlying scientific issue as to whether psi phenomena really exist as facts of nature.

It seems to us axiomatic that no intellectually responsible person, and especially no responsible scientist, should feel entitled to render opinions on this subject without first taking the time and trouble to study the relevant literature. This axiom is regularly violated, however, and in this connection we wish to comment briefly but more generally on the behavior of outside critics of this field, without being drawn into a discussion that could very easily become a book in itself.

The fundamental issue was incisively framed by American philosopher C. J. Ducasse (1969) as follows:

> Although the evidence offered by addicts of the marvelous for the reality of the phenomena they accept must be critically examined, it is equally necessary on the other side to scrutinize just as closely and critically the skeptics' allegations of fraud, or of malobservation, or of misinterpretation of what was observed, or of hypnotically induced hallucinations. For there is likely to be just as much wishful thinking, prejudice, emotion, snap judgment, naïveté, and intellectual dishonesty on the side of orthodoxy, of skepticism, and of conservatism, as on the side of hunger for and belief in the marvelous. The emotional motivation for irresponsible disbelief is, in fact, probably even stronger—especially in scientifically educated persons, whose pride of knowledge is at stake—than is in other persons the motivation for irresponsible belief. In these matters, nothing is so rare as genuine objectivity and impartiality of judgment—judgment determined neither by the will to believe nor the will to disbelieve, but only by the will to get at the truth irrespective of whether it turns out to be comfortably familiar or uncomfortably novel, consoling or distressing, orthodox, or unorthodox. (p. 35)

In our informed and considered opinion, the critiques of psychical research that have so far been offered by outside observers mainly demonstrate the validity of Ducasse's concerns, and routinely though not invariably fail to meet normal standards of scholarly practice. These tendencies were already fully apparent to William James, who took psychical research far more seriously than most present-day psychologists realize (James, 1986; G. Murphy & Ballou, 1960; E. Taylor, 1996), and they have scarcely abated in the subsequent century. We will not dwell on these controversies here, but have included in the Appendix some pointers to recent literature that illustrates their strange character.

We do, however, want to highlight here one particular critical strategy that has been very commonly and inappropriately employed. Most critics implicitly—and some, like Hansel (1966, p. 19), explicitly—take the view that psi phenomena are somehow known *a priori* to be impossible. In that case one is free to invent any scenario, no matter how far-fetched, to explain away ostensible evidence of psi. Because there are no perfect laboratory

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experiments—nor, for that matter, perfect "spontaneous" cases involving psi experiences occurring outside of a laboratory—any positive result whatever can be discredited in this way, and thus any potential accumulation of evidence aborted.[6] The extent to which many critics have been willing to pursue this strategy reveals the depth of their emotional commitment to current scientific orthodoxy, and is to us nothing short of amazing. Contrast this with the attitude expressed by James (1920): "I believe there is no source of deception in the investigation of nature which can compare with a fixed belief that certain kinds of phenomenon are *impossible*" (p. 248). Can there be any doubt which is the scientifically more responsible attitude?

One further point needs to be drawn out in this connection. Many critics also seem to presume that words like "paranormal" or "supernormal" are synonymous with "supernatural." That is not the case, however. Psi phenomena (and certain other unusual phenomena that we will discuss in this book) are in our view inconsistent only with the current materialistic synthesis, summarized by Broad (1962) in the form of widely accepted "basic limiting principles." They do not obviously or necessarily conflict with more fundamental laws of nature, and indeed to claim such a conflict is to presume that we already know all the relevant laws, which hardly seems likely. The authors of this book emphatically do *not* believe in "miracles," conceived as breaches of natural law. Our attitude is that these seemingly anomalous phenomena occur not in contradiction to nature itself, but only in contradiction to what is presently known to us of nature. The phenomena we catalog here are important precisely because they challenge so strongly the current scientific consensus; in accordance with Wind's principle, they not only *invite* but should *command* the attention of anyone seriously interested in the mind.

Finally, we also wish to make clear immediately that in our view "empirical" research includes but is by no means limited to *experimental* research. Laboratory research using random samples of subjects, control groups, and statistical modes of data analysis can be wonderfully useful, but obsession with this as the only valid means of acquiring new knowledge readily degenerates into "methodolatry" (Bakan, 1967), the methodological face of scientism. Laboratory experimentation certainly does not exhaust the means of obtaining valid and important information. Detailed case studies of special individuals, such as persons displaying rare cognitive skills or having unusual neurological deficits, have often provided unique insights and indisputably can play a valuable role in the evolution of scientific understanding. Pertinent modern examples here are the investigations by Luria (1968) and Sacks (1987) of persons displaying prodigious abilities of memory and calculation. Conversely, the experimental literature itself is replete with examples of supposedly "rigorous" laboratory studies which

6. Statistically knowledgeable readers will recognize that critics of this type are acting in effect like Bayesians who have assigned a prior probability of zero to the existence of psi phenomena (see also Radin, 2006).

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were in fact performed under conditions that guaranteed their failure from the outset. A good example here is provided by the many superficial studies of "meditation" carried out by unsympathetic investigators, using as their subjects random samples of undergraduates having little if any experience or interest in meditation (M. Murphy & Donovan, 1997; M. A. West, 1987; see also our Chapter 8).

With these methodological principles in mind, we turn now to the substance of our book. In our opinion, the most systematic, comprehensive, and determined empirical assault on the mind-body problem ever carried out in the suggested spirit, during the entire long history of psychology, is summarized in F. W. H. Myers's (1903) undeservedly neglected two-volume work *Human Personality* (henceforth, *HP*). Myers's friend and colleague William James (1901) declared that "through him for the first time, psychologists are in possession of their full material, and mental phenomena are set down in an adequate inventory" (p. 16). Gardner Murphy (1954) praised "the heroic accumulation of data and amazing integration which the work represents" (p. iv). Ellenberger (1970) described Myers as "one of the great systematizers of the notion of the unconscious mind" (p. 314) and his book as "an unparalleled collection of source material on the topics of somnambulism, hypnosis, hysteria, dual personality, and parapsychological phenomena,...contain[ing] a complete theory of the unconscious mind, with its regressive, creative, and mythopoetic functions" (p. 788). James and various other writers have suggested that to the extent Myers's views are upheld by subsequent research he could rank with Charles Darwin in terms of the character, scope, and originality of his contributions. Myers also powerfully influenced many leading thinkers of the day, including both William James and Pierre Janet, but like James and Janet themselves he was soon pushed aside by the virulent behaviorism nascent at that period. However, just as James and Janet have undergone a major renaissance in recent years, as their central concerns and ideas have begun to reanimate the psychological mainstream, we believe that Myers's work deserves both wider recognition and careful re-examination for the light it can shed on the current situation in psychology.

The balance of our book attempts to further these aims. Chapters 1 and 2 provide essential background. We begin by reviewing modern developments in cognitive science, calling into question the ability of physicalist/computationalist models of the mind in any of their current forms to deal adequately with the most basic, central, and pervasive phenomena of mind and consciousness. We also identify a variety of specific empirical phenomena, and a variety of critical aspects of human mental life, that appear to resist or defy understanding in terms of the currently prevailing physicalist conceptual framework. The central objective of this exercise is to reduce whatever confidence in that framework readers may initially have, and thus to provide justification for revisiting the broader and deeper framework elaborated over a century ago by Myers, James, and their colleagues.

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The following chapter summarizes the contributions of F. W. H. Myers to empirical investigation of the mind-body relation. It begins with a brief summary of the relevant 19th-century intellectual background that suggests why Myers and his type of synoptic approach were ultimately ignored by the nascent materialist psychology. After outlining the purposes and principles in which Myers's work was rooted, the chapter goes on to describe Myers's theoretical model of human personality and consciousness, a model that is the most fully worked out example (so far) of "filter" or "transmission" theories of mind (James, 1898/1900; Schiller, 1891/1894), according to which mind is not generated by the brain but instead focused, limited, and constrained by it. The chapter also includes a description of the methodological principles and empirical phenomena which Myers considered essential for a fully comprehensive and adequate science of psychology.[7]

The next six chapters constitute the empirical heart of the book. We focus in detail on selected large classes of psychological phenomena, several of which were investigated in considerable depth by Myers himself, that appear especially challenging to contemporary mainstream views and capable of yielding new insight into the nature of the mind-brain connection. We do not attempt to review these topics exhaustively, but discuss them selectively in relation to their bearing on this central issue. In so doing, we also begin to assess the degree to which Myers's views have been sustained and confirmed, or must be modified or discarded, in light of subsequent work. Following Myers's own practice, we attempt to lead readers by degrees and without obvious discontinuity from phenomena which, though challenging, are well established and seem at least potentially compatible with current orthodoxy, to phenomena which in our view are just as well or nearly as well established, but clearly cannot be accommodated or understood without radical revision of our most fundamental theoretical ideas. The principal topics we discuss include extreme forms of psychophysiological influence, empirical and conceptual difficulties with current "trace" theories of memory, psychological automatisms and secondary streams of consciousness, the family of "near-death" and "out-of-body" experiences and related phenomena, genius-level creativity, and the world-wide psychological phenomenon of "mystical" experience.

In the final chapter we attempt to re-assess Myers's theory of human personality and to draw out additional implications of this book for future psychological theory and research. We underscore that Myers and James made the most comprehensive effort to date to analyze the mind-body rela-

7. This chapter provides a useful introduction to Myers, but it is no substitute for the real thing. With the hard cover version of *Irreducible Mind*, we included a CD containing the entire text of *Human Personality* in Microsoft Reader ebook format, as well as its most significant contemporary reviews and translations of all foreign text. This digital version and the reader are freely available at the Esalen website (http://www.esalenctr.org/display/hp_ctr.cfm) or through the University of Virginia Electronic Text Center (http://etext.lib.virginia.edu).

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tion *empirically*, and we urge scientific psychology to return to its great central problems with a comparably synoptic empirical approach, but supported now by the tremendous methodological and technical advances achieved in the intervening century. We further argue that Myers's theoretical scheme and the empirical phenomena he deployed to support it have held up remarkably well, and at many points have been substantially reinforced by subsequent research. Remaining difficulties and weaknesses, however, are also pointed out, together with critical opportunities and problems for further investigation. Finally, we attempt to show how the theoretical framework elaborated by Myers and James, although certainly incomplete and imperfect, can be reconciled with leading-edge contemporary physics and neuroscience, and prefigures an enlarged scientific psychology that can potentially overcome the historical fragmentation sketched above.

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to bring these complementary perspectives simultaneously to bear on our central subject matter in a systematic and mutually informed way seems to me altogether welcome, and long overdue.[2]

The History of Cognitive Psychology: A Thumbnail Sketch

The following three sections summarize the history of mainstream psychology in the English-speaking world from the advent of behaviorism to the present. So brief a sketch must necessarily be impressionistic, but I believe it is faithful to the main outlines of the subject as it has developed so far, and would be so regarded by most workers in the field. Useful general sources for readers wanting additional historical detail include Flanagan (1991), H. Gardner (1985), and Harnish (2002).

From James B. Watson to the Cognitive Revolution

The history of scientific psychology in the 20th century can be characterized, somewhat cynically perhaps, as a movement toward progressively less unsatisfactory analyses of the mind. I will pass quickly over the first half of this history. Noam Chomsky remarked parenthetically during a lecture on linguistics in 1964 that in his opinion the first half century of American experimental psychology would end up as a footnote in the history of science. That was a characteristically provocative Chomsky remark, but even then it seemed more right than wrong. The historian of psychology Sigmund Koch, an early advocate of behaviorism who evolved into perhaps its most ferocious critic, has repeatedly derided the simplistic scientism of the period, and marveled at the degree to which behaviorism, having sprung into existence under the banner of a "consoling myth of natural science rigor and systematicity," had so often "proceeded to liquidate its subject matter" (Koch & Leary, 1985, p. 942). During its heyday, from perhaps the 1920s to the late 1950s, behaviorism enjoyed extraordinary, almost monolithic, institutional and professional power. Even as late as 1990, the chairman of psychology at a major American university, in a symposium celebrating the centennial of the *Principles of Psychology,* characterized James's great book as mainly illustrating what psychology should *not* be; its positive attributes consisted mostly of those few scattered passages where James's observations corresponded to truths revealed by *real* psychology—the natural science devoted to analysis and control of behavior (Kimble, 1990).

Certainly one of the chief lessons to be derived here is that entire generations of industrious and able scientists can be captured by an ideology

2. The symposium recorded in Bock and Marsh (1993) provides an outstanding example, and one we have tried collectively to emulate.

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that is fundamentally unsound. I do not mean to say that behaviorism was all bad. Its central methodological impulse, emphasizing the importance of systematic empirical observation and measurement, was certainly healthy and remains so today. Even on that front, however, the early behaviorist program was unnecessarily narrow. Encouraged by the verificationist doctrine associated with the Vienna school of logical positivism (Ayer, 1952), behaviorists simply outlawed in principle all reference to anything not directly observable from the third-person standpoint. Stimuli, responses, and their supposedly lawful connections exhausted the scientifically legitimate subject matter. Even ignoring the often considerable difficulties in defining exactly what constitutes "a stimulus" or "a response," however, this methodological asceticism was not warranted by any independently established conception of the nature of science. Indeed, philosophers of science soon abandoned verificationism in its narrowest construction, recognizing that even classical physics, the archetypal science, did not hesitate to postulate entities and processes that could not be observed directly, but only through their lawful connections to other things that could.

The experimental psychology that began to evolve after Watson's 1913 manifesto can be viewed as a kind of operationalization of 19th-century associationist theories of the mind, in which "ideas" were replaced by behaviors, and complex behaviors were imagined as arising from simple ones through processes of conditioning and reinforcement. The bulk of this work was carried out with simpler organisms such as rats and pigeons, on the view that everything necessary for scientific psychology was present there, and in more accessible form. From the principles that emerged from such studies, it was hoped, we would eventually be able to build a psychology capable of accounting for all the complexities characteristic of human behavior. This specifically included what Descartes had insisted is uniquely ours, and a defining attribute of the mind—our use of language. A few mainstream behaviorists such as Edward Tolman and Egon Brunswick suggested that even the behavior of rats running their mazes might be guided by some sort of inner representation or map, but these suggestions were largely ignored.

Methodological behaviorism was subsequently reinforced for a time by a companion philosophical doctrine called logical or analytical behaviorism, which received perhaps its fullest expression in the influential book by Ryle (1949). It shared Watson's objective of exorcising the mind, "the ghost in the machine," but sought to achieve this objective by redefining mentalistic terms in terms of overt behavior, or dispositions to such behavior. Having a pain, for example, was to be construed as literally consisting in crying out, reaching for the aspirin, and so on. This relentlessly third-person approach to the mind seemed consistent with, and supportive of, the actual practices of behaviorist psychologists, but its problems as a philosophic doctrine soon became apparent.[3] It proved extremely difficult in practice

3. For lucid and accessible discussions of the various philosophic positions briefly canvassed in this section see for example the books of Churchland (1988), Heil (1998), Kim (1998), and Searle (1992).

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to specify, in finite detail and without covert reference to other mentalistic terms, the behavioral conditions in terms of which the original mentalistic terms were to be redefined. From a more commonsense point of view it also seemed to leave out precisely the things that are most important to us as human beings—in particular mental causation and our subjective conscious experience. We all know, for example, that *pain* and *pain behavior* simply are not the same thing. One can have a pain but not show it, or act as if in pain without actually being in pain. For these and other reasons analytical behaviorism fell generally out of favor, although echoes are still heard today—a primary source being Ryle's student Daniel Dennett (1991).

Logical behaviorism gave way in the 1950s and 1960s to a family of positions known collectively as identity theory. Its basic doctrine is that the apparent correlation between mental states and brain states is to be interpreted in one particular way. Specifically, it holds that the relevant mental and physical states are in some sense identical, the same things viewed as it were from inside versus outside. This is regarded not as a logical necessity, but as a fact that we have discovered empirically, through advances in psychology and neuroscience, just as we have discovered that the morning star and the evening star are one and the same.

The identity doctrine came in two forms: The first and stronger form, formulated by writers such Herbert Feigl (1958), U. T. Place (1956), and J. J. C. Smart (1959), holds that mental states can be subdivided into discrete natural kinds or *types*, and that each of these types can be identified with a corresponding type of neural process. A stock example is the supposedly general relationship between "pain" and "excitation of c-fibers." The weaker form of identity theory claims only that each individually occurring or *token* mental state is identical with some corresponding brain state. Note that type identity entails token identity, but not vice-versa.

Type-identity is a strong and interesting philosophic thesis which implies the possibility of reduction, and for these reasons many physicalists welcomed it; but it is certainly false. Quite apart from the difficulties of isolating appropriate "natural kinds" or types in either mental life or brain processes, it is certain that many such mental types, if they existed, would arise under wildly varying neurophysiological conditions. For example, linguistic behaviors involve mainly the left hemisphere in right-handed adults, but a mixture of both hemispheres or even mainly the right hemisphere in left-handers. The mind-brain system is in general enormously adaptable or "plastic." For example, superior general intelligence and linguistic functioning have been observed in a man whose entire left hemisphere had been removed at age 5½ for control of seizures (A. Smith & Sugar, 1975). Fully functioning adults are also occasionally discovered who altogether lack major neural structures such as the corpus callosum or cerebellum, structures that are usually thought to be required for such functioning. In some well-studied cases of hydrocephalus, normal or even exceptional mental functioning has been found in persons who have only 3–5% of the normal volume of brain tissue (R. Lewin, 1980). And to take a still more extreme

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case, high-level forms of learning and memory are certainly present in the octopus, an invertebrate whose nervous organization is radically different from ours. It even lacks a hippocampus, the one structure that everyone agrees plays an essential role in mammalian memory systems (Chapter 4).

Type identity, therefore, has had little appeal for psychologists and neuroscientists, who undoubtedly gravitate in the vast majority—to the extent they think about such things at all—to some sort of token-identity view. This was essentially the position advocated by James in *The Principles*, although he disavowed atomism in all forms and took pains to insist that the level at which the intersection is appropriately sought is that of whole momentary states of consciousness and whole momentary states of the brain. *Token* identity, on the other hand, has had relatively little appeal for philosophers. Among other things it creates a new and serious problem: If the same mental state, say a certain belief, can exist in combination with different sorts of physical states in brains, what is it about all those physical states that makes them "the same" as their common mental counterpart?

A philosophic response to this problem is the doctrine known as *functionalism*, first formulated by Hilary Putnam (1967).[4] Having rejected type identity, on grounds that a given mental state might conceivably occur in extraterrestrial beings, or more relevantly in computers, Putnam went on to propose a novel solution to the problem just noted. His basic idea was to reconceptualize mental states once again, this time not in terms of what they are *made of*, but in terms of what they *do*, their causal role in the functional economy of whatever sort of creature or entity is in question. Just as "cutting tools" can be implemented using rocks, metals, or laser beams, mental states are to be conceived as "multiply realizable"—that is, potentially instantiated in a variety of physical forms including not only token biological states of one or many brains, but also in computers and other suitable kinds of complex physical systems. On this view mental states become simply Xs, defined by their causal relations to stimuli, to other mental states, and to responses, and they can be identified as similar states to the extent they perform similar roles in their respective causal networks.

I will make only a few brief comments on this doctrine, which in various forms has dominated the philosophy of mind for almost 40 years. First, as originally formulated it was inherently and fundamentally third-person and behavioristic, albeit a refined behaviorism that admits the possibility of complex causal processes—mental causes, in effect—mediating between stimuli and responses. Like the earlier forms of behaviorism, it initially avoided all reference to consciousness and subjective features of mental life. This was widely felt to be unsatisfactory, however, and a large part of the subsequent history of functionalism consists of strained attempts to "naturalize" first-person phenomena of these sorts. Second, although functionalism readily affiliates itself with both physicalism in general and token identity theories in particular (and in most of its adherents probably still does), such affilia-

4. But note that Putnam himself subsequently abandoned functionalism, becoming one of its severest critics; see Putnam (1988/1998).

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tions are not an essential or inherent aspect of the doctrine. J. Fodor (1981a), for example, pointed out that functionalist principles might perfectly well apply to the operations of immaterial minds and the like, should any such things exist. Finally, it is fair to say, I think, that functionalism arose not so much *sui generis* in philosophy, but rather as a response to some exciting new developments which had already occurred within scientific psychology itself, and with which Putnam was certainly well acquainted. In any event, it was the confluence of these streams that defined the emergence of the 20th century's most distinctive contribution to mind-brain theory, the "Computational Theory of the Mind" (henceforth, CTM). I turn now to the psychological dimensions of this story.

By the late 1950s discontent with behaviorism was rapidly spreading, as its inherent limitations became increasingly apparent. An influential paper by Lashley (1951) had exposed fundamental difficulties in the attempt to explain complex behavior, notably human linguistic behavior, in terms of linear chains of stimuli and responses. B. F. Skinner, the leading behaviorist, responded to this challenge, but his book on verbal behavior was subjected to a destructive and widely circulated review by Chomsky (1959). Most significantly of all, perhaps, a comprehensive state-of-the-science review organized by Sigmund Koch under the auspices of the American Psychological Association and the National Science Foundation resulted in a sweeping, 6000-page, six-volume indictment of the entire behaviorist platform (Koch, 1959–1963).

At the root of these discontents was a recognition that the old associationist explanatory principles, and their behavioral translations, were in principle unable to cope with the hierarchically organized and orderly character of human language and cognition. We needed a richer concept of mechanism. And as it happened, possible means of overcoming these limitations were just then becoming available, due to fundamental developments in the theory and practice of computation.

The old concept of a "machine"—and perhaps for most of us still the everyday concept—is that of a physical contraption which transforms energy by means of pushes and pulls involving gears, pulleys, shafts, levers, and so on. The fundamental insight underlying the modern developments is the recognition that these physical arrangements are really of secondary importance. The essential attribute of the machine is rather that its normal behavior is *bound by rule*. This insight opened the way to an enormous enrichment of our concept of mechanism, beginning with the contribution of logicians and mathematicians in the 1930s and 1940s and continuing into the present day. These developments, moreover, immediately began to have a profound impact on scientific psychology.

For example, it was quickly recognized that machines can transform data or "information" as well as energy, and that a machine can in principle utilize information about its performance to regulate its own behavior. These ideas had immediate and urgent practical application in the construction of servo-controlled antiaircraft systems during World War II, but pos-

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of theory which is still strong enough to account for the known grammatical facts of language. The result that the corresponding automata are weaker than Turing machines greatly strengthened the presumption that linguistic behavior might be formalizable for computer modeling.

The central idea that minds, brains, and computers could fruitfully be regarded as variants of a more general class of information-processing mechanisms quickly took root, even among neuroscientists (W. J. Freeman, 1998; von Neumann, 1958). The ground was very well prepared. Indeed, these developments seem to me an inevitable outcome of our Western scientific tradition. This is not meant disparagingly, however. I have stressed these results about Turing machines and so on precisely to underscore the impressive depth of the theoretical foundation on which all the ensuing developments rest, a foundation which I feel has not been adequately appreciated by many critics of this kind of work, such as Edelman and Tononi (2000), nor even by some of its enthusiastic supporters, such as H. Gardner (1985).

In practice, the applications came a bit slowly at first. In part this was due to purely technical factors. The early computers were small, slow, and highly prone to malfunction. More importantly, in the early days programming a computer was an exasperating business requiring detailed familiarity with low-level details of the hardware organization. Indeed, the basic elements of the available programming languages referred to data structures and operations virtually at the hardware level. As the technology advanced, however, machines grew larger, faster, and more reliable, and so-called "higher-order" languages were created, such as FORTRAN, whose primitives referred to data structures and operations at a level relatively natural for human problem-solvers. Special-purpose applications programs written in a higher-order language congenial to the user could then be translated by general-purpose "compiler" or "interpreter" programs into the internal language of the computer for execution by the hardware.

I mention these details because they relate to the other main reason for the delay, which is more theoretical in nature and involves a basic question of strategy. The fantastic complexities of the brain can obviously be studied at many different levels from the cellular or even sub-cellular on up. At what level shall we seek scientific explanations of human mental activity? Many scientists, particularly those working at lower levels of the hierarchy of approaches, assume that events at the higher levels are in principle reducible to events at lower levels, and that reductive explanations employing the concepts of the lower levels are necessarily superior or more fundamental. Like many other psychologists, I strongly disagree with this view.

Consider, for example, the problem of understanding the behavior of a computer playing chess under the control of a stored program. It seems obvious that we might observe the behavior of the computer's flip-flops forever without gaining the slightest real understanding of the principles that are embodied in the program and explain its behavior. One of the essential characteristics of both human and animal behavior is that functional invariance at a higher level may be coupled with extreme diversity at lower levels.

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For example, the rats whose cortex Lashley (1950) progressively destroyed in efforts to locate the memory trace could wobble, roll, or swim to their food box, and I can write a given message with either hand, or probably even with my feet if necessary. Attempted explanations based on activities of the participating muscle-groups, neurons, and so on would never get to the essential common feature, which is the approximate invariance of the final behavioral outcome.

Thus it seems appropriate in general to seek a higher and perhaps distinctively "psychological" level of explanation for human cognitive processes. For the computer simulation approach this involves identifying an appropriate set of elementary information structures and processes that seem powerful enough in principle to account for the relevant behaviors. The hypothesized structures and processes should perhaps also be potentially realizable in the brain, given its known properties—or at least not demonstrably inconsistent with these properties—but successful use of the computer as a tool of psychological understanding does not require the obviously false presumption of literal identity between digital computers and brains.

By the late 1950s and early 1960s, a number of higher-order programming languages had been created which emphasized the capacities of computers as general-purpose symbol-manipulating or information-transforming devices, rather than their more familiar "number-crunching" capacities. These languages (such as IPL-V, LISP, and SNOBOL) provided facilities, for example, for creating and manipulating complex tree-like data structures consisting of hierarchically ordered and cross-referenced lists of symbols. Structures of this sort played a central role in theoretical linguistics, and in this and other ways the new languages seemed to many workers to fall at about the right level of abstraction to support realistic efforts to model important aspects of human cognition.

Previous generations of workers had been obliged either to try to force human mental processes to conform to artificially simple but relatively rigorous behavioristic models, or to lapse into the uncontrolled introspection and mentalistic speculations of an earlier era. Now suddenly we were provided with a conceptual and technical apparatus sufficiently rich to express much of the necessary complexity without loss of rigor. The "black box" could be stuffed at will with whatever mechanisms seemed necessary to account for a given behavior. A complicated theory could be empirically tested by implementing it in the form of a computer program and verifying its ability to generate the behavior, or to simulate a record of the behavior. Progress toward machine intelligence could be assessed by means of "Turing's test"—the capacity of a computer program to mimic human behavior sufficiently well that a physically remote human interlocutor is unable to distinguish the program from another human in an "imitation game" (Turing, 1950). The seminal modeling ideas of Craik (1943) could at last be put into practice.

Enthusiasm for the computational approach to human cognition fairly crackles through the pages of influential early books such as G. A. Miller,

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Galanter, and Pribram (1960), Lindsay and Norman (1972), and Newell and Simon (1972). Their enthusiasm seemed justified by the ensuing flood of theoretical and experimental work based on these ideas. In addition to the specific efforts at computer modeling of cognitive functions that is my main concern here, the rise of the computer-based information-processing paradigm also stimulated a healthy reawakening of broader interests in many of the old central concerns of psychology, such as mental imagery, thinking, and even consciousness. I must also acknowledge here that I myself initially embraced the computational theory practically without reservation. It certainly seemed an enormous step forward at the time. Fellow graduate students likely remember my oft-repeated attempts to assure them that the CTM would soon solve this or that fundamental problem in psychology. But all was not well.

Problems in Classic Cognitivism

With the appearance of suitable higher-order languages, numerous research groups set to work to endow computers with capacities for various kinds of skilled performance, including game-playing (especially complex games such as chess), problem-solving (for example, proving theorems in the propositional calculus), pattern recognition (such as recognizing sloppy hand-written characters), question-answering (in restricted domains such as baseball), and natural language translation. An important strategic difference quickly appeared, dividing these efforts roughly into two streams often called computer simulation (CS) and artificial intelligence (AI), respectively. Workers in CS remained faithful to the aim of increasing psychological understanding, in that they sought not only to reproduce particular kinds of human performance, but also to identify testable models of the means by which humans achieve that performance. AI workers, by contrast, disavowed any direct interest in psychology and sought rather to achieve high-level performance by whatever means possible.

I will not attempt to review the substantive accomplishments here.[6] Suffice it to say that many of the individual contributions represented considerable intellectual and technical achievements. Especially dazzling for me was "SHRDLU," a virtual robot that could interact with its handler in more or less ordinary-appearing language while carrying out complex sequences of requested operations on toy blocks of varied color, size, and shape (Winograd, 1972).

However, without intending to disparage these attainments I must add that they still fell very far short of what anyone could plausibly describe as general intelligence. I mention this only because of the extremely inflat-

6. Readers interested in following the history of such work can refer to landmark publications such as Feigenbaum and Feldman (1963), Minsky (1968), Schank and Colby (1973), Winograd (1972), and Winston (1975), or for an overview H. Gardner (1985).

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ed image which many outside observers had at the time of the progress of this work, an image aggressively promoted by some of the research workers themselves. Many extraordinarily grandiose statements and predictions were being bandied about on the basis of very modest concrete accomplishments. Of course the predictions could conceivably have been correct; after all, the theoretical foundation is deep, and the work was still in its infancy. Perhaps we were simply in the early stages of an evolutionary process in which machines would inevitably attain at least the equivalent of human cognitive abilities.

My own confidence in the new paradigm was severely shaken, however, by extensive and sobering experience associated with my dissertation research in the area of natural language processing. The group with which I was working was principally concerned with the applied technical problem of reducing lexical ambiguity in English text, the practical aim being increased precision and power of automated computer-based content analysis procedures. In approaching this problem, we constructed an alphabetized concordance of some half-million words of "typical" behavioral science texts sampled from a variety of sources. This keyword-in-context listing (KWIC) identified the most frequently occurring words and supplied information about their typical patterns of usage. It turned out that about 2,000 *types* (in this context, dictionary entries) covered around 90% of the *tokens* (occurrent words) in an average running text. The striking regularities of word usage evident in the KWIC enabled us to build for each such dictionary entry a small computer routine that could scan the context in which each new token instance of that entry appeared and attempt to determine which member of a pre-established set of meanings was actually present. Crude as it was, the resulting system worked surprisingly well, achieving about 90% accuracy in a large new sample of text (E. F. Kelly & Stone, 1975). So at a practical level the project was rather successful.

My own primary interest, however, lay in determining whether the brute facts of everyday language as we were seeing them could successfully be captured by existing computationalist theories of semantic representation. The main such theory at the time was that of J. J. Katz and Fodor (1964). Their theory built upon the central notions of Chomsky's transformational linguistics and embodied the essential doctrines of the classical computationalist program as subsequently formalized by J. Fodor (1975), Pylyshyn (1984), and others. Specifically, it depicted determination of the meaning of a sentence as resulting from a rule-bound calculation, governed by the syntactic analysis of that sentence, which operates upon pre-established representations of the meanings of its constituent words. The possible meanings of the words themselves were treated as exhaustively specifiable in advance, with their essential semantic contents represented in terms of an underlying universal set of discrete "semantic markers"—in effect, atoms of meaning—or logical structures built out of such markers. In a nutshell, syntax was to provide for the generativity of language, our ability to make "infinite use of finite means," and semantics would go along for the ride.

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Although Katz and Fodor had made their account appear at least semiplausible for a few carefully contrived examples, it failed totally to account for obvious and pervasive features of the language in our database, language as actually used by ordinary speakers for ordinary purposes. For one thing, it was clear that resolution of lexical ambiguity routinely relied upon all sorts of linguistic and non-linguistic knowledge, and not just on some supposedly "essential" knowledge of pre-established word meanings. More importantly, sentence interpretation could not plausibly be viewed as a matter of context-driven *selection* from large numbers of pre-established and highly specific word meanings. Rather, it clearly involved *generation* of appropriate interpretations based on much smaller numbers of more general meanings (see, e.g., the entries provided by different dictionaries for common workhorse words such as *among*, *find*, *fine*, and *line*, and note that all major parts of speech display these properties). Any scheme based on atomization of meaning would necessarily fail to capture what to me had become the most characteristic property of word-meaning, a felt Gestalt quality or wholeness, at a level of generality that naturally supports extensions of usage into an indefinite variety—indeed whole families—of novel but appropriate contexts. The existing proposals could only represent the content of a general term such as "line" by some sample of its possible particularizations, and in so doing rendered themselves systematically unable to distinguish between metaphorical truth and literal falsehood. It seemed especially ironic that Katz and Fodor, for all their transformationalist invective about the need to respect the productivity of language, had erected a semantic theory which in effect elevated the commonplace to a standard of propriety and denied creativity at the level of words or concepts. Their theory failed to account even for *identification*, let alone *representation,* of linguistic meaning (E. F. Kelly, 1975).

I also noted with a certain degree of alarm that this crucial property of generality underlying the normal use of words seemed continuous with developmentally earlier achievements. Skilled motor acts and perceptual recognition, for example, require similar on-the-fly adaptations of past learning to present circumstance. It seemed at least vaguely conceivable that these difficult but undeniably fundamental qualities of embodied action and cognition might somehow be rooted in lower-level properties of the nervous system as a particular kind of computing machine. Early discussions had emphasized similarities between brains and digital computers, for example treating the all-or-nothing neural spike discharge or action potential as the equivalent of a digital relay. In more recent times, however, we had become increasingly sensitized to the ubiquitous presence in real nervous systems of inherently more continuous or "analog" processes, such as the spatial and temporal summation of neural input that leads to spike formation, and the rate and pattern of the resulting spike discharges. Workers in CS and AI had been relying heavily on their doctrine of "multiple realizability," assuming perhaps too cavalierly that they could safely disregard such low-level "hardware" details and pitch their efforts at a level of abstraction that happened

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The most significant by far of these rumblings from within, however, occurred a decade later when a consummate insider, Terry Winograd himself, publicly defected from the program of classical AI. Winograd and Flores (1986) explicitly embraced most of the points already raised above, emphasizing in particular that large parts of human mental life cannot be reduced to explicit rules and therefore cannot be formalized for production by a computer program. Their detailed and fully-informed argument should be required reading for anyone interested in these issues.[9] The best we can hope for along classical lines, they concluded, is special-purpose expert systems, adapted to carefully circumscribed domains that lend themselves to such formalization. Current examples, of course, include things like chess-playing, integration and differentiation of mathematical formulae, and perhaps certain areas of medical diagnosis.

The Second Cognitive Revolution: Connectionism and Dynamic Systems

Since the late 1970s psychology has taken a strongly biological turn, and cognitive science has evolved into cognitive neuroscience. This came as somewhat of a surprise to persons like myself who had been reared in the intellectual tradition of classical or "symbolic" cognitivism and had expected to do all their proper business with little or no reference to biology. When I was a graduate student, for example, the required course in physiological psychology consisted essentially of a smattering of neuroanatomy followed by a lot of boring stuff about appetitive behavior in lower organisms, and that was pretty much that.

I will single out four main threads of this evolution. The first arose within classical cognitivism itself. As indicated above, there was a lot of ferment in the early days as researchers sought to identify appropriate forms of representation for the sorts of knowledge we humans can bring to bear in our thinking, speaking, and so on. Largely reflecting its own historical origins and the available *means* of representation, cognitive theory initially focused almost exclusively on linguistic or propositional forms of knowledge representation. Mental imagery, however, had recently been readmitted onto the roster of acceptable research topics (R. R. Holt, 1964), and a number of important new experimental studies were being carried out, especially by Roger Shepard, Stephen Kosslyn, and various others. On the strength of these investigations Kosslyn (1973) ventured to advance an information-processing theory of visual imagery in which the underlying knowledge structures hypothesized to support the generation of imagery were themselves essentially pictorial and spatial in nature. This provoked an intense reaction from

9. Fundamentally similar views have also been advanced by writers such as Polanyi (1966) and Searle (1992, chap. 8). It is ironic, and perhaps symptomatic, that CTM advocate Robert Harnish (2002, pp. 107–123) reports enthusiastically on SHRDLU without even mentioning Winograd's defection.

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Zenon Pylyshyn (1973), who reaffirmed the classical cognitivist view that *all* knowledge is linguistic or propositional in character (J. Fodor, 1975). The ensuing imagery debate has raged on more or less ever since, its intricate experimental details of interest primarily to the participants.[10] For present purposes the critical event was an important theoretical paper by John R. Anderson (1978). Anderson argued that since *both* kinds of representations (and potentially many others as well) could be made internally coherent, and would lead to identical behavioral predictions, a fundamental theoretical indeterminacy had emerged. Considerations of parsimony and efficiency might lead us to *prefer* one such theory to its competitors, but only physiological observations could potentially determine which was in fact *correct*. Thus it became evident, more generally, that neurophysiological data can sometimes provide important constraints on psychological theory.

The second thread was the emergence and consolidation following World War II of neuropsychology as a scientific discipline. An outgrowth and companion of the medical discipline of neurology, neuropsychology seeks to gain insight into the nature and operations of the mind through careful observation and analysis of the mental disturbances, sometimes highly specific and bizarre, that are produced by gunshot or shrapnel wounds, strokes, degenerative diseases, and other forms of injury to the brain.[11] The general thrust of this work was to suggest that skilled cognitive performances of all kinds characteristically involve cooperation of a number of localized cortical and subcortical regions of the brain, each presumptively specialized for some particular role in the overall performance. Classical cognitivism quickly adapted to this emerging picture of things, assimilating its fundamental theme of the mind as a computational or information-processing mechanism to a "modular" view of its components and internal organization (J. Fodor, 1983; Pinker, 1997).

The third development was the advent and maturation of the functional neuroimaging technologies mentioned in the Introduction, which enable us to observe directly, without opening up the skull, the activity of the working human brain. Two principal classes of such methods are currently available, although others are under development.[12] The first provides measures of the electric and magnetic fields directly generated by neuroelectric activity in populations of suitably located and oriented cortical neurons (elec-

10. But see Brann (1991) and N. J. T. Thomas (1999) for excellent third-party reviews.

11. Some important early landmarks here are books by H. Gardner (1976), Luria (1966), Sacks (1987), and Shallice (1988).

12. In addition to the "macro" technologies described in the text, which operate on scales from the whole brain down to roughly naked-eye-sized parts of it, contemporary neuroscience has developed an impressive arsenal of "micro" technologies, suitable mainly for use in animal studies, that operate down to the cellular or even subcellular level. Other emerging developments at the macro level that look especially promising for future work in humans include transcranial optical imaging and transcranial magnetic stimulation.

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troencephalography [EEG]; magnetoencephalography [MEG]). The second provides measures of hemodynamic or metabolic consequences of neural activity such as local changes of blood flow, glucose consumption, or blood oxygenation (especially positron emission tomography [PET]; and functional magnetic resonance imaging [fMRI]). All these technologies are complex and expensive, and they tend within and between classes to have complementary strengths and weaknesses (see, e.g., Nunez & Silberstein, 2000; Toga & Mazziotta, 1996; Wikswo, Gevins, & Williamson, 1993). Together, they undoubtedly constitute a major methodological advance for cognitive neuroscience. Indeed, scarcely an issue now goes by of any cognitive neuroscience journal that does not contain one or more papers featuring images outfitted with colored spots identifying regions of "significant" brain "activation" produced by some stimulus or task.[13]

The final thread is the one most directly germane to our primary subject, the computational theory of the mind (CTM). Specifically, discouragement with the progress of classical or symbolic cognitivism led in the 1980s to an enormous resurgence of interest in a fundamentally different style of computation that also seems more directly comparable to what actually goes on in brains—namely, propagation of activity through large networks of basically similar elementary units.

I say "resurgence" because this approach actually harks all the way back to the seminal work of McCulloch and Pitts (1943), Hebb (1949), and others. There had been some promising early steps in this direction, notably the work of Rosenblatt (1962) on "perceptrons," but mainstream interest had largely faded under the impact of a devastating critique by AI partisans Marvin Minsky and Seymour Papert (1968). These authors, mathematicians by training, proved rigorously that simple one-layer perceptrons cannot compute even simple things such as the elementary logic function "exclusive OR" (that is, either A or B but not both). In their conclusion, summing up the implications of these results, they conjectured that more complicated networks would fare little better. In effect they urged the field to stay focused on the CTM in its classic symbol-processing form. One of the great ironies here, as pointed out by Dupuy (2000), is that whereas John von Neumann, the great mathematician, had been interested in expanding the possibilities of computation by reference to the actual behavior of the brain, Warren McCulloch, a neuroscientist, had in effect abstracted from the brain a logic machine; and now here was Minsky, who had been von Neumann's doctoral student at Princeton, promoting McCulloch-style symbolic computation in preference to the neurophysiologically grounded approach advocated by his own illustrious mentor.

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13. Most cognitive neuroscientists clearly regard such work as repeatedly and unambiguously confirming the basic mind-brain doctrine of contemporary physicalism, but many significant technical uncertainties remain regarding procedures for acquisition, processing, and interpretation of imaging data, and some of the available results actually conflict with that doctrine. We will touch upon these issues later in this chapter, as well as in our Chapters 4 and 9.

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At any rate the net effect was to drive neural-network models temporarily to the margins of the field, although a few dedicated souls such as James Anderson and Stephen Grossberg soldiered on. The subject burst back into the mainstream, however, with the publication of a two-volume handbook on parallel distributed processing (PDP) or "connectionism" as it came to be called (Rumelhart & McClelland, 1986).[14] Connectionism has subsequently emerged as a serious rival to classical cognitivism, but I will give here only the briefest account of these developments.

The fundamental faith of connectionists is that intelligence emerges from the interactions of large numbers of simple processing units organized in a network of appropriate structure. By modifying features of network architecture such as the number of elementary units, the number of layers, their connectivity patterns (feed forward only vs. recurrent), the rules for activating units (simple thresholds, sigmoid functions), and the rules for modifying the connection strengths among units in light of network performance or experience (Hebb, delta, generalized delta), an enormous variety of interesting behaviors can be produced. Networks have proved especially good at some things at which classical models were conspicuously bad, such as pattern recognition, perceptual learning and generalization, and filling in of incomplete input. They also display psychologically interesting and desirable properties such as content-addressable memory and "graceful degradation"—the tendency, as in Lashley's rats, for performance to decline smoothly and continuously as units are progressively removed. It is perhaps not surprising, given these properties, that unbridled optimism soon reappeared within the field. One leading connectionist, Smolensky (1988), has proclaimed that "it is likely that connectionist models will offer the most significant progress of the past several millennia on the mind/body problem" (p. 3). Many contemporary psychologists agree, and even some philosophers of mind, including in particular Daniel Dennett and the Churchlands, are no less enthusiastic.

Significant problems have also come to light, however. Although network models are often said to be "neurally inspired," the current level of neurophysiological realism is typically very low. Both the "neurons" themselves and their connectivity patterns are routinely idealized and distorted, and the most successful learning rule ("back propagation" or generalized delta) still has no generally recognized counterpart in the nervous system (Crick, 1994). Models often have large numbers of free parameters which must be adjusted to fit specific situations, raising doubts about their generality. Similarly, generation of a targeted behavior is sometimes strongly and unrealistically dependent on the exact content and order of previous network experience or training. Many observers, including Papert (1988), also worry that problems of catastrophic interference between previously learned behaviors will emerge as networks are scaled up to more realis-

14. Excellent general introductions are provided by Bechtel and Abrahamsen (2002) and Harnish (2002), and many of the most important early papers are reprinted, with illuminating commentaries, by J. A. Anderson and Rosenfeld (1988).

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tic dimensions. Most important of all, networks have particular difficulty capturing precisely those characteristics of human cognition to which the classical cognitivist approach seems best suited—features such as its hierarchical organization, temporal orderliness, and productivity. Indeed, despite some new technical wrinkles, connectionism can be viewed as amounting in large part to a modern revival of 19th-century associationism. To the extent that this characterization is correct, connectionism therefore inherits the very same problems that led to the rise of classic cognitivism in the first place (J. Fodor, 2001; Pinker & Mehler, 1988).

