Chapter 7
Defining Psychology

> *A well-defined subject matter, a shared language, and conceptual agreements about the fundamentals are key elements that constitute a mature science. The physical and biological sciences have reached maturity. The psychological sciences have not. Instead, students of psychology are given choices to be or not to be radical behaviorists, cognitive psychologists, evolutionary psychologists, social constructivists, feminists, physiological psychologists, or psychodynamic psychologists, among others. The lack of a shared, general understanding has had unfortunate consequences. Paradigms are defined against one another and epistemological differences justify the dismissal of insights gleaned from other approaches. The result has been a fragmented field and a gulf between the natural and social sciences.*
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> *This analysis suggests that the fragmentation that currently characterizes the field of psychology is unnecessary and a coherent unified theory of psychology is possible. With it, the truth stands a genuine chance of emerging.*
> *G. R. Henriques (2003a, pp. 177–178)*

We are now in a position to address one of the key issues in this book and offer up a solution to the problem of psychology. Recall that my intellectual journey began with a realization that the fragmentation in psychotherapy represented a huge problem that was preventing the advancement of the profession. Yet as I probed deeply into questions of psychotherapy integration, my attention shifted as I realized a unified approach to psychotherapy depended on a coherent conception of psychology. It was out of pursuing the question of "What is psychology?" that the unified theory ultimately emerged. This chapter and the next apply the unified theory to solving the problem of psychology and fostering the move toward developing a unified approach to psychotherapy.

If you have any doubts about the importance of effectively defining the discipline, I recommend the pointed essay, *At War With Ourselves*, by Cummings and O'Donohue (2008). The authors articulate quite clearly the remarkable degree of confusion that has permeated the discipline's identity since its inception and the negative consequences this confusion has had for the field. These authors briefly review the history of the field, pointing how, for the first half of the twentieth century

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in America, psychology was dominated by experimentalists who were "obsessed with learning theory. . .with most of the so called discoveries deriving from rats and pigeons, not humans" (Cummings & O'Donohue, 2008, p. 118). The term "clinical" was frowned upon, and even an applied emphasis was looked at by many with suspicion. Although psychoanalysis was in ascendancy in practice, it was off campus and still practiced mostly by psychiatrists. Reporting on his own training experience in the late 1940s and early 1950s at the University of California, Berkeley, Cummings, who wanted to be a professional practitioner, stated that he. . .

> experienced the shock of [his] life: Psychology did not have anything to do with people. Psychotherapy training was nonexistent, as was the very word itself. We got all our training in psychotherapy off campus. . .Had our graduate faculty learned of this, we would have been drummed out of the program.

Cummings and O'Donohue (2008) go on to recount the rise of professional practice and the ensuing battle for ownership over the discipline. They articulated the conflicts between scientists and practitioners regarding licensure and the professionalization of the discipline, the role of evidence and the scientific method in informing practice, and the way that practitioners became the more dominant force in the American Psychological Association, and how this ultimately resulted in many scientifically oriented psychologists leaving the association and forming the Association for Psychological Science. Additional evidence regarding the enormous confusion about psychology is seen in the fact that originally psychologists in America were scientists experimenting with animals in the lab, and, then with the rise of professional psychology, it evolved to the point where the only people that can legally call themselves psychologists are licensed practitioners. In most states a social psychologist working at a university cannot legally refer to herself as a psychologist! Instead, she must identify herself as a professor of psychology. All of this shows just how much confusion there is regarding the nature and place of the discipline in society.

In an article titled *Psychology Defined* (Henriques, 2004), I argued that the unified theory could successfully define the science of psychology. After briefly reviewing the difficulties psychologists have had in developing a consensual definition, I introduced the ToK System and argued that the diagram suggested a new solution that could help resolve some of the most contentious aspects of the field, including the debate between the mentalists and behaviorists, and the nature of the relationship between animals and humans. The first point I made using the ToK lens was that depiction of the dimensions of complexity and joint points (see Fig. 6.1) indicated that the science of psychology could be as clearly and crisply defined as physics (the science of energy and matter) or biology (the science of life). The second point I made was that, from the vantage point afforded by the ToK System, psychology had spanned two related but separable subject matters. Specifically, the initial focus of individuals like Wundt was on humans, especially human consciousness and culture (Wundt's Volkerpsychologie), which, according to the ToK System, is located primarily on the fourth dimension of complexity. In contrast Watson's behaviorism shifted the focus to animal behavior, which is the third dimension of

complexity (Mind). The discrepancy between the two approaches highlights the utility of the map and taxonomy afforded by the ToK System. Different camps have talked past each other in large part because they were not even located on the same terrain.

To remedy the situation, I argued that, based on the conceptual structure of the ToK System, the science of psychology should be divided into two broad but logically consistent domains. The first domain has as its proper subject matter mental behavior, which is the behavior of the animal-as-a-whole mediated by the nervous system. Such behaviors can be overt (e.g., a beaver building a dam) or covert (e.g., a rat simulating future consequences at a choice point in a maze). The second domain has as its proper subject matter human behavior at the individual level and includes the human mind and human self-consciousness. The other issue I alluded to and argued more specifically in Henriques and Sternberg (2004) is that there is a fundamental difference between the science and the profession because one has as its primary goal the description and explanation of animal and human mental behavior and the other has the improvement of human well-being. Thus, although I view psychology as unified, it also has two major dividing lines, one between animal and human psychology and the other between the science and the profession. I will return to these points below.

But did I effectively define psychology? In his commentary on the unified theory, Haaga (2004, p. 1228) wished I had done so in a clearer fashion. He wrote

> It is a testament to the difficulty of the task he has set himself that in an extremely erudite paper called "Psychology defined," I could not find a concise, quotable definition of psychology. I learned a lot about evolution, some new ways of thinking about Skinner and Freud, and the physics of the Big Bang, but I would not be able to tell you in one sentence what emerged as the definition of psychology. Instead, Henriques (2004) concluded that "psychology," though unified and coherent, spans two realms—psychological formalism ("the science of mind," p. 1) and human psychology ("the science of human behavior at the individual level").

Upon reflection, I realized Haaga was right, and it is an issue I need to remedy. So here is my one-sentence definition of psychology:

> Psychology is the science of mental behavior and the human mind, and the professional application of such knowledge toward the greater good.

This definition contains several key elements that need to be elaborated upon. However, it is my hope that the logic of this definition will be readily graspable. The first element of the definition that needs clearer specification is the concept of mental behavior. Although I have introduced and utilized this conception in the chapters on Behavioral Investment Theory and the ToK System, it has not been elaborated upon, especially with regard to how it both resolves the mentalist versus behaviorist divide and clearly demarcates psychology from biology.

The second element regarding the definition that needs clarification is that although the concept of mental behavior is psychology's foundational subject matter, there nevertheless are two additional great branches of psychology that need to be clearly differentiated from the basic science of psychology. One branch is human

psychology, with its subject matter being human behavior at the individual level and special attention paid to the human mind and self-consciousness. This follows from the notion mentioned throughout this work that human behavior is mediated by an additional dimension of complexity, Culture, which arises as a function of language and consists of the collective justification systems that guide and coordinate human action. Because of this qualitative shift in complexity, human psychology is identifiable as a fundamentally separate branch of the field. In addition to involving another dimension of complexity, it is separate because, as I argued in the target article, it represents a hybrid between the formal science of psychology and the "true" social sciences (e.g., sociology, anthropology).

The second crucial division is between the science and the profession of psychology, a division required because the mission, goals, and competencies of the profession are fundamentally different than that of the science. The goal of the professional psychologist is to enhance human betterment and well-being, whereas the goal of the scientific psychologist is the description and explanation of animal and human behavior (Henriques & Sternberg, 2004). Thus, at the institutional level, the current proposal argues for dividing psychology into the following three great branches: (1) psychological formalism or basic psychology which focuses on mental behavior; (2) human psychology which focuses on the human mind and individual human behavior; and (3) professional psychology which focuses on the professional application of psychological knowledge for the greater good. In subsequent sections I clearly define mental behavior and outline the three great branches and their interrelation.

