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A HERMENEUTIC EXPLORATION OF AND PROPOSED SOLUTION TO THE SCHISM BETWEEN RESEARCHERS AND CLINICIANS IN PSYCHOLOGY

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by

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ABSTRACT

Fragmentation in the field of psychology has persisted throughout its history (Slife, 2000). One example of this fragmentation is the gap between researchers and clinicians (Teachman, Drabick, Hershenberg, Vivian, & Wolfe 2012). Although many attempts have been made to bridge this gap, there is still no consensus regarding its resolution. This dissertation provides an explanation for the gap at the philosophical level and provides a method for communicating across potentially incommensurable philosophies, based on Gadamer's (1960/1989) hermeneutic opus: *Truth and Method*.

ACKNOWLEDGMENTS

During my undergraduate program at Brigham Young University, I was privileged to take courses in behavioral science from Dr. Harold Miller, who studied at Harvard under B. F. Skinner, and Dr. Paul Robinson, who holds national acclaim for his work in behaviorism. These two professors demonstrated how marvelously scientific the study of psychology could be and sparked an interest in me to further pursue that study. I later took courses simultaneously from Dr. Brent Slife and Dr. Ed Gantt, both of whom opened my eyes to the densely complex philosophical questions underlying the science of psychology. I am indebted to these four professors for planting the initial seeds of curiosity regarding the tenuous relationships among parties invested in the study of psychology and for cumulatively setting my initial course within the field. I also feel deeply grateful for Dr. Norman Roberts at BYU, whose mentorship and unexpectedly persistent support prevented me from abandoning the pursuit of a college degree entirely while struggling as an undergraduate student.

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Ultimately, this dissertation is probably best characterized as a love letter to my eternal partner, Emily. Without her, my motivation to complete it would never have exceeded the obstacles. While it is perhaps the least romantic or poetic love letter written in recorded history, I know its value to her could never be quantified.

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CHAPTER 1

THE SCIENTIST–PRACTITIONER GAP

Since the inception of psychology as a field of study, there have been divergent opinions not only about theoretical perspectives, but also about the very philosophical assumptions underlying its scientific processes. Those who point to Wilhelm Wundt as the father of modern psychology (e.g., Boring, 1950) do so on the basis of his controlled experimentation, the hallmark of traditional western science. Those who argue that Freud was the first psychologist emphasize his novel theoretical conceptualization of talk therapy, the substance of applied clinical work (e.g., Ford & Urban, 1965). This is one example among thousands of "apples and oranges" comparisons made throughout the history of the young field of psychology. Nearly all can agree that Freud and Wundt are critical figures in the history of the field, each contributing an essential building block to the foundation of a new scientific discipline. The desire to pick one as more foundational than the other only arises within the context of arguing for one philosophical framework over another. Gage (1989) referred to this ongoing argument as the "paradigm wars." The American Psychological Association (APA) Presidential Task Force on Evidence-Based Practice (2006) pointed out that a diversification of theories and perspectives is something to be celebrated and encouraged; it also carefully explicated the importance of selectivity as available theories and technical approaches multiply. Slife and Williams (1995)

also reminded social scientists of the tremendous responsibility that then falls on us as professionals to examine carefully the underlying assumptions of different perspectives.

Amid the sometimes chaotic discussion of efficacy in research and practice in psychology, it can sometimes appear that one's options are limited to either taking up arms in the paradigm wars or leaving the fight to the "theory people" and resolving to "just do what works." This apparent dichotomy has led to a significant gap between researchers and clinicians. As Teachman, Drabick, Hershenberg, Vivian, and Wolfe (2012) explained, researchers have become frustrated that their findings seem to be dismissed or misinterpreted by clinicians, leading to a failure to implement them properly, while clinicians feel that their experiences in practice are not acknowledged or captured in randomized clinical trials and other research settings. The obvious solution, as has been pointed out repeatedly in recent years (e.g., Drabick & Goldfried, 2000; Kazdin, 2008; Wolfe, 2012), is improved communication between the two sides. I propose, however, that the failure to communicate is far more significant than an unwillingness of either party to express their needs or to acknowledge those of the other. What I will explore herein is the possibility that practitioners and researchers are speaking different languages, based in different cultures, founded on unique assumptions about ontology and epistemology. I will also explore the notion that when the subject of inquiry is human beings, the ideology and morality of the researcher become critical in a way that may be absent when studying material objects. The gaps among these disparate assumptions cannot be bridged by simply agreeing to sit at a table and negotiate a compromise. Rather it requires a methodology that allows for communication between unique entities arising from cultures that may appear fully incommensurate.