The last 20 years or so of computational modeling of the mind have been dominated by the attempts of these two warring paradigms to refine themselves and work out their relationship. Classical symbol-manipulation modelers, for example, are trying to find ways to make their models more adaptable and less brittle, while connectionists are looking for better ways of generating orderly behavior in the absence of explicit rules. Each paradigm clearly aspires to absorbing the other, but some workers are also exploring "hybrid" computational systems in which the connectionist part takes care of things like perceptual learning and category formation—the subsymbolic level or "microstructure" of cognition—while a more classical symbol-manipulation part deals with things like problem-solving and language. It is not at all clear how things will sort out, although the connectionist faction seems presently ascendant.

I must also mention, however, one further recent development emerging from within connectionism itself, one that threatens in effect to devour it from the other side. This movement, dynamic systems theory, has close ties to modern technical developments in the physics and mathematics of complex systems. There can be no doubt that the brain, with its vast network of reciprocally interconnected and non-linear elements, is an example of the kind of systems to which these new developments apply. Dynamic systems theorists often use typical connectionist technical apparatus to synthesize network models, but they bring to bear additional mathematical and graphical tools to characterize and analyze the temporal dynamics of network behavior. The most distinctive property of this school, however, is its more radical theoretical position. In contrast with mainstream connectionism, which it regards as a kind of halfway-house or unholy compromise with classical cognitivism, it advocates abandoning altogether cognitivism's next deepest commitment after computation itself—namely, the concept of a *representational level.* From the dynamicist point of view what goes on inside our heads is nothing like what the classical formulation presumes, with computational processes operating on stored symbolic knowledge-representations of one or more sorts. There is instead only the mechanical operation of a vast neural network in accordance with deterministic rules. As Dupuy (2000) observed, this is clearly the direction in which the early cyberneticians such as von Neumann were headed. It is also the view of

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Second, this "passion" undoubtedly accounts for the apparent inability of many psychological and philosophical defenders of the CTM even to report Searle's message accurately, let alone to appreciate its force. Without dwelling upon the details, see for example Bechtel and Abrahamsen (2002, pp. 303–304), Boden (1990), Chalmers (1996, chap. 9), H. Gardner (1985, pp. 171–177), Harnish (2002, chap. 9 & chap. 13), and Pinker (1997, pp. 93–96). Many others, such as Minsky (1985), Newell (1990), and Thagard (1998), essentially ignore him altogether. Searle defends himself much better against his critics than I could do on his behalf, and fortunately he has taken a number of opportunities to do so. In addition to the original debate over the Chinese room (Searle, 1980), see in particular his encounters with the Churchlands (Searle, 1990) and with Dennett (Searle, 1997).

Third, although the initial targets of Searle's arguments were strong AI and cognitivism in its now classic "symbol-processing" formulation, there is no doubt that his arguments apply to computationalism in all of its forms. Searle made this explicit for standard forms of connectionism in his exchange with Dennett (Searle, 1997), and I think he would certainly do so for dynamic systems theory as well, and for the same reasons. The fundamental commonality of the different sorts of models is underscored by the fact that they all are typically implemented in the same sort of physical architecture, the standard von Neumann architecture used in garden-variety personal computers. Specifics of the computing architecture can profoundly influence the speed and efficiency of computation, but this does not alter its fundamental character and has no bearing on the core theoretical issues.[19]

In sum, I feel sure that Searle's negative judgment regarding the computational theory of the mind will be sustained. Like Nagel (1993a), I have come to regard the CTM as one of the dominant illusions of our age. I hasten to add that this judgment has little *practical* import for cognitive science; for as Searle himself points out, and as I remarked above, use of computers to model particular cognitive functions is a perfectly legitimate and undoubtedly useful scientific activity. But for a fundamental theory of the mind we must look elsewhere.

To the extent that physicalist accounts of the mind are identifiable with the CTM, Searle and his allies have already discredited them. However, physicalism has not yet exhausted its resources. Searle's further and positive contribution is to advocate a novel philosophic position which he calls "biological naturalism," a position which in fact closely approximates the views of many leading neuroscientists. I turn next to this.

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ligiosity in materialism as a philosophic doctrine goes back at least as far as Empedocles, who was called "soter" (savior) by his followers.

19. Parenthetically, shifting the effective *locus* of the computations to a level above (E. L. Schwartz, 1999) or below (Penrose, 1989, 1994) that of single network elements also has no bearing on these issues.

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Biological Naturalism: The Final Frontier

To set the stage I will quote the most trenchant statement known to me regarding the current status of the mind-body problem. It is from philosopher Thomas Nagel (1993b):

> The mind-body problem exists because we naturally want to include the mental life of conscious organisms in a comprehensive scientific understanding of the world. On the one hand it seems obvious that everything that happens in the mind depends on, or is, something that happens in the brain. On the other hand the defining features of mental states and events, features like their intentionality, their subjectivity and their conscious experiential quality, seem not to be comprehensible simply in terms of the physical operation of the organism. This is not just because we have not yet accumulated enough empirical information: the problem is theoretical. We cannot at present imagine an explanation of colour perception, for example, which would do for that phenomenon what chemistry has done for combustion—an explanation which would tell us in physical terms, and without residue, what the experience of colour perception *is*. Philosophical analyses of the distinguishing features of the mental that are designed to get us over this hurdle generally involve implausible forms of reductionism, behaviouristic in inspiration. The question is whether there is another way of bringing mental phenomena into a unified conception of objective reality without relying on a narrow standard of objectivity which excludes everything that makes them interesting. (p. 1)

Searle believes he has a good physicalist answer—indeed, in its general form the only *possible* answer—to Nagel's question. It goes like this: One can accept the reality of conscious mental life without lapsing into dualism, which (he thinks) would be tantamount to abandoning 400 years of cumulative scientific achievement. Consciousness is a biological process as physical as digestion, caused by low-level neurophysiological events in the brain such as neuron firings and the like. But consciousness is not a separate something, over and above the brain action; the causality is not of the boy-hits-ball-then-ball-breaks-window sort. Rather, consciousness *emerges*, in roughly the same way that solidity emerges when water freezes upon reaching a certain critical temperature. It is a system-level property of the brain. And this is not epiphenomenalism, the idea that consciousness is real but ineffectual; like the solidity of ice or other emergent physical properties, consciousness has causal consequences, causal powers.

Although consciousness is thus at least in principle *causally* reducible, it is not *ontologically* reducible; for this would involve showing that what appears to us as consciousness is in reality something else, but in the case of consciousness itself, unlike the apparent rising and setting of the sun, the appearance *is* the reality. For Searle the CTM was on the right track, insofar as it was a materialist theory of the mind, but it was not nearly materialistic enough. To imagine that the mind can be instantiated in computer programs of any kind is to entertain a kind of residual dualism. The brain generates

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mind and consciousness by physical, causal mechanisms inherent in our biology, and we need to respect that unique biological reality. This does not entail that mind and consciousness could not appear in other places, for example in some sort of suitably constructed artifact; but in order for this to occur, the physical substrate of the artifact would need to embody causal powers at least equivalent to those inherent in the brain itself.

That summarizes the central theoretical part of Searle's position. He also goes on to characterize the phenomenology of consciousness in ways that are strikingly reminiscent of James (1890b), and considers in general terms how best to pursue its scientific study. Clearly, given his theoretical commitments, the main order of business is to study the associated neurophysiology. Searle (1997) singled out for special praise neuroscientists such as Gerald Edelman and Francis Crick who were in effect carrying out this program. His admiration for real neuroscience is certainly justified, and it is apparently reciprocated; in a recent paper in the *Annual Review of Neuroscience* he again summarizes his theoretical views regarding consciousness and offers a variety of additional suggestions, both shrewd and detailed, for further neuroscientific investigation (Searle, 2000).

To summarize: In my view John Searle deserves enormous credit for having almost single-handedly altered the terms of the mind-brain controversy. On the negative side, his destructive critique of the CTM should eliminate from contention a family of views that have dominated the past half-century, especially in psychology. On the positive side, his effective advocacy of "biological naturalism" has focused attention clearly and sharply on the position that I believe represents the ultimate development, the necessary culmination, of a conventional physicalist approach to the mind. The fundamental question before us now reduces starkly to this: Can everything we know about the mind be explained in terms of brain processes?

Problems with Biological Naturalism

First, briefly, some conceptual issues. I am not at all persuaded that Searle has succeeded in making meaningful distinctions between his philosophical views and those of a host of others who would describe themselves as advocates of token identity or even property dualism. In particular, his abstract account of the "emergence" of consciousness strikes me as verging upon intellectual sleight of hand. Can mental properties simply be stipulated into physicality in this way? The analogies seem faulty. In the water/ice situation, for example, both ends of the causal relation are indisputably physical things that we know how to observe and measure in conventional physical ways. But that is exactly what is at issue in the relation of brain to mind, as Nagel pointed out.

Related to this, and also captured by Nagel's remarks, we do not in fact have anything even remotely resembling a full causal account of conscious-

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ness, let alone an account that we can understand in the way we understand the freezing of water. Intelligibility of causal accounts is surely something we would like to have, but in this case it seems singularly difficult to imagine how we could possibly get one. I will leave aside here, however, the more philosophic issue whether we should require that a satisfactory causal account be *intelligible*, and focus instead on the prior empirical question whether we can get one *at all*.

For Searle it is virtually axiomatic, a given, that brain processes causally generate every detail of our conscious mental life. Throughout his writings he characterizes this as a demonstrated fact, something we know with complete certainty and beyond any possibility of doubt. In the discussion of his paper in Bock and Marsh (1993), for example, he candidly exclaims "frankly, I just can't take it seriously that my point of view, that brain processes cause consciousness, is denied!" (p. 77).

The vast majority of contemporary neuroscientists and psychologists undoubtedly would agree with Searle in accepting without hesitation this basic premise, although many would perhaps question, as I do, details of his account of the emergence, and his confidence that we will be able to achieve a biological theory that is both adequately complete and theoretically satisfying.[20] As noted in the Introduction, the assumption that brain generates mind—that the mind is what the brain does, full stop—seems plausible in light of much prior scientific experience, and has generally served us well as a working hypothesis. It will undoubtedly continue for most scientists to do so. And this is not a bad thing; for as Dupuy (2000) remarked: "The only way to prove the falsity of materialism is to give it every benefit of doubt, to allow it to push forward as far as it can, while remaining alert to its missteps, to the obstacles that it encounters, and, ultimately, to the limits it runs up against. Any other method—fixing its boundaries in advance, for example—is bound to fail" (p. 25).

I agree strongly with this view. I also appreciate that the human brain is a fantastically complex biological system which undoubtedly harbors layer upon layer of neurophysiological mechanism still to be unraveled by our steadily deepening scientific inquiry. New discoveries are constantly being made, some of which profoundly enrich our appreciation of the brain's resources. For example, the old idea that neurons communicate exclusively

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20. I think this characterization applies to contemporary philosophers as well, although I am not sufficiently well informed to be sure of this. Nagel appears to accept that there must be such a causal linkage, while doubting that we can make it intelligible. Colin McGinn (1999) has staked out a similar position which accepts the existence of causal connections but attempts to deny on principled grounds (unsuccessfully, I think) that we can ever understand them. Searle's most radical philosophical opponent, however, is undoubtedly David Chalmers (1996). Chalmers argues that "zombies," beings exactly like us behaviorally but completely unconscious, are logically conceivable, and that consciousness must therefore be something above and beyond the recognized physical order. Searle will have none of this, but his rebuttal again presumes what is in question, that low-level physical processes in the brain cause consciousness. See their exchange in Searle (1997).

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by means of direct and specific synaptic connections has been enriched by discovery of complementary "volume-conduction" effects caused by diffusion of neurotransmitters through the local extracellular space (Agnati, Bjelke, & Fuxe, 1992). Again, neuroscientists had long believed that the dendrites which make up the input arbor of a typical cortical neuron are essentially passive cables; but in recent years we have discovered that they are enormously more complex and contribute actively to the overall electrical behavior of the cell (see, e.g., the special issue of *Science*, October 27, 2000). Similarly, it had long been believed that glial cells provide only structural and metabolic support for the really important elements, the neurons (which they greatly outnumber), but it has recently been demonstrated that by scavenging potassium ions from the extracellular space, glia can also directly modify the electrical behavior of local neural populations (Kohn, Metz, Quibrera, Tommerdahl, & Whitsel, 2000; Kohn, Metz, Tommerdahl, & Whitsel, 2002).[21]

Other examples could readily be provided, but the point should already be clear: No one can prophesy with certainty how far or even in precisely what directions our evolving understanding of brain mechanisms may lead. Nevertheless, and unlike Dupuy (2000), I believe that sufficient information is already in hand to demonstrate that biological naturalism as currently conceived is not only incomplete but *false* as a theory of the mind.

At this point we make the empirical turn that is the central and distinctive contribution of this book. I will begin by outlining the conceptual framework that underlies our presentation. Imagine if you will two complex streams of activity flowing through time in parallel, one consisting of your conscious mental experience, the other of the myriad physiological processes going on in your brain. Imagine further, even though it is scarcely feasible in practice, that we could divide both streams individually into a sequence of states, and in such a way that the mental states correspond to the brain states. Suppose still further, and again counterfactually, that perfect 1:1 correspondence between the two sequences has been established empirically. We have discovered the Holy Grail of neuroscience and psychology, the neural correlates of consciousness.

All the traditional philosophical positions on the mind-body problem arise from different ways of interpreting this correlation. The current mainstream consensus is that brain processes generate or constitute mental processes, but this is not the only possible interpretation.

Note first that even perfect correlation would not necessarily entail *identity*. For example, like all other mammals we have both hearts and lungs, but hearts and lungs are not identical. It remains at least conceptually possible that minds and brains are distinct, though functionally closely linked.

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21. Perhaps not coincidentally, detailed postmortem anatomical investigation of Einstein's brain revealed that the most striking respect in which it differed from ordinary brains was in having significantly larger numbers of glia (Diamond, Scheibel, & Harvey, 1985).

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has been carried out along these lines, with results more than sufficient to convince me and the other authors of this book that the sheer existence of the basic input/output phenomena—ESP and PK, or in the more theory-neutral terminology of Thouless and Wiesner (1947), "psi"—is an inescapable scientific reality with which we must somehow come to terms.

In this light the anti-psi polemic recently advanced by psychologist/philosopher and long-time skeptic Nick Humphrey (1996) is particularly startling. Throughout his book Humphrey alludes to a supposed killer argument that he will later deploy to demonstrate the impossibility of psi. When we finally get there (Chapter 26), the argument turns out to be that he cannot imagine any possible scenario under which ostensible psi effects could be achieved by some combination of known physical mechanisms. Therefore the reported effects cannot and do not happen, Q. E. D. But whether we like it or not, such effects *do* happen, as a matter of empirical fact (see the Introduction and the Appendix). That is the whole point, and what makes the phenomena theoretically interesting in the first place! Humphrey's "argument" amounts in my opinion to little more than an expression of his deeply felt wish that the phenomena should simply go away. In this he is of course adopting a strategy that has been widely practiced by contemporary scientists and philosophers.[24]

Psi phenomena in general are important because they provide examples of human behavioral capacities that seem extremely difficult or impossible to account for in terms of presently recognized computational, biological, or physical principles. Even more important for our purposes, however, is a further body of evidence suggestive of post-mortem survival, the persistence of elements of mind and personality following bodily death. It is simply false to declare, as does Paul Churchland (1988), that "we possess no such evidence" (p. 10). We in fact possess a lot of such evidence, much of it of high quality (see the Appendix). Ironically, the primary threat to the survivalist interpretation of this evidence arises not from considerations of evidential quality, but from the difficulty of excluding alternative explanations based upon paranormal interactions involving only living persons.

Quite apart from any personal or theological interests readers may bring to this subject, it should be evident that post-mortem survival in any form, if it occurs, demonstrates the presence of a fundamental bifurcation in nature, and hence the falsehood of biological naturalism. We will touch upon various facets of the survival evidence later in the book and summarize our collective sense of the empirical status of the problem in Chapter 9.

Evidence for the occurrence of psi phenomena in general and post-mortem survival in particular thus plays an important though largely implicit

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24. A curious and relevant historical precedent is provided by Turing (1950), who explicitly considered the possibility that telepathy could undermine his proposed Turing-test procedures in favor of the human. Indeed, he evidently took this rather seriously, since it appears last in a list of objections ordered at least roughly in terms of increasing difficulty. His "solution" to the problem, however—putting the human into a "telepathy-proof room"—is patently defective, as he himself probably knew.

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role in our overall argument. Our efforts in this book will be amply rewarded if they lead scientifically-minded readers to take these subjects more seriously than they otherwise might. There are many other kinds of evidence, however, that point in the same general direction.

Extreme Psychophysical Influence

Under this heading comes a variety of phenomena especially suggestive of direct mental agency in the production of physiological or even physical effects. Chapter 3 discusses many such phenomena in detail, but I will give a few examples here to capture their flavor.

Placebo effects and related kinds of psychosomatic phenomena have long been informally recognized and are now widely accepted, but they were accepted by modern medical science only grudgingly, as new mechanisms of brain-body interaction came to light that seemed potentially capable of accounting for them. There remain many types of kindred phenomena, however, that pose progressively more severe challenges to explanation in such terms.

Myers, for example, was interested in hysterical "glove anesthesias," in which a patient loses sensation from the skin of a hand in the absence of organic lesion. In such cases the anesthetic skin region typically corresponds only to a psychological entity, the patient's idea, in complete disregard of the underlying anatomical organization. At the same time, curiously, something in the patient remains aware of the afflicted region and protects it from injury.

Related phenomena have been reported in the context of hypnosis. For example, highly suggestible persons who can vividly imagine undergoing an injurious circumstance such as receiving a burn to the skin sometimes suffer effects closely analogous to those that the physical injury itself would produce, such as a blister. More rarely, the correspondence between the hypnotic blister and its imagined source extends even to details of geometric shape, details which appear especially hard to account for in terms of known mechanisms of brain-body interaction. A closely related and well-documented phenomenon is that of "stigmata," in which fervently devout or pious believers in Christ develop wounds analogous to those inflicted during his crucifixion. The injuries to the skin are again localized and specific in form, and they differ in locus and character in accordance with their subjects' varying conceptions of Christ's own injuries. Similarly dramatic phenomena have occasionally been documented in psychiatric patients in connection with their recall of prior physical trauma.

The conventional expectation, of course, is that even the most extreme of the phenomena just mentioned can ultimately be explained in terms of brain processes. Continuing allegiance to this expectation, despite the explanatory difficulties, is undoubtedly encouraged by the fact that the phenomena described so far all involve effects of a person's mental states on that person's

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own body. Even more drastic explanatory challenges are posed, however, by additional and related phenomena in which one person's mental state seems to have directly influenced *another* person's body. Such phenomena include "maternal impressions" (birthmarks or birth defects on a newborn that correspond to an unusual and intense experience of the mother during the pregnancy), distant healing, experimental studies of distant mental influence on living systems, and cases in which a child who claims to have memories of the life of a deceased person also displays unusual birthmarks or birth defects corresponding closely with marks (usually fatal wounds) on the body of that person. In addition, there has been a considerable influx since Myers's time of other experimental evidence demonstrating the reality of psychokinesis (PK), which by definition involves direct mental influence on the physical environment.

Chapter 3 presents selective and focused discussions of phenomena of these various types, emphasizing their strong association with factors such as emotion, strong beliefs, and unusually vivid mental imagery, and drawing out their implications for an adequate theoretical picture of consciousness, volition, and the mind-body problem. Within-subject phenomena such as placebo effects, hypnotically induced blisters, and stigmata are also more carefully situated in relation to modern developments in "psychoneuroimmunology" and mind-body medicine. The common threads of the chapter are, first, to point out the many, varied, and well-documented phenomena of extreme psychophysical influence for which no conventional physicalist explanation is presently available, or in some cases even seems possible; and, second, to point out the theoretical continuity between normal, conscious volitional acts and these less common phenomena that suggest unconscious (or subliminal) volitional activity, sometimes of wider scope than conscious volition itself. The chapter also provides striking examples of the sometimes pathological interplay between scientific theories and scientific "fact."

Informational Capacity, Precision, and Depth

A number of well-documented psychological phenomena involve levels of detail, precision, or logical depth that seem hard to reconcile with what can be achieved by a fundamentally analog mechanism operating in a statistical way with neural components of low intrinsic precision and reliability. I will give three examples of the sort of thing I have in mind.

The first comes from a case of "automatic writing" observed by James (1889). The subject wrote with his extended right arm on large sheets of paper, his face meanwhile buried in the crook of his left elbow. For him to see what he was doing was "a physical impossibility." Nevertheless, James continues: "Two or three times in my presence on one evening, after covering a sheet with writing (the pencil never being raised, so that the words ran into each other), he returned to the top of the sheet and proceeded downwards,

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dotting each *i* and crossing each *t* with absolute precision and great rapidity" (p. 44).

This extraordinary performance illustrates two features that have often appeared together in the substantial but neglected scientific literature dealing with automatic writing (Koutstaal, 1992; Stevenson, 1978): The subject is in an altered state of consciousness, and the motor performance, itself extraordinary, is apparently guided by an extremely detailed memory record, an essentially photographic representation of the uncompleted page.

The latter property relates to the phenomenon of eidetic imagery, my second example, study of which was revived in modern times by the Habers (see Obler & Fein, 1988, for an overview). Probably the most dramatic demonstration of its psychological reality has been provided using Julesz random-dot stereograms (Stromeyer, 1970; Stromeyer & Psotka, 1970; see also Julesz, 1971, pp. 239–246). These are essentially pairs of computer-generated pictures, each of which by itself looks like a matrix of randomly placed dots, but constructed in such a way that when viewed simultaneously (by presentation to the two eyes separately) a visual form emerges in depth. Stromeyer and Psotka adapted this technology to their aims by presenting pictures of this type to the eyes of their single subject, a gifted female eidetiker, at different *times,* ultimately as much as three days apart. Under these conditions, the subject could only extract the hidden form if she could fuse current input to one eye with an extremely detailed memory-image of previous input to the other eye. Remarkably, she was able to succeed under a wide variety of increasingly severe demands. The original stereograms, for example, were 100 x 100 arrays, but she ultimately succeeded under double-blind conditions with arrays as large as 1000 x 1000, or a million "bits," viewed up to four hours apart.

These results were understandably shocking to many psychologists, who sought to escape their force by pointing to the dependence on a single gifted subject and the absence of replications (R. Herrnstein, personal communication, October, 1972). At least one successful replication has subsequently occurred, however. Specifically, Crawford, Wallace, Nemura, and Slater (1986) demonstrated that their highly hypnotizable subjects were able to succeed with the small (100 x 100) stereograms, but only when they were hypnotized. Moreover, the literature already contains many additional examples of prodigious memory. Stromeyer and Psotka themselves mention the mnemonist intensively studied by Luria (1968) and the case of the "Shass Pollaks," who memorized all 12 volumes of the Babylonian Talmud (Stratton, 1917). Sacks (1987, chap. 22) has reported a similar case of a person who among other things knew by heart all nine volumes and 6,000 pages of Grove's *Dictionary of Music and Musicians.* Other examples could easily be cited. Prodigious memory of this sort appears to be a real psychological phenomenon.

Third in this group is the whole family of "calculating prodigies." Of special interest is the "savant syndrome," often associated with infantile autism, in which islands of spectacular ability appear in the midst of general-

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ized mental disability (Obler & Fein, 1988; Treffert, 1989). The abilities are of many types, but almost invariably involve prodigious memory. The depth of the problems they pose for brain theory is exemplified by the case of "The Twins," also described by Sacks (1987). These individuals, unable to perform even simple additions and subtractions with any accuracy, nonetheless proved able to generate and test prime numbers "in their heads." Sacks was able to verify the primacy up to 10 digits, but only by means of published tables, while the twins themselves went on happily exchanging ostensibly prime numbers of even greater length, eventually reaching 20 digits. Sacks makes the intriguing suggestion that they seem not to be literally *calculating* these enormous numbers, but *discovering* them by navigating through some vast inner iconic "landscape" in which the relevant numerical relations are somehow represented pictorially. The twins themselves of course cannot say how they do it.

Phenomena of the sorts described in this section look hard to explain in terms of brain processes. The most serious attempt to do so known to me (Snyder & Mitchell, 1999) is in fact devoid of specific neural mechanisms. Its central argument is rather that early-stage brain processes like those subserving visual perception, for example, must also be rather savant-like in terms of their speed, precision, and informational capacity; what is unusual about savants, therefore, may consist merely in their access to these mechanisms. This "explanation" of course presupposes a positive answer to the fundamental question at issue, whether the brain alone can accomplish *any* of these things, including perceptual synthesis itself (see below).

I will make just one further observation before leaving this fascinating and challenging subject. The biocomputational approach leads to one further expectation that could readily be tested using brain imaging methods but to my knowledge has not. As proved by von Neumann (1956), the only practical way to get increased arithmetical depth and precision out of individually unreliable computing elements is to use more of them. Although I do not know how to quantify this in a rigorous way, the biocomputational perspective clearly implies that calculating prodigies must use very large portions of their brains in very abnormal ways to achieve the observed effects. The cognitive losses that often accompany savant skills could perhaps be a reflection of such substitution, but we must remember that savant-type skills sometimes also occur in geniuses such as the mathematicians Gauss and Ampére (see also Obler & Fein, 1988, chap. 23).

Memory

The previous section focused on phenomena such as prodigious memory that appear potentially incompatible with the physical properties of the brain considered as a kind of computing device. Problems also arise, however, in regard to memory in its more familiar and everyday forms. Here I

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personality. This personality, whom Anna herself named "Old Stump," was benign, often protecting Anna from her pronounced tendencies toward self-injury. As in the case of Schiller's brother, Stump also typically wrote or drew while Anna herself was occupied with other matters. But Stump also continued writing and drawing even when Anna was asleep, and sometimes in total darkness. This secondary personality also remained calm and rational during periods when Anna herself was feverish and delusional, and it manifested knowledge and skills which Anna did not possess.

The enormous literature on these subjects is reviewed systematically, and its implications discussed, in Chapter 5. The chapter specifically argues for the psychological reality of the phenomenon of co-consciousness (as distinguished from unconscious cerebration and alternating consciousness), which is fundamental both to Myers's own theoretical framework and (as discussed in Chapter 8) to James's later application of that framework to problems in religion and philosophy.

The Unity of Conscious Experience

Under this heading I will briefly address two interrelated problems. The first and narrower is the so-called "binding" problem. This problem emerged as a consequence of the success of contemporary neuroscientists in analyzing sensory mechanisms, particularly in the visual system. It turns out that different properties of a visual object such as its form, color, and motion in depth are handled individually by largely separate regions or mechanisms within the visual system. But once the stimulus has been thus dismembered, so to speak, how does it get back together again as a unit of visual experience?

Only one thing is certain: The unification of experience is not achieved anatomically. There are no privileged places or structures in the brain where everything comes together, either for the visual system by itself or for the sensory systems altogether. McDougall (1911/1961) was already fully aware of this, and used it as a cornerstone of his argument against materialist accounts of the mind. In his view, the evident disparity between the multiplicity of physiological processes in the brain and the felt unity of conscious experience could only be resolved in physicalist terms by anatomical convergence, and since there is no such convergence, physicalism must be false.[25]

25. There is an important historical irony here. Dennett and Kinsbourne (1992) also focus on the absence of anatomical convergence, apparently thinking this is something new, but use it in a completely different way. Whereas McDougall (1911/1961) took the unity of conscious experience as a fundamental and undeniable empirical reality, one which physicalism could not explain, Kinsbourne and especially Dennett (1991) want to use the physiological diversity to undermine that appearance of unity itself, along with other supposedly pre-scientific "folk-psychol-

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McDougall's original argument, although ingenious, relied upon the faulty premise that the only possible physical means of unification must be *anatomical.* All current neurophysiological proposals for solving the binding problem are instead *functional* in nature. The essential concept common to all of them is that oscillatory electrical activity in widely distributed neural populations can be rapidly and reversibly synchronized in the "gamma" band of frequencies (roughly 30–70 Hz), thereby providing a possible mechanistic solution to the binding problem (von der Malsburg, 1995).[26]

A great deal of sophisticated experimental and theoretical work over the past 20 years has demonstrated that such mechanisms do in fact exist in the nervous system, and that they are active in conjunction with normal perceptual synthesis.[27] Indeed, Searle's doctrine of biological naturalism has now crystallized neurophysiologically in the form of a family of "global workspace" theories, all of which make the central claim that conscious experience occurs specifically and only in conjunction with large-scale patterns of gamma-band oscillatory activity linking widely separated areas of the brain (Crick, 1994; Dehaene & Naccache, 2001; Edelman & Tononi, 2000; A. K. Engel, Fries, & Singer, 2001; W. J. Freeman, 2000; Llinás, Ribary, Contreras, & Pedroarena, 1998; Newman & Baars, 1993; Singer, 1998; Varela, Lachaux, Rodriguez, & Martinerie, 2001).[28]

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ogy" intuitions about the nature of consciousness. See also Searle (1997, chap. 5), later sections of this chapter, and our Chapter 9.

26. This paragraph summarizes decades of cumulative progress in the neurobiology of sensory coding. The early work, inspired by Hubel and Wiesel (1962), emphasized "feature detection" by single sensory neurons, and provided for detection and representation of higher-order features or conjunctions of features by means of suitable anatomical connectivity. It was subsequently recognized, however, that combinations of elementary features could potentially occur in numbers far too large to manage exclusively in this anatomically-based way. The new functional proposals overcome the combinatorial explosion by providing for rapid and reversible linkages among groups of cells responding to more elementary properties.

27. Note that this characterization does not imply or require that the oscillatory activity itself satisfactorily *explains* binding. That it does not has already been argued by neurophysiologists such as Crick and Koch (2003) and Shadlen and Movshon (1999). See also the following paragraphs.

28. Although the "global workspace" terminology originated with Baars (1988, 1997), we will use it throughout this book in the more generic sense supplied in the text. Baars certainly deserves much credit for emphasizing that we need somehow to reconcile the unity of conscious experience with the multiplicity of associated neurophysiological processes in the brain, and for stimulating new imaging studies that seek to identify more precisely the critical neurophysiological conditions themselves (see, e.g., Dehaene & Naccache, 2001). His own specific version of a theory of this type, however, is less than satisfactory. In particular, his extended allegory of the "theater of consciousness" (Baars, 1997, pp. 41–47), although providing a colorful vocabulary with which to describe or interpret a variety of psychological phenomena, is conceptually incoherent in a multitude of ways.

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The neurophysiological global workspace, however, cannot be the whole story. A sizeable body of recent evidence demonstrates that organized, elaborate, and vivid conscious experience sometimes occurs under physiological conditions, such as deep general anesthesia and cardiac arrest, which preclude workspace operation. Experiences of this sort fall under the more general heading of "near-death experiences" or NDEs, which are discussed in detail in Chapter 6 together with related phenomena such as "out-of-body experiences" (OBEs) and lucid dreams. In short, it appears to us that McDougall was right after all, albeit for the wrong reason. In effect, we will argue, recent progress in mainstream physicalist brain theory has provided new means for its own falsification as a complete account of mind-brain relations.

Availability of this emerging evidence emboldens me to make some further and even more contentious remarks regarding the larger problem of perceptual synthesis, and the direction in which things seem to me to be moving.

It is an historical fact that mainstream cognitive psychology has always tended on the whole to try to solve its problems "on the cheap," with as little reference as possible to what all of us experience every day as central features of our conscious mental life. The early workers in "mechanical translation," for example, imagined that they could do a decent job simply by constructing a large dictionary that would enable substitution of words in one language for words in the other. This approach failed miserably, and we were slowly driven, failed step by failed step, to the recognition that truly adequate translation presupposes *understanding*, or in short a full appreciation of the capacities underlying the human use of language.

A similar evolution is underway in regard to perceptual theory. Most of the work to date has taken a strongly "bottom-up" approach, along lines formulated in the seminal book of Marr (1982). This school views perceptual synthesis as a kind of exhaustive calculation from the totality of input currently present at our sensory surfaces. Machine vision and robotics, for example, necessarily took this approach, and even in neuroscience it seemed to make sense to start with the most accessible parts of the perceptual systems—the end organs and their peripheral connections—and work our way inward. The great sensory systems themselves—vision, audition, somatosensation, and so on—were also presumed to operate more or less independently, and were in fact typically studied in isolation.

A separate tradition dating back at least to Kant and the early Gestalt theorists, and carried forward into the modern era by psychologists such as Neisser (1967, 1976), has been sensitive to the presence of "top-down" influences, both within and between sensory modalities. Although a few perceptual subsystems (such as those that engender the Müller-Lyer and other incorrigible visual illusions) may be truly autonomous or "cognitively impenetrable" in the sense of J. Fodor (1983), these seem to be isolated and special cases. A very different overall picture of perceptual synthesis is currently emerging in which top-down influences predominate. On this view

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perceptual synthesis is achieved not *from* the input, but *with its aid.* This is necessarily the case for example in regard to ambiguous figures, where the stimulus information itself is not sufficient to determine a uniquely "correct" interpretation. More generally, we routinely ignore information that is present in the input and supply information that is not, speed-reading providing a characteristic example.[29] Something within us, a sort of cosmogenic, world-generating, or virtual-reality system, is continuously updating and projecting an overall model of the perceptual environment and our position within it, guided by very limited samplings of the available sensory information (Simons & Chabris, 1999; Tart, 1993).

As in the case of understanding spoken or written language, an enormous amount of general knowledge is constantly mobilized in service of this projective activity, which freely utilizes any and all relevant sensory data available to it. Top-down and cross-modal sensory interactions have recently been recognized as the rule rather than the exception in perception (A. K. Engel et al., 2001; Shimojo & Shams, 2001). Neuroscientist Rodolfo Llinás and his co-workers have even advanced the view, which I believe is profoundly correct, that *dreaming*, far from being an odd and incidental part of our mental life, represents the fundamental form of this projective activity. Ordinary perceptual synthesis, on this inverted view of things, amounts to oneiric (dreamlike) activity constrained by sensory input (Llinás & Paré, 1996; Llinás & Ribary, 1994). Hartmann (1975) had proposed similar ideas in regard to hallucinatory activity more generally, with dreaming included. On his view such activity is again a ubiquitous and fundamental feature of our mental life, and the critical question is not "why do we sometimes hallucinate?" but rather "what keeps us from hallucinating most of the time?" The answer, he thought, lies in inhibitory influences somehow exerted by the brain activity that accompanies ongoing perceptual and cognitive functions of the ordinary waking sorts. Similar arguments for the primacy and importance of this cosmogenic capacity have more recently been advanced by Brann (1991) and Globus (1987).[30]

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29. Yuo mgiht aslo be srupsired to fnid taht yuo can raed tihs ntoe wtihuot mcuh truoble.

30. Another relevant phenomenon, and one that deserves more attention, is spontaneous hallucinatory experience in normal and awake persons. That such experiences commonly occur was initially demonstrated by the founders of the Society for Psychical Research (Gurney, Myers, & Podmore, 1886; H. Sidgwick et al., 1894) and has subsequently been confirmed repeatedly by others (Bentall, 2000). The mere fact of their occurrence demonstrates that the projective activity can sometimes partially or even wholly *override* current sensory ‘nput. Waking apparitions also share with dreams a tendency to incorporate massive amounts of information about physically remote events (Gurney et al., 1886). Critical readers should not indulge any temptation they may experience to dismiss these case reports wholesale as mere "anecdotes," for they are massively and carefully documented. Penetrating analysis of the pitfalls of eyewitness testimony did not begin with Loftus (1979), as commonly supposed: See Gurney et al. (1886), its review by James (1887), and Chapters 2 and 6.

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So far so good, but where exactly is the "top," the ultimate source of this projective activity? The mainstream neuroscientists who have already recognized its existence invariably presume that it arises entirely within the brain itself. Evidence such as that assembled in Chapter 6, however, like the more direct evidence of post-mortem survival, strongly suggests that it originates outside the brain as conventionally understood. We will return to this in Chapter 9.

Genius-Level Creativity

In the Introduction we quoted Edgar Wind's guiding principle, that "the commonplace may be understood as a reduction of the exceptional, but the exceptional cannot be understood as an amplification of the commonplace." That principle applies with particular force and poignancy, I think, to the topic of this section. Any scientific theory of personality and cognition truly worthy of the name surely must help us to understand this humanly vital topic, but by this standard we have so far made distressingly little progress. The reason, in my opinion, is that for the most part we have violated Wind's principle by trying to understand the exceptional—real genius, in its fullest expressions—as an amplification of the commonplace—"creativity," as we find it in random samples of undergraduates and the like.

Myers and James consciously and deliberately approached the subject from the other end, and in connection with the enlarged conception of human personality which they were struggling to articulate. In Chapter 7 we discuss genius from this point of view, describing Myers's account in some detail and situating it in relation to the main trends in contemporary creativity research. Focusing primarily on the creative process and creative personality structure, we argue that Myers anticipated most of what has been good in more recent work, while also accommodating in a natural way a variety of additional phenomena—including psychological automatisms and secondary centers of consciousness, altered states of consciousness, unusual forms of symbolic thinking, and psi—that are inescapably bound up with this topic but scarcely touched upon in contemporary mainstream accounts. We also show that various expectations flowing from Myers's account of genius have been strongly confirmed by more recent empirical observations.

Mystical Experience

Experiences of this type lie at the core of the world's major religious traditions and have continued to occur throughout history and across cultures. Their existence as a distinctive and important class of psychological phenomena can scarcely be denied. Yet they have largely been ignored by mainstream psychology and neuroscience, and generations of reductionist

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sively took on characteristics of telegraphic speech as the theory evolved (E. F. Kelly, 1975). Much of the contemporary work on computational modeling of metaphor has similar problems (Chapter 7).

Sometimes, however, the homunculus is more brazenly evident. One example is Marr's account of vision, which applies computations to the two-dimensional array of retinal input in order to generate a "description" of the three-dimensional world that provided that visual input, but then needs someone to interpret that description (Searle, 1992). Another is Kosslyn's model of visual imagery, which essentially puts up an image on a sort of internal TV screen, but then needs somebody else to view that image. Draaisma (2000) and our Chapter 4 identify similar homunculus problems in the context of contemporary memory models.

Particularly in its more blatant forms the homunculus problem has attracted the attention of physicalists such as Dennett (1978), who have sought to remove its philosophic sting. Dennett's solution is to "discharge" the homunculus by a process of "recursive decomposition." The basic idea is that the "smart" homunculus appearing at the top of a model can be replaced by progressively larger numbers of less smart homunculi, until we get to a vast bottom layer corresponding to the "hardware" level of computer flip-flops or neuron firings. But as Searle (1992) pointed out, this maneuver fails, because even at the bottom level there has to be something outside the decomposition, a homunculus in effect, that knows what those lowest-level operations mean. Cognitive models cannot function without a homunculus, in short, precisely because they lack what we have—minds, with their capacities for semantics, intentionality, and all the rest built in.

No homunculus problem, however, is posed by the structure of our conscious experience itself. The efforts of Dennett (1991) and others to claim that there *is* such a problem, and to use that to ridicule any residue of dualism, rely upon the deeply flawed metaphor of the "Cartesian theater," a place where mental contents get displayed and I pop in separately to view them. Descartes himself, James, and Searle, among others, all have this right; conscious experience comes to us whole and undivided, with the qualitative feels, phenomenological content, unity, and subjective point of view all built-in, intrinsic features. I and my experience cannot be separated in this way.

Finally, I wish simply to record, without argument, my own deepest intuition as to where these issues lead. All of the great unsolved mysteries of the mind—semantics, intentionality, volition, the self, and consciousness—seem to me inextricably interconnected, with consciousness somehow at the root of all.

The consciousness I have in mind is emphatically *not* that of Chalmers (1996), irreducible but ineffectual, consisting merely of phenomenological properties or "qualia" arbitrarily tacked on to a strong artificial intelligence that does all the cognitive work. Ordinary perception and action are saturated with conceptual understanding, and conceptual understanding is saturated with phenomenological content. Volition too has an intentionality aspect, for as Nietzsche somewhere remarked, one cannot just *will*, one must

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will *something*. Each individual word is in effect "a microcosm of human consciousness" (Vygotsky, 1986/2000, p. 256), and "all *meaning* is in some way ultimately grounded in *being*" (Cassirer, 1957, p. 94). And as William James so forcibly argued at the dawn of our science, all of this perceptual, cognitive, and volitional activity somehow emanates from a mysterious and elusive "spiritual self," which can often be sensed at or behind the innermost subjective pole of our ongoing conscious experience.

Consciousness, in short, far from being a passive epiphenomenon, seems to me to play an essential role—indeed *the* essential role—in all of our most basic cognitive capacities. I can find no better way of ending this section than simply to stand back and applaud the trenchant conclusion drawn by philosopher E. J. Lowe (1998), which encapsulates my own views and the central contention of this book: "Reductive physicalism, far from being equipped to solve the so-called 'easy' problems of consciousness, has in fact nothing very useful to say about *any* aspect of consciousness" (pp. 121–122).

Conclusion

In regard to the deep theoretical issues broached in the previous section, my views as a psychologist closely parallel those of a small minority of modern philosophical writers including E. J. Lowe, Thomas Nagel, and John Searle. I feel an enormous debt of gratitude to them for so vigorously defending, like James (1890b), the reality and importance of our conscious mental life. I find it altogether astonishing, and predict that it will be found so as well by our intellectual descendants, that so much of our science and philosophy from James to the present has sought—consciously!—to slight or ignore these first-person realities of the mind, and sometimes even to deny that they exist. There is perhaps no better example of the power of theory to blind us to facts.

But that has now all changed. We have come full circle and surely will not turn backward again. The question that now confronts us, as it confronted James, is whether all this richness of our conscious mental life really can be accounted for in terms of physical operations in the brain. Searle himself, of course, is sure that it can, at least in principle. But this is just a pious hope, the latest form of "promissory materialism" (Popper & Eccles, 1977, p. 96). Even philosophers fundamentally sympathetic to Searle's point of view are not nearly so confident. Consider for example the gloomy conclusions of physicalist Jaegwon Kim (1998):

> We find ourselves in a profound dilemma: If we are prepared to embrace reductionism, we can explain mental causation. However, in the process of reducing mentality to physical/biological properties, we may well lose the intrinsic, subjective character of our mentality—arguably, the very thing that makes the mental mental. In what sense, then, have we saved "mental"

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> causation? But if we reject reductionism, we are not able to see how mental causation should be possible. But saving mentality while losing causality doesn't seem to amount to saving anything worth saving. For what good is the mind if it has no causal power? Either way, we are in danger of losing mentality. That is the dilemma. (p. 237)

In this passage Kim, a determined but scrupulously honest physicalist, has clearly moved within a hair's breadth of the more skeptical and agnostic Nagel. This surely is a development that ought to worry other physicalists. But whereas *their* troubles with physicalism arose primarily via conceptual analysis, and have led them to no definite conclusion, *ours* are primarily empirical in character, and actually falsify biological naturalism. That is the central contention of this book. We believe strongly that in order to get an adequate scientific account of the mind we *must* be prepared to take seriously all relevant data and to modify as necessary even our most fundamental theoretical ideas. Some of the most relevant kinds of data, however, have been systematically excluded from contemporary scientific and philosophic discussions.

Following chapters, as indicated, will discuss a number of these neglected topics in depth. Let me conclude by briefly recapitulating the argument as it has developed so far: Despite its many significant accomplishments, a century of mainstream scientific psychology has not provided a satisfactory theory of mind, or solved the mind-body problem. Physicalist accounts of the mind appear to be approaching their limits without fully accounting for its properties. The computational theory of the mind has been overthrown, forcing physicalism to retreat into what necessarily constitutes its final frontier, the unique biology of the brain. But this biological naturalism appears destined to fare little better. Some critical properties of mental life can already be recognized as irreconcilable in principle with physical operations of the brain, and others seem likely to prove so as well.

These failures warrant a serious attempt to rethink the entire subject of mind-brain relations from a different perspective—specifically, from the perspective provided by James's generic "transmission" theory. There is no better way to begin such an effort than by reviewing the extraordinary contributions of F. W. H. Myers, James's colleague and friend, who provided the most fully developed version so far of a theory of this type.