Defining the Basic Science of Psychology As the Science of Mental Behavior

Without a doubt the most foundational issue regarding psychology's definition is whether psychology is best characterized as the science of the mind or the science of behavior. From Wundt's conception of psychology as the science of consciousness to Watson's clear rejection of Wundt and proclamation that psychologists must study behavior if they are to be "real" scientists, the mind versus behavior dispute has been central to psychology's definitional problem. The most common current definition of psychology is an amalgamation of these two positions, and the majority of introductory psychology texts now define psychology as "the science of behavior and mental processes."

There are several problems with this definition. First, without clarification the term behavior is seriously problematic because, as elaborated on in Chapter 2, it carries with it two mutually exclusive meanings, one being measurable change in the object–field relationship and the other being the unique behavior patterns of animals. The second problem with the definition of psychology as the science of behavior and mental processes is that it potentially reinforces a problematic form of dualism. At the very least it is ambiguous because it implies that mental processes are not behaviors. This is a problem because, from the vantage point of the unified theory, everything is behavior. That is everything—including your sensations, feelings, and

thoughts—is part of the unfolding wave of complexity and change depicted by the ToK graphic, and all such processes can be characterized as behavior. Consider it this way: Are mental processes *not* forms of behavior? The view of the unified theory is that cognitive processes including sentient experiences (i.e., seeing red or feeling hungry) are emergent entities that arise as a function of the behaviors of neurons and neurotransmitters and the animal acting in context. A MRI or PET scan is a picture of the neurophysiological behaviors that are underpinning and mediating the cognitive processes and felt experiences. In short, then, mental processes are behaviors.

A third problem with the current definition is a mirror image of the second, as it implies that observable animal behaviors are not mental. The teleological behaviorist Howard Rachlin (1999) makes the important claim, much as I do, that overt patterns of human action that can be seen are in fact *mental* behaviors (e.g., the loving kiss or the dogged determination of a boxer in the last round of a bout). Rachlin describes his position as a teleological behaviorist because he argues that all animal behavior is purposeful in the sense of being organized by goal directedness. Anchored to traditional behavioral epistemology, Rachlin goes much further than I do in arguing that such overt patterns of action are the only patterns that exist and at times he seems to completely discount the existence of the subjective, first person point of view. This makes his perspective vulnerable to the rather absurd position effectively highlighted by the old joke about two behaviorists who just made love and one says to the other: "That was great for you. How was it for me?"

Nevertheless, Rachlin's teleological behaviorism does highlight a crucial point about mental behaviorism, and that is we do in fact *see* the mental lives of others unfolding before us. When we are watching our lovers or our children or our dogs, we are watching *mental* behavior—not just behavior caused by covert mental processes.

This is a key point because it is where decades of dispute have occurred between the behaviorist and the mentalist (cf., Rowlands, 2010). The mentalist tends to view behavior as being caused by the mind, whereas the behaviorist argues we should be studying the phenomenon under observation without recourse to unobservable causal agents. Grounded in the map of complexity provided by the ToK System, the unified theory offers a new view that defines Mind as the third dimension of complexity, which consists of the set of mental behaviors. In doing so, overt actions are connected with internal processes because the overt patterns of action that we observe in animals exist on the same dimension of complexity as the covert neurocognitive behaviors (including neuro-computational processes and sentient experiences). To delineate this argument more clearly, I formally introduced a new philosophical approach to bridging the mentalist–behaviorist divide called Mental Behaviorism (Henriques, 2004).

Mental Behaviorism

Mental behaviorism consists of three basic points, each of which I have described in some detail. The first point is that we need an adjective describing the unique kind of behavior animals exhibit, which is what I mean by the term mental. Second,

the idea of the mind as an isolated and disembodied cause of behavior is not tenable; however, as discussed previously the mind can effectively be used to denote the information instantiated in and processed by the nervous system, and it can be conceptually separated from the biophysical material that makes up the brain in the same way a story can be separated from a physical book. Finally, overt animal actions and the covert neuro-computational processes that mediate them both exist on the same dimension of complexity, Mind, which is the set of mental behaviors.

The mental behaviorist position starts with the argument that it is not the mere fact that animals behave (all objects behave) but instead it is the fact that they behave so differently than other objects that requires attention and explanation. Recall the earlier discussion from William James in the chapter on Behavioral Investment Theory where the behavior of frogs attempting to get air was contrasted with iron fillings being attracted to a magnet. Both the fillings and the frog behave; however, as James pointed out, the frog's behavior was qualitatively different because it was goal directed and, unlike the iron fillings, it would vary its action relative to obstacles in order to achieve a particular end state. This is what Rachlin means when he speaks of teleological behaviorism—the overt patterns of animal behavior are organized around a goal or purpose.

It is interesting to note from a historical perspective that in his manifesto for behaviorism, John B. Watson (1913) briefly but explicitly mentioned a positive inclination toward the notion that psychology could be considered the science of mental behavior. From the mental behaviorist perspective, it is tragic that the adjective *mental* was lost because it is essential for conceptual clarity as it allows for a description of the unique kind of behavior that psychologists are interested in trying to explain. By including the adjective mental, we need to be clear about what, exactly, is meant by it in this context. Mental is a descriptive term that refers to the distinctive manner in which animals behave relative to material objects like rocks or organic objects like plants or cells. So we are talking here about the difference between frogs and iron fillings.

To put it in slightly different terms, the mysterious problem of animal behavior to be solved is that they behave as whole units—what I refer to as *coordinated singularities* (Henriques, 2003a)—that produce specific, functional effects on the animal–environment relationship in rapid succession. The mysterious nature of this ability was seen well by Bernstein (1967), a movement physiologist, who characterized it as the "degrees-of-freedom" problem. The problem can be stated in the form of a question:

> How can an organism with thousands of muscles, billions of nerves, tens of billions of cells, and nearly infinite possible combinations of body segments and positions ever figure out how to get them all working toward a single smooth and efficient movement without invoking some clever "homunculus" who has the directions already stored? (Thelen, 1995, p. 80).

Scientific investigations have, of course, allowed us to make much progress in answering the questions regarding the whys and hows of animal behavior, and

according to the unified theory, we can turn to Behavioral Investment Theory for an integrated framework that can provide a natural scientific account of mental behavior. Specifically, as delineated by Behavioral Investment Theory, we now know the nervous system evolved as the system of behavioral investment and that such behavioral investments are accomplished through the hierarchical arrangement of neuro-computational control centers that represent goal states and adjust outputs in response to the demands of the task within a continually changing environment. This general process of behavioral regulation in response to goal states is represented in the P − M => E formulation.

From the perspective of the fragmentation within the field of psychology, the concept of mental behavior bridges the divide between radical behaviorism and traditional psychology because it allows us to retain Skinner's central insights and simultaneously remove key mistakes that he made that prevented his views from being integrated with cognitive neuroscience. From the perspective of mental behaviorism, Skinner's key insight was that animal behavior was not reducible to biological theory and that psychology could be separated from biology with an "evolutionary theory" much akin to the manner in which the modern evolutionary synthesis separates biology from chemistry. Skinner's biggest error was the manner in which he defined radical behaviorism against traditional psychology and conceptually and methodologically separated internal from external determinants and argued that psychology should focus only on the latter.

On the night before he died, Skinner (1990) completed an article for *American Psychologist* summing up his argument for why psychology could never be a successful science of mind. Skinner's anti-mentalistic perspective can be summarized as follows: First, in a manner directly paralleling the ToK System, he argued that human behavior was the product of three separate levels of variation and selection (the parallel dimensions on the ToK are in parentheses): (1) natural selection (Life); (2) behavioral selection (Mind); and (3) verbal selection (Culture). He then corresponded each level to its own discipline: (1) biology; (2) psychology; and (3) anthropology/social sciences. Second, Skinner defined mind as an unobservable cause of behavior, akin to a vitalistic life force that causes organism complexity. (Recall from Chapter 6 that vitalists interpret life as arising from a supernatural force). Third, Skinner argued that Darwin's theory of natural selection provided the framework for understanding how an environmental selection process can create biological complexity, and in so doing it removed the need for vitalism. Finally, Skinner concluded that, in the same manner that natural selection removed the need for vitalism, the concept of behavioral selection removed the need for mentalism. In short, Skinner argued that if psychology were to ever become a real science like biology, the field must give up its notion of unobservable, mentalistic forces causing animal behavior.