Arguments do not typically happen unless there is something of value at stake. Professional psychotherapists owe their ongoing livelihood, in part, to outcome research.

Although many mental health practitioners find the idea of running a profitable business to be off-putting, preferring to think of themselves as benevolent helpers rather than as entrepreneurs, nearly all must eventually face the necessity of defending the value of their product to consumers. Healthcare policies will continue to change, public perception of the mental health field will fluctuate, and new theoretical approaches to treatment will unfold; all the while, psychotherapists must demonstrate that what they have to offer is of sufficient value to society that their services warrant compensation. It is important to acknowledge this contextual background in the analysis the paradigm wars and the "scientist–practitioner" or "researcher–clinician" gap.

The Scientist

In order to address the apparent impasse, I will first provide a historical background regarding how these divergent philosophies of science developed. John Locke is considered in many disciplines to be one of the most important thinkers and philosophical influences in the age of enlightenment. His contributions extend throughout the modern history of social and political philosophy, with sections of his *Second Treatise* (Locke, 1690b) quoted verbatim in the United States' Declaration of Independence. In the field of psychology, he is best known for his contribution to the conceptualization of the self and his proposal that all are born blank slates, or *tabula rasa*. In the realm of epistemology, Locke redefined subjectivity, or self, and intellectual historians such as Taylor (1989) and Seigel (2005) have argued that Locke's (1690a) *An Essay Concerning Human Understanding* marks the beginning of the modern Western conception of the self. Indeed, the notion that humans, as selves, are contained entities, encountering and processing information that grows into a collective body of knowledge to be tested and refined as more information is gathered, has had such great appeal that it remains as an accepted

epistemological assumption throughout many of the social sciences. Locke's (1690a) mechanistic definition of the self fits well within the realms of natural science. As scientists study the world around them, gaining understanding of the physical laws and principles that govern it, humans, as quantified, observable selves, fit nicely into Locke's epistemology. There is an external reality that all individuals are continually mapping onto their blank slates, eventually passing those filled slates onto the next generation of blank slates, so that they may fill theirs and pass them on to the next. The final goal is to discover all that can be discovered of the knowable external reality.

Locke's (1690a) epistemological understanding of the self has provided psychological scientists a way to study human beings that mirrors the way scientists have studied the rest of the natural world, and such scientific work has been undeniably productive in terms of generating data. If this discussion were centered on whether Locke's empirical tradition of science has merit, a brief demonstration of the technological advances from his 17th century world to today quickly and efficiently silences any and all debate on the subject. Amassing knowledge about the external world allows humans to manipulate it and interact with it in ways that Locke could never have dreamed, and technological advances will certainly continue in ways that surpass the most vivid imaginations of even the most creative individuals.

The paradigm wars and the scientist–practitioner gap, however, are not about whether empirical science has merit. Rather the focus of the debate is whether or not the traditional Western scientific method, based on the philosophy of empiricism, is the most appropriate scientific approach to studying human beings. Throughout recorded history, divergent epistemologies, or ways of knowing, have been explored and applied to the study of humans. Aristotle (Taylor, 1995) explained a way of knowing in which the observer and the observed

could eventually become one, thus the observer had knowledge of the subject of study. This was not to say that they literally become the same material object but rather that the truth of the subject was jointly created through its own existence and through the observer's interpretation of it. This abstract notion was supplanted by the more pragmatic explanations afforded by empirical thinkers, and empirical science as described above moved forward with little regard for such philosophical encumbrances. Observation continued to yield evidence of physical laws that governed the universe. Whereas it initially appeared that the sun revolved around the earth, better observation proved otherwise. Notions of the world believed true for centuries were deemed archaic as scientists observed and described the basic laws of physics that offered reliable and replicable explanations for natural phenomena.