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Chapter 2

F. W. H. Myers and the Empirical Study of the Mind-Body Problem

Emily Williams Kelly

> Psychology sometimes seems to suffer from a memory loss that borders on the pathological. Not only is the number of rediscoveries shamefully high, but valuable empirical and conceptual work carried out in older traditions has disturbingly little impact on present-day research. The result is that certain defects in theory formulation diagnosed as long ago as the nineteenth century, are repeatedly reintroduced in psychology. (Draaisma, 2000, p. 5)

The Historical Context

As the above quotation suggests, psychology as a research discipline has not been a steadily progressing advance of knowledge, each generation building on the discoveries and achievements of its predecessors in a systematic march toward new knowledge. Even in the physical sciences, in which there has been a somewhat more linear progression of achievement, many scientists take the all too parochial view that the insights and observations of previous generations have been superseded by the technological, methodological, empirical, and even conceptual developments of the present. Few modern working scientists consider it likely that an examination of the history of their field might not only broaden their perspective on contem-

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porary problems, but even suggest new (or renewed) avenues for attacking those problems. Over a century ago, however, that quintessential spokesman for modern science, Thomas Huxley, lamented the historical ignorance of scientists in his own day and urged them to study their history because "there is assuredly no more effectual method of clearing up one's own mind on any subject than by talking it over, so to speak, with men of real power and grasp, who have considered it from a totally different point of view" (T. H. Huxley, 1874, p. 556).

In the second half of the 19th century, psychology was undergoing a major and rapid transformation from moral philosophy to naturalistic science, and central to this transformation were efforts to grapple with questions as fundamental to psychology as the nature of mind, the nature of the relationship between mental and physical processes, and the relationship of psychology to the rest of science. By the early years of the 20th century, however, such fundamental questions had, for all intents and purposes, been written off as "metaphysical" problems unsuitable for a scientific psychology. Psychology was well on its way to the fragmentation and conceptual impasses characteristic of contemporary psychology, as described in the Introduction and Chapter 1. What led to this abandonment of fundamental questions? Can a return to them aid psychologists in moving the science of psychology forward, both by bringing psychology conceptually into the 21st century and, perhaps even more importantly, by advancing knowledge about issues that are of interest and concern to the general public?

It is our contention in this book that such a return to fundamental questions is not only desirable but essential to psychology, and that a few "men of real power and grasp" in the late 19th century—including in particular F. W. H. Myers and William James—had opened up avenues for attacking these questions empirically, avenues that were quickly closed off by assumptions and beliefs that still overwhelmingly permeate modern psychology. In this chapter, I will first describe briefly the intellectual context in which scientific psychology developed in the 19th century, and then provide an overview of the work of F. W. H. Myers.

The Roots of Scientific Psychology: Dualism, Mechanistic Determinism, and the Continuity of Nature

Science, from Copernicus to the present, has undoubtedly been the single most important influence on the modern world, its reach extending far beyond technological advances to the fundamental changes it has brought to the way humans view themselves and the world around them. The scientific revolution had its roots in ideas that, with every subsequent accomplishment and advance in science, have become ever more entrenched in modern thinking. An important impetus to the development of Western science was the clear articulation of a dualism that split the phenomenal universe into two radically different domains. With Descartes' distinction

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of mental causality was increasingly dismissed as a vestige of primitive, "supernatural" ways of thinking.[2] The resulting intellectual turmoil of the 19th century, however, was more than a conflict between an old, dying order and a new, more advanced one, or even, as it is so often portrayed, between Religion and Science. It was, in essence, a conflict between experience and knowledge (Daston, 1978): Individual first-person experience suggests one kind of world—one of personal agency—but the cumulative third-person knowledge produced by science was suggesting quite a different world—one of impersonal agency. Scientific psychology became the point at which those two world views collided, thus presenting science with the most serious challenge to the strength and sufficiency of its assumptions, principles, and methods.

The potential incorporation of psychology into science, in sum, seemed to present a threat to both. If the "anomaly" of mind, with its apparently volitional, teleological, and subjective phenomena, was to be reconciled with the otherwise increasingly uniform picture painted by Western science of mechanistic, atomistic, physical determinism, then either the concept of mind or the assumptions of modern science would have to be altered. Psychologists could redefine, or reconceptualize, psychology in such a way that it excludes whatever does not fit the framework of the physical sciences, such as consciousness or volitional agency; or they could use the phenomena of psychology to modify the model of science that limits causal agency to physical determinism. They could, in short, either narrow psychology to fit science as it was then understood, or expand science to accommodate psychological phenomena. The nearly unanimous choice of 19th-century scientists was the former course, to force psychology into the framework of assumptions derived from the classical physical sciences, rather than entertain the idea that science might have to be enlarged to accommodate mental phenomena and causality on an equal footing with physical phenomena and causality (Cook [Kelly], 1992). As Boring (1933) succinctly stated: "Historically science is physical science. Psychology, if it is to be a science, must be like physics....The ultimate abandonment of dualism leaves us the physical world as the only reality. Consciousness will ultimately be measured in physical dimensions" (pp. 6, 8).

The Naturalization of Mind: Limiting Psychology

The first major task for the mid-19th-century scientists who sought to transform psychology into science was to reconceptualize mind as a natural, not supernatural, phenomenon. Several lines of influence contributed importantly to this process. The 17th- and 18th-century empiricist philosophy of associationism, which held that all mental phenomena derive solely

2. Today concepts such as volition (along with "belief, desire, fear, sensation, pain, joy, and so on") are similarly dismissed by many as vestiges of "folk psychology" (Churchland, 1988, p. 44).

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from experience (specifically, elementary sensations that bond together to form complex perceptions and ideas) was of immense importance in providing a model of mind that seemed consistent with the scientific model of the physical world as atomistic and deterministic. Advances in physiology, particularly in the localization of function in the brain and nervous system, together with a reflex model of sensorimotor processes that also dovetailed with atomism, associationism, and mechanistic determinism, strongly supported a view of mind as produced by the brain, and thus of physiology as the basis for a science of psychology. The doctrine of the conservation of energy made it increasingly difficult for many scientists to accept the idea of mental processes that could break into the closed causal chain of energy. It also provided a new notion, derived particularly from the physicist Gustav Fechner (considered by many to be the founder of experimental psychology), of mind as a form of energy, the product of a nervous system whose function is to translate physical energy into psychophysical activity. Last, but certainly not least, the biological sciences, in the form of Darwin's theory of evolution and natural selection, played an immense role in undermining old ideas about the uniqueness of human minds and weakening whatever remained of any belief among scientists in teleological or volitional processes in nature.

The naturalization of mind, however, was as revolutionary an idea as Darwinism, and for similar reasons. In the firsthand, subjective experience of all humans, mind seems an indivisible unity, an "I" that is a free, active, causal agent in an otherwise deterministic nature. This concept was being assaulted on many scientific fronts, however, and was rapidly being replaced by a concept of mind as solely the product of a nervous system shaped over the course of evolution in response to the demands of the environment. I use the term "assaulted" deliberately, because this new concept of mind was so antithetical to longstanding and seemingly commonsense ways in which people view their mind, or self, that it quickly became a battleground in which the emerging forces of modern science were arrayed against the old guard of religion, theology, and metaphysics. This war pitted naturalism and the principle of continuity against supernaturalism and dualism; and it was not simply an intellectual matter, but one with enormous implications for the way humans would view themselves, their society, and the world around them. Because so much was at stake, positions rapidly became rigidly polarized, and many of the most well-known and vocal of the scientists advocating for a new science of mind took strong anti-dualistic, anti-teleological views that left no room for compromise.

One of most important results of this militant dichotomization of scientific naturalism and metaphysical dualism was that there was no room in the former for a concept central to the latter, namely, mental causality or volition. A major phenomenon of psychology was therefore automatically excluded because of the assumption that materialistic determinism constitutes the essence of science. For many scientists, causative volition was a nonsensical concept because (they said) it required the introduction of new

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energy into what was otherwise a closed system. Moreover, allowing for the concept of volition in psychology was "a back-door attempt to reintroduce an active ego or soul into the new psychology" (Daston, 1978, p. 202). Volition became "a taboo concept" because scientists thought "it would pull psychology back to its prescientific, mystical days" (Decker, 1986, p. 52). Thomas Huxley (1887/1892), "Darwin's bull-dog" and the personification of the so-called science-religion debate of the 19th century, asked: "The ultimate form of the problem is this: Have we any reason to believe that a feeling, or state of consciousness, is capable of directly affecting the motion of even the smallest conceivable molecule of matter?" (p. 292). His answer, and that of many others, was, certainly not: "If anybody says that the will influences matter, the statement is not untrue, but it is nonsense....Such an assertion belongs to the crude materialism of the savage. Now the only thing which influences matter is the position of surrounding matter or the motion of surrounding matter" (Clifford, 1874, p. 728). Mental as well as physical events are part of a deterministic chain in which one event is the direct antecedent of, and gives rise to, the next event; but "volitions do not enter into the chain of causation...at all....[T]he feeling we call volition is not the cause of a voluntary act, but the symbol of that state of the brain which is the immediate cause of that act. We are conscious automata" (T. H. Huxley, 1874, pp. 576–577).[3]

Huxley's statement expresses another central assumption of the 19th-century founders of scientific psychology—the assumption that matter is the primary, independent factor in the universe and that mind is a secondary, dependent byproduct of it. Henry Maudsley, a prominent physician and physiologist whose *Physiology and Pathology of Mind* (1868) became "a turning point in English psychiatry" (Lewis, 1951, p. 269), summed up the views of many of his scientific contemporaries when he defined materialism as the belief that "mind is an outcome and function of matter in a certain state of organization" (Maudsley, 1879, p. 667). Huxley (1892) argued that "so far as observation and experiment go, they teach us that the psychical phenomena are dependent on the physical...called into existence" by physical processes (p. 43). Alexander Bain (1872/1874), one of the most influential psychologists during the formative years of scientific psychology, argued that all feelings, intellectual capacities, and volitional activities are directly correlated with and dependent on brain states. In France also, prominent psychologists left no room for doubt that mind and consciousness are wholly dependent on physiological processes. Théodule Ribot (1898), Professor of Experimental and Comparative Psychology at the College of France, stated unequivocally that "the organism and the brain...constitute the real personality," and the apparently psychological problem of "the unity of the ego is, in its ultimate form, a biological problem" (pp. 154–156). Early in his career, Ribot had argued, like Huxley, for an automaton theory in which "consciousness is only an adjunct of certain nervous processes, as incapable of reacting upon them as is a shadow upon the steps of the traveler whom

3. For a 21st-century advocate of such views, see Wegner (2002).

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it accompanies" (Ribot, 1882, p. 11). He later modified this extreme view to say that mind, once called into existence by the brain, could have some efficacy.[4] Nonetheless, "the fundamental and active element is the nervous system, [and] the other [i.e., consciousness] is only a concomitant" (Ribot, 1898, pp. 11–12). Psychologists, he said, may treat mental states as causal phenomena, so long as they do not forget that all mental states "have their roots in the organism and are pre-determined by it...[and] that these [mental] causes are in their turn effects" (p. 51). Even a clinician such as Pierre Janet (1893/1901), who described the disturbances he observed as functional and mental rather than organic, emphasized that psychological phenomena are of "cortical origin"(p. 27): "You will understand, once for all, that the word 'mind' represents the highest functions of the brain and probably the functions of the cortex" (p. 52). In sum, for many in the first generation of scientific psychology, the thorough-going unilateral dependence of mind on brain was "a practical certainty....There are numbers of questions relating to the connection of the mind with the body which have ceased to be open questions, because Science has had her word to say about them" (Clifford, 1874, pp. 734, 715).

The Unresolved Dilemmas of Psychology

The principles of 19th-century scientific naturalism took firm hold in psychology. Mechanistic determinism and reductionistic atomism had been pitted against the old "commonsense" or "folk-psychology" principles such as teleology, meaning, and volition, and had apparently emerged victorious. Nevertheless, humanistic principles were not easily relinquished. Throughout much of the 19th century, the conflict between scientific determinism and human volition remained a central dilemma of the age (Chadwick, 1975, pp. 204–205; Daston, 1978, 1982).

In particular, the denial of volition, or mental causality, left some major problems unresolved. Not only did such a conclusion contradict the daily experience of all humans; it also presented major social and ethical problems. If human beings are products of deterministic processes, how can they be held accountable for their actions under any social or ethical codes? Even Huxley (1892), who had contemptuously dismissed "the primitive dualism of a natural world 'fixed in fate' and a supernatural, left to the free play of volition" (p. 4), felt this dilemma so keenly that he was ultimately forced to construct his own "primitive dualism" of Nature and Society, the former characterized by law, the latter by volition or free will (e.g., T. H. Huxley, 1888/1898). The undeniably dual nature of human experience, together with the social and moral necessity for a belief in volition, presented psychologists with paradoxes and problems that seemed insoluble without sacrific-

4. More recently, the eminent neuroscientist Roger Sperry (e.g., 1980) has expressed a similar view.

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ing either human principles on the one hand or scientific principles on the other.

Another major problem that the new psychology not only left unresolved, but actually exacerbated, was the question of whether to view mind as fundamentally a unity or a multiplicity. Is mind an indivisible whole that is the cohesive, organizing factor of mental life, or is it a structure built up from innumerable elements or experiences? Is mind the sum of the parts, or the factor drawing the parts together in the first place? In brief, is mind best understood from the bottom up or the top down? In the 19th century, this problem was central to the conflict between the old dualistic psychology and the new materialist psychology; it was a battle "which pitted the metaphysical 'unity of self' against the scientific 'multiplicity of selves'" (Robinson, 1978, p. 349). The first was the traditional notion of self, and even an associationist such as J. S. Mill (1843/1874) found this a compelling idea: "There is a something I call Myself, or, by another form of expression, my mind, which I consider as distinct from these sensations, thoughts, etc.; a something which I conceive to be not the thoughts, but the being that has the thoughts" (p. 56).

In direct opposition to this view was the "colonial" view of consciousness as a multiplicity built up from innumerable elements of the nervous system working in coordination: "Physiology shows that this verdict [of unity of mind] is an illusion....The apparently simple is, on analysis, found to be complex" (Ribot, 1882, pp. 42, 45). Mill (1843/1874), after acknowledging the compelling sense we all have of a unified self, went on to say that we can have no knowledge of what this something is ("though it is myself") but only of "the series of its states of consciousness" (p. 57). For an increasing number of 19th-century scientists, that knowable "series" was the only conception of mind that psychology needed, especially since the view of mind as a multiplicity conformed much better to the analytic method of science and the atomistic view of matter in 19th-century physics than did the concept of a unitary, indivisible self.

Nevertheless, most psychologists recognized that the multiplicity view of mind left fundamental problems unresolved. As McDougall (1911/1961) later said, the basic problem for all theories of mind is: "What holds consciousness together?" How do we get psychical unity out of physical multiplicity, "the hanging together of a multiplicity of conscious processes in a numerically distinct or individual stream" (p. 164)?[5]

The problem of whether mind is a unity or a multiplicity also raised the problem of whether the traditional analytic methods of the physical sciences are adequate for a science of psychology. If mind is basically a composite structure built up from numerous psychological elements, then such methods are appropriate for psychology. If, however, mind is most fundamentally a unity, then new methods, going beyond quantitative analysis, may be required. In the late 19th and early 20th centuries, a few psycholo-

5. The problem, known today as "the binding problem," remains a fundamental one in psychology and the neurosciences (see Chapter 1).

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> While accepting fearlessly the facts of materialism dwelt upon in these pages, I bow my head in the dust before that mystery of mind. (p. 224)

Even William James, who could hardly be accused of wishing to put psychology into a theoretical or methodological straitjacket, nevertheless argued in *The Principles of Psychology* for a methodological parallelism: "Empirical parallelism...[is] the wisest course....[N]ature in her unfathomable designs has mixed us...of brain and mind,...the two things hang indubitably together and determine each other's being, but how or why, no mortal may ever know" (James, 1890b, vol. 1, p. 182).[7]

In fact, however, the professed ontological neutrality was usually anything but that. Behind the methodological parallelism, and the associated call to get on with studying and describing psychological processes in and of themselves, was the often thinly disguised assumption that mental phenomena are a secondary byproduct of the fundamental constituent of the universe, matter. T. H. Huxley (1892), summarizing his own position, spoke for many of his scientific contemporaries: "I have frequently expressed my incapacity to understand the nature of the relation between consciousness and a certain anatomical tissue...[but] so far as observation and experiment go, they teach us that the psychical phenomena are dependent on the physical" (p. 43). Thus, in the same breath, he expressed both an inability to understand the nature of the mind-matter relationship and a fundamental conclusion about its nature. Huxley was agnostic, therefore, *not* concerning the nature of the relationship of mind and matter—he was convinced that mind is ultimately dependent on (because derived from) matter—but only concerning the specific nature of that dependence. In short, for Huxley as for many other 19th-century scientists, the exact nature of the dependence of psychical processes on physical ones was an open—though unresolvable—question; but the general dependence of mind on matter was a resolved—and thus closed—question.

The outcome of adopting this methodological parallelism in psychology was not difficult to foresee. Psychologists could get on with the business of simply describing psychological processes, professing a vague working assumption of mind-brain unity without having to deal with fundamental theoretical issues such as the nature of mind-brain concomitance and the associated problem of the efficacy of mind in the physical world. The dismissal of such problems as "metaphysical" only, and not "scientific," effectively foreclosed any challenge to the assumption underlying psychology of a unilateral dependence of mind on brain. Although the nature and extent of mind-brain correlation should have become the major empirical problem for a psychology that sought theoretical understanding of its subject matter, it was instead altogether avoided. Perhaps even more problematically, 19th-century psychology had been built on a rigid and unyielding dichotomy of physicalistic naturalism versus dualistic supernaturalism, and the rigidity

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7. This view that the mind-body problem is insoluble is essentially the "mysterian" position of McGinn (1999).

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of the dichotomy precluded any serious consideration of the possibility that the solution to its paradoxes and problems might lie in a close and critical examination of prevailing assumptions about naturalism itself—that is, about whether the equation of naturalism with physicalism and determinism, and of mental efficacy with supernaturalism and lawlessness, exhausted the possibilities.

Scientists instrumental in the development of 19th-century psychology thus in general had chosen to conceptualize science primarily not as a method with which to confront basic questions posed by contradictory aspects of human experience, but as a doctrine to which psychology, if it is to be a science, must conform. Because so many 19th-century scientists refused to question or even critically examine the assumptions of the physical sciences and the world view derived from them, and thus avoided theoretical problems that psychological phenomena alone raised, those assumptions, that world view, and the pattern of avoiding basic theoretical issues became the foundation upon which modern psychology was built. During its subsequent history, psychology, despite its broad expansion in the 20th century, has for the most part remained within that framework:

> Ever since its stipulation into existence as an independent science, psychology has been far more concerned with being a science than with courageous and self-determining confrontations with its historically constituted subject matter. Its history has been largely a matter of emulating the methods, forms, symbols of the established sciences, especially physics. In so doing, there has been an inevitable tendency to retreat from broad and intensely significant ranges of its subject matter, and to form rationales for so doing which could only invite further retreat. (Koch, 1961, pp. 629–630)

F. W. H. Myers: Purposes and Principles

Not all psychologists acquiesced in this retreat from major problems and theoretical issues in psychology. William James, for one, was acutely aware that parallelism, or the Jacksonian doctrine of concomitance, avoided, and did nothing to help resolve, the basic problems of mental causality inherent in psychology. To the injunction of his colleague Charles Mercier—"Having firmly and tenaciously grasped these two notions, of the absolute separateness of mind and matter, and of the invariable concomitance of a mental change with a bodily change, the student will enter on the study of psychology with half his difficulties surmounted"—James replied: "Half his difficulties ignored, I should prefer to say. For this 'concomitance' in the midst of 'absolute separateness' is an utterly irrational notion" (James, 1890b, vol. 1, p. 136). Although he himself had urged psychologists to adopt an empirical or methodological parallelism, he also cautioned them that this was "certainly only a provisional halting-place, and things must some day be more thoroughly thought out" (p. 182). James's close friend and colleague,

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F. W. H. Myers, was one of the few psychologists who attempted to do just this. In numerous papers published between 1880 and his death in 1901, culminating in his (1903) posthumously published *Human Personality and Its Survival of Bodily Death* (*HP*), Myers dissented from the determination of his scientific contemporaries to exclude from psychology its most basic questions.

Myers was born at Keswick, England, in 1843, the son of a liberal clergyman who died when Myers was eight. He went up to Cambridge University in 1860 and lived in Cambridge until his death in 1901. At Cambridge he earned a First Class in both the Classical Tripos and the Moral Sciences Tripos, and he began reading for the Natural Sciences Tripos. In 1865 he was appointed to a fellowship and lectureship in classics at Cambridge, which he held until 1869, when, influenced by J. S. Mill's liberalism, he resigned his fellowship to work for the movement to broaden higher education, and particularly women's education, in Britain. After the passage of the Education Act of 1870, he began work in 1872 as a government school inspector, and in 1875 he was appointed school inspector for the Cambridge District, a position he held for the next 25 years.

Myers's work in education, however, eventually provided simply the background and part of the financial support for the real work of his life. Like many of the intellectual leaders of the mid-19th century, he had rejected the Christianity in which he had been raised because of its insufficient rational basis and "the need of an inward make-believe" (Myers, 1893/1961, p. 36). Although scientists such as those referred to earlier in this chapter were giving widespread currency to the new assumption that mind is a secondary byproduct of elementary material processes, this too seemed to him a gratuitous assumption that required closer scrutiny. To examine this assumption with novel lines of empirical research, Myers helped found the Society for Psychical Research (SPR) in 1882, an organization whose stated aim was "to approach these various problems without prejudice or prepossession of any kind, and in the same spirit of exact and unimpassioned inquiry which has enabled Science to solve so many problems, once not less obscure nor less hotly debated" (Society for Psychical Research, 1882, p. 4).

In the SPR's early years, the phenomena studied included most prominently the study of hypnosis and mesmerism, telepathy, mediumship, and hallucinations. The larger purpose of psychical research, however, as conceived by its most prominent founders, was to examine such phenomena in light of their bearing on questions about the nature and place in the universe of mind or human personality. In addition to Myers, founders and early members of the SPR included prominent scientists and intellectual leaders such as Arthur and Gerald Balfour, W. F. Barrett, W. E. Gladstone, Sir Oliver Lodge, Lord Rayleigh, John Ruskin, F. C. S. Schiller, Henry Sidgwick, Eleanor Sidgwick, Balfour Stewart, Lord Tennyson, and J. J. Thomson, all of whom sought a more satisfactory understanding of human nature than the intellectual climate of the 19th century was providing. For the first two decades of the SPR's existence, Myers was one of its most active inves-

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tigators and prolific writers; and his model of human personality, which he began to formulate in the early 1880s and then presented in detail in the 1890s in a series of nine papers on the Subliminal Self, became the theoretical framework for psychical research, and remained so for decades.[8]

It is readily apparent from even a brief glance at Myers's writings that his ultimate concern was with the question of whether individual personality survives death: "The question for man most momentous of all is...whether or no his personality involves any element which can survive bodily death. In this direction have always lain the greatest fears, the farthest-reaching hopes, which could either oppress or stimulate mortal minds" (*HP*, vol. 1, p. 1). His interest in psychology therefore was not purely academic. Although initially a poet and classicist, he turned to science and psychology because he understood that the question of post-mortem survival was, in essence, the problem of the relation of mind and body, a problem not to be left to "inward make-believe" (Myers, 1893/1904, p. 42) but to be attacked by empirical methods. As William James (1901) said at the time of Myers's death:

> Myers had as it were to re-create his personality before he became the wary critic of evidence, the skillful handler of hypothesis, the learned neurologist and omnivorous reader of biological and cosmological matter, with whom in later years we were acquainted. The transformation came about because he needed to be all these things in order to work successfully at the problem that lay near his heart. (p. 214)

Armed with a fervent belief in the power of scientific method, Myers fought the prevailing tendency in late 19th-century psychology to exclude its most fundamental problems and argued instead for an expansion of psychology's empirical base, the development of its own methods, and an examination of the theoretical assumptions about mind and scientific naturalism that were contributing to the narrowing of psychology.

Before examining Myers's theory of human personality and the avenues of research that he believed important for approaching the mind-body problem empirically, it is first essential to understand some of the purposes and principles that provided the foundation for his thinking. Myers and the field of psychical research in general, for which he was the primary spokesman and theoretician in its first decades, have too often been misunderstood, erroneously portrayed, and contemptuously dismissed as representing "pseudo-science" characterized by "magico-religious belief" and "irrationality" or even "anti-rationality" (see, e.g., Alcock, 1981; Zusne, 1985), or as threatening to return Western society to the superstitious belief in "the operation of 'hidden,' 'mysterious,' or 'occult' forces in the universe" (Kurtz, 1985, p. 505). Nothing could be further from the truth. The central principles guiding Myers were in fact precisely those of most of his scientific contemporaries, including "our modern ideas of continuity, conservation,

8. These nine papers are: Myers, 1892b, 1892c, 1892d, 1892e, 1892f, 1893a, 1893b, 1895d, and 1895e.

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evolution" (*HP*, vol. 2, p. 251), and a central purpose was to encourage the expansion of science and scientific method to address the most fundamental questions about the nature of human personality. For Myers and his colleagues, the "very raison d'être [of psychical research] is the extension of scientific method, of intellectual virtues....into regions where many a current of old tradition, of heated emotion, even of pseudo-scientific prejudice, deflects the bark" (Myers, 1900a, p. 459).[9]

Tertium Quid

In the midst of revolutionary new ideas in the 19th century about the nature and study of mind, not everyone agreed that the rigid dichotomy of the old, theological, personal world view and the new, scientific, impersonal world view, or the acquiescent methodological parallelism to which this dichotomization had led, is the final word. In the introduction to a two-volume collection of some of his essays, Myers's close friend and fellow psychical researcher Edmund Gurney (1887d) wrote:

> Most of the papers deal with matters of contemporary controversy, as to which two antagonistic opinions have been strongly entertained and enforced....In most of these questions I am conscious of "a great deal to be said on both sides",...[and] the truer view seems to me...not one that would extenuate differences...[but one whose] immediate tendency, on the contrary, is rather to make each of the duels triangular. In short, it is a *tertium quid.* (pp. v–vii)

John Stuart Mill had been the leader and exemplar of mid-19th-century liberal thinkers who believed that the cause of knowledge is best served, not by partisans, but by "those who take something from both sides of the great controversies, and make out that neither extreme is right, nor wholly wrong" (Mill, 1910, vol. 2, p. 360). The impact of Mill was particularly strong on intellectual circles at Cambridge in the 1860s; and Myers, Gurney, and other early leaders of psychical research educated at Cambridge fully absorbed this "tertium quid" approach. Fundamental both to Myers's thinking and to psychical research in general, therefore, was the belief that conflicts between ideas or points of view are best settled not by contentious

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9. In the same paper, he went on to say that "we have more in common with those who may criticise or attack our work with competent diligence than with those who may acclaim and exaggerate it without adding thereto any careful work of their own" (p. 459). Unfortunately, most critics of parapsychology and psychical research, past and present, have *not* "criticise[d] or attack[ed] our work with competent diligence," nor have they added any work, careful or otherwise, of their own. With such "critics" in mind, as well as those who "acclaim and exaggerate," Myers (1894–1895) also pointed out that "between the scornfully sceptical and the eagerly superstitious we have virtually had to create a public of our own" (p. 190). Unfortunately, that public still remains small.

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sized the limitations of psychology, Myers, in contrast, wanted to awaken scientists to a sense of the potential power and scope of psychology. We may, he said, so far have insufficiently appreciated "how very far...the possibility of experiment may extend" (p. 119). Myers thus (like James) warned of the danger to science of "the instinct of system, of a rounded and completed doctrine" that prematurely limits what science can and cannot address: A "determined protest against premature synthesis is as much needed now as ever" (Myers, 1889h, p. 392).

In particular, Myers protested against limiting science to the existing subject matter, methods, and concepts of the physical sciences by conceding prematurely that questions about the nature of the relationship between mental and physical phenomena—going beyond the prevailing but ultimately vague assumption of concomitance—are scientifically unapproachable. Instead, "the only line of demarcation which science can draw,...is between things which can, or which cannot, be cognised by our existing faculties," a line which is by no means "permanent and immovable....On the contrary, it is the continual work of science to render that which is incognisable cognisable, that which is imperceptible perceptible....Aristotle...relegated his unknowable to the fixed stars...but we have no more reason than he had to take our [present] mental horizon for an objective line" (Myers, 1881, p. 103).

In addition to lamenting the premature limitation of psychology's subject matter and methods, Myers would also have lamented the current breach in psychology (and indeed in current parapsychology) between its experimental and humanistic or transpersonal branches, that is, between empirically oriented persons who emphasize the objective scientific method and experientially oriented persons who emphasize a more subjective or intuitive approach. The challenge to psychologists—not an easy one, obviously—is to bring the objective method to bear on psychological phenomena without losing sight of their inherent subjectivity. This challenge of bringing scientific method to bear on highly personal experiential phenomena becomes particularly apparent in the study of mystical experience (see Chapter 8).

Psychophysiological Concomitance

Clearly, the most basic problem in psychology needing to be translated from metaphysical to empirical form is the question of psychophysiological correlation. As I discussed earlier, most scientists (then and now) concluded that the mind-body problem is no longer an open empirical question, because advances in physical science seemed to render it certain that mind is a product of the nervous system and wholly dependent on it. For Myers, however, the mind-matter problem was still very much an open empirical problem—and *the* basic theoretical question at the heart of psychology. He argued that the principle of concomitance, or correlation, which states sim-

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ply that "for every mental state there is a correlative nervous state" (Jackson, 1931–1932, vol. 2, p. 72), has not closed off the empirical question of the causal relationship between mind and brain, because it is essentially a neutral statement: "Accompanying the mental phenomena—states of consciousness, there are physical phenomena—brain changes; but no knowledge of the one throws any light on the other" (*HP*, vol. 1, p. 13n). Moreover, merely continuing to observe the parallelism will not advance our knowledge in any qualitative sense:

> However exactly the parallelism between psychical and cerebral energies may be established, the exacter correlation can tell us little more than the vaguer told us—little more than we had always known....But as to the origin or essential significance of this close connection...we avowedly know nothing at all. We do not know whether the mental energy precedes or follows on the cerebral change, nor whether the two are, somehow, but different aspects of the same fact. (Myers, 1891d, p. 635)[12]

Psychologists had in fact limited their observations of mind-brain correlation primarily to situations in which brain is essentially the independent variable and mind the dependent. The observer creates or looks for a condition of damage or alteration to the nervous system and then describes the effects on mental functioning or, at best, looks at a mental state and attempts to identify an associated physiological state. Such essentially one-sided observations are, in Myers's view, bound to lead to inadequate conclusions. When, he said, we look at a partially illuminated globe, the result is

> a familiar optical illusion. When we see half of some body strongly illuminated, and half of it feebly illuminated, it is hard to believe that the brilliant moiety is not the larger of the two. And, similarly, it is the increased definiteness of our conception of the physical side of our mental operations which seems to increase its relative importance, —to give it a kind of priority over the psychical aspect of the same processes. Yet...the central problem of the relation of the objective and subjective sides of the psychoneural phenomena can in no way be altered by any increase of definiteness in our knowledge of the objective processes which correspond to the subjective side. (Myers, 1886a, p. xl)

The Study of Subliminal Phenomena[13]

Achieving a more balanced approach to the problem of mind-brain concomitance requires a thoroughgoing empirical study of mental efficacy, that is, the study of phenomena which suggest that a change in mental state

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12. Those confident that modern neuroimaging techniques confirm the view that mind is a product of the brain would do well to remember this cautionary statement.

13. As I will explain later in this chapter, Myers proposed that the word

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has produced some change in a physiological or physical state (I will discuss such phenomena in depth in Chapter 3). To advance our understanding of the relationship between mind and brain beyond the long-recognized but little-understood parallelism, Myers believed that psychologists needed to begin to single out for special attention situations in which the ordinary relationship between mental and physical functioning seems to be altered or thrown out of gear. In particular, he believed that a newly emerging field—namely, the study of subliminal phenomena in all their myriad forms—had enormous potential for increasing scientific knowledge about the relationship of mental and physical processes. These phenomena are especially important because in them the normal equilibrium, as Myers put it, between mental and physical functioning often seems to be upset, and mental and physical processes operate in unaccustomed and unusual ways. Such phenomena thus suggest that the correlation of mind and brain might not be as straightforward as it appears under normal circumstances.

Myers also believed that such phenomena are important because they sometimes reveal latent mental processes or abilities not apparent in the context of ordinary psychophysiological functioning. The study of subliminal phenomena, which was expanding rapidly during the 19th century (see, e.g., Ellenberger, 1970; Gauld, 1992), increasingly turned up phenomena difficult to reconcile with the prevailing physiological, mechanistic theory of mind. For example, psychosomatic phenomena such as those associated with hypnosis and hysteria suggest that alterations in mental states or processes can have dramatic effects on physiological processes. Such phenomena thus reveal the possibility of experimentally manipulating mental states as the independent or causal variable and observing the effects on physical processes. Moreover, many subliminal phenomena such as hysterical anesthesia or hypnotic hallucinations, occurring in conditions where physical pathology is unlikely, nonetheless sometimes resemble phenomena that are clearly associated with neuropathology. These phenomena suggest that similar effects might not always have similar causes; a blister, for example, might have either a physical cause (a burn) or a mental cause (an hypnotic suggestion). Myers urged the importance of studying such phenomena to determine whether, and under what conditions, mind might be an active initiating factor. Finally, more extreme phenomena such as telepathy[14] even

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"subliminal" be used, rather than "subconscious" or "unconscious," to denote psychological phenomena occurring outside one's normal waking consciousness.

14. The word "telepathy" was introduced by Myers in 1882 (W. F. Barrett, Massey, et al., 1883, p. 147; Myers, 1896a, p. 174) to refer to the phenomenon of one person apparently deriving information directly from another person's mind. Although telepathy and clairvoyance were, and still are, much more controversial than, for example, hypnosis or hallucinations, Myers and his colleagues believed that they had sufficient evidence, both from experiments and from spontaneous experiences, to try to incorporate such phenomena into a broader understanding of the nature of mind; and we agree. We again urge readers to consult the Appendix for references to the serious literature on psychical research.

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more clearly suggest that mental and physical processes do not always operate in the accustomed manner (e.g., Gurney et al., 1886).

To Myers, therefore, subliminal phenomena are particularly important because they suggest that mind is something greater, not only in extent but in capacity, than ordinary psychological phenomena reveal. He argued, however, that the investigation of subliminal phenomena must be approached from a larger perspective than that of most previous studies, which were primarily undertaken for medical or clinical purposes. Although subliminal phenomena were beginning to be widely studied by clinicians (especially in France by Janet, Charcot, Binet, and many others), Myers believed that they should also be examined for their bearing upon central theoretical problems in psychology. The study of hallucinations, for example, "has usually been undertaken with a therapeutic and not with a purely scientific purpose," with the result that pathological aspects of hallucinations have been noted and emphasized, rather than their "absolute psychological significance" (Myers, 1892d, p. 342). Similarly, as I will discuss in more detail later, Myers believed that hypnotism is potentially one of the most effective methodological tools for theoretical psychologists. Yet, here too, in the burgeoning study of hypnosis

> we have to regret the lamentable scarcity of purely psychological experiments over the whole hypnotic field. We are habitually forced to base our psychological inferences on therapeutic practice; and in directions where there has been no therapeutic effort there are gaps in our knowledge, which those hypnotists who have good subjects at their disposal should be invited to fill up as soon as may be. (*HP*, vol. 1, p. 191)

Even hysteria, clearly a severe clinical problem and understandably emphasized as such, is also an important potential source of knowledge about psychophysiological functioning (Myers, 1893a; *HP*, chap. 2). Hysterics often show subliminal control over physiological functioning, producing effects, such as hysterical anesthesias or stigmata, that are practically unknown under normal conditions. A comparison of hysterical and neurological disorders might therefore reveal much about the nature and extent of psychological processes as causal processes, especially the degree to which they are dependent on neurological conditions or, conversely, may themselves alter these conditions.

The New Physics

Just as subliminal phenomena were showing mind to be more extensive and of a different nature than previously assumed, so late 19th-century physics was showing the physical universe to be more extensive—and even of a different nature—than previously assumed. Perhaps its greatest accomplishment was in beginning to reveal just how limited our normal, unaided sensory perception is, in comparison with the character and extent of the

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surrounding universe. Myers recognized the potential importance to the mind-matter problem of this dawning realization. To those whose thinking about the nature of psychophysical processes has been circumscribed by the assumption that our everyday perception of the physical world is somehow a benchmark, Myers (1881) cautioned that "Science, while perpetually denying an unseen world, is perpetually revealing it" (p. 103).

The discovery and study of electromagnetic radiation in particular had begun to reveal just how narrow and limited our sensory perceptions are. The expansion of our knowledge "into regions of rays which no senses born within us have enabled us directly to discern" (Myers, 1894–1895, p. 196) implies that we have not yet exhausted our potential knowledge of aspects of the universe co-existing, undetected, with the perceivable world. Science cannot "conjecture beforehand how many distinct but coexisting environments may now surround us....Her own history has been one of constantly widening conceptions" (Myers, 1894–1895, p. 195). In a prescient remark anticipating the upheavals in scientists' conception of space, matter, and time brought on by 20th-century physics, Myers cautioned that we "must be ready to conceive other invisible environments or co-existences, and in a sense to sit loose to the conception of Space, regarded as an obstacle to communication or cognition" (*HP*, vol. 2, p. 262).

Yet he also emphasized repeatedly that such "unseen" environments must somehow be "fundamentally continuous" and interrelated with the one we know directly; "if an unseen world exists...we must in some sense be in it" (Myers, 1891d, p. 634): Like "a tadpole...who had learned theoretically that what he was breathing in his pond was not the water but the oxygen dissolved therein,—and who then should...raise his head above water...[and] perceive frogs and other animals respiring the translucid air" (*HP*, vol. 2, p. 526), scientists too would probably continue to discover unsuspected environments, co-existing and continuous with the familiar world we perceive directly, even if also differing from it in certain respects.[15]

Mind and Matter

Myers therefore was in a real sense motivated by the expectation that a combined study of the unsuspected range of mind and the previously unsuspected properties and extent of matter would begin to suggest new

15. An important concept in late 19th-century physics was that of the ether. The discovery of radiation had led many scientists to postulate a homogeneous, frictionless, non-material substance filling what we perceive as "empty" space and serving as the transmitting medium for light and electromagnetic forces. Although 20th-century physics abandoned this *particular* concept of ether, it nonetheless added significant support for the larger idea behind the concept of ether—and the one that was of especial significance to Myers—that the imperceptible range of the material universe far exceeds the few aspects of it that are perceptible by our normal, unaided senses.

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well as physical; and second, to attempt to forge a new perspective on old problems concerning the nature of mind by extending psychology's range of observation and data beyond ordinary, familiar phenomena and by broadening its concepts through continually examining assumptions, hypotheses, and views contrary to those currently prevailing. On the basis of this "tertium quid" approach, Myers went on to make two major contributions to psychology. First, he developed a theoretical model of mind that was an important attempt to move beyond the two predominant, but diametrically opposed, views of mind and to develop a more comprehensive view. Second, he identified numerous lines of research by which he thought that the mind-matter problem could be approached, and potentially resolved, empirically. In the rest of this chapter, I will first describe Myers's model of mind and then, by giving a brief overview of his book *Human Personality*, introduce some of the kinds of research that he believed essential for developing an adequate theory of mind.

Myers's Theory of Human Personality

The engine that drove all of Myers's thinking and work was his passionate desire to learn whether or not individual consciousness survives death. As a scientific naturalist in the broad sense, however, he fully recognized that such an enormous question cannot be answered until that problem, and any empirical phenomena relevant to it, can be situated in a framework that makes them theoretically continuous and congruent with other psychological and biological phenomena. This does not mean *reducing* the unknown to the already known, the approach taken in so much of scientific psychology, but instead *linking* the unknown to the already known in a continuous series. Developing such a series, from normal to abnormal to supernormal psychological phenomena, formed the methodological and organizational basis for all of Myers's work.

The immediate challenge for a psychology that might ultimately deal with the question of post-mortem survival is to determine whether human personality is of such a nature that it could even conceivably survive the destruction of the biological organism. In other words, survival research can be conducted productively only within the broader context of psychological research on the nature of mind or consciousness in general: "It became gradually plain to me that before we could safely mark off any group of manifestations as definitely implying an influence from beyond the grave, there was need of a more searching review of the capacities of man's incarnate personality than psychologists...had thought it worth their while to undertake" (*HP*, vol. 1, pp. 8–9). Translating the mind-body problem into an empirical research problem thus became for Myers the primary challenge and task for psychology. It is important to emphasize again that the principle of psychophysiological correlation itself is not what was at issue;

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that there is some fundamental relationship between normal, waking consciousness and the state of the brain was and is evident to scientists and non-scientists alike. Nevertheless, recognizing this correlation still leaves open the question of what it signifies.

The first step toward translating the mind-body problem into an empirical problem, therefore, is to recognize that there is more than one way to interpret mind-brain correlation. A few individuals have suggested that the brain may not *produce* consciousness, as the vast majority of 19th- and 20th-century scientists assumed; the brain may instead *filter*, or shape, consciousness. In that case consciousness may be only partly dependent on the brain, and it might therefore conceivably survive the death of the body.[16]

Myers presented what is so far the most thoroughly worked out and empirically grounded version of this filter interpretation of mind-body correlation. Myers himself did not refer to the brain specifically as the filter, nor does he refer to the transmission model of consciousness as described by James (1898/1900) or Schiller (1891/1894). Nevertheless, his huge body of published writings is largely an elaboration of the view that certain phenomena of psychology, particularly of abnormal psychology and psychical research, demonstrate that human personality is far more extensive than we ordinarily realize; that our normal waking consciousness (called by Myers the supraliminal consciousness) reflects simply those relatively few psychological elements and processes that have been selected from that more extensive consciousness (called by Myers the Subliminal Self) in adaptation to the demands of our present environment; and that the biological organism, instead of producing consciousness, is the adaptive mechanism that limits and shapes ordinary waking consciousness out of this larger, mostly latent, Self. In sum:

> There exists a more comprehensive consciousness, a profounder faculty, which for the most part remains potential only...but from which the consciousness and faculty of earth-life are mere selections....[N]o Self of which we can here have cognisance is in reality more than a fragment of a larger Self,— revealed in a fashion at once shifting and limited through an organism not so framed as to afford it full manifestation. (*HP*, vol. 2, pp. 12, 15)

16. For some persons who have seriously considered this "filter" or (as James called it) "transmission" interpretation of mind-brain relations, see Bergson (1913), Broad (1953), Burt (1968, pp. 58–59), A. Huxley (1954/1990, p. 23), James (1898/1900), and Schiller (1891/1894, pp. 293–295). See also our Chapters 1, 8, and 9.

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The Unity-Multiplicity Problem: "Unitary" versus "Colonial" Views of Mind

Myers's view of human personality had grown out of his attempts, begun in the early 1880s, to bridge the major theoretical gulf between the old, philosophical, mentalistic psychology and the new, scientific, physiological psychology. As in physics—which throughout its history had seen the recurrent waxing and waning of wave versus particle theories of light—psychological theorizing vacillated between, in essence, a wave theory of mind, in which mind is seen as an indivisible unity, and a particle theory, in which mind is seen as the composite product of individual sensations or other "atomistic" psychological elements. Myers quoted from the 18th-century philosopher Thomas Reid to describe the view of mind as an indivisible whole:

> The conviction which every man has of his identity...needs no aid of philosophy to strengthen it; and no philosophy can weaken it....I am not thought, I am not action, I am not feelings; I am something that thinks, and acts, and suffers. My thoughts and actions and feelings change every moment...; but that *self* or *I*, to which they belong, is permanent....[A] person is a *monad*, and is not divisible into parts. (Myers, 1885c, p. 639; *HP*, vol. 1, p. 10)

In the new physiological psychology, in contrast, mind was seen as an aggregate of elements. Its perceived unity derives entirely from the evolved coordination of the parts and processes of the bodily organism, and it is subject to disintegration under pathological conditions. Ribot provided Myers with his description of this "colonial" view of mind:

> It is the organism...which constitutes the real personality....The conscious personality is never more than a small fraction of the psychical personality. The unity of the Ego is not therefore the unity of a single entity diffusing itself among multiple phenomena; it is the co-ordination of a certain number of states perpetually renascent, and having for their sole common basis the vague feeling of our body. This unity does not diffuse itself downwards, but is aggregated by ascent from below...; *the Self is a co-ordination*. (*HP*, vol. 1, p. 10; see Ribot, 1898, pp. 154–155)

These two views seemed completely opposite in nature and apparently "hopelessly incompatible" (*HP*, vol. 1, p. 11); and yet each was supported by empirical observation—"the one by our inmost consciousness," or personal experience, and "the other by [the] unanswerable observation and inference" of advancing scientific analysis (*HP*, vol. 1, p. 11). In keeping with his "tertium quid" approach, however, Myers believed that "the reconcilement of the two opposing systems in a profounder synthesis" is possible (*HP*, vol. 1, p. 11). Neither view, he argued, is wrong; both are simply incomplete. Myers agreed with the colonialists that mind is not the simple unity we generally take it to be (Myers, 1885c, p. 638; *HP*, vol. 1, p. 11): "The old-fashioned conception of human personality as a unitary consciousness known with prac-

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tical completeness to the waking self need[s] complete revision" (*HP*, vol. 2, p. 81). The rapidly multiplying observations of experimental psychology, neurology, psychopathology, and hypnotism clearly showed that the human mind is far more extensive than ordinarily thought, since much psychological functioning remains outside the range of our conscious mental life; that higher mental processes had evolved from lower ones; and that under certain conditions, the ordinary unity of consciousness can break down.