To Skinner and his radical behavioral followers, this argument is straightforward, sound, and confers many scientific benefits. For example, it clearly defines the proper subject matter of psychology as the behavior of the animal as a whole. Second, it differentiates psychology from biology with the same basic logic that biology is differentiated from the physical sciences. Third, it defines psychology as

a science of behavior and removes the problematic concept of something nonbehavioral (i.e., nonphysical), causing something physical to behave. All of these benefits are genuine, and I believe they should be embraced wholeheartedly.

However, the argument is not entirely sound. In fact, from the vantage point of the unified theory there is a glaring problem. According to the ToK System, Mind is the same type of concept as Life. Both are emergent dimensions of informational complexity generated by feedback loops of variation, selection, and retention. Darwin's theory of natural selection removed the need for the concept of vitalism, but it did not, of course, remove the need for the concept of life. Indeed, the idea of Darwin being "anti-life" is absurd. Biology is crisply defined as the science of life, and the set of organic behaviors are what biologists are attempting to describe, explain, and predict, and, in some circumstances, control. Likewise, the unified theory in general and ToK System in particular suggests that psychology can be crisply defined as the science of Mind, and the set of mental behaviors are what psychologists are trying to describe, explain, and predict. Thus, the mental behaviorist answers Skinner's (1990) question, "Can psychology be the science of mind?" with the answer, "Yes, so long as 'mind' is defined as a particular type of behavior."

In contrast to many popular characterizations of Skinner's position, he did not argue that consciousness did not exist. Instead, he simply characterized it as behavior that required explanation and was opposed to thinking of it as a mentalistic force that caused behavior. The mental behavioral position largely agrees with this formulation. First, as Skinner and other behaviorists appropriately argued, simply attributing the cause of behavior to an "inner man" is potentially problematic and circular. And like Skinner I also find it helpful to divide mental behaviors into two broad domains: (1) overt mental behaviors, which are behaviors that take place between the animal and the environment; and (2) covert mental behaviors, which take place within an animal's nervous system. Overt mental behaviors, to put in fairly straightforward terms, are what an animal is observed to be doing and include actions such as a fly avoiding a fly swatter, a beaver building a dam, and a man giving his lover a kiss. Covert mental behaviors are the neuro-computational or neurocognitive processes performed by all brain systems that mediate overt mental behavior. It is important to explicitly note that the adjective mental includes both conscious and nonconscious cognitive processing, as well as overt, observable patterns of behavior.

The mental behaviorist position leaves open various positions that have been taken regarding one of the key debates in animal psychology, which is the degree and kind of anthropomorphizing that has a legitimate place in the science of animal behavior. Anthropomorphizing is the tendency to ascribe conscious thoughts and feelings that would be commonly reported by a human to explain the actions of an animal. For instance, if, after coming home from work and finding that your dog had gotten into the trash and made a mess, whereupon she then presents to you in a submissive deferential position, you then ascribe to her the experience of a guilty conscience and thoughts that she has disappointed you, then you are engaging in anthropomorphizing. The place and utility of such anthropomorphizing in science has been the point of much contention. Early in the history of animal

psychology, Rommanes freely attributed sophisticated consciousness to many animals with limited operationalization, specification, or empirical validation and was justifiably criticized for offering unscientific labeling of phenomena, circular reasoning, and making pseudo-scientific claims that did not really explain anything. More recently, perhaps most notably through the work of Donald Griffin (1976), anthropomorphism has seen something of a qualified revival, although there remain many strong critics of any form of anthropomorphism (e.g., Wynne, 2007).

While not rejecting outright any form of anthropomorphism, the insights afforded by the Justification Hypothesis strongly caution against imputing self-reflective, language-based justifying thought processes in non-human animals. And along those lines, it is clear that many of the objections against the scientific viability of anthropomorphism have validity. At the same time, in contrast to the early behaviorists, Behavioral Investment Theory strongly suggests the need to allow for an animal-centered view, one that includes perceptual, motivational, and affective states that guide and regulate behavioral output (cf., Panksepp, 1998). Importantly, William Timberlake (2007) has outlined precisely such a system, which he argued can integrate the approaches from ethology, the experimental analysis of behavior, and neuroscience. His theromorphic (animal centered) behavioral systems approach is very congruent with the mental behavioral approach to animal psychology advocated for here.

Some have criticized the above argument for mental behaviorism as a form of word play (e.g., Goertzen, 2008), claiming that it does not resolve the fundamental differences and tensions between the mentalist and behaviorist perspectives. My reply is that mental behaviorism rightly identifies the key conundrum regarding the relationship between mind and behavior, and offers a "golden mean" solution, whereby the key insights from both perspectives can be assimilated and integrated into a coherent whole. Specifically, it dispenses with a problematic substance dualism and embraces aspects of the behaviorist criticism when "mind" is conceptualized as an ephemeral, unobservable, disembodied entity that causes behavior. At the same time, by emphasizing "mental" as a description of the unique behaviors in need of an explanation, the conceptual door is clearly opened to theorizing about cognition and consciousness as aspects of mental behavior, as depicted in the Architecture of the Human Mind diagram and the new tripartite model of human consciousness.

There is one other issue that must be addressed if we are to proceed in characterizing the proper subject matter of the basic science of psychology as mental behavior. Mental behavior refers to the behavior of the animal-as-a-whole, but given that, we must now consider the fact that the majority of researchers currently working on animal behavior are biologists. And surely there are those like E. O. Wilson who would disagree with the claim that the science of animal behavior should be characterized as a psychological discipline. Moreover, the significant majority of psychologists currently deal with humans. Along these lines, in his commentary on the unified theory, Kihlstrom (2004, p. 1244) argued that psychology "is first and foremost the science of human mental life." How does that fact square with the analysis that psychology is defined primarily in relationship to the third rather

than fourth dimension of complexity on the ToK? Another important issue that any attempt to define the field must address is the relationship between the science and the practice of psychology.

These issues highlight that there are still some complications to be worked out. In the next section, I spell out why, consistent with the formulation provided by the unified theory and the definition of psychology offered previously, psychology should be divided into three great branches of learning. This formulation elucidates more clearly how the unified theory solves some of the longest standing dilemmas in the field, specifically whether psychology is a natural or a social science, the relationship between psychology and biology, the line between animal and human behavior, and the difference between the science and the profession.

Psychology's Three Great Branches

The current proposal to solve the problem of psychology divides the institution into three related but separable branches. The three branches are (1) the basic science of psychology, what I refer to as psychological formalism or basic psychology, and whose proper subject matter is mental behavior; (2) human psychology, whose proper subject matter is human behavior at the individual level and includes a particular focus on the human mind and human self-consciousness; and (3) professional psychology, which involves the application of psychological knowledge for human betterment. Thus, not only am I attempting to unify psychology but I am also advocating for some fundamental divisions as well. Is it reasonable to argue on the one hand that psychology is a singular coherent entity, and then proceed to divide the concept up into three conceptually separable branches? Kihlstrom (2004, p. 1244) raised this issue when he commented that, "There is considerable irony, I think, in the discussion of the unity of psychology that immediately divides the field." Kihlstrom is correct in noting the irony, but from my vantage point, the irony is revealing. Through its unexpected and somewhat paradoxical solution, the unified theory reveals why the problem of psychology has historically been so difficult to solve.

In responding to the criticism that the unified theory claims to unify psychology but then divides it, it is useful to point out that the soundness of the logic of the divisions proposed by the unified theory can be demonstrated by showing that there are clear parallels between the divisions that I am proposing and the way in which other sciences are organized and divided. Consider, for example, the relationship between biology, neuroscience, and modern medicine. These disciplines are interrelated in the following way: (1) Biology, the science of Life, is the basic science; (2) Neuroscience is a hybrid between biology and psychology; and, finally, (3) Modern medicine can be legitimately defended as the application of biological science in the service of human betterment. The institutional arrangement and conceptual interrelationships between these disciplines directly parallels the arrangement I am arguing for regarding the three separable branches of psychology, a point illustrated in Fig. 7.1. Psychological formalism, the science of Mind, is the

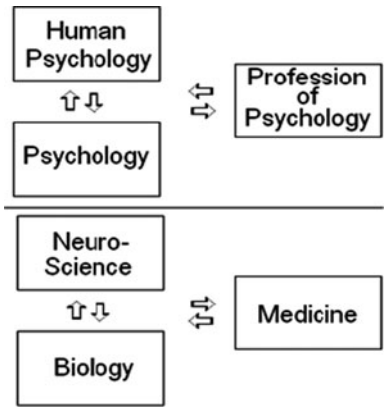

Fig. 7.1 Psychology's three great branches and parallels with biology

basic science. Human psychology is a hybrid between psychological formalism and the social sciences, and professional psychology is the application of psychological science in the service of human betterment.

One additional element that I find helpful in clarifying the nature of three branches is the association of each branch with a major icon in the field. Specifically, I associate B. F. Skinner with the formal science of psychology, Sigmund Freud with human psychology, and Carl Rogers with professional psychology. Associating these icons with the three domains is more of a heuristic than any profound philosophical truth claim. Nevertheless, the unified theory is organized to effectively incorporate the key insights of these major figures into its system of understanding, and by associating each icon with a particular branch a heuristic map is constructed that has good pedagogical value. As will be delineated in greater detail below, Skinner is associated with the formal science of psychology because he argued that psychology: (1) has as its proper subject matter the behavior of the animal-as-a-whole, which we are now referring to as mental behavior; (2) is differentiated from biology with the same logic that biology was differentiated from chemistry because animal behavior evolved as a function of the selection of consequences in a manner that had direct parallel to the evolution of life; and (3) is a purely natural science discipline. Freud is the icon associated with human psychology because he (1) identified key aspects of the dynamic relationship between self-conscious processes and subconscious motives and emotions; and (2) saw the connections between the justifications that individuals offer to maintain psychic equilibrium and the cultural narratives, myths, and taboos that coordinate populations of people. Carl Rogers is associated with the profession of psychology because he (1) identified the centrality of the therapeutic relationship and associated factors like empathy and acceptance in fostering human change during psychotherapy; and (2) recognized that the vision of the human condition afforded by the science of psychology had important implications for how people were viewed and treated.

These three icons, of course, were the primary leaders in the three great paradigms in American psychology—behaviorism, psychoanalysis, and humanistic

psychology—thus suggesting a link between the three great branches of the discipline and the three most historically significant schools of thought. The reasonableness of associating each of the three great branches with these icons is supported by a fascinating book by Demorest (2005) titled *Psychology's Grand Theorists: How Personal Experiences Shaped Professional Ideas.* In it she offers powerful profiles of Skinner, Freud, and Rogers, articulating how their unique life patterns were associated with the ideas they promoted. I found Demorest's justification for choosing these figures especially heartening and supportive of the heuristic formulation offered here. She wrote, "As prime representatives of what historically have been the three dominant [forces] in psychology, Freud, Skinner, and Rogers were all obvious choices" (2005, p. xi).

Psychological Formalism

Psychological formalism is the basic science of psychology, and its subject matter is mental behavior, the set of which makes up Mind and is represented as the third dimension of complexity on the ToK System. I call it psychological formalism to denote that this branch of psychology is a purely natural science discipline. It is hoped that eventually the term will simply be shortened to psychology, but because the generic term psychology can currently be applied as readily to the other two branches of the field (human and professional), the specification of "formalism" is needed for now.

The prior argument regarding the definition of mental behavior suggests that most research on animal behavior should fall under the discipline of psychological formalism. This raises some significant questions regarding the relationship between biology and psychology, a relationship and dividing line that is currently not clear. The histories of the disciplines attest to the fact that they meet in the domain of animal behavior. However, the dividing line between psychology and biology remains poorly delineated, and currently the science of animal behavior is an interdisciplinary enterprise that consists of both psychological and biological scientists. Early in the twentieth century, psychology was probably the more dominant force in the science of animal behavior. Behaviorists conducted the majority of laboratory research on animal behavior patterns, and comparative psychologists like C. Lloyd Morgan, T. S. Schneirla, Robert Yerkes, and Harry Harlow were hugely influential. The landscape has changed since that time. With the ascent of cognitive and humanistic perspectives in the latter part of the twentieth century, psychology shifted more toward focusing on human behavior. This combined with the rise of ethology, sociobiology, and behavioral ecology, and it is now the case that most who study animal behavior are biologists. Given these considerations, it should be noted that my prescription requires a shift in the gravitational center of the discipline.

Although this is likely to be a concern for some, there are good reasons to suspend judgment and entertain the possibility that animal behavior mediated by the nervous system constitutes the proper subject matter for the formal science of psychology. First, the current conventional definition of psychology as a science of behavior and

mental processes either includes the vast majority of animal behaviors with a general definition of "mental" (as advocated for here) or struggles enormously with ambiguity of what animal behaviors to consider if it is anchored to a conception of mental as being equivalent to sentience or consciousness or a disembodied, nonmaterial force that causes behavior. Second, American psychology was essentially defined as the science of animal behavior for much of the twentieth century; thus, there already exists a rich tradition in which this conception has been the rule. Third, humans are, of course, a type of animal and thus are obviously included. Fourth, even the simplest nervous systems, such as that in the planarian, have been found to exhibit basic psychological phenomena such as associative learning. Fifth, defining psychology solely in terms of human behavior opens up a host of serious problems. For example, if only human behaviors are psychological behaviors, what kinds of phenomena are sensation, perception, motivation, emotion, motor development, memory, attachment, dominance, eating, mating, etc. that are currently studied in animals? Finally, according to the ToK System, there is a clear dividing line between Life and Mind, suggesting that there is a specifiable way to differentiate biology from psychology.

To understand the nature of the dividing line between biology and psychology we can ask the question: Is animal behavior reducible to biological theory? In other words, can standard biological theory adequately explain animal behavior or are new theoretical ideas required to achieve a complete explanation? Interestingly, two scholars who have figured prominently in this book so far offer strikingly different views on this question. Edward O. Wilson, the prominent sociobiologist, believes animal behavior is reducible to biology. In contrast, as mentioned above, B. F. Skinner fundamentally disagreed with this claim and argued strongly that the behavior of the animal-as-a-whole was not reducible to biological theory. He argued instead that the science of animal behavior was as conceptually distinct from biology as biology was from chemistry and physics, and for a very similar reason. Animal behavior, like life generally, evolves as a consequence of variation and selection giving rise to emergent properties, and thus could only be understood with psychological concepts (e.g., operant principles) rather than biological ones (see Naour, 2009).

In contrast to Wilson's version of reality, the ToK System depicts a clear demarcation between biology and psychology and thus is aligned with Skinner over Wilson in the claim that the behavior of animals with a nervous system represents a qualitative shift in complexity that is not fully reducible to biology theory. The ToK System adds the point that animal behavior mediated by the nervous system represents a qualitative shift because the behaviors are organized by a second information processing system (the nervous system, in addition to the genetic system). I associate Skinner with psychological formalism because of his view of animal behavior as an emergent level of complexity.

Given this formulation, what are the disciplines that constitute the formal science of psychology? The list would include the following: Comparative/Animal Psychology, Behavior Analysis, Ethology, Behavioral Ecology, Behavioral Genetics, Cognitive and Computational Neuroscience, Behavioral Neuroscience, Psychophysiology, Biopsychology, Psychobiology, and Psychophysics. The unified

theory further posits that Behavioral Investment Theory provides the integrative theoretical formulation to account for animal behavior.