Nevertheless, Myers also believed that even though these observations were correct, the theoretical conclusion drawn from them—that human personality is a mere aggregate of separate elements—is a premature and superficial conclusion. He believed that, when psychologists probe more deeply into the problem, the analysis, paradoxically, reveals an underlying continuity, and a fundamental unity, of human personality.

An Expanded View of Consciousness

An important first step that Myers took in this direction was to try to clear up the confusion that many people—then and now—have felt about the notion of an "unconscious mind." Most people naively equate their mind, and especially the term "consciousness," with their ordinary awareness. To propose that there are unconscious mental states, therefore, seems an oxymoron. This belief gave rise in the 19th century to interpretations of unconscious phenomena such as the physiologist William Carpenter's hypothesis of unconscious cerebration, according to which all unconscious processes, being by definition devoid of conscious awareness, are reflexes of the brain (Carpenter, 1874/1882).[17]

This hypothesis was severely challenged, however, by multiple kinds of evidence then emerging for complex mental functioning that occurred outside an individual's ordinary waking awareness. Such evidence included in particular the alterations in consciousness seen in connection with mesmerism and hypnosis, as well as numerous clinical reports of cases involving alternate, or secondary, personalities, with what appeared to be separate memory chains, streams of consciousness, and thus self-identity comparable in kind (if not always in degree) to the original personality.[18] In these situations, processes occurring beyond the margins of ordinary consciousness displayed all the characteristics that we attribute to conscious beings, such as memory, intention, volition, and creativity. Myers unequivocally denied that any variant of the "unconscious cerebration" hypothesis can accommodate such observations: "I wish to protest against the undue extension of such phrases as 'unconscious cerebration,' and to insist that we have as good ground for attributing consciousness to some at least of these subliminal

17. The same idea lives on today in the form of "the cognitive unconscious" (see Chapter 5).

18. See, for example, Binet (1890, 1891/1896), M. Prince (1905/1908), Sidis (1898/1906, 1912), and Sidis and Goodhart (1905). See also Chapter 5.

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operations in ourselves as we have for attributing consciousness to the intellectual performances of our neighbors" (Myers, 1892c, p. 327).

Myers was thus led to a definition of "conscious" radically different from our usual equation of it with what goes on in our ordinary, waking, aware self. For him "*conscious* means *memorable*," that is, something that is "capable of being comprehended within some chain of memory," either of the primary consciousness or of a secondary one, given the appropriate conditions (Myers, 1885d, p. 129; 1891c, p. 117):

> When we conceive any act other than our own as a conscious act, we do so either because we regard it as *complex*, and therefore *purposive*, or because we perceive that it has been *remembered*....The *memorability* of an act is, in fact, a better proof of consciousness than its complexity....I cannot see how we can phrase our definition more simply than by saying that any act or condition must be regarded as conscious if it is *potentially memorable*. (*HP*, vol. 1, pp. 36–37)

In other words, something is "conscious" if it is capable of entering waking awareness, given the appropriate condition or the discovery of an "appropriate artifice" or experimental method to elicit it (Myers, 1891c, p. 115). Given this new, expanded conception of what is "conscious," Myers (1892b) therefore considered such terms as "'Unconscious,' or even 'subconscious,'...[to be] directly misleading," and he proposed instead the words "supraliminal" and "subliminal" to distinguish between streams of consciousness that are and are not, respectively, identifiable with ordinary awareness (p. 305).

This notion of something within us being conscious, even though it is not accessible to our ordinary awareness, is an exceedingly difficult one for most of us to accept, since it is so at variance with our usual assumption that the self of which we are aware comprises the totality of what we are as conscious mental beings. Nevertheless, it is essential to keep in mind Myers's new and enlarged conception of consciousness if one is to understand his theory of human personality as something far more extensive than our waking self.

A Jacksonian Model of Mind

Myers's model of mind was deliberately patterned on Hughlings Jackson's hierarchical model of nervous system functioning, which in turn had derived from the 19th-century philosopher Herbert Spencer's ideas about the evolution and dissolution of complex systems (Jackson, 1884). Jackson described the nervous system as a hierarchy of three general levels, ranging from the oldest and most basic biochemical processes, shared with primitive organisms, to mid-level sensorimotor processes, to the most recently evolved cerebral centers with which the higher mental processes are associated. Development occurs as older processes, through repeated functioning, become more organized, automatic, unconscious, and stable. Reced-

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range of perception which rises above the threshold—the spectrum, as I call it, of my supraliminal consciousness—may merely have been determined by natural selection" (Myers, 1894–1895, p. 197). The ordinary waking self is not the *only* possible self that could have developed out of the entire, mostly latent Self; nor, as Myers frequently emphasized, is it necessarily psychologically superior to or more important than the rest of the spectrum of consciousness:

> I hold that we each of us contain the potentialities of many different arrangements of the elements of our personality....The arrangement with which we habitually identify ourselves,—what we call the normal or primary self,—consists, in my view, of elements selected for us in the struggle for existence with special reference to the maintenance of ordinary physical needs, and is not necessarily superior in any other respect to the latent personalities which lie alongside it. (Myers, 1888a, p. 387)

Myers's model of the evolution of mind echoed certain further ideas of Spencer, from whom Jackson had derived his model of the evolution and dissolution of the nervous system. In Spencer's evolutionary theories, the universe—like an embryo—began as a simple homogeneity, or formless unity, which began to divide and differentiate into parts, and then integrated to form new units that become increasingly complex in the ongoing process of adapting to their environment. Jackson had applied these general ideas about the evolutionary differentiation and increasing complexity of systems to physiology and the nervous system in particular (R. M. Young, 1968, 1970).

Implicit in these ideas about the evolution of the universe from a formless homogeneity to complex forms of life was the idea that all of the latter were somehow inherent in the former. An important aspect of Myers's ideas about the evolution of mind or consciousness, therefore, was that, just as the forms of all living organisms were somehow latent in the original homogeneity, or "primal germ," from which all life developed, all forms of consciousness were likewise inherent in the homogeneous primal germ. All life "starts from an *X* of some sort; and for my present argument it matters not whether you call *X* a carbon-atom or an immortal soul. Whatever it was, *X* had certain propensities, which must have dated in any case from some age anterior to its existence upon our recent planet...[and] on which earth's forces began their play" (Myers, 1892b, p. 318).

Thus, Myers suggested, there had been a "primitive simple irritability" (*HP*, vol. 1, p. 95), or "undifferentiated sensory capacity of the supposed primal germ" (Myers, 1896a, p. 167), which he called *panaesthesia*. Out of this homogeneous or undifferentiated sensibility have developed the particular senses we now have. For example, the evolutionary process eventually reached "a point...where vision differentiate[d] itself from various indefinite forms of perception...with the growing sensibility of the pigment-spot to light and shadow" (*HP*, vol. 1, p. 224). Similarly, other senses evolved out

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of some pre-existing latent potential. Just as importantly, other forms of perception may yet be emerging and evolving:

> Whatever be the part which we assign to external influences in its evolution, the fact remains that the germ possessed the power of responding in an indefinite number of ways to an indefinite number of stimuli. It was only the accident of its exposure to certain stimuli and not to others which has made it what it now is. And having shown itself so far modifiable as to acquire these highly specialised senses which I possess, it is doubtless still modifiable in directions as unthinkable to me as my eyesight would have been unthinkable to the oyster. (Myers, 1889e, p. 190)

Myers also pointed out that, on both the individual and evolutionary levels, the process of evolution has involved not simply the adaptation of an organism to its environment, but also, with increasingly complex sensory processes, the widening perception of that environment, the "gradual *discovery* of an environment, always there, but unknown" (*HP*, vol. 2, p. 95). The implication for Myers was that, as physics was also revealing, there probably are "unseen" environments, imperceptible to our senses as they have so far evolved, but nonetheless "fundamentally continuous" and interrelated with what we do perceive.

Human beings have "evoked in greatest multiplicity the unnumbered faculties latent in the irritability of a speck of slime" (*HP*, vol. 1, p. 76). Nevertheless, it does not thereby follow that our present sensory capacities and our normal waking consciousness mark the final point of the evolutionary process: "To anyone...who takes a broad view of human development, it must seem a very improbable thing that that development should at this particular moment have reached its final term" (*HP*, vol. 1, p. 186). Just as in the individual spectrum of potential consciousness some contents and capacities have become supraliminal and some remain subliminal, so in the evolutionary spectrum of consciousness, some faculties have been evoked and some remain latent (*HP*, vol. 1, p. 119); but there is "no apparent reason why these latent powers should not from time to time receive sufficient stimulus" to appear sporadically, and even ultimately to develop more fully (*HP*, vol., 1, p. 186).

The Subliminal Self: A "Tertium Quid" Theory of Consciousness

With this evolutionary model of a larger Self whose latent capacities gradually emerge and whose emergent manifestation grows increasingly complex in response to the demands of the environment, Myers thought that psychology could resolve the apparent conflict between the old concept of mind as a unity and the new concept of mind as a multiplicity, and affirm that *both* views are in fact correct, although incomplete: Consciousness is, he insisted, "at once profoundly unitary and almost infinitely complex" (*HP*, vol. 1, p. 34). The Subliminal Self or Individuality—the original whole

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light ray, in the metaphor of the electromagnetic spectrum—registers or otherwise incorporates within itself everything that comes within its range of experience. These are "the elements of our personality," and those few "elements selected for us in the struggle for existence" are bound together in a more or less stable chain of memory, our ordinary waking self (Myers, 1887b, p. 387). In certain circumstances, however, other chains of memory or groupings of elements may form: "The letters of our inward alphabet will shape themselves into many other dialects;—many other personalities, as distinct as those which we assume to be *ourselves*, can be made out of our mental material" (Myers, 1889e, p. 195). Moreover, the number of such groupings or personalities is potentially endless: "The fresh combinations of our personal elements...may be evoked, by accident [e.g., spontaneous somnambulism or multiple personality] or design [e.g., hypnosis or suggestion], in a variety to which we can at present assign no limit" (Myers, 1888a, p. 387). If any of these new chains of memory become sufficiently complex and stable, they thus develop into one or more secondary personalities, or subliminal selves.

Nonetheless, behind the "shifting" elements and groupings of elements of our being, there is a "perdurable Unity" (Myers, 1889g, p. 343; see also 1885a, 1885c). It is erroneous, he thought, to conclude that the analysis of personality into many components means that there is no ultimate unity behind it (1887a, p. 260). Myers found it particularly significant that, in certain hypnotic and psychopathological cases, the various personalities were not totally isolated; some of them were, in varying degrees, aware of others. In Janet's case of Léonie (or Madame B.), for example, the secondary personality, Léontine, was fully aware of Léonie, although Léonie was unaware of her; and the third personality, Léonore, possessed the memories of both the other two, even though they were both unaware (directly) of her existence (see *HP*, vol. 1, pp. 322–326). Similarly, in Morton Prince's case of Christine Beauchamp, there was a hierarchy of selves in which each one knew about the one(s) lower in the hierarchy, but not the one(s) above it (Prince, 1900, 1905/1908). Although this "hierarchy" of memory was not straightforward or even present in every case of multiple personality, it was a common enough feature to be noteworthy, and particularly because the same sort of hierarchy could also be evoked experimentally by hypnosis (Gurney, 1884, 1887c): "We all know that the hypnotised subject as a rule remembers waking life, but that the awakened subject as a rule has wholly forgotten the effects of this hypnotic trance. The full significance of this fact...has hardly yet, I think, been realised in any quarter" (Myers, 1892b, p. 303). The significance is that there may in fact be an underlying unity to human personality.[20]

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20. Hilgard (1977) has more recently called attention to the importance of the "covert" contents of consciousness that can be uncovered by means such as hypnosis. Observing that people who profess to be unaware of events occurring while they were hypnotized can sometimes recover memories of these events when re-hypnotized, Hilgard proposed his "neo-dissociationist" model of hypnosis,

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Many critics of Myers's theory (e.g., Jastrow, 1906; Mallock, 1903; McDougall, 1926, p. 523) have mistakenly attributed to him the view that the subliminal and the supraliminal selves act as two co-existing, discrete selves. This completely misrepresents his actual view. Myers's theory was not simply a hypothesis of the multiplicity of personality, but went further and tried to reconcile the paradoxical multiplicity *and* unity of human personality. Myers in fact explicitly said: "I do not...assume that there are two correlative and parallel selves existing always within us" (*HP*, vol. 1, p. 15):

> My contention is, *not*, as some of my critics seem to suppose, that a man (say Socrates) has within him a conscious and an unconscious self, which lie side by side, but apart, and find expression alternately, but rather that Socrates' mind is capable of concentrating itself round more than one focus, either simultaneously or successively. I do not limit the number of *foci* to *two*. (Myers, 1885d, p. 129)

One contemporary historian has even alleged that "the cornerstone of his [Myers's] conception was the fact that consciousness had no essential unity" (E. Taylor, 1996, p. 81), an assertion that clearly misses the essence of Myers's theory of human personality, his oft-stated conclusion that there *is* a "perdurable Unity."

To understand that in Myers's theory mind is *both* a unity and a multiplicity, it is essential to understand the clear distinction that he drew between "Individuality" and "personality." By *Individuality*, or *Self*, he meant to refer to "the underlying psychical unity which I postulate as existing beneath all our phenomenal manifestations"; by *personality*, or *self*, he meant those "more external and transitory" chains of memory, including the ordinary or supraliminal self of which we are customarily aware, as well as the potentially infinite number of selves that may be formed from "the elements of our being" (Myers, 1892b, p. 305; 1888a, p. 387). Each of us is *one* of the former, even though *many* of the latter may be formed from that larger Self: "The human individuality [is] a practically infinite reservoir of personal states;—as a kaleidoscope which may be shaken into a thousand patterns, yet so that no pattern can employ all pieces contained in the tube" (Myers, 1892e, p. 363).[21]

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suggesting that hypnosis creates an amnesic barrier preventing mentation in the hypnotic state from entering ordinary waking consciousness. In important respects, therefore, Hilgard's hypothesis of a "Hidden Observer" who has access to these covert memories is similar to Myers's hypothesis of an underlying Subliminal Self who, as hypnosis sometimes shows, is aware of events that the supraliminal consciousness is not. Hilgard, however, distances his theory decisively from any theories of another "self," primarily because, unlike Myers, his theory is built on the assumption that all forms of consciousness, covert as well as overt, are brain processes alone. See Chapter 5 for further discussion of Hilgard's views.

21. Oxford philosopher F. C. S. Schiller (1891/1894, pp. 279–282) drew a similar distinction between the phenomenal "self" and the transcendental "Ego." Like Myers, he emphasized that the Ego is not a second self, but that the ordinary self

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Understanding the distinction Myers drew between "Individuality" and "personality"—or between "Self" and "self"—can help clear up the sorts of confusion that Gauld (1968) apparently experienced with regard to Myers's theory. Gauld complained about "the abstruseness and complexity of the concepts central to his theory, such as consciousness, mind, soul, spirit, personality, psychical activity," and he argued that in Myers's theory the "soul," *not* the "subliminal self," is the "unifying principle...'behind' all mental phenomena....The concept of the 'subliminal self' is simply not qualified to act as a unifying theoretical principle" (Gauld, 1968, pp. 278, 295). Further, Gauld complained, "Myers offers little elucidation of these terms" (p. 278).

Some of this confusion undoubtedly derives in part from Myers's own somewhat inconsistent use of the terms "subliminal self" or "subliminal consciousness." On some occasions, he used the term "subliminal" to refer in general to "all which lies below that threshold [of ordinary consciousness]" (Myers, 1892b, p. 305). On other occasions he used it to refer more specifically to secondary personalities or chains of memory such as those that occur spontaneously, in hysteria or multiple personality, and those that are artificially induced, as in hypnosis. On still other occasions, he used the term "subliminal self" (usually, but unfortunately not consistently, spelled with capital letters as "Subliminal Self" or "subliminal Self") to refer to the underlying unity or larger Self. These "concepts central to his theory" are undoubtedly difficult, but despite some inconsistency in his usage or spelling Myers was quite clear in his intent to distinguish between a subliminal "self" (a personality alternate or in addition to the normal waking one) and the Subliminal "Self" or "Individuality" (which is his real "unifying theoretical principle"). In this book we will try to keep the distinction clear in readers' minds by using the term "subliminal consciousness" to refer to any conscious psychological processes occurring outside ordinary awareness; the term "subliminal self" (lower case) to refer to "any chain of memory sufficiently continuous, and embracing sufficient particulars, to acquire what is popularly called a 'character' of its own" (pp. 305–306n); and the term "Individuality" or "Subliminal Self" (upper case) to refer to the underlying larger Self.

The Permeable Boundary: A Psychological Mechanism

In Myers's model, evolution of consciousness involves the shifting of the supraliminal segment up the spectrum into the ultraviolet region, as more and more psychological processes are mastered and then relegated to the

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is an extract of the Ego (p. 410). In many respects, in fact, Schiller's and Myers's theories of mind are parallel; and in a review of Morton Prince's *The Unconscious* (Schiller, 1915), Schiller specifically advocated Myers's theory of human personality as providing the best account of the facts described by Prince. Ducasse (1951, p. 495) later drew a similar distinction between individuality and personality.

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vol. 1, p. 72).[23] Nonetheless, there is "real psychological danger in fixing our conception of human character too low. Some essential lessons [concerning the nature and functioning of human personality] are apt to be missed" (*HP*, vol. 1, p. 50).

On the other hand, our waking or supraliminal consciousness is also not inherently superior, or even necessarily "the most important part of the psychical operations which are going on within us" (Myers, 1887a, pp. 258–259). Myers agreed with Jackson (1884), who, when discussing the evolution and dissolution of the nervous system, had insisted that manifesting states are the "'fittest,' not 'best'....[T]he evolutionist has nothing to do with good or bad" (p. 591). The distinction, Myers emphasized repeatedly, between the subliminal and the supraliminal, as well as between the evolutive and dissolutive, is "a purely psychological one" (Myers, 1900d, p. 289; *HP*, vol. 1, p. 72).

To those, therefore, who asked: "Are we then to believe that the subliminal self is both wiser and more foolish, truer and more false, more understanding and more ignorant, more reliable and more untrustworthy than the normal self?" (Dallas, 1900, p. 288), Myers's answer was "Yes." Depending on the conditions under which they emerge, the elements of the Subliminal Self or Individuality can fall into numerous patterns or even "selves," running the gamut from the most primitive, elementary, fragmentary, and pathological to the most advanced, complex, complete, and beneficial.

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23. Jastrow was among those who mistakenly thought that Myers's hypothesis of human personality was based on the notion that subliminal processes are "ipso facto" superior to supraliminal ones (see, e.g., Jastrow, 1906, p. 537). Jastrow seems, however, to have completely misunderstood Myers's hypothesis in general. He criticized it as based "upon a fundamental emphasis on the schism of conflicting personalities," and went on to argue that his own hypothesis of the "subconscious as a natural function with the most intimate relations to consciousness,...both parts of a common synthesis,...is diametrically opposed to that of the subliminal self" (1906, pp. 537, 539–540). Elsewhere (1903) he criticized Myers's hypothesis as one of discontinuity and argued that the concept of the subconscious will not be recognized in psychology as important until the hypothesis of the discontinuity of consciousness and the subconscious is replaced by one recognizing their underlying continuity. Jastrow's hypothesis, in fact, was in many ways closely similar to Myers's hypothesis, *particularly* with regard to the ultimate continuity of conscious and subliminal processes. Some of the larger implications that Myers drew from the same premises (such as the potential post-mortem survival of human personality) were undeniably different from Jastrow's conclusions; and this may explain why Jastrow (and others) have been so prone to misread and misrepresent Myers's hypothesis. It may also help explain why Jastrow (1900) and others have also been so prone to misread and misrepresent psychical research generally.

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Automatisms and the Expression of Subliminal Functioning

Another important concept in Jackson's model of nervous system functioning that was paralleled in Myers's model of mind was that of automatisms.[24] Jackson (1884) had proposed that the older and more habitual a process became, the more stable, unconscious, and automatic its execution became, leaving the organism free to develop more advanced and complex processes. Similarly, according to Myers, as a species evolves or an individual develops, older psychological processes become more stable and automatic. There is, he pointed out, an evolutionary advantage to "relegating voluntary ends to automatic execution," because learned, stable, automatic processes get "the needed thing done...with a verve and a completeness which conscious effort finds it hard to rival" (Myers, 1900c, p. 415). But Myers's concept of automatisms went beyond that of Jackson to include not only the automatic execution of older, more established modes of functioning (those in the "infrared" region), but also the emergence of new, more complex processes originating in the "ultraviolet" region. Myers therefore defined psychological automatisms generally as any form of communication or exchange of material from the subliminal regions of the psychological spectrum to the supraliminal. They include dreams, secondary personalities, hypnosis, automatic writing, trance speaking, and the "uprushes" of subliminally generated creative inspirations into supraliminal expression. Automatisms, moreover, may take the form of influence upon primarily organic processes over which the supraliminal self ordinarily has no control (as in the phenomena of hysterical or hypnotic anesthesia), as well as of new and sporadically occurring processes (such as telepathy) over which the supraliminal similarly has little or no control.

On Myers's model of mind, subliminal processes emerge when consciousness is deflected from its normal, supraliminal functioning: "To some extent at least the abeyance of the supraliminal life must be the liberation of the subliminal" (*HP*, vol. 1, p. 122). More specifically, "it seems as though this supersensory faculty assumed activity in an inverse ratio to the activities of everyday life" (Myers, 1886b, p. 287). Supernormal processes such as telepathy do seem to occur more frequently while either the percipient or the agent (or both) is asleep, in the states between sleeping and waking, in a state of ill health, or dying; and subliminal functioning in general emerges more readily during altered states of consciousness such as hypnosis, hysteria, or even ordinary distraction. Thus, whereas supraliminal functioning usually reflects "the familiar parallelism between bodily and mental states," subliminal mental processes might vary "inversely, rather than directly, with the observable activity of the nervous system or of the conscious mind" (Myers, 1890b, p. 320; 1891d, p. 638). For Myers, therefore, the importance of studying psychological automatisms and other aspects of

24. Because the concept of automatisms is so central to Myers's model of consciousness, and so important to address adequately in any attempt to understand consciousness, we have devoted an entire chapter to this topic (see Chapter 5).

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subliminal functioning derives largely from the light it might shed on mind-body relations.

Myers also proposed that the emergence of subliminal material in automatism may be more likely when one's habitual "paths of externalisation"[25] are in abeyance. As a possible example, he suggested that because the left hemisphere, the seat of verbal capacity, has become the predominant expressive vehicle for cognitive and other intellectual functioning, then subliminal functioning, or automatisms, might more readily emerge when the left hemisphere is damaged, inhibited, or otherwise prevented from functioning fully: "In graphic automatism [automatic writing] the action of the right hemisphere is predominant, because the secondary self can appropriate its energies more readily than those of the left hemisphere, which is more immediately at the service of the waking mind" (Myers, 1885b, p. 43).[26]

This suggestion that the subliminal portions of our spectrum of consciousness might find their "readiest path of externalisation" through the right hemisphere has received modest support from modern observations indicating that right-hemisphere functioning (in right-handers, at least) is for the most part nonverbal (see, e.g., Springer & Deutsch, 1985). Myers had noted that "our subliminal mentation is less closely bound to the faculty of speech than is our supraliminal" (*HP*, vol. 1, p. 98). More specifically, the "language" of subliminal consciousness seems to be primarily pictorial and symbolic, rather than verbal and propositional (e.g., Myers, 1892f, p. 460; 1897, p. 70; *HP*, vol. 1, pp. 100, 277), and he suggested that the "study of visual and motor automatism will afford us sufficient proof that symbolism, at any rate pictorial symbolism, becomes increasingly important as we get at the contents of those hidden [subliminal] strata" (*HP*, vol. 1, p. 100). Thus, he said, art, music, and even poetry (whose "material...is the very language which she would fain transcend") are expressions of this subliminal language (Myers, 1897, p. 70; *HP*, vol. 1, p. 101).

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25. This phrase refers to a common belief in the 19th century (and one that remains with us today; see Chapter 4) that psychological functioning produces physical changes or "traces" in the brain and that the nerve-currents accompanying psychological processes take the "paths" of least resistance, carving out "established" paths that subsequent nervous activity will become more likely to follow (see, e.g., Carpenter, 1874/1882, p. 442; James, 1890b, vol. 1, pp. 108, 563, 659). In contrast, Myers was concerned to emphasize that here, as elsewhere, psychologists and physiologists still have no real understanding of mind-brain correlation. Therefore, to forestall readers who might be tempted to take his terminology of "brain paths" literally, Myers (1889a) explained that he was using the terminology as a metaphor and *not* "a real transcript of the unknown processes which actually occur" (p. 535).

26. In the wake of discoveries of the localization of function in the brain (beginning especially with Broca's localization of a center for spoken language), the concept of hemispheric asymmetry and differences became an important one in the late 19th century (see Harrington, 1987). Myers was one of the earliest to suggest that subliminal phenomena might find their readiest expression through what was considered the non-dominant hemisphere.

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Myers also proposed that variations in the complexity of subliminal functioning or automatisms might be correlated with the amount of time a person has spent, say, doing automatic writing or in an hypnotic state or secondary personality. He noted the observations of Elliotson and Janet about variations in "the amount of personality which the hypnotised subject is able to manifest" (Myers, 1888a, p. 390) and suggested that such differences depend on the stock of memories accumulated in the secondary state. Hypnotize a person once or a few times, and the "little scrap of memory" associated with these states is wholly insufficient "to dignify...with the name of a secondary personality. Repeat the process, however, many hundred times, and at last the time spent in the hypnotic trance, the experience gained therein, will become comparable with the time spent in normal existence, and the experience gained in the common routine of life." For example, "Mme. B. has been so often hypnotised, and during so many years,...that Léontine has by this time acquired a very considerable stock of memories which Mme. B. does not share" (p. 391).

Myers also offered a possible explanation for a frequently noted characteristic of automatisms: the puerile, silly, trivial nature of much of the content, "quite independent of the intellectual level of the automatist" (Myers, 1887a, p. 212). This feature had, perhaps understandably, made automatisms a target for much levity and repugnance,[27] but Myers suggested that automatisms, such as the "much-derided phenomenon of 'table-tilting'," might be early, rudimentary attempts at subliminal communication, just as gestures or sounds of animals are early forms of communication (*HP*, vol. 2, p. 92). Thus, "the interest [of most automatisms]...certainly does not lie in the wisdom of the oracle received" (Myers, 1885a, p. 239). The interest lies instead in the possibility of finding means of gaining greater access to subliminal regions of our consciousness: "If once we can get a spy into the citadel of our own being, his rudest signalling will tell us more than our subtlest inferences from outside of what is being planned and done within" (*HP*, vol. 2, p. 91).

A Law of Mental Causality

Myers fully expected that there are laws of mental causality, or psychological laws in addition to those of the physical world and not derived from these. Moreover, he believed that some such concept as telepathy—the hypothesis that individual minds (or Selves) can, at some now-subliminal level, interact directly with other minds—will be an important element in

27. Huxley (1913), for example, spoke contemptuously of the "twaddle" produced by many spiritist mediums (vol. 2, p. 144). Stevenson (1978), commenting on the "vapid writings" that an automatic writer had attributed to the deceased William James, remarked that survival of death with such "a terrible post-mortem reduction of personal capacities...makes it, at least to me, a rather unattractive prospect" (p. 323).

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the major law or principle of psychology that remains to be discovered. Myers thought that this "law" of psychology would demonstrate in general "the Interpenetration of Worlds," that is, the interaction between the physical world that our senses have evolved to perceive and what he called the "metetherial" world, the larger universe that is beyond our direct sensory perception but that "co-exist[s] with, and manifest[s] itself through, the material universe that we know" (Myers, 1892f, p. 534). The belief that there is a world beyond the known physical one has of course been fundamental in most religions (although many religions, particularly Western ones, have, in contrast to Myers, traditionally seen the physical and the spiritual worlds as discontinuous); but Myers believed that the demonstration of "telepathy...would be the first indication of a possible scientific basis" for this belief (Myers, 1886a, p. lvii).

Myers found an important hint to what the new law of psychology might look like in a concept from mesmerism. Many mesmerists believed that the effects they were able to elicit from their subjects result from a yet unknown physical radiation or force passing from the mesmerist to the subject, creating a connection that they called *rapport*. Myers and Gurney believed that the mesmerists might have been on the right track, but that instead of being a physical phenomenon, the influence might be a psychological one, some kind of "a specialised relation between two minds," a resonant link, or a "subtle inter-communication" between subliminal minds (Myers, 1886a, p. lvii; 1886b, p. 287; *HP*, vol. 1, p. 209). This notion of a psychological link between minds became the basis for Myers's concept of telepathy and, indeed, his concept of all supernormal interaction. The nature of the relation remained entirely unknown, as it had for the mesmerists. Certain people made good mesmerists or hypnotists, but no one understood why. The "rapport" does not seem clearly related "either to kinship or to affection" (1884–1885, p. 100), and there are even some telepathic or other such cases in which the nature of the presumed rapport is particularly puzzling, since the people involved are strangers (1884–1885, p. 122). Myers suggested, however, that in some sense this telepathic or mental "rapport" might be the psychological equivalent of the concepts of molecular attraction (Gurney & Myers, 1884a, pp. 814–815) or of gravitation (*HP*, vol. 1, p. 38) in the physical world; and it remained for psychologists to identify and describe this link more adequately.

Methods for Psychology

Myers's vision for a new psychology included more than a theoretical model of mind that could carry psychology beyond the dichotomy of the old mentalistic psychology and the new materialistic one. He also repeatedly emphasized the need for psychologists to develop their own unique methods, suitable to the particular problems and phenomena of psychology and

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he said, Carpenter's hypothesis of unconscious cerebration to explain subliminal phenomena in general, and Faraday's hypothesis of unconscious muscular action to explain table-tilting in particular, "were, so far as they went, not only legitimate, but the most logical...to explain the scanty evidence with which alone Faraday and Carpenter attempted to deal"; they were not, however, hypotheses applicable to the full range of available and pertinent data (Myers, 1886a, p. lxii). Similarly, Janet's observations of automatic writing had been confined to hysterical patients, and to relatively few even of them, and as a result "a good many passages of M. Janet's...seem to me...lacking in width of purview," although containing "much which I hold to be true and important" (Myers, 1889e, pp. 189, 191).

Myers and his SPR colleagues thought also that the conflict between "mesmerism" and "hypnotism" was probably the result of limited observations on both sides (W. F. Barrett, Gurney, Myers, et al., 1883). Mesmerists believed that the phenomena produced by Mesmer and his successors were the result of an actual physical force or "effluence" passing from the mesmeriser to the subject. The new hypothesis of hypnotism, in contrast, attributed the phenomena to suggestion, or the subject's belief that a certain procedure or cause will lead to a certain result. In Myers's view, both theories were inadequate because advocates on both sides often confined their observations to too narrow a range. In the case of hypnosis in particular, there was often a "confinement of attention to some few of the commoner and more obvious manifestations" (Myers, 1898b, p. 101).

Likewise, he said, the Nancy view of hypnosis as a psychological phenomenon of suggestion had prevailed over the Salpêtrière view of it as primarily a physiological phenomenon, because advocates of the former, such as Bernheim and Liébeault, had experimented with more, and more varied, subjects than had Charcot and his colleagues at the Salpêtrière.[28] Now, however, the Nancy school was itself in danger of becoming trapped in its own brand of dogmatism by "insisting that *all* in hypnotism is suggestion....I must adhere to the view which I have often expressed....Has not the history of hypnotism thus far been a slow but repeated justification of those who, in each successive controversy, took the wider and less exclusive view?" (Myers, 1889e, p. 198). In appealing for a wider gathering of data, he thus urged "a freer communication between opposing schools" in a joint effort to attack the problems involved (Myers, 1892b, p. 326).

Myers similarly cautioned against the too rigid dismissal of hypotheses alternate to one's own. He defended the approach of explicitly maintaining multiple hypotheses as working possibilities, believing that this breadth of view might enable one ultimately to identify some more comprehensive or "tertium quid" principle that encompasses aspects of the competing hypotheses as well as a wider range of data. His attempt to reconcile the unitary and colonial views of mind exemplified how he thought conflicting views could be reconciled within a larger perspective. Maintaining mul-

28. For a discussion of the Salpêtrière and Nancy schools and their differences, see Gauld (1992), especially pp. 306–362.

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tiple hypotheses or interpretations as working possibilities is also important because this encourages a broader range of observations. With regard to mesmerism/hypnotism, for example, people who favored the mesmeric hypothesis usually emphasized quite different kinds of phenomena than did those who favored the suggestion hypothesis.

Myers believed that the study of phenomena and beliefs found among ancient cultures and so-called primitive peoples should also play an important part in expanding psychology. One of his earliest papers was an attempt to suggest some parallels between ancient Greek oracles and divination practices and more recent phenomena such as table-tilting and automatic writing (Myers, 1880/1888). He contended that primitive beliefs and reports were not necessarily invalid superstitions simply because of their origin. At a time when most Westerners regarded non-Westerners as "childish" savages, Myers (1886a) urged the potential importance of comparative ethnology and anthropology to psychology and "hoped that shamans and medicine-men will not vanish before the missionary until they have yielded some fuller lessons to the psycho-physicist [i.e., psychologist]" (p. xlv).

Thus, instead of judging "the worth of ideas by tracing their *origins*," as scientists following in the footsteps of E. B. Tylor, Sir John Lubbock, and Herbert Spencer tended to do, Myers urged that we adopt "a somewhat more searching criterion. Instead of asking in what age a doctrine originated—with the implied assumption that the more recent it is, the better—we can now ask how far it is in accord or discord with a great mass of actual recent evidence" (*HP*, vol. 2, p. 91). Gurney's comprehensive survey of witchcraft literature, for example, had shown that firsthand (*not* secondary) accounts of phenomena attributed to witchcraft bore a remarkable resemblance to modern phenomena of hypnotism and hysteria (Gurney et al., 1886, vol. 2, pp. 116–120, 172–185). This example suggests that, faced with unusual or abnormal phenomena, one is not limited to accepting traditional explanations and beliefs about them in their original form or to rejecting the observations altogether. Invalid or insufficient interpretations may have derived from perfectly valid observations. The important question to ask is "whether hypotheses, now admitted to be erroneous, had ever been based in past times on evidence in any way comparable to that which we have adduced" (Myers, 1886a, p. lxix).

In addition to considering a breadth of phenomena and of explanatory hypotheses, Myers believed it was equally important to maintain a breadth of method. The ultimate goal of any science, he said, is to arrive at an explanation of a phenomenon sufficient to allow one to predict or produce that phenomenon (Myers, 1880/1888, p. 56). All sciences, however, must pass through two prior methodological stages before reaching that advanced stage: "First, [the phenomena] will occur spontaneously. Next, they will be empirically produced. And lastly they will be produced scientifically; produced, that is to say, with real knowledge of the conditions on which they depend." Psychical researchers and others attempting to push psychology beyond the commonplace and toward "real discovery" were, he thought,

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"just entering" the second stage, that is, the early experimental stage "at which we can sometimes set the machinery going, but have no notion how it works." Nevertheless, as long as one remains at this second stage, and has not yet progressed to the third stage of understanding fully how to produce the phenomena, then the observational method must continue in conjunction with the experimental work. It remains "important to take stock, so to say, of the whole range of *spontaneous* phenomena corresponding to the phenomena which we are endeavouring to produce. We shall thus learn how far we are likely to be able to go, and we may get hints as to the quickest line of progress" (Myers, 1892c, p. 333).

In short, the methodological approach for psychology that Myers advocated was above all else a comparative one: comparing observations from widely differing conditions, places, or times; comparing spontaneous phenomena and experimentally produced phenomena; comparing different hypotheses or perspectives. Furthermore, because of his fervent belief in the ultimate continuity of all phenomena, he emphasized the necessity of showing continuity and interrelationship among apparently disparate phenomena. It was particularly important to understand the continuity between normal psychological phenomena and the refractory and rare abnormal and supernormal phenomena of psychology and psychical research, not only to bring the latter out of the realm of superstition and into the realm of science, but also to strengthen science itself by expanding its framework to include, not just some, but all phenomena of human experience. As James summarized this, Myers brought "unlike things thus together by forming series of which the intermediate terms connect the extremes":

> Myers's great principle of research was that in order to understand any one species of fact we ought to have all the species of the same general class of fact before us. So he took a lot of scattered phenomena, some of them recognized as reputable, others outlawed from science, or treated as isolated curiosities; he made series of them, filled in the transitions by delicate hypotheses or analogies, and bound them together in a system by his bold inclusive conception of the Subliminal Self, so that no one can now touch one part of the fabric without finding the rest entangled with it....Through him for the first time, psychologists are in possession of their full material. (James, 1901, p. 16)

Empirical Phenomena for the Study of Mind: An Introduction to *Human Personality*

In addition therefore to outlining a new model of mind and urging the development of methods uniquely suited to psychology, Myers sought to call the attention of psychologists to "their full material" by describing an enormous range of phenomena, both spontaneously occurring and experimentally induced, that he thought not only must be accounted for in any

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adequate theory of human personality but also are essential for stimulating the development of such a theory. The most fully developed and complete form in which the theoretical, methodological, and empirical themes of Myers's work were presented is the massive two-volume *Human Personality and Its Survival of Bodily Death* (1903). Although it was published posthumously, two years after Myers's death, most of it had been finished and was ready for publication at the time of his death, large parts of it having been drawn from or based upon his numerous publications from the 1880s and 1890s.[29] As Myers had requested when he realized that he was seriously ill and might die soon, Richard Hodgson and Alice Johnson served as editors after his death.[30] When the book appeared, it was quickly reviewed in numerous journals and periodicals.[31]

Human Personality consists of 10 chapters and lengthy Appendices that present much of the empirical data and case reports supporting the primary material. In the first chapter Myers introduces his overall purpose and his theory of the Subliminal Self. In Chapters 2 and 3 he provides a more detailed account of the theory by discussing two seemingly different kinds of phenomena that he believed are closely related psychologically, namely, hysteria and genius. In Chapters 4 and 5 he discusses the emergence of sub-

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29. The William James scholar Eugene Taylor (1984, p. 179; 1996, p. 147) made an egregious error when he stated that *Human Personality* was published posthumously by Myers's widow and son because Myers himself had largely abandoned the project. He further seriously misrepresented the facts when he opined that Myers had not completed the book because he had "dallied around" during the 1890s (Taylor, 1996, p. 147). In fact, Myers published nearly 50 papers and reviews in the 11 years before his death, including the important series of nine lengthy papers on the Subliminal Self, published between 1892 and 1895 (see footnote 8 for the references), on which much of *Human Personality* was based. Would that we could all "dally" like this.

30. Although the primary task remaining for the editors was to put the unfinished Chapter 9 and the Appendices in order, at least two major changes occurred that probably deviated sharply from Myers's own wishes. First, as I shall discuss further below, a large body of material that Myers had intended to include in Chapter 9, concerning the medium Mrs. Thompson, was omitted. Secondly, Myers himself had apparently intended a different title for his book than the one that appeared. James (1902/1958, p. 386n) stated that it had already been announced by Longmans, Green, and Company as in press under the title *Human Personality in the Light of Recent Research*—a title that far more accurately reflected Myers's approach than did the title that was ultimately used. The change was apparently made at the last minute by the editors or the publishers and, I suspect, would not have been approved by Myers himself. As I have already mentioned, although the question of survival after death was certainly Myers's central concern, he fully understood that it could be approached adequately only within the much larger context of the nature of consciousness. Unfortunately, the title that was used has probably turned away many scientific readers who would have examined the book if Myers's own title had been used.

31. We have placed the most significant of these contemporary reviews (Flournoy, James, McDougall, Stout) on our digital version of *Human Personality* (see our Introduction, p. xxx). See also Gauld (1968, chap. 12).

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liminal functioning in two altered states of consciousness: sleep and hypnosis. In Chapters 6 through 9 he presents a wide variety of evidence, both spontaneous and experimental, for psychological "automatisms" of subliminal origin. Chapters 6 and 7 deal with sensory automatisms, or "messages which the subliminal self sends up to the supraliminal in sensory form," especially visual or auditory form, "externalised into quasi-percepts" (*HP*, vol. 1, pp. 23, 222). Chapters 8 and 9 deal with motor automatisms, or subliminal impulses or ideas expressed through motor functioning, whether "by movement of limbs or hand or tongue" (*HP*, vol. 1, p. 222). The final chapter is an Epilogue assembled by the editors from some of Myers's more speculative writings. It consists largely of a "Provisional Sketch of a Religious Synthesis" and several appendices in which Myers outlines his hope and belief that science ("that great wedding between Reason and Experience" [*HP*, vol. 2, p. 295]) and religion ("the sane and normal response of the human spirit...to the known phenomena of the universe, regarded as an intelligible whole" [*HP*, vol. 2, p. 284]) will come together so that the methods of science can be directed toward questions that religion alone has thus far asked (*HP*, vol. 2, p. 305).

In the remainder of this chapter, I will briefly sketch, using as an outline the chapter divisions of *Human Personality*, some of the many types of phenomena and lines of research that Myers believed important for a truly comprehensive and instructive science of psychology. This skeletal presentation, however, can provide only a glimpse of the richness, depth, and orderliness of the evidence that Myers marshaled in support of his theory. As Gardner Murphy (1954) cautioned years later,

> The reader who would grasp what Myers is doing must simply keep his fingers in the appendices, often the appendices of both volumes, and indeed sometimes several fingers at once, to trace out the carefully marshalled evidence which is offered by the author at each point to support the generalization which he offers. This is the only way in which the documentary strength and philosophical significance of Myers can be understood. (p. iv)

Chapters 2 and 3: Hysteria and Genius

Myers noted that Breuer and Freud had been puzzled by their seemingly paradoxical observation "that amongst hysterics we find the clearest-minded, the strongest-willed, the fullest of character, the most acutely critical specimens of humanity" (translated and quoted by Myers, 1893a, p. 14;[32] see Breuer & Freud, 1893/1957, p. 13). More generally, the apparent relationship between genius and insanity had long been noted and debated (and still is; see our Chapter 7). In Myers's model of mind, this relationship is to

32. This paper (Myers, 1893a, pp. 12–15) provided the first published account of Freud's work in English (Fuller & Fuller, 1986; E. Jones, 1961, vol. 1, p. 250).

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Similarly, Myers argued that the study of genius can teach us about the structure and evolutionary dynamics of mind, since the same psychological mechanism that produces a narrowing of consciousness in hysterics produces an expansion of consciousness in geniuses (Myers, 1892d; *HP*, chap. 3): Both involve an unusual instability or permeability of the barrier or filter between the subliminal and supraliminal, in one case leading primarily to a "down-draught," in the other to an "uprush." Believing that the evolution of mind involves a general process of "gaining a completer control over innate but latent faculty," Myers defined genius as "an emergence of hidden faculty" (Myers, 1895b, p. 6). In particular, it involves "a power of utilising a wider range than other men can utilise of faculties in some degree innate in all," as well as "a *subliminal uprush*, an emergence into the current of ideas which the man is consciously manipulating of other ideas which he has not consciously originated, but which have shaped themselves beyond his will, in profounder regions of his being" (*HP*, vol. 1, p. 71). As an "uprush," an inspiration of genius is a psychological automatism, a subspecies of subliminal phenomena. What distinguishes the phenomena of genius, however, is that they involve not so much the emergence of new faculties as the intensification of familiar ones (*HP*, vol. 1, p. 96). In the analogy of the spectrum, they "make the bright parts of the habitual spectrum more brilliant," rather than drawing on subliminal faculty "beyond the limits of the ordinary conscious spectrum" (*HP*, vol. 1, p. 78).

Myers's conception of genius was thus quite different from that of Maudsley, Lombroso, and others who considered genius to be indicative of pathology (Myers, 1889e, p. 192; *HP*, vol. 1, p. 71). Unlike them, he believed that geniuses, with their "perceptions of new truths and powers of new action," represent instead the "highest product of the race" (*HP*, vol. 1, pp. 96, 71). Because genius and madness both involve similar psychological mechanisms—namely, a permeability of the psychological boundary—it is to be expected that they might frequently occur in the same person (Myers, 1885d, p. 130; 1892d, p. 355); but any nervous disorders that accompany genius signal, not dissolution, but a "*perturbation which masks evolution*" (*HP*, vol. 1, p. 93).

Genius is customarily associated with an unusually high level of intellectual functioning or extraordinary artistic achievements of a scientist, writer, artist, musician, or dancer. Psychologically speaking, however, any uprush of heightened faculty belongs to the same class: "A man may have a sudden and accurate inspiration of what o'clock it is, in just the same way as Virgil might have an inspiration of the second half of a difficult hexameter" (*HP*, vol. 1, p. 78). A psychological conception of genius, Myers insisted, is entirely different from the aesthetic conception. Whereas from the aesthetic view the important consideration is the perceived quality or value of the product, from the psychological perspective the important consideration

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we had been a populace of hysterics we should have acquiesced in our hysteria. We should have pushed aside as a fantastic enthusiast the fellow sufferer who strove to tell us that this was not all that we were meant to be" (p. 25).

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is the psychological mechanism behind the phenomenon, that is, a sudden uprush from the subliminal that is "*incommensurable*" with ordinary conscious effort (Myers, 1898b, p. 104; *HP*, vol. 1, pp. 75, 99). Two works of art or two different kinds of phenomena may thus be "in the same *psychological* class" without being "in the same *artistic* class" (*HP*, vol. 1, p. 75).

In Chapter 7 we will return to a discussion of the phenomena of genius, including one that Myers thought particularly useful for studying the psychological mechanism involved, namely, arithmetical prodigies. According to Myers, the "calculating boy" is of the same psychological genus as a Shakespeare, although clearly not of "the highest order of art" (Myers, 1898b, p. 104; 1892d, p. 349). Nevertheless, the products of the calculating prodigy, unlike those of the artist, can be judged on purely objective grounds; the answer is either right or wrong. Thus, the study of such persons may provide a relatively objective way to study the otherwise subjective processes of inspirational uprush (Myers, 1892d, pp. 356, 360; *HP*, vol. 1, pp. 78–85).

Chapter 4: Sleep

Myers believed that the study of sleep and dreams should occupy a prominent position in psychological research. In keeping with his view that consciousness has evolved out of a primitive "panaesthesia," Myers described the evolution of consciousness as a process in which, in response to environmental demands, we become "more and more awake." Sleep is thus a reversion to an earlier stage of development. Furthermore, just "as sleep precedes vigilance, so do dreams precede thought" (Myers, 1892e, p. 363); dreams, he thought, represent "the kind of mentation from which our clearer and more coherent states may be supposed to develop" (*HP*, vol. 1, p. 58).

Myers's (1898b) psychological definition of sleep, therefore, was that it is "an alternating phase of our personality" (p. 105) in which the organism reverts to a more primitive state of consciousness for reparative purposes: "It is a fully admitted, although an absolutely unexplained fact, that the regenerative quality of healthy sleep is something *sui generis*, which no completeness of waking quiescence can rival or approach" (*HP*, vol. 1, p. 123). Myers attributed this characteristic feature of sleep to its being a primitive, now subliminal, state of consciousness. Just as in Jackson's hierarchical theory of nervous functioning a lower level takes over when a higher level ceases to function, so in Myers's theory, when waking consciousness ceases, the infrared portion of the spectrum of consciousness, with its "increased control over organic functions at the foundation of life" (*HP*, vol. 1, p. 123), takes over and makes sleep the "regenerative phase of our personality" (*HP*, vol. 1, p. 152; 1898b, p. 105).

For Myers (1892e), therefore, sleep was "no mere abeyance of waking activities, but rather a phase of personality with characteristics definitely

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its own" (p. 365). The most obvious and important of these characteristics are the reparative organic processes; but there are also others indicating a kind of psychological functioning different from that in the supraliminal waking state. Because of the "heightening effect of sleep" in allowing subliminal impressions "to cross the threshold of consciousness," particularly by appearing in dreams (W. F. Barrett, Massey, et al., 1883, p. 140), sleep and dreams can provide an important source of knowledge about subliminal functioning.

First, Myers argued, dreams provide a readily available means of studying the "language" of the subliminal, a language that may underlie other, less common forms of automatism or subliminal processes. Just as sleep is not simply an absence of waking functioning, dreams are not just "echoes or fragments of waking experience, fantastically combined" (Myers, 1892e, p. 365). Dreams are the evolutionary precursor of thought (p. 363), expressed in a form or language that is often symbolic in content rather than literal. So that psychologists might begin to learn this symbolic, primarily nonverbal language, "dreams should be subjected to an analysis far more searching than they have as yet received from any quarter" (pp. 365–366).

The study of sleep and dreams might also provide information about enhanced or even novel psychological processes emerging in subliminal functioning (1892e; *HP*, chap. 4). For example, dreams most commonly, but also hypnagogic and hypnopompic[34] illusions, reveal a latent capacity for internally generated imagery going far beyond the person's ordinary voluntary waking capacity. For many people, in fact, dream imagery is "the highest point" that their visualizing faculty reaches (Myers, 1892e, p. 370). Moreover, in most people dreams display a creative and dramatizing capacity far greater than they normally show (p. 371); and in some dreams cognitive or problem-solving processes seem to have been enhanced, as in cases in which solutions to mathematical or scholarly problems have appeared in dreams (pp. 392–397; *HP*, vol. 1, pp. 134–135, 372–379).

Another cognitive function that can be enhanced in dreams is memory. For example, "we occasionally recover in sleep a memory which has wholly dropped out of waking consciousness," a phenomenon Myers considered common enough so that "no one will raise any doubt about it" (Myers, 1892e, pp. 380–381). More interestingly, however, there are also occasional dreams involving facts of which the person had never supraliminally been aware (pp. 381–392). Such extensions of memory suggested to Myers that, in sleep as well as in other subliminal states of consciousness, memory may be more wide-ranging than is supraliminal, waking memory—even if it is also less focused or controlled than supraliminal memory (*HP*, vol. 1, p. 129). In other words, the study of enhanced memory, or hypermnesia, in dreams—including the memory of events once known but now forgotten, as well as events perceived with the normal senses but never consciously

34. Myers (1892b, pp. 314–315) coined this word to refer to images that may occur as a person is waking up, comparable to the hypnagogic imagery that precedes sleep.

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noted—is of interest in showing that both memory and sensory perception extend beyond our ordinarily noted supraliminal range. Furthermore, Myers believed that the study of memory in dreams will reveal an underlying continuity of memory between the dream state and related conditions such as hypnotic and somnambulistic states, and thus provide further support for his theory of a fundamental unity of human personality behind its multiple and often seemingly disparate manifestations (Myers, 1892e, pp. 378–379; *HP*, vol. 1, pp. 128–134, 370–372).

Myers's model of mind predicts that if sleep is a state of consciousness in which subliminal processes take over from supraliminal ones, then sleep should facilitate subliminal functioning, not only in the organic or "infrared" region, but also in the "ultraviolet" range of the psychological spectrum, such as the emergence of telepathic impressions in dreams. This does, in fact, seem to be the case (W. F. Barrett, Massey, et al., 1883, p. 140; *HP*, vol. 1, pp. 135–150, 379–436).[35] Moreover, Myers (1892e) conjectured that dreams which seem particularly vivid or otherwise impressive to the dreamer—and especially dreams that lead the dreamer to take some action once he or she wakes up—might more often be those which later prove to have been supernormal (pp. 366–367). He thus suggested studying the qualitative intensity of dreams as part of an effort to identify supernormal dreams more readily.

Although dreams, by their very nature, are spontaneously occurring phenomena, Myers nevertheless believed that they can occasionally be brought under some experimental control:

> I have long thought that we are too indolent in regard to our dreams; that we neglect precious occasions of experiment for want of a little resolute direction of the will....[W]e ought to accustom ourselves to look on each dream, not only as a psychological *observation*, but as an observation which may be transformed into an *experiment*. We should constantly represent to ourselves what points we should like to notice and test in dreams; and then when going to sleep we should impress upon our minds that we are going to try an experiment;—that we are going to carry into our dreams enough of our waking self to tell us that they *are* dreams, and to prompt us to psychological inquiry. (Myers, 1887a, p. 241)

What he was proposing was the study of what today we call lucid dreams, a phenomenon now generally acknowledged even though, like many of the phenomena Myers thought important for psychologists to study, it first had to go through a prolonged period of resistance on the part of many scientists (Green, 1968a; LaBerge, 1985; see also our Chapter 6).[36]

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35. Numerous studies of spontaneous supernormal phenomena since Myers's time have shown dreams to be a frequent vehicle for telepathic impressions (e. g., Stevenson, 1970, p. 2) ; and for more recent experimental evidence of the facilitating effects of sleep on psi functioning, see Child (1985) and Ullman and Krippner, with Vaughan (1973).

36. Myers put himself to the task that he proposed, even though he knew that he

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Chapter 5: Hypnotism

From the spontaneous subliminal phenomena associated with sleep, Myers moved on to what he called "that great experimental modification of sleep," hypnotism (*HP*, vol. 1, p. 152). Gurney and Myers had long argued that hypnosis is far more important than a curious anomaly or stage entertainment (Gurney & Myers, 1885, p. 422; *HP*, vol. 1, p. 158). The method that had evolved from Mesmer's original discovery had, in fact, been "the first really intimate, really penetrating method of psychological experiment" (Myers, 1892f, p. 444) and could provide a "corner-stone of a valid experimental psychology" (Gurney & Myers, 1885, p. 422; Myers, 1885c, p. 641n). Its psychological importance lay for them in its potential as "an experimental method of reaching the subliminal self" (Myers, 1891b, p. 83).

As a psychological method, however, hypnosis was in its infancy. It had been used in clinical therapy, but its theoretical implications remained largely unexamined (*HP*, vol. 1, pp. 22–23). As I mentioned earlier, Myers repeatedly emphasized that the usefulness of hypnosis as an experimental method depended on an adequately broad conception of the nature, and hence the phenomena, of hypnosis. Although the terms "mesmerism" and "hypnotism" are sometimes used loosely or interchangeably, the former being considered simply an older term for the latter, there is in fact a distinct difference between the two concepts, and the two schools of thought emphasized not only different interpretations of the phenomena, but even radically different phenomena. One major motivation behind the work of Myers and Gurney on hypnosis was to keep alive both sets of interpretations and observations and arrive at a better, more comprehensive view. They believed that both the mesmeric hypothesis and the hypnosis hypothesis had merit, since both had empirical observations supporting them. On the other hand, they also believed that both remained inadequate. The "effluence"—whatever it is that seemingly emanates from the hypnotist to cause an effect in the subject—is probably not a new physical force, as the mesmerists thought, but a psychological one. Likewise the concept of "suggestion," central to the hypnosis view, is inadequate because suggestion itself is wholly unexplained. They thus cautioned against a too-thorough abandonment of the old mesmeric hypothesis and a too-eager readiness to adopt the suggestion hypothesis as the new dogmatism (see, e.g, Gurney & Myers, 1883).

A point to which Myers returned frequently, however, stemmed from his fear—justified, in light of what actually happened after the turn of the century—that the problem of hypnosis as a theoretical issue in psychology would not be adequately pursued because of the mistaken perception that it has been "explained" in terms of suggestion. Suggestion, it is said, produces effects because in a "suggestible" person it leads to the suspension of

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was both a poor dreamer (Myers, 1887a, p. 241) and a poor visualizer (Myers, 1892e, p. 370). Perhaps predictably, he succeeded on only three nights out of nearly 3,000 on which he tried (Myers, 1887a, p. 241).

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cumstances my subject simply *cannot* see a tiger at will; nor can I affect the visual centres which might enable him to do so" (*HP*, vol. 1, p. 233).[38]

Myers also believed that studying experimentally induced hallucinations is "an important prerequisite" for understanding spontaneous hallucinations (Myers, 1892b, pp. 319–320). For example, he conducted some experiments (Myers, 1892f, pp. 460–461) in which two hypnotized subjects were given different suggestions about what they would see on a blank surface or speculum. They were then brought together and asked to describe what they saw. Each subject described what had been suggested to him, and neither was able to influence the other to see or report anything else. Such experiments clearly are pertinent to the question of collective hallucinations and particularly the hypothesis that the comments or reactions of one person having an hallucination influenced others present to have an hallucination they otherwise would not have had.

Additionally, some hypnotic phenomena appeared to involve hyperesthesia, or the enhancement of the normal five senses. The philosopher Henri Bergson, for example, reported a case of a boy who could, while hypnotized, identify objects reflected in the corneas of the experimenter's eyes (Myers, 1887b; *HP*, vol. 1, pp. 477–479). Recognizing and ruling out such sensory hyperesthesia is, of course, necessary before one invokes an explanation involving psi phenomena such as telepathy or clairvoyance.[39]

In addition to enhanced control over perceptual processes, the phenomena of hypnosis sometimes involve enhancements of cognitive processes. Experiments in post-hypnotic suggestion by Gurney (1887b; 1888), Delbœuf (1892), and Bramwell (1896) showed that some level of subliminal consciousness can conduct complicated arithmetical calculations or keep track of a specific, often lengthy lapse of time (see *HP*, vol. 1, pp. 502–510). Such experiments might contribute, for example, to an understanding of arithmetical prodigies or the claims of some people that they can awaken themselves at pre-determined times (Myers, 1898b, p. 104; H. Sidgwick & Myers, 1892, pp. 605–607).

Finally, from Myers's conception of hypnosis as a means of accessing subliminal strata of consciousness, it follows that phenomena suggestive of supernormal modes of perception, such as telepathy or clairvoyance, would

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38. Modern social psychological theories of hypnosis, which attribute hypnotic effects primarily to the compliant behavior of subjects wishing to fulfill the role of good subjects, still fail to address this fundamental question about *how* subjects comply with effects not ordinarily under their conscious control. This failure in large part is surely because of what Myers (1898b) had lamented as "confinement of attention to some few of the commoner and more obvious manifestations" (p. 101; see also our Chapter 5). I will discuss this problem more fully in Chapter 3.

39. Nonetheless, Myers also warned against carrying such explanations to unreasonable extremes. For example, he noted, someone had proposed that subjects hypnotized at a distance had fallen into trance because they heard "the changed sound accompanying the hypnotiser's quickened circulation" (Myers, 1887b, p. 535).

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also be observed in connection with hypnosis (see, e.g., *HP*, vol. 1, pp. 543–546, 553–559). Indeed, such phenomena had long been reported in the older mesmeric literature (Gauld, 1992); and SPR members conducted experiments with hypnosis in which the hypothesis of some supernormal mode of perception had to be considered (see, e.g., Gurney et al., 1886, chap. 2). In the century since these early experiments, many other studies have supported the prediction from Myers's model that hypnosis can sometimes elicit or enhance supernormal functioning (for reviews, see Honorton & Krippner, 1969; Schechter, 1984; Stanford & Stein, 1994; van de Castle, 1969).

Chapters 6 and 7: Hallucinations—Sensory Automatisms and Phantasms of the Dead

Myers considered research on hallucinations particularly important, because they provide an instructive means of studying the relationship between "subjective," mental, internal perception and "objective," physical, external reality. As Myers noted, even our senses do not provide us with an entirely objective representation of external reality; sensory perception is itself a mental construct that "is in its own way highly symbolic" (*HP*, vol. 1, p. 277). Likewise, hallucinations "further...confound our already doubtful contrast between objective and subjective,...between 'real' and 'unreal' things" (Myers, 1891c, p. 125).

The study of hallucinations, however, had to be carried out in conjunction with attempts to understand the relationship of hallucinations to other modes of perception and imagery (*HP*, vol. 1, pp. 224–231). Myers and Gurney had first attempted to fit hallucinations into a general scheme of perception in 1884 (Gurney & Myers, 1884b, pp. 77–82; see also Gurney, 1885). They argued that visual perceptions and hallucinations are produced by the same neurological sensory apparatus, but in the former the primary stimulus has come from peripheral sensory mechanisms, whereas in the latter the stimulus has come from mental processes directly activating the relevant cortical areas. Hallucinations therefore fall on a continuum with other perceptual processes, including not only normal sight, but after-images, illusions, memory images, and dreams (*HP*, vol. 1, pp. 224–231).

Myers further argued that not all hallucinations are pathological, as most psychologists then (and now) assumed. Many hallucinations are indeed generated by pathological, physiological agents such as drugs, alcohol, or disease, but others represent internal imagery generated spontaneously or by suggestion. One of the most important accomplishments of Myers, Gurney, and their colleagues in psychical research was in demonstrating the previously unsuspected, but as it turns out not infrequent, occurrence of hallucinations in normal, healthy individuals. In contrast to psychologists such as Janet, who believed that hypnotic phenomena such as post-hypnotic hallucinations could be produced only in hysterical subjects (Janet, 1893/1901, p. 277), the psychical researchers were showing that normal individuals could

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in fact be hypnotized and induced to experience vivid hallucinations (e.g., Myers, 1892f, p. 470). In addition, they demonstrated the frequent occurrence of spontaneous hallucinations among normal persons, conducting extensive investigations of hundreds of such cases (e.g., Gurney et al., 1886; E. M. Sidgwick, 1922), as well as two major surveys (Gurney et al., 1886, chap. 8; H. Sidgwick et al., 1894). In both the first survey (which yielded 5,705 replies) and the second (17,000), the results showed that approximately 10% of the persons questioned reported having experienced an hallucination of sight, sound, or touch when awake and in good health. These early estimates of the prevalence of hallucinations among normal persons have proved remarkably accurate (see Bentall, 2000, pp. 94–95, for a review).

These studies and surveys also demonstrated that such hallucinations are not always purely subjective in origin. Some, in fact, are veridical—that is, they involve seeing, hearing, or otherwise sensing some event happening at a physically remote location. For example, many of the experiences reported to Gurney, Myers and their colleagues involved seeing an apparition of someone who was undergoing some kind of crisis (usually death) at about the same time.[40]

In these studies, the researchers dealt with two major issues. First they addressed the all-important question of the reliability of the evidence, and in the problems that they identified and the standards of evidence that they set for their material, they were pioneers in the psychology of eyewitness testimony (Gurney et al., 1886, chap. 4). Second, they asked whether the observed correspondences between hallucinations and crisis events could have occurred by chance. Using their own figures for the frequency with which people report having hallucinations in a waking, healthy state, together with statistics regarding the incidence of death in the United Kingdom, they concluded that hallucinations coinciding with a death happened too frequently to be attributable to chance (Gurney et al., 1886, chap. 8; H. Sidgwick et al., 1894).

Veridical hallucinations are, however, in some sense *both* subjective and objective. Even when the stimulus for the hallucination is external and objective, as seems to be the case with veridical hallucinations corresponding to some real but distant event, the percipient's mind often contributes by modifying that original stimulus in idiosyncratic ways, such that the hallucination may take symbolic, expected, or familiar forms (Gurney & Myers, 1884b, pp. 81–82; see also Tyrrell, 1943/1953). Hallucinations "are not mere crude externalisations....They are in most cases elaborate products—complex images which must have needed intelligence to fashion them" (*HP*, vol.

40. Although no subsequent study of veridical hallucinations has approached those of the early SPR in scope or thoroughness of investigation, such experiences have continued to be reported (see, e.g., Dale, White, & Murphy, 1962; Green, 1960; Stevenson, 1970, 1995; S. H. Wright, 1999). An ongoing study at our research unit in the University of Virginia over the past several years has identified more than 200 cases of dreams, telepathic impressions, or hallucinations occurring at the time of some crisis (usually death) occurring to a person at a distance.

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1, p. 234). To further understand the relative contribution of subjective and objective elements, Myers believed that it is necessary to examine *both* sides of a case—that of the percipient and that of the person undergoing the crisis, including the state of consciousness of each and the emotional relationship between them (W. F. Barrett, Massey, et al., 1883; Myers, 1884–1885). Myers particularly warned against "the error of attributing too much importance to the person who sees the phantom, because his account of the matter is the only one which we can [or do] get" (1886b, p. 301).[41]

Undoubtedly, the cases that raise in particularly acute form the problem of the subjectivity versus objectivity of hallucinations, and the ones that Myers believed to be the most important to study, are collective cases, in which more than one person perceives an apparition simultaneously (see, e.g., Myers, 1886b; 1886d; 1890b; 1898a). Because collective cases suggest some kind of objective stimulus for the hallucinations, they raise "this perplexing problem of the relation of psychical operations to space" (Myers, 1886b, p. 302). Myers proposed the idea that subliminal elements of Person A's mind may be drawn to a particular place, perhaps by some form of psychological "rapport" with one or more people there. These subliminal elements then, in some yet unknown way, modify an actual point in space, not in a material way perceptible to ordinary senses, but nonetheless in some manner sufficient to stimulate perception of Person A at subliminal levels of the percipients' minds. His hypothesis was, in short, a *spatial* one without being a *sensory* one (Myers, 1886b; 1898a, pp. 323–325). It resulted from his attempt to find a more satisfactory view than, on the one hand, the animistic interpretation of apparitions as some kind of objective "ghost" that multiple people present will see and, on the other hand, Gurney's hypothesis that apparitions are subjective hallucinations produced by a telepathic impression, with collective cases involving further telepathic transfer of this impression from a primary percipient to bystanders. There is still no consensus in sight on this complex and theoretically important phenomenon (for a review, see Gauld, 1982, chap. 15).

Like crisis apparitions of persons who may be dying but not yet deceased, apparitions of people that the percipient already knows to be dead have been reported in all times and cultures, and numerous such cases were reported to the SPR. Although most such cases cannot be attributed to anything other than subjectively generated imagery, some cases do suggest a more objective origin, including collective hallucinations of a deceased person; cases in which veridical information unknown to the percipient was conveyed by the apparition; and cases in which the apparition was later recognized by the percipient in a photograph of someone he or she had not known in life (Myers, 1889d; 1890b; *HP*, chap. 7).

Myers also called attention to hallucinations perceived by a seriously ill or dying person, experiences that in recent years have been called near-death experiences and deathbed visions. We have devoted a whole chapter to this topic (Chapter 6) because such experiences seem to us particularly

41. Stevenson (1987, pp. 106–107) has since had to repeat this warning.

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important, suggesting as they do "the persistence...of consciousness under pathological conditions which would seem to negative its possibility"(Myers, 1891c, p. 116).

I have already referred to the importance Myers placed on the study of hypnotically induced hallucinations. Another important method for inducing hallucinations—one that leaves the subject more or less in an ordinary state of consciousness, "undisturbed" by suggestion from another person—is "scrying" (Myers, 1892f, p. 449). Throughout history and across cultures, people have deliberately generated visual and auditory hallucinations with various forms of speculum (or crystal) gazing and "shell-hearing." Although such methods have long been associated with occultism and superstition, Myers believed that they could be usefully adapted and developed as an experimental method in psychology (pp. 458–459, 465).[42]

In early attempts at scrying the content will often be nothing more than "confused reminiscences" or other more or less random imagery (Myers, 1892f, p. 483). In keeping with his idea that the complexity and extent of subliminal functioning correlate with the amount of time spent in the altered state, Myers suggested that, as the scrying is pursued further and develops, the material will become more complex. As with dreams, there may be material once known but now forgotten, or material that had been within one's sensory range but never consciously perceived. Most importantly, but most rarely, the hallucinations may contain information not known normally by the automatist (Myers, 1892b, pp. 318–319; 1892d, p. 348; 1892f; 1899; *HP*, vol. 1, pp. 575–598). Because he believed that complex material, and particularly the latter kinds, will emerge only with sustained attempts, Myers (1896b, 1899) chastised those whose efforts at inducing automatisms had been brief and superficial and who had drawn their conclusions from those limited observations.

Chapters 8, 9, and the Epilogue: Motor Automatisms, Trance, Possession, and Ecstasy

When subliminal material is expressed through motor functioning, Myers classified it as an active, or motor, automatism. For example, if attention is sufficiently diverted from the act of writing, certain persons may produce automatic writing, something more than the simple doodling common to many people, and often attaining "a degree of complexity hitherto little suspected" (Myers, 1885a, p. 248). Because of this complexity, together with the conviction of most automatists that the writing seems not to have originated within themselves, many people believe the phenomenon is produced

42. He was right: The Ganzfeld method now widely used in parapsychology can be viewed as a modern variant of the crystal-gazing and shell-hearing techniques that Myers advocated for psychological research, in that a uniform visual and auditory field is used to heighten internal imagery and focus the subject's attention on it (for some references, see the Appendix).

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305). Whatever one thinks of the personal religious convictions that Myers drew from his work, his general goal of expanding science and psychology to include *all* aspects of human experience, from the most primitive physiological reflexes to the highest manifestations of creativity and mysticism, was one to which, we contend, scientists must return after more than a century of avoiding those "larger questions which the human heart will rightly ask."

Conclusion

Aldous Huxley (1961), comparing *Human Personality* to better-known writings on the "unconscious" by Freud and Jung, said: "How strange and how unfortunate it is that this amazingly rich, profound, and stimulating book should have been neglected in favor of descriptions of human nature less complete and of explanations less adequate to the given facts!" Evaluations of Myers and his work, however, both by his contemporaries and by later psychologists or historians of psychology, have been extraordinarily varied, both in the accuracy with which they have portrayed his ideas and in the conclusions they have drawn about the value of his work. Myers himself recognized that his ideas and theories were far-reaching and at many points possibly premature; but his conjectures and speculations were part of a deliberate attempt to encourage further empirical research: "My excuse for the bold and comprehensive way in which I have set forth [my] hypotheses ...[is that if] there is to be widespread effort there must be widespread interest; and such interest can only be evoked by an understanding of the vast importance of the discovery to which these small and scattered inquiries do manifestly, although remotely, tend" (Myers, 1892f, p. 534).

One of the most cogent evaluations of Myers was that of his friend and colleague William James,[48] upon whom Myers's ideas had a considerable and lasting impact.[49] Myers, he said, had identified psychology's most important

48. James's discussion of Myers's contributions to psychology (James, 1901) and his review of *Human Personality* (James, 1903) are reproduced in our digital version of *HP*.

49. Some James scholars have mistakenly credited James with developing the idea of the subliminal consciousness, upon which Myers then drew. Barzun, for example, claims that James, "with his usual generosity," gave credit to Myers, even though "two years before Myers, he [James] had written an article on 'The Hidden Self'" (Barzun, 1983, p. 230n); and McDermott (1986, p. xviii) echoes Barzun's claim. These writers are clearly unaware that Myers's ideas about a subliminal consciousness long predated the series of nine papers on the Subliminal Self that he began publishing in 1892 (see, e.g., Myers, 1884, 1885a, 1885b). As we will discuss further in Chapter 5, James himself said that the "discovery" of a consciousness "extra-marginal and outside of the primary consciousness" was made in 1886 (James, 1902/1958, p. 188). Although matters of priority are often difficult to sort out, especially since important ideas usually do not spring suddenly out of a void,

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problem: "*The precise constitution of the subliminal*...is the problem which deserves to figure in our science hereafter as the *problem of Myers*" (James, 1901, pp. 17, 18). Moreover, "Myers has not only propounded the problem definitely, he has also invented definite methods for its solution....He is so far the only generalizer of the problem and the only user of all the methods" (p. 17). James similarly appreciated the vast range of psychological phenomena that Myers identified as pertinent to the problem (p. 16).

Additionally, James considered Myers's theory of human personality to be an important one: "It is a vast synthesis, but a coherent one....No one of the dots by which his map is plotted out, no one of the 'corners' required by his triangulation, is purely hypothetical. He offers empirical evidence for the concrete existence of every element which his scheme postulates and works with" (James, 1903, p. 30). To those who found Myers's theory "unsatisfactory," James pointed out that "no regular psychologist has ever tried his hand at the problem....Myers's map is the only scientifically serious investigation that has yet been offered" (p. 33).

James (1903) did express some reservations, to which we will return in Chapter 9, but one particularly worth noting here is:

> Most readers, even those who admire the scheme as a whole, will doubtless shrink from yielding their credence to it unreservedly....The types of case which he uses as stepping-stones are some of them, at present, either in quality or quantity, decidedly weak supports for the weight which the theory would rest upon them, and it remains at least possible that future records may not remedy this frailty. (p. 31)

In the remaining chapters of this book, we will examine many of the "stepping-stones" to which James refers, and we will argue that "future records" *have* to an unappreciated extent remedied their frailty. In the century since Myers's death, many of the observations he made have been powerfully reinforced by subsequent research. Perhaps more importantly, the intervening century of psychological research has reinforced the need for a theory of human personality which—like his—encompasses the full range of human experience.

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it seems clear that the basic outlines of Myers's theory of subliminal consciousness were well in place in the 1880s.

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Chapter 3

Psychophysiological Influence[1]

Emily Williams Kelly

> Phrases about "the influence of the mind on the body" are so often loosely adduced as though they were themselves the explanation needed, that it is as well to keep the real obscurity of the physiological problems in view. (Gurney, 1887a, p. 105)

The naturalization of mind begun in earnest in the 19th century has continued unabated, and the assumption that mind is wholly derivative from brain processes has strengthened and grown more pervasive over the last century. The consensus of nearly all scientists and philosophers today is that all aspects of mind and consciousness are byproducts of an evolving nervous system; and extremists such as the "eliminative materialists" even hold not only that all mental processes and concepts are in principle reducible to brain processes but that any reference to them in language other than that of physiology constitutes mere pre-scientific "folk psychology," to be abolished through further advances in physiology.

This widespread presumption of equivalence between mind and brain is based on the observations, both scientific and everyday, that the evolution of mind is correlated with the evolution of the nervous system and that changes in or injuries to the brain result in changes in or even abolition of consciousness. It is easy to forget, however, that correlation is not causation. As I pointed out in Chapter 2, the assumption that the correlation implies a

1. This chapter has been inspired largely by two great works, both of a remarkable breadth and depth of scholarship: *Reincarnation and Biology*, by Ian Stevenson (1997), and *The Future of the Body*, by Michael Murphy (1992).

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unilateral dependence of consciousness on the brain has been exacerbated and entrenched because observations of the mind-brain relationship have been limited primarily to situations in which a change on the side of the brain is the independent variable and changes on the side of behavior or consciousness the dependent. What happens, however, when we expand our observations to phenomena in which a change in mental state clearly seems to be the initiating cause, and a change in a physiological or physical state the result—phenomena, in brief, relevant to a problem long neglected by psychologists, namely, the problem of volition?

In *Human Personality*, Myers laid out a wide variety of phenomena that had to be addressed by scientific research before the books were closed on the question of the nature of mind-brain correlation. Some of these—previously ignored, denied, or derided by scientists because they resisted a ready physiological explanation and seemed instead to harken back to a pre-scientific, magical way of thinking—are now finding their way back to the mainstream of scientific and medical thinking. This has happened not only because the phenomena continue to be observed, but much more importantly because scientists have begun to identify neurobiological processes that seem to bring them safely within the framework of the prevailing physiological model. But how complete and adequate are such models, even in principle if not yet in actuality, to explain the enormous variety of phenomena in which a mental state seems to have triggered a physiological reaction? As we will see, it is less than fully clear that they succeed even for phenomena that are becoming more widely acceptable. Moreover, many additional phenomena discussed by Myers continue to remain outside the mainstream of scientific and medical thinking, still ignored, denied, or derided, even though observations of these too have continued and, in some instances, grown substantially in number and quality.

There is, in short, a continuum of phenomena suggesting effects of mental state on physiological processes, ranging from those now increasingly accepted by the scientific community and seemingly explainable by physiological mechanisms to phenomena still routinely dismissed by most scientists as outside the explanatory framework of science. In this chapter I take the position that many of the latter phenomena rest on just as firm an empirical basis as the former and that, like the former, they must somehow be brought within the framework of science—science, that is, as a method and not as an ideology—before we can arrive at an adequate understanding of consciousness and volition. I will begin with phenomena of mind-body interaction that have gradually become accepted by scientists, and the theories that have begun to make them more acceptable, and then move further along the continuum to address phenomena that become progressively more difficult to account for within present models of mind and brain. For most of these phenomena there is an extensive amount of biomedical literature, which I will certainly not attempt to review exhaustively. The primary purposes of this chapter, rather, are: first, simply to call attention to the extraordinarily wide range of documented phenomena of psychophysi-

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cal influence which seem to originate in or depend upon a person's beliefs or expectations, however those were generated; and second, to show how the more extreme and unusual of these phenomena challenge physiological models of any conventional sort.

I will begin by briefly describing the revolutionary developments of the last few decades in regard to mind-body medicine and "the faith that heals."[2] It is no exaggeration to say that there has been an explosion of interest in recent years in the question of whether, and when, psychological states or traits affect physical health, promoting both health and disease. Medical journals as well as the popular press have published numerous reports on studies of the relationship between physical health and spirituality, religion, personality, stress, depression, humor, imagery, meditation, and—that quintessential emblem of patient expectation and faith—the placebo. To be sure, many physicians and other scientists still support a statement made 20 years ago by a former editor of the *New England Journal of Medicine*: "The venerable belief that mental state is an important factor in the cause and cure of disease...is largely folklore" (Angell, 1985, pp. 1571–1572). Such resistance may be breaking down, however.

What has led to this increasing breakdown in resistance to, and the explosion of interest in, phenomena suggesting that mental factors contribute importantly to physical health? In this section I will first review developments in psychosomatic medicine and especially the burgeoning field of psychoneuroimmunology (PNI) that have recently made the concept of psychological influence on health more palatable to scientists. I will then briefly review how the findings of PNI have been applied to various phenomena. Finally, I will discuss research on the placebo effect, which illustrates the general shift in scientific opinion about mind and health.

Psychosomatic Medicine

Many physicians and historians of medicine have maintained that Western medicine is deeply tied to Cartesian-style dualism (see, e.g., G. L. Engel, 1977; Lipowski, 1984) and that "from Hippocrates on [it] has tended to be staunchly naturalistic and somatic, or physiologic" (Lipowski, 1984, p. 159). With the growing identification of specific agents of diseases in the 19th century, medicine became increasingly wedded to reductionism and a mechanistic model of disease in which the role of medicine was to repair malfunctioning of the biological machine. It is a mistake, however, to attribute a reductionistic view of disease and "noninteractionist" dualism to Des-

2. Two articles with this title have appeared in medical journals (Frank, 1975; Osler, 1910), and, although separated by 65 years (and even by culture, if the distinct difference in linguistic style can be taken as a measure of that), they both conveyed the same message: the importance in medicine of releasing the patient's own powers of healing.

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breaking down the resistance to the idea that psychological factors can influence the body, because it essentially erases the problem by insisting on the unity, if not identity, of mind and brain. In essence, however, the change to the classical psychosomatic models consisted of recasting the "psychological" part as itself biological, with ideas, beliefs, expectations, and the like to be understood as patterns of neural activity. In consequence, some scientists have begun to speak of this unified psychophysiological entity as "the brain-mind" (e.g., H. Spiegel, 1997, p. 617).

A few authors have cautioned that there has been a too eager abandonment of the original "psychogenic" idea that psychological factors play "an important etiologic role in the production of disease" (Nemiah, 2000, p. 299). Moreover, although the currently prevailing view is that "the mind-body problem...cannot be viewed as the subject matter of psychosomatic medicine" (Lipowski, 1984, p. 168), a minority still think that "as a discipline, psychiatry should be deeply interested in the mind-body problem" (Kendler, 2001, p. 989). It is important to note also that the theoretical assumption of mind-body unity, or holism, is nevertheless itself frequently accompanied by a methodological dualism: "Mind and body may be regarded as abstractions derived for methodologic purposes," and the "most appropriate" position for medicine is "a *methodologic* and *linguistic* approach to the mind-body problem rather than a metaphysical one" (Lipowski, 1984, pp. 168, 161). The editor of the *American Journal of Psychiatry* spoke for many when she suggested that "the relationship between mind and brain has been extensively discussed...without any decisive resolution....One heuristic solution, therefore, is to adopt the position that the mind is the expression of the activity of the brain and that these two are separable for purposes of analysis and discussion but inseparable in actuality" (Andreasen, 1997, p. 1586).

As I discussed in Chapter 2, however, this methodological parallelism not only permits but encourages the evasion of important questions about the "how" of psychophysical interaction. In rejecting dualism and embracing a holistic systems model, "we have been evading the question of the 'how' of physical symptom formation, and so far extremely limited attention has been given to the matter of transition from a purely mental concept, such as consciousness, to very specific somatic alterations" (Sheikh, Kunzendorf, & Sheikh, 1996, p. 153). Moreover, most contemporary scientists go much further and believe that this assumption of mind-body inseparability is not simply a "heuristic solution" but an established fact: "We *know* that mind and brain are inseparable....Mental phenomena arise from the brain" (Gabbard, 2000, p. 117).

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Psychoneuroimmunology

Resistance to the idea that mental factors can influence physical states has primarily been rooted in the lack of any theory to explain the interaction: "Physicians and scientists until recently dismissed such ideas as nonsense, because there did not appear to be a plausible biological mechanism to explain the link" (E. M. Sternberg, 2001, p. 16). The expression of an anti-dualistic, holistic approach to mind and body in the biopsychosocial model laid the groundwork, but the most important impetus to the readmittance of the idea that mental factors influence the body has come from a burgeoning field that seems to many scientists to provide a plausible biological mechanism, namely, psychoneuroimmunology (PNI). This field had its roots in the work of the physiologist Walter Cannon, the physician Hans Selye, and others, who showed that the body maintains its proper state of functioning by a self-regulating internal process (called by Cannon "homeostasis") and that stress is an important factor in upsetting the normal balance because, reflecting the body's reaction to environmental changes, it has widespread biochemical and neurophysiological effects. Solomon and Moos (1964) extended this picture by hypothesizing that stress could be immunosuppressive and in this way could influence health, but there was much resistance to this idea because it was then widely assumed that the immune system is autonomous and beyond the reach of influence by the central nervous system (see, e.g., Solomon, 1993; E. M. Sternberg, 2001).

Although evidence continued to accumulate, especially in psychosomatic medicine, for the influence of psychological factors on disease and health, and although it seems intuitive that "the two great systems that relate the organism to the outside world [that is, the nervous system and the immune system] 'ought' to talk to each other" (Solomon, 1993, p. 357), it took an experiment by Ader and Cohen (1975) to demonstrate such interaction conclusively. Using a classical conditioning paradigm, Ader and Cohen showed that the immune system of rats could be suppressed by exposing them first to an immunosuppressive drug coupled with saccharine, and then to the saccharine alone. Once the immune system had been shown to be responsive to conditioning of the central nervous system, this finding could be extended to account for the interaction between the immune system and central nervous system activity in response to stress.

PNI seeks to delineate physiological and functional connections between the brain and the immune system, and as such has been dubbed "the field of mind-body communication" (E. M. Sternberg, 2001, p. xi). The literature of PNI is now vast, but the key point for purposes of this chapter is that "even the greatest skeptic must now admit that a wealth of evidence exists to prove in the most stringent scientific terms that the functions of the mind do influence the health of the body....This level of proof [provided by PNI] of the myriad connections between the brain and the immune system was needed" (E. M. Sternberg, 2001, p. xvi). In particular, "by understanding these [mind-body] connections in modern terms, in the language of mol-

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Bereavement and Mortality

One of the most poignant examples of the relationship between negative emotions and disease comes from the numerous studies showing increased mortality after bereavement. As Jacobs and Ostfeld (1977) pointed out, the study of conjugal bereavement is particularly useful for examining the relationship between stress and illness, both because of its specificity and because of its severity. Jacobs and Ostfeld reviewed eight studies that had been conducted up to that time, all showing an increased risk of mortality during the first two years after bereavement, and especially among men during the first six months. McAvoy (1986) and J. R. Williams (2005) provide more recent reviews. In addition, Williams provides a brief review of studies showing both an increased incidence of depression among the bereaved and an association between depression and increased mortality from cardiovascular disease. In a large prospective study of nearly 100,000 widows and widowers, Kaprio, Koskenvuo, and Rita (1987) found a significantly higher rate of mortality immediately after bereavement, even as early as the first week.

Although most of the studies of the relationship between bereavement, depression, and mortality are epidemiological studies, some studies have attempted to pinpoint physiological mechanisms underlying the increased mortality. Bartrop, Lazarus, Luckhurst, Kiloh, and Penny (1977) measured lymphocyte function in 26 bereaved spouses, both two weeks and six weeks after their spouse's death. Their findings of depressed T-cell functioning showed "for the first time...a measurable abnormality in immune function" resulting from severe psychological stress (p. 834). Schleifer, Keller, Camerino, Thornton, and Stein (1983) replicated and extended this study by measuring lymphocyte stimulation responses in the spouses of 15 women with advanced breast cancer, measuring the responses several times before the spouse's death, as well as two months and one year after the death. The responses were significantly weaker after bereavement than before, "demonstrat[ing] that suppression of mitogen-induced lymphocyte stimulation in widowers is a direct consequence of the bereavement event" and not "a preexisting suppressed hormone state" (p. 376).

Sudden and "Voodoo" Death

Another example of the association between depression, bereavement, cardiovascular disease, and increased risk of death is the phenomenon of sudden death. Engel (e.g., 1966, 1968) wrote extensively on the relationship between stress, hopelessness, and mortality, which he called the "giving up-given up" complex, a phenomenon that had been demonstrated earlier in experimental studies of rats exposed to conditions of danger accompanied by hopelessness (Richter, 1957; see also G. L. Engel, 1978). In particular, Engel (1971) discussed numerous examples of people who have died sud-

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denly, usually from cardiac arrest, shortly after receiving a sudden shock, such as news of a death or other serious loss, a sudden fright, or, occasionally, at a time of unusual joy. Although Engel's cases were ones he learned about anecdotally, either from media reports or from medical colleagues, a systematic study of 26 men who died suddenly found that both depression and an event causing acute anger or other emotion immediately preceded the death (W. A. Greene, Goldstein, & Moss, 1972); and a study of 100 men under the age of 70 who died suddenly found that the vast majority of them had been under unusual stress, within 30 minutes, 24 hours, or six months before the death (A. Myers & Dewar, 1975, p. 1137).

I mentioned above that some sudden deaths have occurred at a time of fear. Among the most well known of these cases of being "scared to death" are those commonly labeled "voodoo death," but also "hex death," "bone-pointing," or death by sorcery. In such cases, a person who has been cursed or otherwise led by another, usually authoritative, person to believe that he or she is going to die at a particular time does in fact die. The phenomenon has periodically been discussed in the Western medical and anthropological literature ever since physiologist Walter Cannon's 1942 paper describing some cases reported by anthropologists and physicians and proposing a physiological mechanism for them. It is often assumed that such cases are found primarily in preliterate societies. Despite the many difficulties in penetrating preliterate cultures by Western investigators and in obtaining adequate medical documentation in connection with suspected voodoo deaths, medical observers have continued to report similar observations. A. A. Watson (1973), for example, reported witnessing 9 or 10 such cases (in about four years) while he was medical officer at a small mission hospital in Zaire. One of them involved a native nurse who, now a Christian, had been "outspoken on the foolishness of accepting the belief in the death curse," but who nonetheless died within three days of learning that he himself had been cursed (p. 194). Even in the United States, belief in voodoo death, and associated cases, persist among particular sociocultural groups such as African-Americans (Golden, 1977, 1982; Tinling, 1967).