Human Psychology

From Descartes' dualistic conception of the conscious human mind and the physical body and concomitant assertion that animals were purely mechanistic creatures that did not think or feel to Wundt and Tichtener's early conception of psychology as the science of human consciousness, a number of historical traditions have explicitly claimed that psychology only applies to humans. In contrast to these positions the functionalists, spurred on by the work of Darwin, argued strongly for at least some forms of mental continuity between humans and other animals, thus opening the door for animal psychology. With Watson (1913), the behaviorists took this position a step further and explicitly proclaimed that there was no dividing line between man and brute. And yet, more recently, the shift in the science of psychology has been back toward the human. In his critique of the current status of the field, Robinson (2002, p. 10) wrote the following:

> It might now be time to open the cages, let the birds fly south and the rats find their way back to barns and marshes. Whatever the study of non-human animals might yield at the level of fact, it is doubtful in the extreme that it will contribute significantly to an understanding of the civic, aesthetic, moral, and transcendental dimensions of human life. . .Find creatures with the power not only radically to alter the world as it is given but to do so in a deliberate manner based on a critical appraisal of themselves and of that very world, and the conditions under which an Evolutionary Psychology would be credible are simply eliminated.

As with so many other domains, the unified theory assimilates and integrates both of these seemingly contradictory positions. It achieves this by showing via Behavioral Investment Theory that human behavior is continuous with other animals, while simultaneously positing via the Justification Hypothesis that human behavior is qualitatively different than that of other animals. Thus, according to the unified theory, just as animal behavior is not reducible to biological theory human behavior is likewise not reducible to Behavioral Investment Theory and the formal science of psychology. Instead, as is suggested in Robinson's critique, individual human behavior is (a) mediated by another information processing system (symbolic language), (b) self-reflective, and (c) embedded in a macro-level cultural context of justification. Because of this qualitative shift (depicted on the ToK System as the emergence of Culture), human psychology must be separated from the formal science of psychology. In short, Watson was wrong—there is a dividing line between man and brute. Moreover, unlike basic psychology which is conceptualized here as a purely natural science, human psychology is part of the social sciences and must then grapple more with issues of value, epistemological relativism, and cultural context because of the problem of the double hermeneutic mentioned in Chapter 1.

Sigmund Freud is the icon I associate with the discipline of human psychology. It would certainly be justifiable to challenge the appropriateness of this association

given how separate Freudian theory is from academic human psychology and how much Freud originally got wrong. As I mentioned above, the association of the icons with domains is more of a pedagogical heuristic rather than a deep analytic truth claim. My primary reason for doing so is because the Justification Hypothesis aligns well with Freud's fundamental insight pertaining to the nature of human self-consciousness, which is that there are systematic motivational reasons behind the reasons that people give for their behavior. In the language of psychoanalytic theory, Freud discovered the dynamic unconscious. Freud was, of course, not the first to question the completeness of the conscious rationales people offered for their behavior. But he was by far the most influential individual in articulating the systematic nature of the relationship between conscious and unconscious thought. In essence, Freud ultimately observed that the justifications that people offer for why they do what they do could be understood as arising from the inherent tension between biopsychological drives that guide behavior and the socio-linguistic system in which the individual is immersed. This is, of course, directly in line with the Justification Hypothesis, and it is the reason I associate Freud with human psychology. I also think we should be looking to separate the psychodynamic baby from the psychoanalytic bathwater and build bridges with academic psychology so that we can move forward toward building a cumulative science of human psychology. For example, we should recognize that social psychologists' work on cognitive dissonance has many parallels with psychodynamic notions of ego defense.

I use the icons of Skinner and Freud to make an additional point pertaining to the theoretical unification of psychology. Although they are arguably the two greatest figures in the field, the two perspectives appear on the surface to be wholly incompatible. Skinner pejoratively dismissed "mentalistic" approaches and placed the focus on the causal role of the environment in the selection of behavioral responses. He also took an extreme fact-based approach to science and even questioned the need for deep theoretical constructs in psychology. The foundational database for his behavioral selection paradigm was the behavior of animals in the lab. Conversely, Freud's psychoanalytic paradigm was unabashedly mentalistic in nature. Stemming from observations of troubled humans' free-associating on a couch, Freud wove together powerful insights with wild speculations and formulated an elaborate but problematic grand theory of the human mind. Of course, both Freud and Skinner are much maligned in opposing circles and the vast majority of psychologists view each of their respective paradigms as incomplete and at least partially incorrect. Yet both Skinner and Freud remain pillars of the field, and there is not currently a way to blend the insights of the two together in a coherent fashion.

If there is in fact an elephant to be seen, a unified theory of psychology would coherently unite the ideas of Skinner and Freud within the same overarching system, clearly spelling out the errors and inconsistencies in each paradigm while retaining the key theoretical insights from both perspectives. My argument in using these icons is that the unified theory aligns the central insights of Skinner and Freud both with one another and with science at large, allowing us to finally see the elephant. Some additional graphics offered in Fig. 7.2 might help communicate this point. Specifically, the diagram on the left shows how psychology

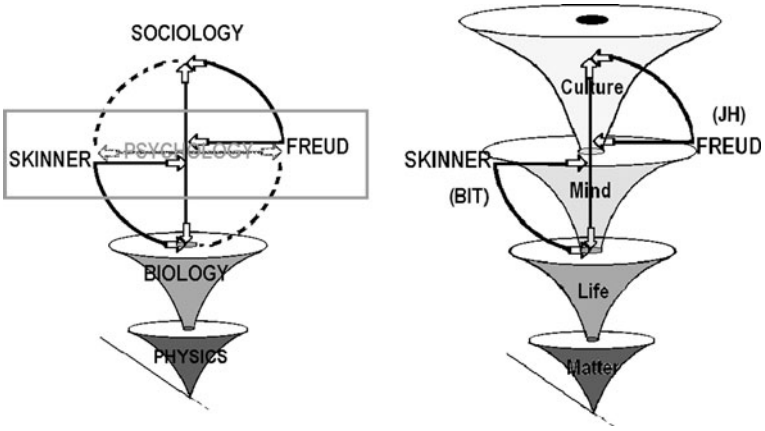

Fig. 7.2 Solving the problem of psychology with the unified theory

is currently a fragmented discipline that exists between biology and the social sciences, with psychologists on a spectrum of positions in the philosophy of science, the ends of which are occupied by Skinner and Freud. The diagram on the right represents the argument that by recasting Skinner's central insight in light of Behavioral Investment Theory and Freud's central insight in light of the Justification Hypothesis, we can align their insights with each other and biology from below and the social sciences from above and see a coherent vision of the whole.

Given this discussion and the overarching goal of developing a precise definition of psychology, the following question arises: If psychological formalism corresponds to the third dimension of complexity on the ToK System, where is human psychology? Because humans are animals, psychological formalism provides the appropriate framework to view human behavior from a bottom-up perspective. We can and should attempt to understand people's behavioral investments from the general principles derived from Behavioral Investment Theory. But it is further argued that the behavior of human objects is qualitatively different from other animals because human behavior is imbedded in a meta-level societal context and is associated with a novel information processing system, language. Thus to be complete, human psychology must effectively allow for the top-down socio-cultural perspective as well.

According to the ToK System and alluded to earlier in the context of the discussion of meta-levels, the human individual is seen as the smallest unit of analysis in the social sciences. Many share this conception. For example, Baumeister and Tice (1996) proposed a very similar formulation, although their focus was on personality. The point here in reference to the question raised is that human psychology should be thought of as existing at the base of the social sciences and should be thought of as a hybrid between psychological formalism and the social sciences. Moreover, it is human psychology that is a subset of psychology more generally. Humans are, after all, a subset of animals, rather than the reverse.

Because some may find the notion that human psychology is a hybrid discipline between psychology and the social sciences confusing, it may be useful to point out other hybrid disciplines that have quite impressive track records. Molecular genetics, for example, is a hybrid between chemistry and biology and has seen some of the most impressive scientific accomplishments of any discipline in the past 70 years. Similarly, neuroscience is a hybrid between biology and psychology, and it has also been witness to explosive growth and numerous revolutionary discoveries in recent decades. As with my proposed conception of human psychology, both of these disciplines adopt an object-level perspective (molecular and cellular, respectively) on phenomena that simultaneously exist as part of meta-level informational system processes (Life and Mind, respectively).