Such cases, however, are by no means limited to preliterate or "folk" societies; they also occur in modern Western cultures. Although superficially different, the general phenomenology of Western cases parallels that found in aboriginal or preliterate cultures. The belief that one is going to die may be generated, not by a witch doctor's curse, but by more culturally congruent phenomena such as a fortune teller's prediction (Barker, 1968), a doctor's pronouncement of a hopeless condition (Milton, 1973), or some other suggestion accepted by the patient. Both Myers (1895e, pp. 528–529) and Tuke (1884, pp. 112–113) described cases in which a prediction of death seemed to have contributed to the person's sudden death. More recently, Boitnott, Friesinger, and Slavin (1967) reported a case in which a midwife who had delivered three babies on the same day had predicted that the first would die before her 16th birthday (she did so, in an automobile accident), the second would die before her 21st birthday (she died on her 21st birthday),

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and the third (the subject of the report) would die before her 23rd birthday. This woman was admitted to the hospital, anxious, "terrified," and convinced that she was "doomed" because of the prediction; and she died there two weeks later, the day before her 23rd birthday, apparently of pulmonary hypertension.

Walters (1944) described a case of a woman who, because of a complex family situation, believed that she would die at the same age (42) as her mother. As Walters described the events: "Her last two weeks were marked by extreme excitement and fearfulness....She lapsed into a coma on the anniversary of her mother's death and died the day after, in the seventh month of her forty-second year....It is probable that the cause of death was renal failure brought about by acute emotionalism" (p. 84).

Another Western case more closely resembles voodoo death. Mathis (1964) reported the case of a man who first developed asthma at the age of 53 and died nine months later of an asthmatic attack. A closer examination of his history revealed that he suffered his first attack two days after his mother had cursed him for going against her wishes, saying "something dire will happen to you" as a result. His attacks all seemed to be precipitated by similar encounters with his mother. On the day of his death, at a 5:00 p.m. interview with a physician, "he was in excellent physical and mental condition." At 5:30 p.m. he had a telephone conversation with his mother, in which she repeated her warning. At 6:35 p.m. he was found semicomatose, and at 6:55 p.m. he was pronounced dead.

A letter to the editor of the *British Medical Journal* in 1965 (Elkington, Steele, & Yun, 1965) described the case of a 43-year-old woman who died following a minor operation. Years earlier she had been told by a fortune teller that she would die at 43, and before her operation she told both her sister and a nurse that she would not survive the operation. This report precipitated numerous additional letters to the editor, in many of which physicians reported their own observations of similar cases (e.g., Barker, 1965; Ellis, 1965; Hunter, 1965; Nelson, 1965; Nixon, 1965; P. J. W. Young, 1965). Barker (1965, 1966) appealed in two major medical journals for other cases of "auto-suggestion," particularly those generated by remarks of fortune tellers, and he subsequently published a short book on this and related phenomena (Barker, 1968).

A corollary of the belief in voodoo death is the belief that the curse can be overridden by the counter measures of a more powerful figure. Kirkpatrick (1981) described the case of a 28-year-old Philippine-American woman diagnosed with and treated for systemic lupus erythematosus. Although the treatments were at first successful, when her illness recurred she refused further treatment and returned to the Philippines, where her village's witch doctor "removed the curse placed on her by a previous suitor." She returned three weeks later, apparently cured, and continued in good health with no further conventional treatment for at least two years, when she gave birth to a child. Golden (1982) similarly described the case of an American man, admitted to a Veterans Administration hospital, whose wife had put a "spell"

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Possible Mechanisms Behind Psychological Factors in Mortality

Noting that before the 20th century physicians routinely took into account the influence of mental state on the health of patients, Engel (1968) asked: "How is it that such insights could have vanished so completely from medical writings for so long?" (p. 363). In a later paper he further commented that "consideration of the relationship between emotion and sudden death has virtually disappeared from the medical literature, or at best the idea is greeted with scepticism if not incredulity or downright ridicule" (G. L. Engel, 1971, p. 772). Since Engel made those remarks, and perhaps to a great extent because of him, the relationship between stress and illness or death has become a major focus of research, the attitude shifting from incredulity to a search for the underlying physiological mechanisms.[5] As I mentioned earlier, that search has been prompted primarily by the findings of PNI that stress and associated strong emotions produce physiological effects which can precipitate disease. Phenomena such as voodoo death have thus begun to be taken more seriously precisely because of the growing belief that "such deaths may be explained in physicalistic terms" (Lachman, 1982–1983, p. 347).

Particularly important to this change in attitude has been research showing that the activation of the hypothalamic-pituitary-adrenal (HPA) axis by stress leads to the release of corticosteroids, which have profound effects on the immune system. Explanations along these lines have been proposed in particular for the mechanism by which stress and emotion can lead to sudden death. Cannon (1942) himself suggested that voodoo death was the result of "the persistent excessive activity of the sympathico-adrenal system," precipitated by extreme fear and unrelieved by any action on the part of the victim, who believes that he can do nothing to prevent his death.[6] In an editorial on the 60th anniversary of the publication of Cannon's paper, E. M. Sternberg (2002) credited his work with "form[ing] the basis of much of our modern understanding of the physiological response systems

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5. See, for example, an extensive review of studies showing a relationship between psychological factors and cardiovascular disease, as well as a discussion of some physiological mechanisms that may be behind the relationship (Rozanski, Blumenthal, & Kaplan, 1999).

6. Similarly, Fry (1965) and, more recently, Wittstein et al. (2005) have suggested that increased activity of the sympathetic nervous system produced by stress can lead to sudden death. In contrast, Spieker et al. (2002) thought that "a contribution of the sympathetic nervous system to the derangement of vascular function after mental stress could be excluded" (p. 2819); and Richter (1957) suggested that the deaths of the rats in his study seemed to involve overactivity of the parasympathetic nervous system. Other researchers (G. L. Engel, 1971, 1978; Lex, 1974) have proposed that *both* the sympathetic and the parasympathetic systems—or, in more operational terms, the "flight-or-fight" and "conservation-withdrawal" reactions of the biological defense system—are involved. Such debates, however, concern only the detailed mechanisms involved, and not the basic premise that stress has profound effects on the body.

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involved in linking emotions, such as fear, with illness" (p. 1564). Moreover, she concluded that "most of Cannon's proposed explanations" have been upheld by subsequent research on the role of the massive release of stress hormones and other neurochemicals in causing disease. The picture remains complicated, because research on the relationship between depression and specific immune system measures and diseases (such as HIV) has often produced inconsistent results (M. Stein, Miller, & Trustman, 1991). Nonetheless, increased understanding of the mechanisms seems likely to come from advances in basic PNI research. Meanwhile, perhaps the best lesson to take away from all these phenomena is that "unless you have something to live for you die before your time" ("Pertinax," 1965, p. 876).

Mind and Health

The relationship between psychological factors and health is not limited to the role of mental states in the etiology or prognosis of disease. If negative emotions can contribute to disease and even death, positive ones ought conversely to contribute to improvements in health or even healing. Cousins (1976) was instrumental in bringing widespread attention to this idea. Confronted with a serious illness with little hope of full recovery, he recalled Selye's (1956) demonstration that stress and negative emotions can lead to illness, and he wondered whether the opposite might not also be true. As a result, he undertook a regime that consisted, among other things, of improving his psychological state by daily doses of humor and laughter. He recovered, and concluded that "the will to live is not a theoretical abstraction but a physiologic reality" (p. 1462). Since then he has supported research on the relationship between positive emotions and health, including the study of laughter and its impact on stress hormones and other physiological measures (Berk et al., 1989). More generally, numerous studies have shown the efficacy of interventions such as relaxation training, meditation, imagery, biofeedback, and hypnosis in alleviating pain and perhaps also in improving conditions associated with cancer, cardiovascular disease, and surgery (for a review, see Astin, Shapiro, Eisenberg, & Forys, 2003).

Postponement of Death

One example of the "will to live" described by Cousins is that some people seem to have postponed their death until after some meaningful occasion, such as the arrival of a loved one or a significant day (Callanan & Kelley, 1993). I mentioned above Meador's (1992) patient, who announced his desire to live through Christmas, and then died shortly afterward. A more famous example is that of Thomas Jefferson and John Adams, both of whom died on July 4, 1826, exactly 50 years after the Declaration of

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inconsistent and often methodologically weak. Moreover, they regard the introduction of spirituality and religion into medical practice as ethically suspect. As a result of both problems, they argued that "suggestions that religious activity will promote health, [and] that illness is the result of insufficient faith, are unwarranted" (p. 667). Nearly all reviewers on this subject have acknowledged that most research to date is based on epidemiological data or on prospective cohort studies, and not on randomized clinical trials where contributing factors can be more carefully controlled (e.g.,, Koenig et al., 2001, p. 382). Most researchers, however, have considered this weakness as one to be remedied in future research, and not as a reason for dismissing what has already been done (e.g., McCullough, Larson, Hoyt, Koenig, & Thoresen, 2000, p. 220; L. H. Powell et al., 2003, p. 50).

Moreover, recent statistical and methodological reviews have shown that there have in fact been studies methodologically sufficient to warrant further research. In a series of papers in the *American Psychologist* (W. R. Miller & Thoresen, 2003; L. H. Powell et al., 2003; Seeman, Dubin, & Seeman, 2003), the authors used a levels-of-evidence approach in which they judged and ranked studies in terms of methodological soundness. The strongest finding was of "a strong, consistent, prospective, and often graded reduction in risk of mortality in church/service attendees" (L. H. Powell et al., 2003, p. 36). Consistent with this finding is a study that used a meta-analytic, rather than levels-of-evidence, approach (McCullough et al., 2000). In 42 independent studies based on samples of almost 126,000 people, the meta-analysis also revealed a significant correlation between religious involvement and reduction of mortality from a variety of causes.

Meditation and Healing

Another strong finding from the analyses in the *American Psychologist* series concerns the relationship between meditative practices and better health (Seeman et al., 2003). I will return to the topic of meditation below in regard to the apparent ability of yogis and other contemplatives to control autonomic functions; but here I will mention just briefly a few additional meditation studies not covered by Seeman et al. G. R. Smith, McKenzie, Marmer, and Steele (1985) described the case of "an experienced meditator" with a hypersensitivity to a viral antigen. Apparently by a combination of her usual daily meditation and a process of visualization, she "could voluntarily modulate her immune responses by a psychic mechanism....the subject, acting with intention, was able to affect not only her skin test response but also the response of her lymphocytes studied in the laboratory" (p. 2111).

Meares (e.g., 1977, 1979, 1980) has described his work with advanced or terminal cancer patients who undertook, under his training and supervision, an intensive program of meditation. Of 73 patients thus treated, nearly all received relief from pain and anxiety. In about 10% of the patients the

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growth of the tumor was slowed, and another 10% also "far outlived" the original prognosis of their oncologists (Meares, 1980, p. 323). More dramatically, five had a complete regression "in the absence of any organic treatment which could possibly account for it," and five more seemed "well on their way" to a similar regression. Meares (1981, 1983) proposed a mechanism for the remissions: On the assumption that cancer is related to a failure of the immune system, he proposed that meditation, by lowering the patient's anxiety and feelings of stress, also lowers cortisone production, adrenaline levels, and the activity of the sympathetic nervous system, thus boosting the functioning of the immune system.[7]

Meares suggested that a similar process might explain some cases of spontaneous regression of cancer (Meares, 1977, p. 133). There have been several reviews of this phenomenon (see in particular Challis & Stam, 1990; O'Regan & Hirshberg, 1993). All defined spontaneous regression as the partial or complete disappearance of a malignancy in the absence of any treatment, or with treatment generally considered inadequate to bring about the observed results (Challis & Stam, 1990, p. 545). Even with this restrictive definition, it is clear that hundreds of such remissions have been reported in the medical literature, and furthermore that the usual "explanation" in terms of mistaken diagnosis is wholly inadequate. Unfortunately, however, practically none of the reports include a description of the psychological conditions surrounding the remission (one exception is Ikemi, Nakagawa, Nakagawa, & Sugita, 1975, but their report was limited to five cases). Any attempt to evaluate either Meares's hypothesis or any other more comprehensive, "biopsychosocial" view of remissions must await more detailed reports.

Faith Healing

The phenomenon known as faith healing, spiritual healing, and a variety of other names has been defined as "any purely mental effort undertaken by one person with the intention to improve physical or emotional well-being in another" (Targ, 1997, p. 74). The medical community has occasionally responded to the continuing reliance among the general public on such healing by examining the history and claims for alternative forms of healing (see, e.g., a section of the 1910 *British Medical Journal* devoted to this topic). In 1893 Myers and his brother A. T. Myers, a physician, published a paper in response to the then widespread interest in apparent cures of diseases

7. Another contributing factor may have been Meares himself. In line with my earlier discussion of the role of authoritative figures in creating or removing a "curse" of death, Meares's own apparent conviction of the efficacy of meditation in healing, together with his insistence on a particular practice—even ritual—that was not to be deviated from, may have profoundly influenced his patients' beliefs and expectations. A similar factor seems to be involved in placebo/nocebo effects, which I discuss later in this chapter.

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with mesmerism, at the shrine at Lourdes, and by "mind-healers." Although deploring the too frequent lack of firsthand or medical testimony in such cases, they concluded that the *occurrence* of some such cures was "certain....Cures are and always have been effected by other than demonstrably physiological means" (Myers & Myers, 1893, p. 164). What was still very much unresolved was the *explanation* of the cures. In their view there was at that time no adequate evidence for any mechanism beyond the activation of the person's "own inward forces" by self-suggestion (p. 207). Nevertheless, even if healing is the result of self-suggestion, the mechanism—be it faith in mesmerism, in Lourdes, in the healer, or in something else—is not to be dismissed as insignificant. In the first place, the mechanism behind self-suggestion has shown "the power of evoking the imagination to a degree and in a manner in which nothing else has ever evoked it" (Gurney & Myers, 1885, p. 406). Also, it remains unknown how the "imagination" or faith can activate these self-healing forces—themselves yet another unknown.

Now once again, with the recent burgeoning of alternative and complementary medicine in this country and in Europe, the scientific and medical community has turned its attention to evaluating seriously some of these claims. In the United States, the Office of Alternative Medicine (now the National Center for Alternative and Complementary Medicine), at the National Institutes of Health, was founded in 1992, and its budget grew from $2 million in 1992 to $123.1 million in 2005. Among the many topics of interest, especially among the general public, has been faith or spiritual healing. Such healing includes not only that occurring under the auspices of a particular religion, but also healing by practitioners who profess no faith except in their ability to heal by transferring some power or energy to the patient, usually to correct some imbalance thought to be causing the disease and to activate the patient's self-healing capacities (R. D. Hodges & Scofield, 1995, p. 205). I will discuss the phenomenon of distant healing and prayer under double-blind conditions later in the chapter, but here I limit the discussion primarily to healing in a context in which patients know they are being treated by a healer and hence in which self-suggestion seems likely to be a factor.[8]

One example of healing apparently through some form of faith on the part of the patient is that of John Fagan, which received widespread publicity. A physician knowledgeable about this case published a report concerning its medical aspects (Curran, 1976). Diagnosed with and operated on for invasive cancer of the stomach in the spring of 1965, Fagan slowly deteriorated for the next two years. By 1967 he had deteriorated to the point that during the weekend of March 4th–5th his doctor said that his death was imminent. On March 6th, however, he roused, asked to eat, and from

8. I emphasize here that self-suggestion is likely to be *a* factor, perhaps even the most important one; but it is important to keep in mind that the source of the effect is not always clear. As I will discuss later in the chapter, there is also strong evidence supporting the idea that one person can deliberately influence the physiology of another person.

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died," her almost instantaneous recovery after the praying, from whatever condition she was in, is at least noteworthy.[9]

In the seventh case reported by Gardner, attributing the recovery to the patient's "faith" or to self-suggestion again becomes more problematic. This case involved an infant in England diagnosed with advanced fibrosing alveolitis, for which the prognosis "is *almost* uniformly fatal" in such a young child (p. 1928). He failed to respond to conventional treatment and after three months in the hospital was discharged home with a "hopeless" prognosis, with "maintenance" medication only. After being taken to a local prayer service—at the suggestion of his physician—he began a rapid and ultimately complete recovery.

Gardner compared these contemporary cases, in most of which there is "no doubt as to the accuracy of the diagnosis or clinical details" (p. 1930), with similar cases reported by the historian Bede in the 7th century; and he concluded from the similarities that older cases lacking in adequate medical documentation are not necessarily to be dismissed on that account alone. Moreover, one might add, cases with clear medical documentation cannot be ignored or dismissed as "anecdotes" or on the grounds that the reporters had, like Bede, a Christian orientation and, presumably, interpretation of the healings. Another physician has published reports of "miraculous" healings in a strongly Christian context (Casdorph, 1976). Casdorph described 10 cases of rapid and complete healing of serious and longstanding illnesses, including rheumatoid arthritis, multiple sclerosis, various kinds of cancer (bone, brain, and kidney), and other debilitating and life-threatening diseases that had not been, or could not be, cured by conventional medical treatment. All occurred during the 1970s either during or shortly after the patient attended a large public service conducted by the well-known healer Kathryn Kuhlman; and all are extensively documented with medical records studied by Dr. Casdorph, as well as by testimony obtained from the patients and the physicians involved.

Although Gardner's report was published in the *British Medical Journal*, most medical journals have refused to publish studies on faith healing (Benor, 1990, p. 9).[10] As a result, almost all of the research studies, as well as reviews of them, have been published in relatively obscure specialty journals, such as parapsychology journals (which are, however, peer-reviewed) or ones devoted to alternative and complementary medicine (e.g., Abbot, 2000; Benor, 1990; Schouten, 1993a, 1993b; Solfvin, 1984).

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9. Interestingly, although this case occurred in rural Thailand in 1963, the woman also described an experience that seems much like the near-death experiences that have been widely reported in the West in recent decades (see our Chapter 6): She said that "she had met Christ, had seen into heaven, but was told she must go back and report what she had seen" (Gardner, 1983, p. 1932).

10. Casdorph has published research papers on other topics in prestigious medical journals, but his reports of unusual healings were published in a popular book. One wonders whether he tried, without success, to publish these reports in professional journals.

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Most reviewers have covered a highly heterogeneous group of studies, including studies conducted on nonhuman targets, on human subjects aware of the intended intervention, and on human subjects kept blind as to whether or not they were being treated. Abbot (2000), for example, reviewed 22 studies involving randomized clinical trials, which were almost evenly split between distant healing studies and studies involving contact between the healer and the patient. The results of the studies were also almost evenly split, with 10 showing a significant positive effect (five of them involving distant-healing and five direct contact). Moreover, there did not seem to be a relationship between the methodological quality of the study and the results. Because of these mixed results, and because of the "significant heterogeneity" in the studies with regard to healing method used, medical condition treated, outcome measure, and control intervention, Abbot decided—as have most other reviewers and researchers—that no firm conclusion can yet be drawn, but that there has been enough positive evidence from studies of good quality to warrant further and better research on faith healing.[11]

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11. I mentioned earlier that few medical journals would publish results of faith or spiritual healing studies, regardless of their quality, until recent years. When the prestigious *JAMA* (*Journal of the American Medical Association*) finally broke this ban in its pages, it did so in an unusual and revealing way. Specifically, in 1998 the editors of *JAMA* published a science-fair project of a 9-year-old 4th-grader who herself had designed and carried out an experiment to test the practice of Therapeutic Touch (TT), and in particular "whether TT practitioners can actually perceive a 'human energy field'" (Rosa, Rosa, Sarner, & Barrett, 1998, p. 1005). Finding that the "practitioners were unable to detect the investigator's 'energy fields,'" the authors concluded that this study provided "unrefuted [sic] evidence that the claims of TT are groundless and that further professional use is unjustified" (p. 1005).

However, as 12 letters to the editor about this paper unanimously pointed out, such a sweeping conclusion was premature, biased, and irresponsible. One clinician summed up the study as "simpleminded, methodologically flawed, and irrelevant" (Freinkel, 1998, p. 1905). Several others described some of its serious methodological flaws. Others recognized that the authors failed to make the important distinction between the efficacy of the method and the theoretical underpinning proposed by practitioners: "The definitive test of a healing practice is whether healing takes place, not whether the practitioners have a flawless grasp of the natural forces at work" (Lee, 1998, p. 1905). It is remarkable—but unfortunately not uncommon—that the editors of this major journal would publish a paper that sweepingly dismisses a whole complex and controversial phenomenon solely on the basis of one small and highly flawed study—especially since they have not, to my knowledge, published a review or research paper with more moderate and reliable conclusions. One can only conclude that this affair reflects a deep-seated bias on the part of the editors, where "one would expect medical professionals to be more concerned with whether real healing takes place" (Lee, 1998, p. 1906). It is not difficult to guess what would have been the fate of a paper submitted by similar authors, with similarly flawed methodology and conclusions, if the results had been positive and not in accord with editorial bias. For more responsible reviews of TT, see Astin, Harkness, and Ernst (2000), Peters (1999), and Wardell and Weymouth (2004).

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occurring recovery in patients "whose recuperative powers seemed suddenly to reassert themselves" (p. 13).[12]

West was quite right to point out that what occurs at Lourdes is not unique, in that similar cases can be found in other contexts. Nonetheless, was he right in saying that there is a "rational physiological basis" (p. 119) for the Lourdes cures and that they are thus "readily explainable"? Even if a disease is "functional" and not "organic"—a distinction that West himself emphasized is not one that can be clearly made—do we understand what these "recuperative powers" are or how and when they work? Do we really understand the physiological basis of rapid healing by hypnosis or any other means of suggestion? Such questions are fundamental, not only in connection with Lourdes cases, but in connection with all the phenomena that I am discussing in this chapter, including the effects of self-suggestion on physiological conditions.

Since 1954, moreover—and therefore after the occurrence of the cases examined by West—the medical evaluation at Lourdes has been extended such that medical dossiers on cases that seem worthy of further follow-up have been passed on to an international body of physicians. This committee examines all available medical documentation to determine whether a correct diagnosis was made; whether the disease was both organic and serious; whether there was a possibility that the disease disappeared spontaneously or in response to medical treatment; whether the symptoms disappeared; how sudden and complete the cure was; and how long the cure persisted. Out of 38 dossiers sent to this committee between 1954 and 1984, 19 have been judged "medically and scientifically inexplicable" (Dowling, 1984, p. 637).[13]

Much of the literature on Lourdes is in French. There are, however, several good sources in English published after West's book, and also after the 1954 establishment of the international body of medical evaluators (e.g., Dowling, 1984; Garner, 1974; M. Murphy, 1992, pp. 267–271). These report several cases not discussed by West, including one older case (the 1923 case of John Traynor) and three more recent ones (those of Vittorio Micheli [1963], Serge Perrin [1970], and Delizia Cirolli [1976]), in all four of which there was extensive medical documentation. In the case of Vittorio Micheli, for example, X-rays showed "an almost complete destruction of the left pelvis" as a result of sarcoma (Garner, 1974, p. 1257). At Lourdes in May 1963, Micheli felt an immediate disappearance of his long-standing pain and a subjective sense that he was cured. Within a month, he was walking, and within three months X-rays "showed that the sarcoma had regressed and the bone of the pelvis was recuperating" (p. 1259). In a similar case, 12-year-

12. As examples of such recuperative powers, he cites (pp. 19–20, 119) some of the kinds of phenomena that I will discuss at greater length later in this chapter, such as accelerated healing of skin diseases by hypnosis.

13. As Dowling and others have emphasized, however, declaring a cure "medically inexplicable" is *not* synonymous with declaring it a "miracle"—the latter being a theological, not scientific, designation.

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old Delizia Cirolli went to Lourdes in August 1976 with a diagnosis (made by X-rays and a biopsy) of a bony metastasis of a neuroblastoma. There was no improvement, and she continued to decline, X-rays in September showing further growth of the cancer. Villagers prayed for her, however, and her mother regularly gave her Lourdes water. By Christmas, weighing less than 50 pounds, she began to recover. Subsequent X-rays showed the bone repairing, and ultimately cured. Although "there was no doubt she had been cured," the exact nature of the diagnosis was in some doubt. Doctors ultimately decided that Ewing's tumor was more likely than a neuroblastoma; but whether a neuroblastoma or a Ewing's tumor, spontaneous remissions are either rare or unknown (Dowling, 1984, p. 636).

Again, what are we to make of such cases? Clearly we cannot dismiss them as based on unreliable or distorted "anecdotes." But is it then an adequate explanation simply to say that some self-healing power, perhaps within the neuroimmunological system, has been activated in some unusually potent way? What is this self-healing power, how is it activated, and how does it work, not only in the repair of a bone seriously damaged by cancer, but even in less dramatic illnesses? Do "spontaneous" cures and remissions—whether those that are frequently seen or those that are rare—occur only by "chance"?

Placebo and Nocebo

Closely related to all the phenomena I have discussed so far—some now accepted as genuine, some still viewed with considerable skepticism—is a phenomenon that has occupied an odd place in the history of modern medicine, namely, the placebo effect and its obverse, the nocebo effect. Scientists have struggled even with the problem of *defining* placebo/nocebo[14]—let alone explaining it—primarily because most of them wish to avoid charges of dualism, and thus they reject definitions that involve any "tortuous attempts to define the placebo as a belief state separable from the purely physical effect of a drug or of surgery" (Wall, 1996, p. 163). Many insist, as we saw earlier, that mind and body are a unified entity—"brain-mind"—and on this basis some have suggested, rather vaguely, that placebo/nocebo effects are "meaning responses" (Moerman, 2000, p. 56) or "context effects" (Kleijnen, 2000). As I will argue throughout this chapter, however, such assumptions and terms provide neither definitions nor explanations.

A placebo (or nocebo) is an intervention that has no known direct physiological consequences but nevertheless improves (or worsens) a person's health. For a more precise definition, however, "dualistic" terms seem unavoidable to convey the basic idea that a psychological factor seems to have precipitated a change in a physiological condition. As Kihlstrom

14. Thompson (2005) discusses the problem of defining placebo (pp. 27–28) and provides a table listing 18 different dictionary definitions from 1785 to 2001 (pp. 18–21).

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(1993a) cautiously recognized, the placebo response suggests "the reverse of the conventional way of thinking about the mind-body problem. We usually think about mental states as emerging from physiological processes. In placebo, there is a mental state that seems to alter physiological processes" (p. 215).

There has thus been widespread resistance to and skepticism about the notion of a placebo effect—despite the professed belief of many scientists in a unified "brain-mind"—because "it poses a serious challenge to much of the ideology of biomedicine...[that] disease is a mechanical phenomenon" (Moerman, 2000, p. 65). The placebo's odd place in the history of modern medicine derives from the conflict between this relatively new but now prevailing mechanistic assumption in Western medicine and the old, long-standing assumption in medicine that "faith heals." The issue came to a head after the 1940s with the emergence of the double-blind, randomized clinical trial as the standard for medical research, and also with the publication of some influential papers, especially that of Beecher (1955), that seemed to show "the oxymoron-like enigma of an effect produced by something that is inert" (Kaptchuk, 1998, p. 1723). In clinical research, it became essential to show that a new procedure or therapy was better than "nonspecific effects" produced by a placebo, and placebo thus became simply the control condition, a "nuisance" to be eliminated in the search for truly effective treatments.[15]

Placebo's "odd" place, therefore, is that, on the one hand, it has been so thoroughly accepted by the medical community that it is now an obligatory factor in the experimental design of studies of the efficacy of medical treatments; and yet on the other hand, there has been virtually no effort until recently to understand the "enigma" of the placebo itself and its apparent conflict with the biomedical model. As Kaptchuk (2002) cautioned, "dismissing a treatment as 'just a placebo' may not be enough" (p. 817).

Benson and Epstein (1975) were among the first to urge scientists to drop their "disdain" for the placebo and study it as a phenomenon in its own right, but 24 years later some researchers were still asking "why the bald facts of the placebo phenomenon...have not yet launched a thousand inquiries into the mind's treatment powers" (Dientsfrey, 1999, p. 233). Efforts to understand the placebo itself, however, have increased in recent years, largely because a physiological hypothesis emerged that "gave it instant respectability in 20th century terms" (Wall, 1993, p. 197). Specifically, J. D. Levine, Gordon, and Fields (1978) reported a double-blind study in which 40 post-dental-surgery patients were given a placebo as a pain-killer. An hour

15. This attitude that the placebo is simply a nuisance to be eliminated, rather than an important phenomenon begging to be explained both for theoretical reasons and so that it might perhaps be used deliberately, remains the dominant one. For example, two physicians (both of them, incidentally, supported by grants from drug companies) recently said that "a detailed understanding" will allow scientists "to decrease placebo response in clinical trials" (D. J. Stein & Mayberg, 2005, p. 442).

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primarily on subjective perceptions—although one might then ask, as one researcher did, "If it's all in your head, does that mean you only think you feel better?" (Ader, 2000, p. 7). I have already mentioned studies showing a relationship between placebo and the release of endogenous opioids. More recent studies on pain and placebo have added to the evidence that placebo can produce highly specific and objectively measurable physiological changes. For example, two brain-imaging studies, one with positron emission tomography (PET) (Petrovic, Kalso, Petersson, & Ingvar, 2002), and one with functional magnetic resonance imaging (fMRI) (Wager et al., 2004), have shown decreased activity in brain areas associated with pain (such as the thalamus, insula, and anterior cingulate cortex) in response to placebo administration as an analgesic. In another study (Zubieta et al., 2005) PET imaging specifically showed activation of the endogenous opioid system in response to a placebo analgesic.

Another condition whose responsiveness to placebo might be thought purely subjective is depression. Effects of placebo on depression have been recognized since the introduction of pharmacologic treatments (e.g., Malitz & Kanzler, 1971), and a meta-analysis of studies in which patients had been assigned randomly to an antidepressant medication or to a placebo provocatively suggested that much of the effect of the medications themselves is attributable to placebo effects (Kirsch & Sapirstein, 1999). Most studies have indeed used subjective measures of effectiveness, such as scales in which patients rate the severity of particular symptoms; but at least two recent studies have focused on more objective outcomes. Both studies found alterations in brain activity, one using electroencephalography, or EEG (Leuchter, Cook, Witte, Morgan, & Abrams, 2002), the other using PET (Mayberg et al., 2002); and both also found that the physiological response to placebo was in some way different from that to the antidepressant. Such findings suggest not only that placebo is an active, and not a "no-treatment," condition, but also that "the two treatments [placebo and pharmacological] are not physiologically equivalent" (Leuchter et al., 2002, p. 125).

Other conditions known to be responsive to psychological factors, such as asthma and ulcers, have also been responsive to placebo, and several studies have measured physiological reactions to placebo treatments of these conditions. For example, asthmatic patients given a nocebo (an inhalant of saline solution that they were told was an allergen) showed a significant increase in airway resistance; all the resulting asthmatic attacks then responded to a placebo (the same saline solution that they were now told was a medication) (Luparello, Lyons, Bleecker, & McFadden, 1968; McFadden, Luparello, Lyons, & Bleecker, 1969; see also Butler & Steptoe, 1986). Moerman (1983, 2000) reviewed numerous studies comparing placebo and the drug cimetadine in the treatment of ulcers, in which the effect was determined by an objective measure (an endoscopic examination). The studies showed "broad variation—from 0 to 100 percent—in placebo effectiveness rates" (Moerman, 2000, p. 51). Related to this, in one of the earliest studies to obtain objective measures of placebo (and nocebo) response, Sternbach (1964) reported a small study in which six subjects were given three pills

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on separate occasions. They were told that one was a stimulant to stomach activity, one was a relaxant, and one was a placebo. In fact, all three pills were placebos, but in four of the six subjects the measured gastric motility rate reflected what the subject thought was the nature of the pill—that is, highest for the "stimulant," lowest for the "relaxant," and intermediate for the "placebo."

Placebo treatment has not been confined to the use of dummy pills or saline solution. There are some indications that the type of placebo used may influence the strength of the response, and specifically that a type of intervention believed by the patient to be more effective might produce a stronger response.[16] Placebo injection, for example, may have more efficacy than a placebo pill (Kaptchuk, Goldman, Stone, & Stason, 2000). If so, it would be surprising if surgery, one of the most radical interventions possible, did not occasionally show a placebo response. There are, of course, ethical issues that often preclude sham surgery (Hornig & Miller, 2002), and because of methodological problems, the results of such studies can be considered only suggestive (for example, there are no studies comparing the surgery and sham surgery with a third, no-treatment condition). Nevertheless, surgeons themselves have recognized that surgery, like medications, should be evaluated when possible for a placebo effect (A. G. Johnson, 1994), and there have been a few studies comparing the effects of certain surgical procedures with those of a sham surgery.

Among the earliest such studies were several in the 1950s involving a procedure for the treatment of angina, in which mammary arteries were tied off in the belief that this would increase blood flow through other channels. Suspicion about the rationale of this procedure led to the testing of it with sham surgery. Neither the patients nor the physicians evaluating them after the surgery knew which group they were in. In one study (Cobb, Thomas, Dillard, Merendino, & Bruce, 1959), both groups improved with regard to the number of nitroglycerine tablets taken, with the sham-surgery group showing a slightly greater reduction than the real-surgery group. The sham-surgery group also reported slightly greater subjective improvement, the most dramatic improvement being that of a sham-surgery patient. Neither group improved significantly with regard to exercise tolerance or electrocardiographic changes, but the only three patients who did show improvement on these measures were all from the sham-surgery group. In another study (Dimond, Kittle, & Crockett, 1958), there was significant improvement in 10 of 13 real-surgery patients, as well as in all five sham-surgery patients. Apparently the success of this procedure had depended more on the enthusiasm of the surgeons performing it than on the procedure itself (Beecher, 1961; Benson & McCallie, 1979). More recently, two physicians suggested that laser treatment for angina may also have "a potentially marked placebo effect" and that, because it has been evaluated primarily with subjective measures and by physicians "presumably enthusiastic" about the pro-

16. For some references to studies suggesting that the size and color of a placebo pill may influence its effectiveness, see Thompson (2005, p. 41).

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Another study (Benedetti et al., 2004) showed changes in neuronal activity in association with a placebo saline injection (in contrast to a no-treatment control group), and the authors concluded that "these STN [subthalamic nucleus] neuronal changes are likely to be induced by the placebo-activated dopamine" (p. 588).

Placebo effects have also been found in some studies of a surgical procedure to implant fetal (nigral) tissue in the brains of Parkinson's patients, again prompting some physicians to emphasize the need to include, when possible, a placebo (sham surgery) in studies to evaluate the effectiveness of such surgery (Albin, 2002; T. B. Freeman et al., 1999). In one such study (Freed et al., 2001), among patients reporting improvement there was no difference between the real and placebo treatment. In a later reanalysis of the same data (McRae et al., 2004), expectation seemed to produce a "very strong" placebo effect, because those who *thought* they had received the real treatment had better scores, regardless of which treatment they had in fact received. Similarly, a study (de la Fuente-Fernández, 2004) of patients who had received both real and simulated deep-brain stimulation for Parkinson's showed that the magnitude of effect was equivalent in both conditions, as measured objectively by a standardized scale of motor function.

Despite recent studies of placebo showing objective measures of improvement, many physicians still insist that, regardless of its mechanism, placebo "helps people to feel better..., but it cannot cure diseases"; or more specifically that "evidence that diseases like cancer yield to placebos are [sic] limited to anecdotes, few, if any, of which can be believed....no one has yet convinced me that tuning up the nervous or immune systems repels the overwhelming forces of disease" (Spiro, 2000, p. 26).

One of the "anecdotes" to which Spiro may have been referring is the well-known case reported by Klopfer (1957). In this case, a patient who was clearly near death from lymphosarcoma learned that his hospital was to participate in studies of a promising new drug, Krebiozen. Although he did not qualify for the study because he was so close to death, he was so insistent on receiving the drug that his doctor agreed. Within three days of his first injection, he was up and walking, and his "tumor masses...were half their original size." In 10 days he was discharged, and he continued in "practically perfect" health for two months. At this time, he began to see in the media conflicting reports about the drug; as a result, "he began to lose faith...[and] relapsed to his original state." His doctor decided to tell him not to believe what he had read because there was an improved, stronger version available, and "with much fanfare" he gave him an injection—this time, of water. The patient again recovered, the results "even more dramatic" than before, and the water injections were continued for another two months. When, however, the patient learned that further studies had shown the drug to be worthless, he almost immediately declined again and died within days (pp. 337–339).

Recovery from such serious conditions in response to a placebo treatment is undoubtedly rare, and necessarily such reports must be "anecdotal," if what is meant by this is a case report and not an experimental study.

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Nevertheless, there is no good or compelling reason to dismiss such reports in the absence of evidence of fraud or incompetence. Moreover, Klopfer's report is not unique. Earlier I discussed cases of remission of cancer in connection with Lourdes and with meditation, again reported by presumably competent physicians, and I suggested that, without more knowledge of the psychological conditions involved in the numerous reports of spontaneous remissions of cancer, it is premature to dismiss them as attributable solely to chance.

In sum, we have convergent evidence, not only from formal studies of placebo but also from other phenomena such as those I have so far discussed, that health can be significantly influenced by psychological factors such as belief or suggestion. It seems more useful at this point, therefore, to ask not *whether,* but *how,* this occurs. As I mentioned earlier, PNI studies suggest strongly that psychological factors such as stress or anxiety, or converse conditions such as relaxation or hope, can, through the interaction of the nervous and immune systems, significantly impact a person's health. In placebo studies in particular, there have been two primary candidates for a more specific mechanism. Some have argued, in the wake of the Ader and Cohen (1975) study that essentially launched PNI, that placebo is a conditioned response (e.g., Ader, 1997), but others (e.g., Kienle & Kiene, 1997) have pointed out the weaknesses of evidence for relevant conditioning in most clinical situations (pp. 1314–1315). In particular, how can there be "conditioning" of responses that have not occurred before? The other and more common proposal, therefore, is that the response to placebo depends on the patient's expectations (e.g., de la Fuente-Fernández, Schulzer, & Stoessl, 2002). G. H. Montgomery and Kirsch (1997), for example, concluded that "conditioning...is completely mediated by expectancy" (p. 111); the "effect of placebos depends on the strength of the person's expectations, not on how those expectations were formed" (p. 108). In practice, however, disentangling a conditioned response from the influence of expectation in an adult human is not straightforward, and studies comparing the two hypotheses have found that both are in fact involved, perhaps in different ways (Amanzio & Benedetti, 1999; Benedetti et al., 2003; Stewart-Williams & Podd, 2004). For example, Benedetti and his colleagues found that expectation seemed to be the primary factor in the release of endogenous opioids in response to placebo given for pain relief, whereas conditioning seemed more involved in the release of hormones such as dopamine in Parkinson's patients (Amanzio & Benedetti, 1999; Benedetti et al., 2003).

Whether expectation, conditioning, or both are involved, a much more fundamental problem remains. Placebo has received increased attention in recent years because of evidence that it activates physiological mechanisms, such as the release of dopamine or endogenous opioids or a more basic reaction of the immune system in general. Similarly, other phenomena such as "voodoo death" or faith healing can be taken more seriously as the mutual interaction of the nervous and immune systems. But how adequate are these explanations? Even if we assume that expectation is the fundamental factor, "a remaining question is how these expectancies then generate the cor-

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responding responses" (Kirsch, 2004, p. 341). More directly, "how does a social situation, a psychosocial factor, initiate a physiological process in the body?" (Dienstfrey, 1999, p. 230). As the neurophysiologist Wall cautioned (1993), explanations offered so far seem to be no more than "labelling an unknown process" (p. 214).

Behind these vague descriptions of psychophysiological mechanism, however, is the presumption that has grown steadily over the past century or more—discussed throughout this book—that mind and brain are not separable but are in fact coterminous, different words for the same phenomenon. With regard specifically to the placebo, Byerly (1976) proposed an "alternative to either a physicalistic or mentalistic interpretation," in which "physical and psychological factors are not treated as two separate substances"; with this "softening of the distinction," the problem of "how mind and body can causally interact seems less of a problem" (p. 433). Similarly, H. Brody (1980) devoted an entire volume to placebo as seen by the new "philosophy of medicine," an anti-Cartesian, holistic search for "theories which avoid the rigid distinction between mental and bodily phenomena" (p. 23). Numerous others writing about the placebo effect have rejected the "outmoded dualism" (Wall, 1977, p. 365) and "misleading dichotomy" of mind and brain (Cardeña & Kirsch, 2000, p. 16) that "obfuscates and stigmatizes" such phenomena as placebo (Hahn & Kleinman, 1983, p. 16). Instead, the new "conceptualization [is that]...mind and beliefs are literally *embodied* and, conversely, the bodies of persons literally *mindful*" (Hahn & Kleinman, 1983, p. 16), and "without the Cartesian straightjacket, the issue then becomes not whether mental, 'non-material' processes can bring about significant changes, but rather whether one type of physical events can have a substantial effect on others" (Cardeña & Kirsch, 2000, p. 16). Underlying this "holistic" approach to psychophysiology, however, is a very clear-cut assumption: Most scientists today take a "neuralist" view that "obviates the mind-body mechanism problem because it treats subjective phenomena (the conscious mind) as products of nervous system activity....this neuralist approach avoids the problem of how subjective 'mind' could act on the objective and physical body" (Fields & Price, 1997, p. 94).

The noteworthy word in this last quotation is "avoids." In their determination not to be saddled with the stigma of dualism—a word synonymous for most modern scientists with "unscientific"—scientists have opted for a position that in fact explains nothing and is really only an empty restatement of the obvious, that we are psychophysiological beings. One can readily embrace the "biopsychosocial" view of humans as products of inextricably interwoven factors, and one can also reject the view—mistakenly called Cartesian dualism—that mind and body are separate entities that cannot interact. But we cannot—or should not—gloss over the problem that remains. Despite statements and assumptions about "holism" or "unity," we observe that there are mental events and physical events, and few would argue that they are not somehow related. No one, however, can say how. *How*, for example, does "a person's belief in a sham treatment...send a mes-

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& Freeman, 1946, pp. 35–36; see also Barahal, 1940; and R. Jones, 1902). Some (Ephraim, 1959, p. 233) have thought that this mechanism has been confirmed in some cases, whereas others (Jelinek, 1973, p. 529) have thought not, but in any event air bubbles cannot account for cases, by far the most common type, in which the change was permanent.

Another proposed explanation has been that a sudden loss of hair in which dark pigmented hairs fall out, but white ones do not, would give the illusion that the hair had changed color rapidly (Helm & Milgrom, 1970; Jelinek, 1973; P. R. Montgomery, 1967): "In recent years, most patients with rapid whitening of scalp hair have been found to have either alopecia aleata [loss of hair] or vitiligo [whitening of patches of the skin]" (Guin, Kumar, & Petersen, 1981, p. 577). Although this explanation almost certainly covers some cases, it cannot cover all, especially those in which the patient had originally had no (or very few) white hairs and those in which the reporting physician denied any substantial hair loss (see Stevenson, 1997, vol. 2, p. 1730).

Because sudden stress or other intense emotion affects the immune and nervous systems, resulting biochemical changes might somehow contribute to loss of pigment (Ephraim, 1959, p. 233; Ornsteen, 1930); and Guin et al. (1981) have pointed out that both alopecia aleata and vitiligo have been associated with immune disorders. Clearly, however, sudden whitening of hair or skin pigment involves a more precise mechanism than that behind the general systemic responses in the conditions I have discussed so far, and it remains "physiologically difficult to understand how the hair, which, once formed, is a structure without nerves or blood supply, can throughout its length undergo rapid physicochemical changes directly due to emotional influences" (Ephraim, 1959, p. 228).

False Pregnancy

In contrast to general stress, a quite specific idea seems to have precipitated a specific physiological response in the condition known as pseudocyesis, in which a woman who falsely believes herself to be pregnant shows many of the physiological symptoms of pregnancy (for a succinct review, see Small, 1986). The condition has been known to physicians at least since the time of Hippocrates, who described 12 cases that he had observed. Another well-known case from antiquity was that of Mary Tudor (Aldrich, 1972). Pseudocyesis also is—or was—not an uncommon condition. Bivin and Klinger (1937) reviewed 444 cases, most from the English-language literature of the 19th and 20th centuries, and another review 22 years later by Murray and Abraham (1978) reported an additional 68. T. X. Barber (1984) commented that a "surprisingly large number of...excellent hypnotic subjects (60% of those asked)" in one of his studies had experienced pseudocyesis on one or more occasions (p. 112). There have been reports of other cases since 1978 (e.g., five in Devane, Vera, Buhi, & Kalra, 1985;

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six in Whelan & Stewart, 1990; and six in Signer et al., 1992), but it is clear that the reported incidence (if not the real incidence) is declining, probably because of several factors. Improved diagnostic techniques, for example, do not allow a woman to maintain the illusion of pregnancy for long; and increased sociocultural options for women besides motherhood may have lessened the pressure to become pregnant that many women probably felt in earlier times (L. M. Cohen, 1982).