What would the discipline of human psychology look like? A glance at the current American Psychological Association is instructive in this regard. A perusal of the subdivisions that currently make up the APA will demonstrate a relative lack of correspondence between psychological formalism and the primary concerns of the APA. While some might be inclined to use the lack of correspondence as strong evidence that my conception of psychology is faulty, the answer regarding the lack of correspondence is found in the fact that what has happened historically to the field of psychology is that it has shifted its center of gravity from its base in psychological formalism to now locating its center in human psychology. Although this shift has occurred, the term "human" is very rarely used as a modifier, which results in semantic confusion from the vantage point of the unified theory. Nevertheless, in this regard, it becomes clear that the APA is primarily an organization of human psychology (along with the profession), rather than psychological formalism. In other words, according to this analysis the APA should technically be renamed the American Human Psychological Association. This insight offers readily available explanations as to why so many individuals in the basic psychological sciences (e.g., behavior analysis, cognitive science, biological psychology) have expressed serious objections that their interests have not been well served by the APA and have changed affiliations to organizations such as the Society of Neuroscience, the Psychonomic Society, the American Psychological Society, and the Association for Behavior Analysis.

Professional Psychology

In contrast to the relatively few proposals that have advocated for a break between basic and human psychology, much attention has been focused on the relationship between the science and the profession of psychology, and several major figures in the field have advocated that the two are separate branches in much the same way that I advocate for here. And there have long been institutional demarcations between the science and the profession. For example, the American Psychological Association only accredits programs in professional psychology, specifically programs in clinical, counseling, or school psychology or some combination therein. Likewise, professional psychologists are demarcated in society by

virtue of licensing and other public safeguards such as the requirement to maintain continuous education. Thus, the proposal to formally separate the science from the profession is not new and has already been done to some degree at an institutional level by virtue of the different regulations and competency requirements. Nonetheless, unlike the clear distinction between biology and medicine, there has not been nearly as clear a division and distinction between the science and the profession as there ought to have been and it remains yet another example of the field being plagued by confusing semantic issues.

I noted previously that the sciences are separated from the humanities because at its core the former concerns itself with questions of descriptive accuracy, whereas the latter consist of creative–expressive enterprises that inform us regarding how the world ought to be (Jones, 1965). Recalling the difference between these two cultures is important here because it highlights what underlies the fundamental distinction between the science and the practice of psychology (Henriques & Sternberg, 2004; see also Kimble, 1984). The fundamental task of professional psychology is not to describe animal or human behavior but instead is to improve the human condition. This is what makes it a more value-laden and prescriptive than the science of psychology. It is not accidental, then, that Carl Rogers is the icon I associate with profession of psychology, as he was the most influential humanistic psychologist. He argued passionately that the vision of humanity offered by both psychoanalysis and behaviorism was too deterministic, limited, and pessimistic, and that psychology could and should offer a more hopeful, uplifting message regarding human potential. I also emphasize Rogers because of the way he valued people, and that one of his foundational insights was that the quality of the therapeutic relationship is central to the psychotherapeutic processes. As I tell my students, good therapy begins with Rogers.

Donald Peterson has been one of the most prominent and vocal leaders of the professional psychology movement and has offered a vision very congruent with the one promoted here. Characterizing the field as evolving from a pre-professional phase through the scientific–practitioner phase to a full professional phase, he has articulated with clarity and precision why professional psychology was ready to establish itself as an independent field and how that identity is separate from psychological science (e.g., Peterson, 1991, 2002). Defining professional activity as discipline inquiry, Peterson specified the distinction between science and practice as follows: "Science and practice differ in fundamental ways. Science begins and ends in a body of systematic knowledge. . .Professional activity begins and ends in the condition of the client" (Peterson, 1991, pp. 425–426). Thus, the goals of the scientist are qualitatively different from the goals of the practitioner. The goal of the psychological scientist is to contribute to the fund of general, scientific knowledge. By positioning him or herself as an objective observer, the task at hand becomes fundamentally descriptive. False positives are a major concern and experiments are conducted to discard errant leads. The primary task of the practitioner is different. It is not to describe change, but to affect it, usually in terms of improvements in the functioning and well-being of the client. To the practitioner, psychological knowledge is not the end, but a means to the end. It is through the clear recognition of

this fundamental difference that the complementary roles of the scientist and the practitioner are both seen as necessary and good.

Interestingly, many seminal thinkers in the field have *not* shared this conception of the relationship between the science and the profession. O'Donohue and Halsey (1997) reviewed the views of Freud, Skinner, Ellis, and Rogers and found marked differences in the substance of their views regarding the science–practice relationship. Both Freud and Skinner had visions that were in fairly direct contrast with the science–practice relationship envisioned by Peterson and shared by the unified theory. For example, Freud saw psychoanalysis as both a therapy and a scientific process. Consistent with the claim that there are tensions between the goals of therapy and the goals of science, Wachtel (1993, p. 180) reported that Freud experienced many "epistemological anxieties" viewing psychoanalysis both as a treatment and a method for conducting research on the unconscious mind. Specifically, Freud struggled with making suggestions and giving advice in psychoanalysis in part because, although such interventions might be helpful, they nevertheless would taint the objective nature of the results revealed. Regarding this conflict, Freud (1916/1943, cited in Wachtel, 1993) wrote, "The danger still remains that our influence upon the patient may bring the objective certainty of our discoveries into doubt; and *that what is an advantage in therapy is harmful in research*."

Skinner also did not see a clear difference between science and practice. This in large part was because Skinner reduced everything to behavior and argued that the control of behavior was primary, and he also construed it as the ultimate goal of science. As a consequence, he tended to view behavior therapy as a logical extension of lab-based principles. His writings on how humans should give up the "myths" of freedom and dignity speak to his views that behavior was all. And yet, as is made very clear by Skinner's *Walden II* and the criticisms it evoked, much value-focused debate is elicited in the attempt to determine which behaviors to control and why, and one cannot use the scientific method to resolve these issues.

I argued above that because it was explicitly concerned with promoting human betterment, the profession was more value-laden and more closely connected to the humanities. Although this is true, I want to be clear that I am not characterizing the profession as a purely humanistic enterprise like literature. I view professional psychology as an applied social science and health profession. And I argue professional psychologists should be considered as scientific practitioners. The logic of this argument is seen when professional psychologists are compared to other helping professions, such as ministers or lawyers. In contrast to these professions, professional psychologists are trained in scientific methodology and anchor their knowledge base to a scientific discipline. The parallels between professional psychology with medicine and engineering offer further clarity in seeing how professional psychologists can be considered scientific practitioners. As stated by the National Council of Schools in Professional Psychology guidelines and consistent with the model advocated here, "The properly trained professional psychologist is a scientist in the sense that a skilled physician is a local clinical, biological scientist and the skilled engineer a local physical scientist" (Peterson, Peterson, Abrams, & Stricker, 1997, p. 376).

Debates between scientific and professional psychologists constitute some of the most contentious and long-standing issues within the field. One flashpoint in these debates is the conflict surrounding Empirically Supported Treatments (ESTs). In the 1990s, a movement emerged in professional psychology that practicing psychologists should employ only treatments that have been supported by scientific research. At first glance, such a proposition sounds very reasonable. After all, who would choose a treatment that was not supported by science over one that was? It turns out, however, that the issue is extremely complicated. In the following section, I articulate some of the problems associated with the EST movement, and explain how the unified theory sets the stage for a much healthier relationship between the science and the profession.

The Relationship Between Research and Practice in Professional Psychology

From 1999 to 2003 I directed a randomized controlled clinical trial exploring the efficacy and effectiveness of a brief cognitive therapy intervention for patients presenting to an emergency room following a suicide attempt. I learned many valuable, eye-opening lessons in carrying out this work. One lesson was that the magnitude and complexity of the problems these participants faced was humbling and, at times, even demoralizing. Consider that the modal number of psychiatric diagnoses was three, two-thirds had a serious problem with substance abuse, the average scores on a depression inventory were in the severe range, most were unemployed, a quarter homeless, histories of abuse were the norm, and over 70% made less than fifteen thousand dollars a year.