The symptoms are often objective ones, so much so that the condition "may tax the diagnostic abilities of the ablest physician" (Fried, Rakoff, Schopbach, & Kaplan, 1951, p. 1330). In one study of 27 cases occurring between 1937 and 1952, in every case at least one doctor had concurred with the woman's belief that she was pregnant (Schopbach, Fried, & Rakoff, 1952, p. 130). The commonest symptoms are, in order of incidence: abdominal enlargement, often progressing at approximately the rate of a normal pregnancy; menstrual disturbances (usually a complete cessation of menstruation for several months); sensation of fetal movements, felt not only by the woman but by others (including doctors); nausea and other gastrointestinal symptoms; breast changes, including secretions; labor pains; enlargement of the uterus; and changes in the cervix (Bivin & Klinger, 1937). Most authors since 1937 have reported similar symptoms.[17]

Some physicians have hypothesized that pseudocyesis represents a psychosomatic response to conflicting wishes and fears of pregnancy (e.g., Bivin & Klinger, 1937; Bressler, Nyhus, & Magnussen, 1958; Fried et al., 1951). According to this interpretation, it is a kind of conversion disorder, that is, a condition in which "a change in physical functioning mimicks a physical condition as an expression of a psychological conflict or need" (O'Grady & Rosenthal, 1989, pp. 506–507). Many cases of pseudocyesis do seem to have occurred at times of psychological distress, although few patients have been considered hysterical, as are most individuals in whom conversion reactions are commonly observed. More generally, Pawlowski and Pawlowski (1958) suggested that the extent to which the pseudo-pregnancy progresses depends in large part on "the extent to which the idea of pregnancy takes possession of the patient's entire personality" (p. 439). Clearly, however, such psychological interpretations constitute at best descriptions of the precipitating conditions, and not an explanation.

Most observers of pseudocyesis therefore have emphasized a psychophysiological approach according to which disturbances in the hypothalamus, perhaps brought on by depression or anxiety, have led to alterations in

17. There have even been a few cases reported of pseudocyesis in a male, although the symptoms are generally much less pronounced than in women. Also, unlike women, the men almost invariably have severe psychological disorders contributing to the delusional belief that they are pregnant (see Silva, Leong, & Weinstock, 1991, for references to reports of such cases). A related and much more common phenomenon is that of couvade, in which someone (usually the father), witnessing the expectant mother's symptoms and suffering, experiences similar ones in apparent empathy (Klein, 1991).

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toms [of pregnancy] generated by the individual...reduce anxiety" (O'Grady & Rosenthal, 1989, p. 507) or a "depressive mechanism induces neuroendocrine changes through cortical or limbic connections at the level of the hypothalamus" (Whelan & Stewart, 1990, p. 105) tell us nothing about how such changes are "generated" or "induced."

In sum, there is much evidence for an association between a woman's belief that she is pregnant and objective physiological changes in her body consistent with that belief, but so far the mechanisms underlying this association remain unknown. Unfortunately, although "the role of psychogenic factors in the control of the neuroendocrine system is becoming one of the most exciting areas of psychosomatic medicine" and pseudocyesis has been called "a paradigm of psychosomatic research" (Murray & Abraham, 1978, pp. 629, 631), opportunities for understanding the condition may be diminishing.[18] Pawlowski and Pawlowski (1958) predicted that, as diagnostic methods improve and especially "as people become more cultured and sophisticated, their emotional conflicts will seek a more profound, sophisticated mode of expression than that of spurious pregnancy" (p. 440).[19] If so, this is unfortunate (from a scientific point of view), because not only do we not have an adequate picture of the psychophysiological mechanisms involved, we are even less knowledgeable about how the specific idea of pregnancy can trigger the specific physiological systems necessary to produce the symptoms.

Stigmata

Among the most well known and hotly debated of the phenomena I discuss in this chapter are cases of stigmata, in which a person develops marks, and even bleeding, corresponding to the sites of wounds Christ is thought to have suffered at his crucifixion. Hundreds of cases have been reported from the 13th century to the present time, although one of the early reviewers of the phenomenon concluded that up to his time only 50 had been reported with adequate testimony (Thurston, 1952, p. 121). Most of the cases have

18. The neuroscientist V. S. Ramachandran (Ramachandran & Blakeslee, 1998) likewise thought that "pseudocyesis provides a valuable opportunity for exploring the mysterious no-man's-land between mind and body" (p. 218). He also noted (p. 216) that the incidence has declined from 1 in 200 pregnancies in the late 18th century to about 1 in 10,000 today, but unfortunately he gives no source for these figures.

19. An alternative (or additional) explanation for the decline of reports of cases of pseudocyesis is one that applies more generally to many types of controversial phenomena: The decline may represent not so much a decline in the *incidence* as a decline in the *reporting* of such cases. Because such cases do not fit with our current understanding of the possible mechanisms, observers may fail to report them or, at another level, journal reviewers and editors may decline to publish reports, believing that such cases cannot really occur.

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occurred in young, single females, often Catholic and usually highly religious. The marks are usually on the palms, the backs of the hands, and the soles and top of the feet, corresponding to sites where nails were thought to have pinned Christ to the cross. Another common site is in the side, corresponding to the spear wound. Less common but still repeatedly reported are marks on the head, back, or shoulders (corresponding to the crown of thorns, the lashings Christ received, and the site where he bore the cross), and, more rarely, bloody tears. Although usually called "wounds," the marks vary in nature from relatively simple red marks to blisters to actual bleeding. Rarely are actual lesions seen; the bleeding instead seems to erupt from unbroken skin. The emergence of the marks is almost always periodic and regular, usually occurring on Fridays and repeating weekly for years. They frequently appear when the stigmatic is in some kind of altered state of consciousness, such as an ecstatic state or trance. Finally, however severe the nature of the wounds or bleeding, no sepsis or inflammation occurs, and the wounds disappear rapidly, leaving little or no mark, until the next recurrence.[20]

Among the most well known cases are those of St. Francis of Assisi in the 13th century, usually considered the first stigmatic, and cases from the 19th and 20th centuries, such as Louise Lateau, Gemma Gelgani, Thérèse Neumann, and Padre Pio. Stigmata are, however, still occurring. Early and Lifschutz (1974) reported the case of a 10-year-old girl in California who was "intensely religious" and strongly identified with Christ. She showed no signs of psychopathology. Unlike most cases, in this one the stigmata appeared on only one occasion, a period of 19 days just preceding Easter 1972. A week before the stigmata appeared the girl had read a book about the crucifixion, and three days later she had seen a movie depicting it. Both portrayals of the crucifixion apparently affected her deeply, and on the night she saw the movie she also had a vivid dream about it. Her stigmata consisted of bleeding from the usual sites, including the hands, feet, and forehead. As in many cases, there were no lesions; the blood seemed instead to ooze from the skin. Although the physician (one of the authors) never saw the actual onset of bleeding, on one occasion she "observed the blood [on her palm] to increase in volume four fold" (p. 199). Bleeding from all sites occurred for the last time on Good Friday; up to the time of publication of the report they had not recurred.

Whitlock and Hynes (1978) reported the case of Mrs. H., a Polish Catholic woman who had wanted to enter a convent as a young girl, but became pregnant at age 16. After years of unhappiness, in 1958 at age 49 she developed hysterical anesthesia and began having visions and pains in her limbs. Shortly afterward a weekly cycle began in which every Friday she would pass into a trance state and late that afternoon blood would appear "below her closed eyelids." Because the blood seemed to be oozing from the skin

20. For some reviews and summaries of cases in English, see Klauder (1938), M. Murphy (1992, pp. 484–502), Ratnoff (1969), Stevenson (1997, vol. 1, pp. 34–53), Thurston (1922, 1952), and Whitlock and Hynes (1978).

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and there were no lesions or broken blood vessels, the authors conjectured that the bloody secretions came from her tear ducts (p. 191). She remained under the care of one of the authors of the paper until her death in 1963, and the authors were "satisfied that the phenomena of trance, muscular rigidity and the blood on her eyelids were genuine and not fraudulent" (p. 192). Like many stigmatics, Mrs. H. was "generally regarded as hysterical" (p. 192); but, unlike most, her stigmata took only one form—bloody tears. Moreover, that form is one of the most rare, having been reported in only a few other cases, such as those of Thérèse Neumann, Gemma Galgani, Elisabeth K., and Delfina (Stevenson, 1997, vol. 1, p. 39; Whitlock & Hynes, 1978, p. 198).[21]

J. G. Fisher and Kollar (1980) reported the case of a 23-year-old Mexican-American woman, who was initially raised Catholic but later became Pentacostal. Her religiosity intensified when she married a man equally active in the Pentacostal church. In 1971, six months after her marriage, stigmata began to appear, first on her hands, later on her feet, head, back, and left side, "usually (but not always) associated with religious ecstasy" (p. 1461). The authors began psychological and physiological studies (an MMPI that "revealed a normal profile," with no signs of psychopathology), but unfortunately they were unable to complete their studies because she and her husband "disappeared abruptly without notice" (p. 1462).

An even more recent case was phenomenologically quite different from either Mrs. H. or Early and Lifschutz's patient. Margnelli (1999) reported the case of Anna Maria T., an Italian Catholic woman whose stigmata first appeared in 1990, when she was 64. They continued to appear regularly on the first Friday of every month and would last two to three days before suddenly disappearing, leaving no scars. They were still continuing to appear regularly up to the time of the publication of the report. The marks occurred only on her palms and consisted of red rounded blotches, and sometimes blisters, that most closely resembled a burn but never bled. Anna Maria was not unusually religious, but she did pray and go to mass once a week, especially after the death of her husband in 1987, and she was an admirer of St. Francis and Padre Pio (both of them stigmatics themselves). Unlike most stigmatics, she apparently did not go into a trance-like state, although the stigmata had begun when, while praying, she had a vivid vision of Jesus approaching her and taking her by the hands.

She was closely followed by the author for five months, during which time color and infrared photographs were taken, various physiological reactions measured, and psychological tests (MMPI and Rorschach) administered. Measurements and photographs were made on days when the stigmata were present as well as on days when they were not. Physiological measurements showed differences in temperature, blood flow, and electrodermal response

21. Related to this phenomenon may be that of the secretion of bloody sweat under emotional circumstances, of which there have also been a few reports, usually among persons who were not religious stigmatics (Dunbar, 1954, p. 608; Tuke, 1884, p. 295).

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by prolonged fasting, may have led to a general depletion of the immune system resulting in diseases such as herpes simplex (Simpson, 1984, p. 1747; Whitlock & Hynes, 1978, pp. 198–199). Others have noted that in hemophiliacs, as in stigmatics, bleeding sometimes occurs in connection with severe emotional stress (Ratnoff, 1969, p. 157; Whitlock & Hynes, 1978, p. 196).

The most specific suggestion, however, has come from Ratnoff (1969), who compared stigmata cases with a phenomenon known as autoerythrocyte sensitization (AES), "the lay equivalent of stigmata" (p. 162). AES is a rare condition in which the patient, usually a woman, has suffered some severe physical trauma and subsequently develops bruising, inflammation, pain, swelling, or other internal bleeding at times of emotional stress. Because the symptoms can be induced by injecting small amounts of their own blood into patients, it has been suggested that the original trauma created an unusual sensitivity to their own blood, a sensitivity apparently somehow activated by subsequent stress. It has further been suggested that AES is an autoimmune disorder, but tests have not yet demonstrated this (Ratnoff, 1969, p. 160).

The psychological similarities between AES patients and stigmatics are notable. Like stigmatics, AES patients are nearly all women, their symptoms "sound like the table of contents of a monograph on hysteria," and most patients were "under severe emotional stress when symptoms first appeared" (Ratnoff, 1969, pp. 161–162). The physiological parallels, however, are not so clear-cut. Stigmatics do not develop bruising or inflammation, the hallmark of AES. J. G. Fisher and Kollar (1980) concluded that AES was an unlikely diagnosis in the case they investigated, and, moreover, "our review of the literature failed to identify any stigmatic in whom this interesting psychosomatic entity would have been a likely mechanism" (p. 1463).

Phenomena Related to Stigmata

Any proposed mechanism of stigmata will also have to take into account that it is not an isolated phenomenon. There have been many reports of similar, but "non-religious" cases in which strong emotion seems to have produced a specific physiological reaction related to the emotion, in particular, "somatic repetitions of previous experiences" (R. L. Moody, 1948). Stevenson (1997, vol. 1, p. 68–78) has reviewed many such cases. In some cases the rush to psychoanalytic interpretations unfortunately overshadowed the re-porting of the phenomenon itself. In one of these, a 31-year-old man bled on the palms of his hands on three occasions, all involving a stressful situation (Needles, 1943). In another case a man was reported to have bled from his armpits, in the absence of any visible wound, for four to five days every month for at least seven months (Hadley, 1929–1930).

Other cases that have also been interpreted psychoanalytically may have been related to AES, since the somatic reactions reflected an earlier injury. In a case reported by Graff and Wallerstein (1954), a male patient in a psychiatric ward developed, on two occasions, some unusual swelling

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on one of his several tattoos. The reactions occurred two days apart, both times after a psychiatric interview in which he talked about various traumatic memories. Because the wheals remained for 24 hours after the first occasion, photographs and plaster casts of them were made (pp. 510, 512). In another case (Lifschutz, 1957) a patient reported that, when she was 13, her father had "scratched her down her back with his fingernails, leaving three long scars." Four years later she left home, but when her father announced he was coming to visit her, the scars, healed for four years, bled, and they did so again on several subsequent occasions when her father was coming to visit (p. 529). Dunbar (1954) described two similar cases. In one of them a patient, recalling that a physician treating her for typhoid fever would "jokingly...grasp her neck with his hand," developed a red spot on the left side of her neck and three red spots ("of the size of a finger tip") on the right side (p. 614). In the other case, a woman developed a bruise and swelling on an arm in association with recalling her husband beating her (p. 622).

Two of the most remarkable cases were witnessed and documented by R. L. Moody (1946, 1948). In one, a man was hospitalized for several months in 1935 because of frequent somnambulism. On one occasion, to restrain him from his wanderings, his hands were tied behind his back while he slept. Nine years later he was again hospitalized because of somnambulism, as well as aggressive behavior. One night he was seen writhing on his bed with his hands clasped behind his back, after which he got up and walked outside in a somnambulistic state, his hands still behind his back. Because of the patient's aggressive behavior, Moody had ordered that he not be followed in his somnambulistic wanderings "for the safety of the staff." He returned 20 minutes later in an apparently normal state. The nurse then saw "deep weals like rope marks on each arm" (1946, p. 934). They remained and were observed by Moody and others for two days. On the evening after they disappeared, "the incident was abreacted under narcosis."[22] While he was in an apparently "completely dissociated state" and being observed by Moody, "weals appeared on both forearms; gradually these became indented; and finally some fresh petechial hæmorrhages appeared along their course" (p. 934). Although trickery could not be ruled out on the first occasion, since the patient was unobserved just before the marks were noticed by the nurse, on the second occasion the marks appeared under Moody's direct observation. These remained until the next morning, when they were photographed. The photograph published by Moody shows the many indentations on the arm and their close resemblance to rope marks (p. 935; reproduced in Stevenson, 1997, vol. 1, p. 72).

In another case, of a woman whose father had beaten her repeatedly when she was a child, "swelling, bruising, and bleeding were observed by me on at least thirty occasions" while she was reliving various traumas she had suffered (R. L. Moody, 1948, p. 964). Moody described several examples,

22. R. L. Moody (1946) defined "abreaction" as "an uninhibited reliving of the traumatic incident...[that] differs fundamentally from the mere recall of a forgotten event" (p. 934).

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currant juice." The next day it was inflamed, and it kept her bedridden for several days (p. 285).

Such cases "cause initial incredulity" (Stevenson, 1997, vol. 1, p. 74), especially when they occur in times and places other than our own. More recently, however, Rantasalo and Penttinen (1959) reported the case of a mother who, on six occasions, developed blisters on her arm. The six occasions were, first, during her first pregnancy, then on each of the four occasions when her three children were vaccinated, and finally during a time of stress again involving the welfare of her children.

Later in this chapter I will discuss the numerous instances in which bleeding, blistering, or other marks have been induced deliberately, usually by hypnotic suggestion, but one such case is worth mentioning in this section because it is apparently the only one in which wounds closely resembling religious stigmata have been produced by hypnotic suggestion. This is the case of Elisabeth K., a German girl observed by Lechler beginning in 1929. Lechler's report has never been translated; the best summary of the case in English, including five photographs of the stigmata, is by Stevenson (1997, vol. 1, pp. 43–49). Similar to the cases reported above of people who developed reactions in response to seeing another person's injuries, Elisabeth had an "extraordinary capacity to translate images" of another person's suffering into similar symptoms on her own body (p. 44). After attending a lecture on the crucifixion on Good Friday 1932, she reported to Lechler that she had felt severe pain in her hands and feet while watching depictions of Jesus nailed to the cross. Lechler had been hypnotizing her for several years for treatment of a variety of hysterical symptoms, and he decided to test her apparent ability to produce symptoms in sympathy with other people's suffering. He hypnotized her and suggested that she would dream that night of nails being driven into her hands and feet. The next day she had red, swollen areas, "with the skin somewhat opened up," at the suggested sites. Lechler told her about the hypnotic suggestion and, with her consent, gave her the further suggestion that the wounds would become deeper and that she would produce bloody tears—suggestions that were also effective. On subsequent occasions he was able to induce actual bleeding, as well as swollen red marks on her forehead and shoulder. The marks on the forehead, flecked with blood, were especially interesting because some of them were "distinctly triangular in shape, and therefore corresponding to the wounds that real thorns might sometimes make" (Stevenson, 1997, vol. 1, p. 45; photographs of these triangular marks are on p. 48). After the first inductions, she was kept under constant surveillance, and Lechler witnessed the onset of the bleeding on several occasions.

Specificity of the Wounds

An adequate explanation of stigmata will have to take into account not only the existence of these other kinds of cases, both spontaneous and induced, but also several further noteworthy features. First, as we have seen in the above examples, marks appear at quite specific locations correspond-

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ing to the image in the person's mind, whether the image is of Jesus' wounds or of wounds on someone else. For example, stigmata corresponding to the spear wound on Jesus' side may be located on either the left or the right side, apparently reflecting the stigmatic's belief about the location of that wound (Stevenson, 1997, vol. 1, pp. 40–41): "There seems to be little doubt that the representation of this wound in pictures or sculptural crucifixes had a powerful influence on which side of the body the lesion appeared" (Whitlock & Hynes, 1978, p. 187). Some stigmatics have had wounds in the palms of their hands, others wounds in the wrists (Stevenson, 1997, vol. 1, pp. 38, 41). An additional example of the specificity of location is provided by a rare phenomenon known as "espousal rings," in which a red line, often accompanied by thickening of the skin, appears encircling a finger, apparently as a symbolic token of the person's devotion to Jesus (Thurston, 1952, pp. 130–140). One example was the case of Marie-Julie Jahenny, a 19th-century Breton girl whose stigmatic ring appeared in 1874 and was still visible in 1891 (M. Murphy, 1992, pp. 491–492; Thurston, 1922, pp. 206–208).

In addition, wounds often take specific shapes, again corresponding to the image in the person's mind. The triangular marks on Elisabeth K.'s forehead provide one example. In the case of Anna Emmerich, Y-shaped stigmata on her chest resembled a Y-shaped cross at the church of her childhood (M. Murphy, 1992, pp. 486–488; Stevenson, 1997, vol. 1, p. 42). I have already mentioned the detailed shape of rope marks on one of R. L. Moody's patients; Stevenson (1997, vol. 1, pp. 38–39) describes three similar cases among stigmatics (two from the 17th century and, again, Marie-Julie Jahenny in the 19th century), in which rope marks on the wrists appeared, apparently corresponding to images of Jesus being bound to the cross by ropes rather than by nails.

Other features of stigmata cases are difficult to reconcile with the notion that they are the result of some normal disease such as herpes simplex. One is their temporal regularity: Many recur regularly, often for years, at a specific time, usually Friday. Another is "the almost invariable absence of sepsis when they were open or healing" (Whitlock & Hynes, 1978, p. 188). Additionally, the wounds disappear and heal rapidly, usually leaving no scars, inflammation, or other residue. Thurston (1922) summed up the problem presented by features such as these: "For the symmetrical arrangement and narrowly limited area, the periodicity extending over a long term of years, and for such deep wounds...—wounds that never suppurate but heal with extraordinary rapidity—there seems to be no adequate analogy" (pp. 200–201). More recently, Margnelli (1999) asked what "activates the nerve trunks, the contained area of the lesions, the wounds' topographical precision and their long duration" (p. 464).

Predisposing Characteristics

Certain psychological characteristics or conditions seem conducive to psychophysiological phenomena such as stigmata. The "unmistakable symptoms of hysteria" in many stigmatics is a clue (Thurston, 1952, p. 122),

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but the essential factor does not seem to be psychopathology per se, but rather a trait variously manifesting as suggestibility, absorption or intense concentration, capacity for vivid imagery, hypnotizability, or dissociation. As M. Murphy (1992) put it, "images, it seems, often work upon the flesh most effectively in states of deep mental absorption" (p. 500), and clearly an important feature in most cases has been the person's heightened, intense "monoideism" or concentration on the relevant images (Stevenson, 1997, vol. 1, pp. 50–51, 80–83).

Another characteristic is what has been called "abnormal suggestibility" (Thurston, 1952, p. 122) or unusual "impressionability" (Stevenson, 1997, vol. 1, p. 52). Stigmatics seem to be "model hysterics in whom suggestibility/auto-suggestibility would reach the maximum visibility" (Margnelli, 1999, p. 464). Ratnoff (1969) likewise noted the importance of suggestibility among patients with AES syndrome (p. 162).

Related to both absorption and suggestibility is the observation that stigmata frequently occur when the person is in some kind of altered state of consciousness, such as religious "ecstasy" or a trance (Margnelli, 1999, pp. 466–467; Whitlock & Hynes, 1978, pp. 187, 189). Such an altered state may also be conducive in non-religious cases, as seen in that of R. L. Moody (1946) in which the marks appeared during a somnambulistic state. Such altered states figure so frequently in these cases that Whitlock and Hynes (1978) called them "essential precursors" of stigmata-like lesions: "One fact appears to be fairly well established: the existence of a state of trance or some other altered state of consciousness seems to have a facilitatory effect on the production of changes that culminate in bleeding from open wounds in the skin" (p. 500).

Another important factor, and one surely tied to the absorption in relevant imagery, is the intense emotion accompanying stigmata and related cases. Many people have noted, and many of the phenomena described in this chapter suggest, "that intense emotional experience can activate specific psychophysiological mechanisms" (Whitlock & Hynes, 1978, p. 200). The fundamental question, however, behind stigmata as well as all the other phenomena described in this chapter, is "How?" Although some may presume with R. L. Moody (1946) that "neural pathways undoubtedly exist by which psychic contents may be projected on to the body in a highly specific manner" (p. 935), we must ask what exactly are the neural pathways underlying a physical symptom, such as stigmata, with a highly specific form, location, and temporal occurrence: "It is difficult to see how underlying systemic disorders on their own could cause painful recurrent bleeding, often for periods of many years" (Whitlock & Hynes, 1978, p. 199). More specifically, "we have no understanding of how the brain could instruct local blood vessels and other tissues to represent the various forms in the skin" (Stevenson, 1997, vol. 1, p. 87); and vague suggestions that "an anomalous activation of nerve fibers...induc[es] the liberation of histamine in points where the lesions are formed" are—or should be—clearly inadequate (Margnelli, 1999, pp. 463–464).

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taneously occurring phenomenon, hypnosis as the experimental production of the same or similar phenomena—we might begin to unravel the "paradoxical" notion of a physical symptom induced by an idea.

As with all the phenomena I am discussing in this chapter, most attempts to explain hysteria have "wavered between the two terms of Cartesian dualism" (Ey, 1982, p. 11), considering them either "real" (that is, organic) or "imaginary" (that is, psychological or malingering). The term "hysteria" itself derived from the longstanding belief that the disease, far more common in women, resulted from a disorder of the uterus; but the 18th-century physician Robert Whytt proposed that it was instead a derangement of the nervous system, not the uterus, and 19th-century scientists took up this idea enthusiastically (Slavney, 1990, pp. 22–24). Maudsley, for example, vaguely attributed hysteria to an underlying "molecular disorder" in the nervous system. Janet thought it was a deficiency in neural "energy" resulting in an inability to integrate sensory input and thus a "dissociation" of sensorimotor processes. Some thought it primarily a disorder "in the higher cerebral centers" (Yazící & Kostakoglu, 1998, p. 166); and others have suggested that the symptoms are the result of changes in cerebral blood flow (Sierra & Berrios, 1999, p. 272).

In the absence of detailed knowledge of the physiology involved, however, such explanations amounted to "the delineation of a metaphor, not the demonstration of a mechanism" (Slavney, 1990, p. 24). Psychodynamic interpretations thus began to take over. Breuer and Freud, for example, proposed that the primary mechanism is the conversion of a strong idea or emotion into somatic expression—specifically, the observed symptoms of motor or sensory dysfunction or loss—"in much the same way as kinetic energy can be converted into various forms of movement and power" (Whitlock, 1967, p. 146).

The conversion hypothesis, however, is itself metaphoric and does not explain but *embodies* the problem. It "fails to state how it bridges the gap between the mental and the physical. How does the psychic energy or libido actually become transformed into something physical to produce, say, a paraplegia?" (E. Miller, 1987, p. 166). Psychological explanations such as conversion have seemed to many to be "very much tied up with dualist notions of mind/brain relationships" (E. Miller, 1999, p. 188), and, because of the modern determination to avoid at any cost the "pain of Cartesian dualism" (Halligan & David, 1999, p. 161), the pendulum has swung away from psycho-dynamic interpretations of hysteria and back again to seeking neurophysiological mechanisms—but this time rooted more squarely in the notion of the "brain-mind," that mysteriously unified entity in which the terms on either side of the hyphen are, in the final analysis, synonymous.

It has been known since the late 19th century that the sensorimotor functioning of an hysterical patient remains intact at some level, although part of it is prevented somehow from reaching the patient's awareness. For example, a patient suffering from hysterical blindness may, when hypnotized or producing automatic writing, give evidence of having perceived a

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particular visual stimulus when he or she is unaware of having done so. Similarly, patients with hysterical anesthesias rarely suffer any injury to the affected area, as often happens when sensation is blocked by, say, chemical anesthesia (*HP,* vol. 1, pp. 46–47). Since the 19th century, therefore, neurophysiological theories have described hysteria not so much as a *loss* but as an *inhibition* of normal sensorimotor processes, with a resulting dissociation of sensory input and perceptual awareness. Ludwig (1972), for example, attributed hysteria to "a dysfunction of attention and recent memory due to increased corticofugal inhibition of afferent stimulation" (p. 771).

Building on such proposals, researchers in recent years have attempted to identify more specifically the neural correlates of hysteria. There have not yet been many such studies. Some writers (Athwal, Harrigan, Fink, Harshall, & Frackowiak, 2001) have expressed surprise at the "dearth of relevant neuropsychological or anatomicophysiological reports in the literature" (p. 217); but the development of neuroimaging techniques has encouraged scientists that we now have the "means of elucidating the neural correlates of conversion disorder" (Yazící & Kostakoglu, 1998, p. 163). The earliest neuroimaging studies involved EEG recordings of evoked potentials, using somatosensory, visual, or auditory stimuli corresponding to the clinical deficits. In general these studies have tended to show that there is little or no alteration of "early" components (those closely tied to physical aspects of the stimuli), but significant changes in later components associated with attention and other aspects of psychological response (Lader, 1973; Marsden, 1986; Sierra & Berrios, 1999). Other evoked potential studies have similarly supported a "gate control theory" of pain, in which pain is prevented somehow from reaching consciousness (Sierra & Berrios, 1999, pp. 276–277; Slavney, 1990, p. 28), even though the painful stimulus appears to be processed normally up to the cortical entry level.[24]

Tiihonen, Kuikka, Viinamäki, Lehtonen, and Partanen (1995) used SPECT imaging to measure changes in cerebral blood flow in a patient with both paralysis and loss of sensation in her left arm. They took measurements during the time she was symptomatic and after she had recovered, and found differences suggesting that the hysterical symptoms were "associated with the simultaneous activation of frontal inhibitory areas and inhibition of the somatosensory cortex" (p. 134).

Two years later Marshall and colleagues reported a study with a woman suffering from left-sided hysterical paralysis of her arm and leg (Marshall,

24. It remains unresolved at this point whether hysterical sensory deficits do sometimes reach down to earlier levels of the corresponding pathways, as suggested by Levy and Mushin (1973) and some other early studies. An ideal way of pursuing this question would be to use natural tactile stimuli (not electrical stimuli, which bypass the tactile receptors in the skin) applied to anesthetic versus normal hands in the same subjects. This would circumvent the problems that arise in connection with visual and auditory stimuli stemming from changes of head or gaze orientation and provide each "hysterical" evoked response with a natural within-subject control.

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the brain is activated, inhibited, or in some other way altered; obviously, "hysterical paralysis must express itself through the physiological medium of brain anatomy" (Athwal et al., 2001, p. 216). The critical question is *how* the physiological change producing the symptoms is set in motion in the first place: "How is the meaning of events translated into the pathophysiology of neurons?" (Slavney, 1990, p. 29); "how the neurotransmitters were affected by the psychological state...still requires explanation" (E. Miller, 1999, p. 188); "how [do] psychological mechanisms translate (convert) from an emotional reaction into physical symptoms" (Halligan & David, 1999, p. 161)? The problem, again, is particularly acute when the symptoms "do not reflect known anatomical functions, do not correspond to known neurological pathways...,and do not follow known principles of neurophysiologic response patterns" (Ludwig, 1972, p. 771).

The problem becomes still more acute when we recognize what has made hysteria so notoriously difficult both to diagnose and to define nosologically—namely, its protean nature. Symptoms may change location, may respond to suggestion, or may reflect some meaningful response to psychological precipitating conditions. Another important point is one that Myers called attention to more than a century ago, but has rarely been appreciated: Both hysteria and hypnosis involve not just a functional *loss* or *inhibition* but also a *gain,* in that the hysteric or the hypnotized person seems to have attained a level of control—albeit at a subliminal level—over processes not ordinarily under volitional control. Moreover, physiological studies of hysteria seem to be showing that there is no underlying organic damage and that the nervous system is operating normally—"the brain receives normal sensory input...and can issue normal motor commands." What seems to be affected, instead, is the cortical activity ordinarily associated with consciousness, and "it is here that contemporary neurobiology faces a major challenge. The cerebral mechanisms of consciousness are not understood" (Marsden, 1986, p. 287).

In sum, as in the case of stigmata an adequate account of hysterical conversion will have to account for the specificity of the symptoms, as well as for the facts that they often do not fit any anatomical pattern (as in "glove anesthesias"), may change location, and can often be changed, alleviated, or cured by suggestion. General psychophysiological mechanisms, therefore, such as that behind "the tears that follow tragedy and the facial flushing that accompanies shame" (Slavney, 1990, p. 23) or the perspiration, tachycardia, and other symptoms accompanying stress, anxiety, or fear cannot adequately account for the "more dramatic" symptoms of hysteria (E. Miller, 1999, pp. 186–187). Scientists may increasingly be "elucidating the neural correlates of conversion disorder" (Yazící & Kostakoglu, 1998, p. 163), but, as Myers frequently emphasized, "correlates" is a neutral term and implies nothing about causation. Describing what is happening at the physiological level in the modern terminology of the neurosciences brings us no closer to an adequate theoretical understanding of the phenomena than we were a century ago. Modern neurophysiological terminology, like

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the old psychodynamic terminology, can too easily "distract attention from careful investigation of how beliefs and related mental images about the body come to implement physiological changes in the persons affected" (Stevenson, 1997, vol. 1, p. 53n).

The search for an adequate theoretical model, however, has been hampered by the fear of being chained to an outmoded dualism. Like most modern scientists, those addressing the problem of hysteria have called for a reconceptualization of the phenomenon that avoids the pitfalls of dualism (E. Miller, 1999) and offers instead "a more all-embracing concept" that integrates rather than separates *"the organic and functional, somatic and psychological"* (Ey, 1982, p. 18). Again, however, I emphasize that such "holism" also brings us no closer to a real understanding of psychophysiological phenomena and too often leads instead to such vacuous statements as "an individual's hysterical symptoms correspond to his/her ideas about what his/her symptoms should look like [because] his/her symptoms are his/her ideas" (M. Turner, 1999, p. 200).

Multiple Personality and Dissociative Disorders

Another phenomenon involving profound altered states of consciousness, and closely related to hysteria, is multiple personality disorder (MPD), now more generally called dissociative identity disorder (DID). The main feature of MPD is not, as in hysteria, the so-called conversion or somatic symptoms, but the dissociation. Nonetheless, perhaps not surprisingly, there are frequent reports of MPD patients manifesting strikingly different physiological characteristics or symptoms in association with different "alter" personalities. In Chapter 5 we will discuss MPD in detail. In this section, I will concentrate on neurophysiological findings in MPD patients, especially from the point of view of their relevance for the central message of this chapter, namely, that "experimental investigations of the processes by which persons with MPD accomplish such changes may eventually aid the understanding of normal mind-body processes" (S. D. Miller & Triggiano, 1992, p. 57).

There have been several reviews of psychophysiological changes associated with MPD (Birnbaum & Thomann, 1996; Coons, 1988; S. D. Miller & Triggiano, 1992; F. W. Putnam, 1984, 1991). Systematic observation and research in this area was initially motivated by continuing skepticism and controversy over the clinical validity of MPD and the hope of determining whether it was "real" or simply an extreme form of role-playing. But as Putnam (1991) pointed out, hoping to "prove" the reality of MPD with psychophysiological studies is probably unrealistic, since "there are few physiological measures that can readily distinguish between separate people" (p. 491). Thus, the goal of research on the psychophysiology of MPD has gradually shifted away from trying to "prove" the reality of MPD and instead toward trying to identify the neurophysiological processes underly-

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ing MPD and thereby aid in our understanding of the mechanisms of consciousness in general (e.g., S. D. Miller & Triggiano, 1992, p. 57; F. W. Putnam, 1991, p. 493; Reinders et al., 2003)—a goal more in line with Myers's approach of studying abnormal and unusual phenomena to shed light on normal psychophysiological processes. All the reviewers have emphasized that research in this area is in its infancy: There have been few studies yet with adequate experimental controls, and many of them are unpublished, presented only in conference reports. Nonetheless, these preliminary studies, as well as frequent clinical observations, have shown that psychophysiological changes associated with MPD constitute a robust and potentially important phenomenon.

Psychophysiological changes between alter personalities have been observed "in virtually every organ of the body" (Coons, 1988, p. 47). Although such observations are often derisively labeled as mere "anecdotes," the persistence and number of them was what suggested the need for more controlled studies in the first place. Many of the changes, in fact, are sensorimotor changes similar to those seen in hysterical conversion. There have been reports, for example, of anesthesia or analgesia in one personality but not others (e.g., B. G. Braun, 1983a, pp. 87–88; Ludwig, Brandsma, Wilbur, Bendfeldt, & Jameson, 1972, pp. 305–306); estimates of the incidence of these are 25–38% (Coons, 1988, p. 48; F. W. Putnam, Guroff, Silberman, Barban, & Post, 1986, p. 287). Related phenomena such as deafness or auditory hallucinations, or muteness or speaking with different accents, have also been reported (Coons, 1988, p. 48; S. D. Miller & Triggiano, 1992, pp. 54–55).

There have also been reports of changes in handedness or handwriting across personalities (Coons, 1988, p. 49); in one study, 37% of the 100 patients surveyed showed changes in handedness (F. W. Putnam et al., 1986, p. 289). As many as 26% of MPD patients show allergies in some personalities but not in others (F. W. Putnam et al., 1986, p. 289). In their book about their famous patient "Eve," for example, Thigpen and Cleckley (1957, p. 132) reported that the alternate personality Eve Black had an allergic reaction when she wore nylon stockings, whereas the original personality Eve White did not. B. G. Braun (1983b) described a case in which one personality could eat oranges normally, whereas all the other personalities were allergic to citrus. In another case one personality was allergic to cats, whereas another was not. In still another a woman had an allergic response to smoke in one personality but not in another. In a survey of 100 cases, 35% involved alter personalities which responded differently to foods, and in nearly half the cases they responded differently to medications (F. W. Putnam et al., 1986, p. 289). For example, B. G. Braun (1983a) reported a case in which a woman who developed adult-onset diabetes "required variable amounts of insulin depending on which personality had control" (p. 87).

Related to stigmata and the recurrence of traumatic wounds in some psychiatric patients are phenomena that B. G. Braun (1983b, p. 127) observed in two patients. One woman, whose mother had repeatedly burned her with cigarettes, developed several red marks on her skin while recalling the abuse

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were intended as a replication). In the first study Shepard and Braun, examining seven MPD patients, found "clinically significant optical differences between alter personalities" on six measures: visual acuity, manifest refraction, color vision, pupil size, corneal curvature, and intraocular pressure (S. D. Miller, 1989). On two additional measures, eye muscle balance and visual fields, there were no significant changes except in one patient. In his attempt to replicate this study, Miller added a control group of nine people who attempted to simulate alter personalties, for comparison with nine MPD patients.[26] An ophthalmologist, blind to which persons were the patients and which the simulators, administered and evaluated the ophthalmological tests. He tested these 18 people on five of the eight measures that Shepard and Braun had used: visual acuity (both with and without correction), manifest refraction, pupil size, eye muscle balance, and visual fields. There were 4.5 times more changes among different personalities in MPD patients than in those of the simulating controls. Some of the measures are "subjective," requiring the responses of the person being examined, whereas others are "objective," or measured directly by the ophthalmologist; but Miller questioned whether even the subjective measures were "transparent" enough so that a feigning patient would know the appropriate responses to make, and furthermore whether patients would be knowledgeable enough to produce the consistent results observed over multiple trials (p. 485).

S. D. Miller et al. (1991) replicated this study, extending it to 20 MPD patients and 20 simulating controls, and they again found significantly greater differences, both statistically and clinically, between the alter personalities of the MPD patients than between the "simulated" personalities of the controls. In a table summarizing the results of the three studies, however, they also show that the measures on which significant changes were found were not the same across all three studies, with changes in visual acuity and manifest refraction being the most consistent.

In addition to these controlled comparisons, Miller, like clinicians before him, noted some "highly unusual personality-specific" physiological changes that were "not amenable" to statistical study (p. 483).[27] In one

26. Although experimental studies with "control" subjects are usually considered the ideal to which all psychological research must conform to be acceptable, studies of MPD involving simulating controls raise the question of how useful such a research model is when confronting phenomena produced by highly unusual subjects. This question arises again in connection with experimental studies of hypnosis that fail to acknowledge the importance of using highly hypnotizable subjects (discussed later in this chapter) and in connection with phenomena associated with creativity, genius, and mystical experience (Chapters 7 and 8).

27. S. D. Miller and his colleagues (1991) caution that there has been no systematic research on whether observed differences are consistent over time within personalities, and that "psychophysiological differences between personalities may be more labile than previously thought." Nevertheless, they believe that the study of such differences should move away from demonstrating the phenomena toward seeking the "underlying processes by which these patients develop such differences" (p. 135).

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patient, for example, a condition known as accommodative-type esotropia, or a rotation of the eyes that sometimes appears in 4- to 5-year-old children but eventually corrects itself, was observed only in a personality that was four years old, and not in the adult personalities. In another case, an adult personality showed presbyopia, or deterioration of the ability of the eye to adjust thickness and curvature of the lens, whereas two child personalities did not, consistent with normal aging of eyesight. One personality of a third patient had 20/15 visual acuity in both eyes and no muscle balance problems; but in another personality, vision "markedly deteriorated to 20/30 in the right eye, and 20/50 in the left eye," and there was also a muscle balance disorder (a rotation outward of the left eye). Both conditions "*completely* resolved" when the patient switched back to the first personality (p. 484).

As with hysterical conversion, the development of neurological measuring techniques has prompted studies looking at measurable physiological differences between MPD alter personalities. Unfortunately, most of these studies date from the 1970s and 1980s, when techniques were less developed than they are now, but they are worth noting because many of them did demonstrate quantitative differences between personalities that could, and should, be followed up using more sophisticated imaging and statistical techniques.

The first such study, published nearly 100 years ago (M. Prince & Peterson, 1908), looked at galvanic skin response (GSR), or changes in skin resistence, in three personalities of one patient. Presented with emotionally laden words, the personalities reacted differently to them, as measured by the GSR. It took more than 60 years for someone to follow up on these promising results, but in 1972 one study measuring, among many other things, GSR in four personalities was reported (Ludwig et al., 1972). Emotionally laden words were determined for each of the four personalities and presented to the appropriate personality; again there were differential responses consistent with the clinical picture. In a second study, however, with a different patient who also had four personalities, Ludwig and his colleagues (Larmore, Ludwig, & Cain, 1977) found no unusual changes in GSR. Two additional studies involving measurement of skin conductance (Bahnson & Smith, 1975, p. 86, and Brende, 1984) found changes that "var[ied] by emerging personality." More recently, F. W. Putnam, Zahn, and Post (1990) looked at skin conductance in the context of other activity of the autonomic nervous system (ANS), such as heart and respiration rates. They found overall differences among the alter personalities of nine MPD patients, but they also found differences among the alter personalities of five simulating control subjects. Nevertheless, the patterns in the various changes led the authors to suggest the "differences in ANS activity between alter personality states may be arrived at in different ways" by MPD patients and controls (p. 256).

Several studies have used EEG methods to look for possible differences among personalities, not only in patterns of spontaneous electrical activity but also in average evoked responses, usually from visual stimulation. Two

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(unpublished) studies similarly found "a significant personality effect on the evoked potentials" (S. D. Miller & Triggiano, 1992, p. 50).

More recent studies have involved contemporary neuroimaging techniques, including mapping of EEG frequencies (Hughes, Kuhlman, Fichtner, & Gruenfeld, 1990), SPECT (Saxe, Vasile, Hill, Bloomingdale, & Van der Kolk, 1992), and PET (Reinders et al., 2003). Although the results have again suggested reproducible differences between alter personalities in individual patients, these studies can at best be considered preliminary only, given the large number of problems that have yet to be satisfactorily controlled. Nonetheless, discussion of the possible mechanisms and implications of physiological differences between alter personalities of MPD patients has begun. F. W. Putnam (1991) outlined the five primary models that have been proposed—namely, that alter personalities are the product of: (1) an autohypnotic or trance-like state; (2) a functional disconnection between the cerebral hemispheres; (3) epileptic-type seizures in the temporal lobes; (4) discrete psychophysiological and behavioral states produced by mechanisms such as conditioning or state-dependent learning and memory; and (5) feigning or role-playing. Putnam concluded that there is little if any evidence thus far to support (2) or (3) and considerable evidence to refute (5). The "most congruent" model to Putnam seems to be (4), although he also pointed out, in support of (1), the relationship between hypnotizability and clinical dissociation in adults.

Much of the neurophysiological evidence to date can, in fact, be viewed as supporting a Myers-like view of alter personalities as the products of an hypnotic-like state in which some "slackening of the centralising energy" can start a process leading to "a new mnemonic chain" in which state-dependent learning and memory can accrue and eventually form a new personality. Moreover, he believed, these "fresh combinations of our personal elements...may be evoked, by accident or design, in a variety to which we can at present assign no limit" (Myers, 1888a, pp. 383, 387).[28] Since Myers's time, much more data has accumulated suggesting the high hypnotizability of MPD patients (already well recognized in the late 19th century by Myers, Charcot, Janet, and others), as well as in support of a Myers-like "behavioral state model...that alter personalities represent discrete behavioral states of consciousness with personality state-specific encoding of certain types of memory, behavior, and psychophysiology" (F. W. Putnam, 1991, p. 499)—in short, that partly distinctive personalities can develop within the same organism and influence it in somewhat divergent fashions.

The major remaining question, however, is not so much why, but how these states form and, especially, how the accompanying physiological changes are produced. The one finding on which probably all observers of MPD can agree is the central role of emotion, and especially stress, in

28. Some of these conditions of "accident" and "design" which Myers suggests can produce these new chains of consciousness include dreams, somnambulism, automatic writing, trance, certain intoxications, hysteria, and hypnotism (Myers, 1888a, p. 387; see also our Chapters 2 and 5).

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engendering and maintaining dissociative states; and certainly the general importance of emotional states in altering both the autonomic and central nervous systems is well known. It seems likely, therefore, that EEG, blood flow, and GSR or other autonomic changes reflect the different emotional states of the various personalities, because of changes in arousal, anxiety, muscle tension, and the like that must accompany these emotional changes (Coons, Milstein, & Marley, 1982; F. W. Putnam et al., 1990). Ludwig et al. (1972), for example, found that psychophysiological changes are prominent in "emotionally relevant areas" but "tend to disappear for most emotionally neutral or non-affect-laden material" (p. 308). Mathew, Jack, and West (1985) found marked changes in regional cerebral blood flow in the right temporal lobe between alter personalities in two patients, and they suggested that, because the right hemisphere plays an important role in general in emotional states, it follows that it would play a similar role in dissociative states.

Such explanations may well be correct for generalized physiological changes, but the picture becomes more complicated when we encounter more specific and selective changes, such as the vision changes found by S. D. Miller and his colleagues, different allergic responses, anesthesias, or state-specific stigmata-like reactions, all of which have been reported repeatedly and for years. For such effects, researchers have so far suggested only rather vague models involving some kind of inhibition of function. B. G. Braun (1983b), for example, speculated that when patients do not show an allergic reaction in one state that they have in others, this is because "the final common pathway, the allergic response, can be blocked" (p. 133). Reinders et al. (2003) similarly suggested "an inability...to integrate visual and somatosensory information," that is, a "'blocking' of trauma-related information" (p. 2122), and Forrest (2001) concluded that the orbitofrontal cortex produces "a pattern of lateral inhibition between conflicting subsets of self-representations which are normally integrated into a unified self" (p. 259).

But do such proposals really constitute an explanation of the mechanism leading to specific physiological changes? Or are they merely redescriptions of the original phenomena in more "modern," physiological terms? As with studies of hysteria, scientists may be starting to close in on some neural correlates of some dissociative phenomena. But, once again, the larger underlying problem is: How is the differential and often quite specific and time-limited release of particular neurotransmitters, or the "blocking" of somatosensory information, or the "lateral inhibition" between the various states accomplished? This central problem, already serious enough in the context of the spontaneously occurring phenomena I have discussed so far, becomes even more serious when we consider psychophysiological changes that are induced *deliberately.* I turn next to these.

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Specific Physiological Effects Induced Deliberately

Deliberately induced changes in autonomic processes that are not ordinarily under voluntary control have long been reported (Tuke, 1884), particularly in connection with yogic, Zen, or other meditative practices intended to alter the ordinary influences of physical, and particularly sensory, processes on consciousness.[29] In this section, I will describe some of the autonomic changes that have been observed and some proposed explanations, concentrating especially on phenomena suggesting unusual kinds or levels of deliberate, volitional control over autonomic processes.

I begin with two cases, reported in a medical journal and later described by Dunbar (1954), that are reminiscent of cases I described earlier, such as those of R. L. Moody (1946, 1948), in which a person remembering a traumatic experience developed marks or wounds on the skin corresponding to injuries suffered during that experience. Dunbar described two people who could apparently deliberately produce urticaria, or wheals on the skin. One was a physician who had been "able from childhood to produce urticaria on his arms and trunk 'by the strength of his will'." The other was "able, under the eyes of the physician, to produce an urticarial wheal at any designated spot on his forehead just 'by thinking about it,' without any previous touching of the spot" (p. 612).

Among the earliest studies describing deliberately induced autonomic changes were several of persons who claimed to be able voluntarily to increase their heart rate. The physician Tuke (1884, pp. 371–372) confirmed the claim of an acquaintance that he could voluntarily increase his heart rate 10 to 20 beats a minute; on the occasion that Tuke observed, his pulse went from 63 to 82 in two minutes. Tuke was inclined to think "the mere direction of the Attention to the heart is sufficient," but because few people can accomplish this feat, it might have been more appropriate to suggest that a focus of attention on the heart might be necessary, but certainly not sufficient. Between 1872 and 1968 at least 18 more such cases were reported in the scientific and medical literature. Favill and White (1917), for example, reported Favill's own ability to increase his heart rate by 30 to 96 beats a minute, the increase usually beginning immediately and reaching its peak after "several beats." Another example is Luria's (1968) remarkable subject S., who demonstrated for Luria his ability to control his heart rate by first raising it to 100 from a resting state of 70 to 72 and then lowering it to 64 to 66 beats a minute. He said that he did the first by imagining himself running to catch a train, and the second by imagining himself in bed trying to fall asleep (p. 139). Most observers of these effects reported no correlation with respiration rate, and most subjects could shed little light on how they were able to produce them. Some reported, rather vaguely, exerting considerable

29. For a comprehensive bibliography of about 1,700 studies of physical and psychological effects of meditation from 1931–1996, see M. Murphy and Donovan (1997).

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Yogis

I turn now to autonomic changes voluntarily induced by yogis or other trained meditators. A frequent claim is that some yogis are able to survive long periods of time in a meditative state without food, water, or even air. Several studies have examined the hypothesis that they do this by lowering their metabolism, one indication of which would be a decrease in heart rate and hence in oxygen consumption. Although a lowered heart rate could be presumed to be the result of the relaxation, at least one case suggested that something more may be involved. Hoenig (1968) observed that, while the yogi was in a meditative state (maintained in the experiments for up to nine hours), there was an unusual pattern in his heart rate: It gradually decreased from 100 to 40, and then gradually increased again to 100, in regular cycles of 20 to 25 minutes. Despite the unusual pattern, Hoenig suggested that it was probably "a by-product of relaxation of an extreme degree" (p. 88).

Some yogis have claimed that they can survive conditions of deprivation not simply by lowering heart rate, but by stopping the heart altogether. Again, Tuke (1884, pp. 372–373) provided examples of Westerners who have made such claims; but when modern devices such as EEG and EKG allowed better examination, in most cases it was found that the heart did not actually stop. The heart sounds and the pulse were reduced enough not to be detectable by ordinary examination, and thus the heart seemed to have stopped without actually having done so. This reduction was apparently accomplished by some variation on the Valsalva maneuver, in which contraction of chest muscles, while holding the breath, puts pressure on the heart. Again, however, the picture is not quite so simple because in at least two cases EKG seemed to show that the heart *had* in fact stopped. McClure (1959) reported the case of a non-yogi in which EKG showed "slowing of the sinus rate progressively to the point of sinus arrest for a period of a few seconds" (p. 440). Moreover, no breath-holding or Valsalva maneuver was observed; "the patient simply abolished all sympathetic tone by complete mental and physical relaxation" (p. 441).

A much more extreme case, observed and reported by Kothari, Bordia, and Gupta (1973a, 1973b), involved a yogi who was confined to a small underground pit for eight days, connected to an EKG with 12 leads "short enough not to allow any movement" (1973b, p. 1646). Almost immediately after the pit was sealed, a significant sinus tachycardia developed and progressed until it reached 250 beats per minute, but without any sign of ischemia. This tachycardia continued for 29 hours when, suddenly and with no prior slowing of the heart rate, "a straight-line had replaced the [EKG] tracing" (p. 1647; a reproduction of the tracings is in the report). The investigators wanted to terminate the experiment, understandably fearing that the yogi was dead, but his attendants insisted that it continue. The flat-line state persisted for five more days until, half an hour before the experiment was scheduled to end, sinus tachycardia again developed. This continued for two hours after the yogi was removed from the pit, when his heart rate

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finally returned to normal (98 beats per minute). The obvious explanation, that the EKG leads had been disconnected, was ruled out, first because the machine was immediately checked for any malfunctioning, but more importantly because no electrical disturbance ever appeared, such as would accompany the disconnection of the leads; subsequent attempts by the investigators to disconnect the leads always produced "gross and irregular electrical disturbance." Moreover, malfunction of the machine was highly unlikely, since "the [EKG] re-appeared spontaneously on the last day" (p. 1649), an "extraordinary coincidence" if it had been a malfunction of the machine (M. Murphy, 1992, p. 535).[31] Having ruled out such explanations, the authors candidly admitted that they were "not prepared" to accept that the yogi had voluntarily stopped his heart for five days and survived; but they could "offer no satisfactory explanation for the [EKG] record before us" (p. 1649).

Other studies (Anand, Chhina, & Singh, 1961b; Hoenig, 1968; Karambelkar, Vinekar, & Bhole, 1968; Vakil, 1950) have also involved putting yogis in underground pits, for periods ranging from hours to days, and observing metabolic changes including oxygen intake and carbon dioxide output. Most have shown a significant decrease in oxygen consumption, "much more than what could be produced even by sleep" (Anand et al., 1961b, p. 89), a finding replicated in the studies by Wallace and Benson (Wallace, 1970; Wallace & Benson, 1972) of Transcendental Meditation practitioners. Concurrently, there is usually a significant decrease in carbon dioxide output.

Most investigators have concluded that yogis can survive these conditions not only because of their reduced metabolism but also because "airtight" underground pits are not, in fact, airtight, even when sealed; oxygen and other gases seep in from the surrounding earth. In one study (Anand et al., 1961b), however, the pit actually was airtight, not one dug in the earth but a specially constructed metal box. In these conditions the yogi showed that he could voluntarily "reduce his oxygen intake and carbon dioxide output to levels significantly lower than his [ordinary] requirements" (p. 87).

Other physiological changes suggest even more direct control of autonomic functions. Luria (1968, pp. 140–141) reported that S. deliberately raised the temperature of one hand by 2° C and then immediately lowered the temperature on the other hand by 1½° C. He said that he did this by imagining one hand being placed on a hot stove and the other being immersed in cold water. Similarly, a yogi was reportedly able to induce an 11° F difference in the temperature of the left and right sides of the palm of one hand, with the color of the skin changing to pink on the hot side and grey on the cold side (M. Murphy, 1992, p. 532). Wenger and Bagchi (1961) observed a yogi who could produce forehead perspiration within 1½–10 minutes of being asked to do so; moreover, the temperature on his forehead

31. The sudden reappearance of the heart beat half an hour before the scheduled end of the experiment also suggested an unusually precise internal clock, since the yogi had been in complete isolation and darkness for eight days (M. Murphy, 1992, p. 535).

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another person may in large part be attributable to auto- or self-suggestion.

Psychophysiological changes have been deliberately induced most frequently through hypnosis. (As we will see in the following pages, however, many of the same phenomena have been produced by suggestions that did *not* involve a formal hypnotic induction.) Like other phenomena discussed in this chapter, hypnosis (and its predecessor, mesmerism) has a long, complicated, and often contentious history, and there is an enormous literature about it. The definitive history may be found in Gauld (1992). In this section I will limit the discussion to an especially important aspect of hypnosis—the localized and often quite specific physiological changes that have been induced by hypnotic suggestion.

It is first important to emphasize, as have Myers and others, that hypnosis is much more than an entertaining oddity. Myers (1885c) considered hypnotism "as above all things *a method of psychological experiment*" (p. 641n), "an experimental method of reaching the subliminal self," and particularly a "means of artificial displacement of the psycho-physiological threshold" (Myers, 1891b, p. 83). Others since Myers have echoed its importance. For example, Bramwell (1903, p. 176) remarked that, although its therapeutic use in surgery and medicine is necessarily limited because only a small proportion of patients are sufficiently responsive to it, it is nonetheless of great theoretical importance for both psychology and physiology. T. X. Barber (1984) said bluntly that because suggestion can produce physiological changes, "the royal road to solving the mind-body problem involve[s] unraveling the mystery of hypnosis" (p. 77).

Much of the research on hypnosis over the past century has revolved around the question of whether hypnosis is a discrete "state" with its own unique phenomena, or whether it is simply an extension or variation of normal states of consciousness and psychological processes associated with suggestion. The variety of positions taken over the years on this question has prompted a corresponding variety of attempted explanations for the physiological changes induced by hypnosis, especially hypotheses about the role of cortical inhibition or alterations in blood flow, and about the central role of emotion, imagery, and belief. I will return to the question of the adequacy of these explanations after I have provided some examples of specific psychophysiological changes induced by hypnotic suggestion.[32] In the meantime, there are two important points to keep in mind. First, those (like Barber) who have repeatedly argued that many phenomena produced by hypnotic suggestion can also often be produced by non-hypnotic suggestion or by voluntary efforts are certainly correct. One of the main points of this chapter is, as Myers emphasized, the continuity and interrelationship of phenomena such as I have been discussing, both with each other and with other psychophysiological processes. We will see, therefore, that many

32. For reviews of such phenomena, see T. X. Barber (1961, 1965, 1978, 1984), Crasilneck and Hall (1959), Gorton (1949), M. Murphy (1992, chap. 15), and Stevenson (1997, vol. 1, pp. 56–68).

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unusual phenomena described earlier, including stigmata and other skin changes, changes in allergic reactions, sensory changes such as anesthesia and analgesia, changes in heart rate and other autonomic functions, and the healing effects associated with placebo, occur not only in waking states involving intense emotion, imagery, or belief, but also with hypnosis. Hypnosis may in some way facilitate these phenomena, but there seem to be no phenomena absolutely unique to hypnosis.

The other crucial point to keep in mind is that effects seen in connection with hypnosis often go far beyond general systemic effects of stress or relaxation, in terms of their specificity of location and other characteristics, and that any adequate explanation must address this specificity. Many theorists have contended that hypnosis is a simple extension of "normal" psychological processes, produced by compliance with the experimenter's instructions, "role-playing," or outright feigning.[33] They have been able to do so, however, only by confining their observations primarily to studies involving the most elementary, commonplace phenomena, and conducted with ordinary, even non-hypnotizable, subjects.[34] I therefore dissent sharply from the position taken by McDougall (1908). Contrasting himself with those such as Myers who "fix their attention on the most strange and perplexing of the phenomena of hypnotism," McDougall regarded as "more sober" and "more consistent with scientific principles" an approach that "concentrate[s] attention upon the simplest and least astonishing of them" (p. 242). I instead agree with Myers, who, as I pointed out in Chapter 2, believed that confining one's observations to a narrow range can lead only to a position that, lacking "width of purview," may be completely misleading.[35] In this section, therefore, I will concentrate on the more rare, extreme phenomena that are too often ignored in theorizing about hypnosis.

Most of the research on physiological effects of hypnosis, as well as most of the extreme phenomena reported, have occurred in connection with the effects of hypnosis on pain and on a variety of conditions involving the skin. Before turning to these two primary areas, I will first mention briefly some effects that have been induced by hypnosis on autonomic and sensory processes.

Autonomic Effects

Many studies have examined changes induced by hypnosis in autonomic functions such as glucose level, gastrointestinal effects, skin temper-

33. For a detailed discussion of this "sociocognitive" position, see Chapter 5.

34. This "college sophomore as subject" methodology has been a major factor in the inadequacy of much psychological research and has led, in our view, to the trivialization not only of hypnotic phenomena, but also of important areas of human experience such as creativity, meditation, and mysticism (see also Chapters 7 and 8).

35. I remind readers here of "Wind's principle," quoted in the Introduction.

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ature, salivation, and heart rate, but the conclusion of most reviewers of the phenomena has been that these changes are secondary results of changes in emotional arousal (e.g., T. X. Barber, 1961, 1965; Crasilneck & Hall, 1959; Gorton, 1949; Reiter, 1965). In one patient, for example, temporary cardiac arrest was induced by suggesting that the patient recall previous episodes of fainting (Raginsky, 1959), an effect perhaps related to the emotion generated by the memories. In another study, however, reminiscent of the deliberately induced changes in heart rate among yogis and other persons, a dramatic acceleration of heart rate was induced by hypnosis, an effect apparently induced directly and not by any suggested or accompanying emotions (van Pelt, 1965).

I mentioned earlier some cases of multiple personality disorder in which the subject, while exhibiting the personality of a young child, showed physiological effects appropriate to that age: In one case, the child personality had a childhood eye muscle disorder that disappeared in the adult personalities, and in another the older personalities had myopia, whereas two child personalities did not (S. D. Miller, 1989). A similar age-specific alteration in myopia was induced hypnotically by Erickson (1943). An even more important study was reported by Gidro-Frank and Bowersbuch (1948), in which three subjects, regressed to the age of about five to six months and below, consistently exhibited the Babinski reflex, a reflex that is produced by stroking the sole of an infant's foot, and that normally disappears and gradually changes to a different kind of reflex by the age of about one year. The Babinski reflex appeared spontaneously, with no specific suggestion for it by the experimenters and in subjects unaware of its existence and properties. One reviewer (Gorton, 1949) commented shortly after the publication of this study that it was "the best single piece of evidence available at present to support the thesis that hypnotic suggestion *properly administered to suitable subjects* can bring about psychobiological changes in the total organism which are impossible of attainment in the waking state" (p. 478; italics added).

Another interesting case involving autonomic changes is related to the phenomenon of false pregnancy, discussed earlier. Its subject, a male undergoing hypnotherapy in an attempt to quit smoking, was on one occasion given the suggestion to imagine himself as the person he would like to be. Instead of imagining himself as a non-smoker—presumably the hypnotist's intention—he imagined himself as a pregnant woman. A homosexual whose partner had recently died, he had long thought of himself as a woman and had wished he could bear a child. After the initial suggestion by the hypnotherapist, the patient continued on a daily basis to produce vivid imagery of himself as a pregnant woman, and by the time he came to a hospital three months later, he had an enlarged abdomen, morning nausea, nipple secretion, and "noticeable" enlargement of one breast (D. Barrett, 1988).

A related series of studies similarly suggests that strong imagery in connection with suggestion can contribute to structural changes in the body. In these studies 70 women who wished to increase the size of their breasts

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when exposed to full Indian sunlight: "The muscles of the eye, and iris,... lose their contractibility, and the eye becomes as motionless and insensible to light as that of a dead man" (pp. 46, 82–83). Similarly the French physician Féré reported that the pupils of two hysterical patients contracted and dilated appropriately in response to a suggestion given in the "cataleptic" (or deep hypnotic) state, but not to the same suggestions when they were in a normal state (Tuke, 1884, p. 377). More recently, Schwarz, Bickford, and Rasmussen (1955) found that in two of three subjects who responded to a hypnotic suggestion of total blindness, the pupils became "much more dilated and sluggish in their reaction to light" (p. 567). More dramatically, Erickson (1977) reported "subjects who would dilate the pupil of one eye and contract the pupil of the other in hypnotic trance, when looking at the same light" (p. 9). Unfortunately, he published no detailed report of these observations; but he did describe (1965) the case of one girl who could produce unilateral pupillary responses even when *not* hypnotized, reminiscent of the three subjects reported by Tuke and Luria (discussed earlier) who could voluntarily control pupil contraction or dilation. Significantly, this girl was "an excellent hypnotic subject"; she had extensive experience with producing suggested visual hallucinations, and she was also "remarkably competent in developing autohypnotic trances to obliterate pain."

Whether changes of these sorts have been induced by hypnosis or by nonhypnotic suggestion, our oft-repeated question remains: How? To say that it is simply a matter of voluntarily, or even involuntarily, diverted attention (see, e.g., T. X. Barber, 1961; McPeake, 1968) demonstrates the inadequacy of explanations that deal only with the more commonplace phenomena. The hypothesis of diverted attention becomes somewhat strained when one considers a report in which a hypnotized subject did not react "even when a pistol was discharged close beside him" (Esdaile, 1846, p. 278). Perhaps the important diverted attention here is not that of the subject from the relevant stimulus, but that of the theoretician from the relevant phenomena.

Recent imaging studies seem to be moving us in the direction of seeing hypnosis more comprehensively, neither as a mere extension of normal capacities nor as a discrete and homogeneous altered "state" associated with unique phenomena, but instead as a means of facilitating—in ways still unknown—physiological changes that go far beyond those producible by simple imagining or role-playing. For example, two recent PET studies of hypnotically induced hallucinations (Kosslyn, Thompson, Costantini-Ferrando, Alpert, & Spiegel, 2000; Szechtman, Woody, Bowers, & Nahmias, 1998) both found changes in regional cerebral blood flow consistent with what is seen during "real" sensory perception, but unlike the changes seen with nonhypnotic imagery. As the authors of one of these studies concluded, "hypnosis is a psychological state with distinct neural correlates and is not just the result of adopting a role" (Kosslyn et al., 2000, p. 1279).

Autonomic and sensory changes such as I have been discussing present a significant enough challenge for those seeking to develop an adequate theoretical understanding of hypnosis and suggestion. When we examine the

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phenomena of hypnotic analgesia and effects involving the skin, we encounter even more serious challenges.

Hypnotic Analgesia

Hypnosis—or, more properly in this context, mesmerism—developed primarily as a therapeutic tool in late 18th- and early 19th-century medicine, but observers quickly realized that perhaps its most powerful medical use, in the days before chemical anesthesia, was as a means of reducing and even eliminating the severe pain that otherwise accompanied every surgical procedure. The heyday of mesmeric analgesia occurred between 1829, when a cancerous breast was removed from a mesmerized patient who showed no signs of pain and no changes in pulse or breathing during the surgery, and 1854, when a similar surgery was performed in the presence of several observing physicians (see Gauld, 1988). By far the most surgeries carried out in conjunction with mesmerism were performed by James Esdaile in Bengal between 1845 and 1851. These surgeries included amputations of breasts, limbs, and penises, as well as less severe operations, but the largest number (161) were for removal of often enormous (up to 80 pounds) scrotal tumors, a condition distressingly common in those days in Bengal. Even after the introduction of chemical anesthesia in the mid-19th century, which quickly superseded the need for any other kind in most cases, mesmeric or hypnotic analgesia continued to be used occasionally, especially in situations when a chemical agent might have been dangerous. Bramwell (1903, chap. 9), for instance, described numerous cases of his own as well as those of other physicians, particularly for the removal of teeth (an excruciatingly painful procedure without anesthesia), but also for eye surgery, removal of tonsils and uterine and breast tumors, and childbirth. Even in more recent years, numerous painless surgeries under hypnosis have been reported (for a list of 32 reports of such surgeries between 1955 and 1974, see Hilgard & Hilgard, 1975/1983, p. 134).

As with hypnosis in general, attempts to explain hypnotic analgesia have fallen primarily into two camps, which can be roughly described as (1) physiological theories that seek an explanation in terms of changes in the brain, especially the inhibition of memory, attention, or perceptual processes; and (2) psychological theories that seek an explanation in terms of normal psychological processes such as expectation, relaxation and anxiety reduction, or "role-playing" enacted in compliance with the social or interpersonal context. These two groups of theories are by no means mutually exclusive. Nevertheless, the acceptance of hypnotic analgesia has been hindered by the frequent contention that hypnosis has not really produced analgesia, but has only prompted subjects to adopt a variety of strategies either for coping with the pain or for hiding it from observers. Thus, as with other phenomena I have been discussing, some investigators have tried to go

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that the sensory and discriminative aspects of painful stimuli are handled primarily by somatosensory cortex, whereas aspects of emotional response, such as judgments of pain unpleasantness, are handled by anterior cingulate cortex and related frontal structures (Faymonville et al., 2000; Rainville, Carrier, Hofbauer, Bushnell, & Duncan, 1997, 1999). Hypnotic analgesia may therefore in part involve a functional dissociation between these normally cooperating parts of the brain, which in turn enables a blunting of the pain experience.

Although research on the neurophysiology of hypnotic analgesia (and of hypnosis in general) is becoming a robust activity, two major questions remain. The first, asked repeatedly throughout this chapter, is—*how* does it occur? Over a century ago, neurophysiologists were already suggesting that hypnosis involves the inhibition of higher cortical centers, and most neurophysiological theories since then have suggested one or another cortical or subcortical process as being inhibited or otherwise blocked from functioning normally during hypnosis. But how is this inhibition effected? What sets in motion the complex and specific processes involved in, say, complete analgesia of the right forearm or a left lower wisdom tooth? Some neurophysiologists have recently begun to recognize, as Myers (1886a) already had, that hypnosis involves more than "inhibitory cerebral action" (p. xlii). Crawford, Knebel, and Vendemia (1998) have suggested that hypnotic analgesia requires the "activation of a supervisory attentional control system...involving the anterior frontal cortex" (pp. 22, 29). But, again, what activates this "supervisory control system," and how does it know to release the relevant neurochemicals, or block the relevant neural pathways, or do whatever else is required to produce the desired result?

The second primary question lurking behind all the numerous and varied studies of hypnotic analgesia over the past century is prompted by the uneasy feeling that should be felt by anyone who has paid sufficient attention to what has actually been reported in connection with mesmeric and hypnotic analgesia—specifically, a feeling that somewhere along the way we have missed the boat, or, perhaps more precisely, that scientists have become so focused on the minor leaks in the hold that they have failed to go on deck and see the typhoon bearing down on the boat. I am referring here to the question of how adequate all proposed theories—neurological as well as psychological—are to account for the more extreme phenomena that have repeatedly been reported by qualified medical observers. For many people, there is "astonishment" when reading reports of these phenomena, because they challenge the belief "that hypnotic states are really no more than states of heightened awareness or attentiveness [or] states of conscious conformity and obedience" (Robinson, 1977, p. xxvi). The usual response of such people is simply to dismiss the reports as "anecdotes" that can be ignored in light of more "rigorous" experimental and clinical studies (Spanos & Chaves, 1989). This is bad enough, but it is even more intellectually irresponsible to distort and misrepresent what was actually reported, as T. X. Barber (1963) does to Esdaile's reports. Barber suggests that Esdaile's patients "may not

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have been free of anxiety and pain," because they "moved" or "gave a cry." He fails to mention, however, that such signs were extremely infrequent, as Esdaile himself reported, or that such signs of "moving" or "moaning" also occur occasionally in connection with chemical anesthesias such as ether, chloroform, or nitrous oxide (Gauld, 1988, p. 21). Moreover, Esdaile recognized that some patients were less responsive to suggestion than others, and may have felt some pain, and thus he soon introduced measures to ensure that a patient was adequately analgesic before proceeding with the surgery.

Moreover, as Bramwell (1903) and others have pointed out, there is a vast difference between (in Bramwell's terms) the "pinprick" involved in most experimental studies and the "faradic brush" (an extremely painful DC electrical stimulus) involved in other situations (p. 94). Most contemporary experimental studies use relatively minor and brief pain stimuli (understandably, given ethical considerations). The reports of surgical procedures, however, are quite another matter. There are, as I mentioned earlier, numerous reports of major surgeries, including amputations. Gauld (1988) points out that one response of some contemporary theorists has been to suggest that "the pain caused by surgical procedures is not as great as is commonly supposed"; but he cautions us to recognize that "it is easy, in these comfortable days, to forget what pre-anaesthetic surgery was like for the patient" (pp. 20–21).[37]

A few examples, selected from numerous similar ones, should illustrate the kind of extreme phenomena that need to be accounted for, and not dismissed cavalierly as "anecdotes," fabrications, or exaggerations. Moll (1901) cites "a cynical experiment of the American physician, Dr. Little, who thrust a needle through the cornea of a subject whom he suspected of simulation" (p. 113). A dental surgeon reported operating on several of Bramwell's hypnotized patients, extracting in all about 40 teeth, including a young girl's "two left lower molars, which were decayed down to a level with the alveolus, with pulps exposed; also two right lower molar stumps, and a lower bicuspid: all difficult teeth" (Bramwell, 1903, pp. 162–163). Mason (1955b) reported the case of a woman who had two impacted wisdom teeth extracted under hypnosis, involving an incision in the gum and removal of bone by chisels. Later, also under hypnosis alone, she had a follow-up surgical bilateral mammoplasty in which scars, skin, breast tissue, and fat were cut out and the breasts completely reshaped. This procedure took 70 minutes, during which time she showed no sign of pain or shock. She later said that she felt and remembered nothing.

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37. Darwin, who ultimately rejected a medical career in part because of his inability to tolerate the suffering he witnessed, reminds us of this: "[I] attended on two occasions the operating theatre in the hospital at Edinburgh, and saw two very bad operations, one on a child, but I rushed away before they were completed. Nor did I ever attend again, for hardly any inducement would have been strong enough to make me do so; this being long before the blessed days of chloroform. The two cases fairly haunted me for many a long year" (Darwin, 1892/1958, p. 12).

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One of Esdaile's nearly 300 surgeries illustrates in particularly vivid detail that we are not here talking about the "pinpricks" of most contemporary experimental studies.[38] The case was that of a man who for two years had suffered from "a tumour in the antrum maxillare; the tumour has pushed up the orbit of the eye, filled up the nose, passed into the throat, and caused an enlargement of the neck" (Esdaile, 1846, p. 147). Although the patient proved difficult to mesmerize, Esdaile finally succeeded in doing so. Then, he reports,

> I performed one of the most severe and protracted operations in surgery.... I put a long knife in at the corner of his mouth, and brought the point out over the cheek-bone, dividing the parts between; from this, I pushed it through the skin at the inner corner of the eye, and dissected the cheek back to the nose. The pressure of the tumour had caused the absorption of the anterior wall of the antrum, and on pressing my fingers between it and the bones, it burst, and a shocking gush of blood, and brain-like matter, followed. The tumour extended as far my fingers could reach under the orbit and cheek-bone, and passed into the gullet—having destroyed the bones and partition of the nose....The man never moved, nor showed any signs of life, except an occasional indistinct moan.[39] (pp. 148–149)

With this description firmly in mind, consider now this statement by T. X. Barber (1963), who purports to explain hypnotic analgesia in terms of the patient's "motivation for denial of pain":

> [He] has often formed a close relationship with the physician-hypnotist and would like to please him or at least not to disappoint him....[He] is aware that if he states that he suffered, he is implying that the physician's time and energy were wasted and his efforts futile....this may at times be sufficient for him to try to inhibit overt signs of pain such as moaning, wincing, or restlessness....[The patients] "bravely made no signs of suffering at all." (pp. 306–308)

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38. T. X. Barber (1963) contemptuously dismisses those who "almost always rel[y] heavily on Esdaile's series" in support of their contention that truly painless surgeries have been performed under hypnotic analgesia alone (p. 316). Similarly, Spanos and Chaves (1989) "view with alarm the retreat to nineteenth century anecdotes" (p. 131). I make no apologies for being another who is profoundly impressed with Esdaile's reports. I can only wonder at the intransigence of those who simply dismiss reports that run counter to their beliefs, and moreover in doing so grossly misrepresent them. Furthermore, the suggestion that such reports are limited to one reporter or to the 19th century or any other "pre-modern" or "pre-scientific" period is simply false (again, see Hilgard & Hilgard, 1975/1983, p. 134).

39. A "moan," no doubt, that was behind Barber's dismissal of this case as not being truly painless—but as Esdaile commented later, "he declares most positively, that he knew nothing that had been done to him ...—and I presume he knows best" (p. 150).

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629). Decades later T. X. Barber (1984) concluded that "abnormally reactive (allergic) skin responses to pollen, house dust, tuberculin, and many other allergens can be reduced and at times totally blocked by suggestions not to react," particularly among highly hypnotizable persons (p. 75). Mason (1960), for example, reported that the lesions caused by skin allergies were improved in eight of nine patients by hypnotic suggestion. Among the most interesting studies, however, have been those in which allergic reactions to a skin test have been abolished. Mason and Black (1958) reported a case in which not only the patient's symptomatic signs of asthma and hay fever, but also her reactions to a skin test with the allergens that had previously affected her were abolished. Mason (1960) also reported a case in which he injected both of a patient's arms with tuberculin, but told the patient that one arm was being injected with water only. The skin reaction, as indicated by the Mantoux test, was inhibited in that arm, but normal in the other arm. Several weeks later, the test was repeated, with the experimental ("water"-injected) and control (tuberculin-injected) arms being reversed, and again the reaction was inhibited only in the arm that the patient believed had been injected with water (p. 336).

Black, Humphrey, and Niven (1963) extended this study to four more Mantoux-positive patients, this time evaluating not only the observed swelling and redness but also skin biopsies. Again, there was significant reduction in the overt symptoms, but no changes were found in the cellular patterns in biopsy specimens. They concluded that the reactions to hypnotic suggestion took place, not at the cellular level, but at the level at which fluids are exuded, suggesting "a vascular constituent...in the mechanism of inhibition" (p. 1652). Similarly, in a review years earlier of hypnosis and allergies, Gorton (1949, pp. 337–339) had suggested that the effect was on the "cutaneous reaction" and not on the underlying "allergic constitution."

In a more recent study (Laidlaw, Booth, & Large, 1996), 32 of 38 subjects were able to reduce significantly the size of skin wheals produced in reaction to a histamine solution when exposed to a hypnotic-like procedure involving relaxation, imagination, and visualization.[41]

Bleeding

Hypnotic suggestion has also been successful in controlling bleeding, an especially important use for hemophiliacs. Myers (*HP*, vol. 1, p. 490) had called attention to this phenomenon, particularly in connection with a case in which a young boy was cured of "a most desperate case of hæmorrhagy." But possibly the best-known case is that involving the Russian monk Rasputin, who was said to have been able to stop the bleeding of the hemophiliac son of the Czar by hypnotic suggestion (Stevenson, 1997, vol. 1, p. 58). Blood flow has also been slowed or stopped in an effort to

41. The subjects had been chosen randomly, all had comparable reactions to the histamine solution in a pretest, and all were given a hypnotic susceptibility scale, but there was no relationship between hypnotizability and the response of the skin reaction to the hypnotic suggestion (p. 245).

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promote healing. McCord (1968) reported that, in a patient whose frequent and severe nosebleeds had responded only "poorly" to conventional treatments, a single hypnotic suggestion ("given...in a definite and purposeful manner") stopped the bleeding entirely, at least up to a three-month followup. Clawson and Swade (1975) reported immediately stopping the bleeding of a severe laceration. Bleeding that does not involve the skin has also been stopped, for example with severe gastrointestinal bleeding from ulcers (Bishay, Stevens, & Lee, 1984; L. E. Fredericks, 1967). Perhaps the most common use, however, has been to control bleeding in dental surgery, particularly with hemophiliacs (for reviews, see T. X. Barber, 1984, pp. 95–96; Crasilneck & Hall, 1959, pp. 15–16).

Hypnosis has also been used to increase blood flow. I noted earlier in the chapter reports that yogis could raise or lower the temperature in a hand, sometimes unilaterally. Similar results have been obtained with hypnosis (see T. X. Barber, 1978; Conn & Mott, 1984; McGuirck, Fitzgerald, Friedmann, Oakley, & Salmon, 1998; *HP,* vol. 1, p. 491). Because altered blood flow produces such temperature changes, a few investigators have asked whether hypnotic suggestion could help patients suffering from Raynaud's Disease, a painful condition caused by low peripheral blood flow, usually to the fingers or toes. Grabowska (1971) and Conn and Mott (1984) reported significant increases in blood flow in four patients, and Crasilneck and Hall (1975, pp. 127–128) reported that among 48 patients whom they had treated between 1950 and 1975, there was remission or marked improvement in about 60%.

Burns

In light of studies such as these showing that hypnotic suggestion can affect blood flow to specific sites (T. X. Barber, 1978, 1984), the hypothesis that healing of wounds might be accelerated by hypnosis seems more than plausible. In one study (Ginandes, Brooks, Sando, Jones, & Aker, 2003), for example, surgical incision wounds healed faster in a hypnotic suggestion group than in two control groups. In addition to—or perhaps because of—its effectiveness as an analgesic, hypnotic suggestion has also been used successfully to promote healing of severe burns. Most of the reports have been of clinical cases, such as that of Ewin (1979), who reported that 13 of 14 severely burned patients "healed rapidly and without scarring," including a man whose right leg had briefly been immersed up to the knee in 950° C (1750° F) molten aluminum. The 14th patient had "scoffed at the idea" of hypnosis, perhaps contributing to a self-fulfilling *lack* of effect.

Few controlled studies have been conducted, but in 1983 some preliminary findings were reported (Hammond et al., 1983; Margolis, Domangue, Ehleben, & Shrier, 1983; Moore & Kaplan, 1983). Two of these provided suggestive results consistent with accelerated healing, but the results in the third (Moore & Kaplan, 1983) were even more impressive. The patients served as their own controls in that each had suffered similar burns on both sides of the body (usually both hands), and the hypnotic suggestion was directed

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A few investigators have compared the efficacy of hypnotic and nonhypnotic suggestion. The first such study was by R. F. Q. Johnson and Barber (1978), who gave the same suggestions to a group exposed to an hypnotic induction procedure and to another group instructed in "focused contemplation." Three of 11 patients in the hypnotic group were cured, as opposed to none of the "focused contemplation" group. Because these numbers were so small, Spanos and his colleagues attempted to replicate and extend these findings. In one experiment the hypnosis group did better than either a placebo or a no-treatment group. In a second experiment both a group given an hypnotic suggestion and a group given a suggestion without hypnotic induction did better than a control group (Spanos, Stenstrom, & Johnston, 1988). In a third experiment the hypnosis group did better than a medication-treated group, a placebo group, and no-treatment group (Spanos, Williams, & Gwynn, 1990).

All four studies provide some evidence for the effectiveness of hypnotic treatment, even though the subjects were not chosen on the basis of their hypnotizability, but solely because they had warts and volunteered for the study. Nonetheless, Spanos and his colleagues did find that subjects who lost their warts, whether in the hypnosis, suggestion, or placebo groups, reported more vivid imagery than those who did not. Similarly, two studies (Asher, 1956; Ullman & Dudek, 1960) found that patients capable of deep trance fared significantly better in being cured of their warts by hypnosis than did those who were not.

Others have reported the successful treatment of warts with hypnosis (e.g., Clawson & Swade, 1975; Ewin, 1974), but many such results have been criticized because they could not exclude the possibility of spontaneous remission, which does occur frequently. Recognizing this problem, Clawson and Swade (1975, p. 165) had remarked that a better experiment than curing all warts would be one in which specific warts were targeted. Successful experiments of this kind would weaken the explanation that the warts had disappeared spontaneously, as part of their "natural history," such as some general systemic change in the patient's immune system or vasomotor processes. In fact, there have been some studies in which patients had warts on both sides of the body (usually on their hands), the experimenters suggested that the warts on only one side would disappear, and only the selected warts were cured (Dreaper, 1978; Sinclair-Gieben & Chalmers, 1959).[42] In

42. Several other studies have *not* provided evidence for a side-specific effect, in that in these studies the warts disappeared on *both* sides and not just on the selected side (R. F. Q. Johnson & Barber, 1978; Spanos et al., 1988; Surman, Gottlieb, & Hackett, 1972; Surman et al., 1973); but there may be a fairly straightforward explanation. In a study that produced significant healing but not a side-specific effect, Spanos et al. (1988, p. 257) noted that the effect of suggestion was greater on the hand that had the most warts, whether it was the target or control side; and in their highly successful study, Sinclair-Gieben and Chalmers (1959, p. 481) had always selected the hand with the most or the largest warts as the target hand. As with burn cases, therefore, it seems likely that the patient's primary motivation would be to cure the worst, and preferably all, of the warts, rather than comply with the experi-

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Dreaper's case, when treatment was begun the intention was to remove all warts, which were on both sides of both hands. When they began to shrink, however, he suggested to the patient that she allow one wart to remain, as a control; and "after ten months' treatment the only wart remaining was the suggested one" (p. 308). Attention was then directed to the remaining wart, and it too disappeared after two months.

Sinclair-Gieben and Chalmers thought that, to be successful, subjects had to reach a depth of hypnosis such that they could carry out a post-hypnotic suggestion—a logical hypothesis since the gradual disappearance of warts after hypnosis is, in essence, the continuing operation of a post-hypnotic suggestion. In their study, in nine of the 10 patients who could reach this level of hypnosis the warts disappeared on the treated but not on the untreated side.

Nearly all the proposed explanations of curing warts by suggestion have been variations on the idea that the blood supply to the warts has been altered, a change produced by some vaguely characterized neurological mechanism. Sulzberger and Wolf (1934) were among the earliest to propose this kind of explanation, saying that the "permeability of the capillaries may be affected by psychic influences," and more specifically that the hypothalamus receives impulses from the cortex, "which are, in turn, transmitted to the sympathetic and parasympathetic nerve fibres leading to the particular part. In this way, vasomotor and other changes are brought about which, in turn, can cause local trophic and physico-chemical changes. These changes are probably sufficient to make the soil unsuitable for further activity of the wart virus" (p. 555). In reviewing and summing up similar theories, Ullman (1959) emphasized the important role of emotion, and particularly "vasomotor effects emotionally induced": "The mechanism of healing in the case of cures by suggestion is in all likelihood dependent on local vascular changes brought about by vegetative impulses concomitant with the affective changes experienced by the patient" (p. 483).

Sulzberger and Wolf (1934), however, also cautioned that such descriptions "may, at first, seem to be a satisfactory explanation—but, on further analysis, how utterly hypothetical and incomplete!" (p. 556). We would do well to remember this cautionary remark, even—perhaps especially—now. I have asked repeatedly throughout this chapter whether more precise identification of physiological effects accompanying the phenomena discussed, even if correct, really gets us any closer to understanding what has set those effects in motion, particularly when they involve not simply a generalized systemic response to stress, relaxation, fear, or some other emotion, but instead a specific and localized response. As Dreaper (1978) asked, when commenting on his own impressive case in which the "mechanism" allowed one single wart among many to remain, "what can be the mechanism...

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mental goal of removing only the randomly chosen ones. More clear-cut results might be obtained, therefore, by assuring patients that after the "target" warts have been removed, the treatment will later be directed toward the "control" warts.

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[that] could cause the geographically selective destruction of warts" (pp. 308–309)?

Whatever the mechanism, most observers of the phenomena agree that, because warts are a kind of skin tumor produced by a virus, if warts "can be cured by suggestion, then we are forced to admit that we are concerned with something which deals with the most fundamental processes in medicine" (Sulzberger & Wolf, 1934, p. 553). Understanding this phenomenon might, therefore, lead to an "understanding of the bodily mechanisms involved in immunity, resistance against disease, malignancy, and other vital problems" (Gravitz, 1981, p. 282). More specifically, Clawson and Swade (1975) called warts "a model for metastasizing tumors," and they conjectured that if suggestion can cure warts by stopping the blood flow to the capillaries nourishing them, then "we think likewise tumors can be destroyed" (p. 165).[43]

Other Skin Diseases

Although the remission of tumors by hypnotic suggestion remains a hypothetical suggestion for now, hypnosis has nevertheless already been used successfully to treat skin diseases far more serious than warts. For example, Osgood (see *HP*, vol. 1, pp. 471–472), Bramwell (1903, p. 264), Asher (1956, pp. 311–312), Mason (1960), and others (see Crasilneck & Hall, 1959, p. 15) have reported curing or significantly improving cases of eczema with hypnotic suggestion, and Frankel and Misch (1973) reported significant improvement in a case of psoriasis that had resisted 20 years of conventional treatments. Dunbar (1954, pp. 616–623) reviewed numerous reports of cures, often almost instantaneous, of longstanding eczema, psoriasis, and other skin disorders. She also described a case of a physician who had suffered a severe X-ray burn, the effects of which had persisted for 14 years as painful swelling, eczema, and scars so severe that another physician had recommended amputation. The symptoms were almost completely cured after four weeks of hypnotic treatment, and a year later were completely healed.

Most striking, however, have been reports of the improvement in some cases of congenital ichthyosiform erythrodermia, or "fish-skin disease," a congenital disease appearing at birth or shortly thereafter in which a thick, black, horny layer of skin, inelastic and subject to painful lesions, covers part or even all of the body. There have been 10 cases in which this disfiguring and painful condition has responded favorably, with varying degrees of success, to hypnotic suggestion, improvements that were initially "unbelievable" to T. X. Barber (1984, p. 77), given that spontaneous remissions, or

43. Whereas patients can see warts and the effect that suggestion may be having on them, most tumors are not visible to the patient, and this may hinder the effectiveness of any suggestive techniques. Now, however, tumors can be viewed directly with imaging techniques, and it is worth considering that patients given appropriate suggestions together with the feedback of such images might respond to suggestion as effectively as patients with warts have done.

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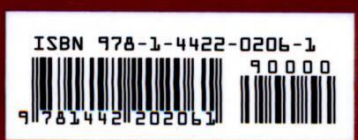

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