Another lesson was that the current system was failing these individuals. Psychiatrists would offer diagnoses and medication management, and social workers and counselors would lend a sympathetic ear and perhaps encourage patients to enter a group or music therapy program in the afternoon. With rare exceptions, professional psychologists were nowhere to be found. Brief hospital stays followed by missed outpatient appointments were the norm. My sense that there were serious problems with the ecology in which these folks lived and with the health system that was providing treatment was greatly reinforced when I discovered that the degree of psychopathology exhibited by the population of suicide attempters in Philadelphia had markedly increased over the past 30 years (Henriques, Brown, Berk, & Beck, 2004). In the early 1970s, Beck and his colleagues had evaluated hundreds of suicide attempters with the same basic instruments we were using in our outcome trial. I realized that this afforded us a remarkable opportunity for a cohort comparison—and when the comparison was made, the results were striking. On every substantive measure of psychopathology and distress, the present-day suicide attempters were worse. Perhaps the most striking and disconcerting finding was the difference in subsequent suicide attempts. Four times as many present-day suicide attempters made

a suicide attempt in the year following the index attempt than was the case in the 1970s (40% compared with 10%).

What does this have to do with the unified theory? *Everything*. Why? Because there is no current single-school approach that has the conceptual sophistication and depth necessary to address the incredibly complex and multifaceted nature of the problems associated with the suicide attempters. This is a key point because one of the major impediments to a healthy relationship between the science and the profession has been the fact that the scientific frames offered to date have been either ambiguous, wrong, or too narrow to effectively guide the practitioner. But in this case, what about cognitive psychotherapy? After all, the treatment was generated by the founders of cognitive psychotherapy and was derived from general principles of cognitive theory and therapy for the emotional disorders (e.g., Beck, Rush, Shaw, & Emery, 1979), and it was found to have a positive impact cutting the subsequent suicide rate in half, as well as reducing self-reported levels of depression and hopelessness relative to controls (Brown et al., 2005).

Although I am a strong believer that the treatment principles in cognitive psychotherapy can be useful and effective, as the individual who directly conducted or supervised the majority of the therapy, I can say with firsthand knowledge that the treatment included many additional elements that are beyond the scope of cognitive psychotherapy's focus and consequently were not included in the written reports of the project. By reviewing some of these issues, I want to help the reader understand some of the complexities associated with conducting research on psychological treatments and why the debate about ESTs is so complicated.

To understand some of the complications, let us start with the development of the study. I arrived at the University of Pennsylvania just before the grant for the suicide attempter project was awarded. Upon its award, I was placed in the role of project director, which essentially required me to assume primary responsibility for the day-to-day operations of the project. The original design of the study was to screen for eligible participants in the ER, and after obtaining informed consent, have participants who were randomized to the cognitive therapy condition assigned to therapists working at the Center for Cognitive Therapy. These therapists were well-trained and highly qualified. There became a serious problem, however, with the study design. Participants would generally not show up, or if they did show up, it was frequently at the wrong times. Moreover, we were supposed to track patients for as long as 2 years, but given their relatively chaotic lifestyles and transitive living arrangements, we quickly lost contact with many of the patients. During the first year, we brought sixty subjects into the study, with thirty assigned to the cognitive therapy condition and thirty assigned to the treatment-as-usual for a control group. Of the thirty people assigned to the cognitive therapy condition, only about one-third received four or more sessions, whereas one-third received between one and three sessions, and one third never received any treatment. We also lost track of approximately 50 percent of the patients in both conditions after the first month of tracking. Not surprisingly given these numbers, when we examined the data on the folks we did have, there were few differences between the groups.

Given all of these difficulties, we made a number of changes. Indeed, the changes were so dramatic that we scrapped what we called "Study 1" and began "Study 2," and started the data collection process completely from scratch. For Study 2, we changed the protocol for tracking participants, and assigned an undergraduate research assistant to each participant to maintain contact in order to facilitate follow-up. We also changed the way we paid subjects, changed the way we interviewed them, and especially ensured that we maintained contact as they were transitioned out of the hospital. Perhaps most importantly, we changed the basic structure by which the therapy was provided and who provided it. Instead of referring patients to the Center for Cognitive Therapy, our research unit became responsible for delivering the therapy. I personally provided therapy to many of the patients and directly supervised many others. The reason for this was because, as a researcher, I had much more flexibility in my schedule and could assume much more responsibility for getting the participants to their sessions. (I should also comment that, given my role in the project, I had a stronger vested interest in having participants attend sessions). For example, I could see the patients if they showed up on Monday when their therapy was on Tuesday or if they were in the hospital or if they had no transportation and needed to be seen at home. In addition, although I helped develop the treatment manual and certainly followed it to the best that situations allowed, it is nevertheless true that I was also trained in psychodynamic and humanistic approaches and inevitably employed lenses and techniques from these traditions in my work. There was quite a dramatic shift from the Study 1 results. In Study 2, we successfully tracked the vast majority of the patients and 75% of those in the cognitive therapy condition completed four or more sessions. And, as mentioned, the intervention was found to have a significant and substantial impact on patient functioning.

From a research perspective, these facts raise interesting points. For example, given that I provided much of the treatment, either directly or through supervision, and that I also used humanistic and psychodynamic treatment principles (by which I mean I would sometimes use deep empathy to elicit powerful emotional experiences and sometimes helped patients gain insights into defensive processes of which they were not aware), how are we to glean the extent to which these techniques influenced outcome? Moreover, from the point of view of basic research design, we did in fact have an A-B design with Study 1 representing phase A and Study 2 representing phase B. And we had good evidence that what we were doing in phase A had no notable impact, but what we did in phase B did. Interestingly, cognitive therapy was constant across both phases; what had changed was how it was delivered and who provided it. In short, we had strong data suggesting that the novel developments that distinguished Study 1 from Study 2 were central to the efficacy and effectiveness of our intervention.

And yet in the major outcome article publishing the findings, it was the cognitive therapy that was claimed to be the primary agent of change (Brown et al., 2005). And although articulated in professional presentations, none of the above factors were reported in the primary written record of the study. Why not? The answer is fairly straightforward. The complexity of delineating these additional factors, the fact that

they did not fit clearly within the scope of cognitive therapy, and the desire to promote the successes of cognitive therapy all combined to result in the biased selection of methodological reporting and framing of the results. I do not say this with any smug sense of superiority. I am proud of the work we did, what we published, and stand by it. But it remains the case that if our broad and general frame of understanding were different, we would have reported the results from the study in a different way.

For example, imagine that after the initial trouble in conducting Study 1, Beck had connected with a colleague in the Social Work Department who agreed to serve as a consultant on the condition that if the changes were undertaken and the results were successful, they would be the focus of the research report. If this had occurred and resulted in the above-mentioned changes, the write-up would have been quite different, and the message to the scientific and therapeutic communities would have been significantly altered. But given cognitive therapy's scope—which is broad but, unlike the unified theory, is not comprehensive—we only reported our findings through that lens, with the consequence being that those who read about the study are given the impression that it was a rather straightforward application of cognitive therapy when the actual reality was notably more complicated.

This would not be the first time that theoretical and conceptual frameworks employed by clinical researchers resulted in written accounts that cloaked processes that in fact were much more complicated than the language allowed for by the operating theory. Consider, for example, the story and perspective offered by Paul Wachtel. Wachtel was trained in psychoanalytic theory and, based on his reading of behavior therapy, "found it foolish, superficial, and possibly even immoral" (Wachtel, 1997, p. xix). And yet when Wachtel actually observed behavioral therapists relating to patients and conducting interviews and interventions, he found much sophistication and relational grace that was virtually never the primary focus of the written accounts. He was so impressed by what he saw that he set out to develop a theoretical rapprochement between behavioral and psychoanalytic perspectives.

What relevance do these stories have for psychology's fragmentation and the consequent need for the unified theory? On the one hand it highlights the lack of comprehensiveness of the language of current paradigms. Although some proponents of cognitive therapy have called it a "unified theory" (e.g., J. Beck, 1995), when one takes an interdisciplinary viewpoint it is clear that the frame offered by cognitive therapy is of a relatively limited scope. The lack of comprehensiveness gives rise to problems in terms of the reporting and interpreting of data, problems that are greatly exacerbated when individuals from different backgrounds and paradigms interpret the findings. The combination of incomplete paradigms, biased data reporting, and political antagonisms create a volatile brew, especially when it comes to making policy decisions about how practitioners ought to practice. This is why the debates about ESTs are so heated.

One does not need to look hard to see the struggle regarding the relationship between research and practice playing out in the field. A recent editorial in *Newsweek* (Begley, October 12, 2009) proclaimed baldly that psychologists

(note that she meant professional practitioners) ignore scientific evidence, and she explicitly called for the public shaming, stigmatization, and marginalization of practitioners who fail to employ cognitive or cognitive behavioral treatments for depression, anxiety, bulimia, PTSD, and other conditions. The editorial was based on a recent report (Baker, McFall, & Shoham, 2009), which in turn was accompanied by a strongly endorsing foreword by the noted psychologist Walter Mischel (2009). The Baker et al. (2009) report is notable mostly for the vigor and militant voice with which the authors make their case, rather than the novelty of the message. The content of the message is straightforward: Cognitive and cognitive behavioral interventions have been shown to reduce symptoms associated with many psychiatric disorders equal to or better than competitors, and, consequently, there is an ethical obligation to train psychological practitioners in these methods, and ensure that they employ them. Moreover, the argument continues, many psychological practitioners are soft thinkers, often anti-science, and tend to go on personal experience rather than scientific evidence, and this is a trend that, if anything, has gotten worse over time. Consequently, researchers, and psychological policy makers (i.e., accreditors) need to insist on better scientific training and greater conformity to the manuals that have been supported by the hard fought, scientific data gathering process.

As one who worked directly with the Father of Cognitive Therapy on a randomized controlled clinical trial and who now directs a practitioner-oriented doctoral program, I have a rather unique vantage point on this perennial arm wrestle within the field. From my perspective—a view that is informed by the unified theory and my personal experiences on "both sides of the aisle"—the resolution proposed by Baker et al. (2009) and endorsed by Begley and Mischel is doomed to failure. That is, instead of working toward achieving their stated goal of closing the gap between science and practice, it will almost certainly add to the antagonisms and splits between scientists and practitioners. The reason is quite simple. Like many clinical researchers, the authors are methodological fundamentalists and thus tend to be blind to larger issues of theory, conceptual analyses, and sociopolitical forces. By this I mean that the authors believe in the power of the scientific method to reveal divine truths that all should pay homage to, while failing to recognize that facts and fact gathering via the scientific method are only part of the equation.

In their excellent article in which they liken psychology's epistemological structure to a triangle that consists of facts, theories, and concepts, Machado, Lourenco, and Silva (2000) show in a number of different ways that psychological research is dominated by attention to fact gathering at the expense of theoretical and conceptual analyses, and this results in a number of detrimental consequences for the field. I made a similar point when I argued that psychology has serious problems with research proliferation sans conceptual consolidation. Returning to the issue at hand, consider, for example, the question of what kind of enterprise is psychotherapy? Two conceptually distinct models have been offered (Wampold, 2001). On the one hand, some argue that psychotherapy should be modeled on medicine in general,

and thus consists of specifiable and conceptually transferable diagnoses of disorders and concomitant treatments. Yet psychotherapy can just as readily be seen as primarily a relational process between two individuals with the unique nature of the relationship and the quality of the therapeutic alliance being the central defining features that result in change. Baker et al. (2009) are committed completely to the former conceptual framework and seem to be defined against the relational perspective, despite the fact that the empirical case can be made strongly for it. The situation is obviously complicated at the theoretical and paradigmatic level as well. Consider, for example, that the authors only explicitly endorse cognitive and cognitive behavioral treatments, despite the fact that there is empirical evidence for treatments that emphasize psychodynamic, interpersonal, and humanistic perspectives.

Thus, although Baker et al. (2009) frame the issues in terms of good scientific researchers committed to the truth versus feel-good anti-science practitioners that go with their gut, the fact of the matter is that the conceptual and theoretical issues are muddled and debatable, and their emphasis at the empirical level is clearly influenced by political horse racing. Yet the authors pay virtually no attention to these issues, and certainly offer nothing in the way of resolving them. Consequently, I can say with much confidence that their argument will be seen by those who have different conceptual and theoretical frameworks for psychotherapy as simply another power play, an effort to ensure the dominance of cognitive and cognitive behavioral interventions over humanistic and psychodynamic ones. As one who saw firsthand the manner in which the limitations of such frames can combine with ego investment and political aspirations to result in the somewhat biased reporting of particular data, I am inclined to agree.

I believe the unified theory can help move the field of psychotherapy toward a more comprehensive and coherent view of psychotherapy, one that sets the stage for building more effective bridges between science and practice because the scientific frame it offers allows for the assimilation and integration of numerous different major viewpoints into a coherent, comprehensive picture of people in general. This is in contrast to the Baker et al. (2009) position, which employs a frame of what is "scientifically plausible" that is narrowly associated with behavioral and cognitive science perspectives and does not effectively include scientifically grounded experiential or psychodynamic lenses. In short, much of the debate and confusion surrounding ESTs is confounded with the theoretical confusion and the horse racing between the paradigms. If a coherent unified theory of psychology were shared by both scientists and practitioners alike, the tensions between research and practice would change from fundamental disputes about paradigms and concepts to much more specific debates about what principles and processes are actually supported by theory and research in the service of human betterment. In the subsequent chapter, I articulate in greater detail how the unified theory leads to a unified approach to conceptualizing people that effectively integrates and assimilates insights from the major perspectives and sets the stage for a general approach to psychotherapy that could fundamentally alter the research–practice relationship because it offers a comprehensive view of people grounded in psychological science.

Conclusion

The goal of this chapter was to demonstrate how the structure of the unified theory can be applied to solve the problem of psychology. Psychology can be defined as the science of mental behavior and the human mind, and the professional application of such knowledge toward the greater good. The unified theory characterizes psychology both as a unified discipline and as having three branches that should be clearly delineated and guided by different philosophical guide posts and assumptions. This paradoxical solution helps us to understand why the field has resisted an effective definition for so long.

How do we move the institution forward in the direction depicted by the unified theory? I believe we should take a kind of family therapy solution to solving the problem of psychology. In many forms of family therapy, problems are identified in the roles, structures, and boundaries that members of the system have and the therapy focuses on clarifying the nature of the overall system and the specific role each member plays. I am arguing that there is a subset of psychologists, the basic psychologists or formalists, who should identify as natural scientists, whose subject matter is mental behavior, and whose charge it is to delineate the basic relationships between cognitive processes, the brain, and overt action. This of course includes animal behavioral science, but also investigations of isolated or specific aspects of human psychological functioning, such as the psychophysics of human perception. In contrast, the human psychologists are concerned with human behavior at the individual and small group levels. These theorists and researchers are concerned with what Quackenbush (2005) calls the cultural person-as-a-whole and study developmental, personality, and social psychology. Their identities should be of social scientists because in addition to being grounded in the basic science of psychology, they also need to understand the impact of culture, human social systems, and language on thought and behavior. As social scientists, there likely will be a wider variety of epistemological leanings, ranging from those who align themselves more with natural science epistemologies to those who question the values and benefits of such natural science approaches and advocate for more relativistic and value-reflective approaches. Third, there is the profession of psychology, made up of licensed individuals who are trained primarily as mental health professionals with a specifiable set of competencies shown to positively impact human functioning and well-being. Such individuals should be informed by the science of psychology to engage in the practices most likely to be effective in alleviating human suffering and enhancing human flourishing. The last point to be made is that although I am proposing clearly identifiable boundaries between the domains, this does not mean we would not have individuals working on the boundaries. For example, we need clinical researchers who work between the science of human psychology and the profession.

It is my hope that an effective definition of psychology will allow us to rise above the tendency to define ideas against one another and instead develop a more harmonious conception of the field. The chapter has been primarily concerned with definitional and conceptual issues and the institutional alignment. Although these

issues are central, as a trainer of budding clinicians, I am well aware that many pragmatic and practice-oriented issues have not been addressed. Indeed, I argued at the end of the chapter that one of the serious impediments to an effective relationship between the science and the practice is that the single schools have not provided comprehensive models for human functioning and the EST movement was entangled in politics and the promotion of cognitive and behavioral orientations over psychodynamic and humanistic ones. In contrast to this state of affairs, the unified theory opens up the possibility of a broader, more holistic perspective on personality and psychotherapy. In the next chapter, we address more directly the implications the unified theory has for how we understand and help people.