The empirical scientific method uncovered information at such an astounding rate that it was only a matter of time before it occurred to Wilhelm Wundt to begin to apply it to the human mind (Boring, 1950). Over and over again scientists demonstrated that what once seemed completely unobservable and attributable only to supernatural forces (e.g., magnetism, chemical reactions, or meteorology), in fact simply demanded better means of observation. Wundt is credited as the first to attempt to observe and develop explanations scientifically for mental disorders and abnormal behavior (Boring, 1950). He logically extended the philosophy of empiricism and applied it to humans, hoping that perhaps it would enable him to discover the structures within the brain that were responsible for sadness, anger, psychosis, love, or religious beliefs. Although there have been some arguments along the way, Wundt's pursuit is still being championed, as neuroimaging techniques provide ever increasing information about the structures in the brain and their relationships to emotion, cognition, and behavior (e.g., Moran & Zaki, 2013).

All the while, philosophical debates have not ceased regarding what many perceive to be a fundamental difference between studying the external world and studying human minds. While physical human bodies may, in fact, be subject to the same physical laws and principles that govern the rest of the universe, many hold (e.g., Buber, 1970; Cushman, 1990; Hillman & Ventura, 1993; Martin & Sugarman, 1999) that there is something unique about us as volitional beings. Whereas to date no one has ever observed a falling object that suddenly chose, of its own free will, to stop falling and hover in midair, there are countless examples of human beings who, by all natural explanations, should be depressed and simply choose not to be. Empiricists would argue that there is a pragmatic and parsimonious explanation for these exceptions to the rules. They simply have yet to discover the physical laws that will eventually prove that these individuals are merely following rules that will then be evident. Just as a bird in flight is not choosing an exemption from the law of gravity, but rather employing other physical principles that suspend it in air in a manner no more mysterious than climbing a ladder, empiricists will eventually demonstrate that no one chooses exemptions to any physical laws. What once appeared to be a willful determination to overcome the effects of neurotransmitters in the brain will eventually have simple physical explanations akin to the principles that allow a 700,000 pound 747 jumbo jet to soar through the air. When studying the human mind, however, there is increasing evidence that "the quest to understand and predict human behavior will as likely require patience with complexity as a desire for parsimony" (Cauce, 2011, p. 228).

Modern psychological science is largely based on the assumption that such complexity must be studied using the empirical scientific method. As Slife and Williams (1995) pointed out,

> Psychology developed during the period of history (the late 19th century) when logical positivism was the dominant force in the natural sciences, and so researchers in

> psychology and other behavioral sciences adopted that view. . . . In psychology, unlike the natural sciences, researchers settled on methods before they developed their questions—that is, they did not decide psychology was a science because they were faced with questions that seemed to require a scientific method to answer. Rather, psychologists seem to have first made the decision to use scientific methods and then framed their disciplinary questions according to what could be studied using that method. (p. 179)

It was this fundamental difference in the initial approach to developing a human science that led to the current paradigm wars. The appropriateness of the traditional scientific method is not questioned in the natural sciences, because the object of study is not one in which "why" holds relevance. As Brown University professor of biology Kenneth Miller (as cited in Slack, 2007) defined science, it is the systematic attempt to

> provide natural explanations for natural phenomena. . . . Explanations must be limited to things that can be observed, tested, and verified. Everything in science is open to critical examination, replication, peer review, and discussion by other scientists. I could never publish a result saying I had made an observation on a particular protein, without also telling people what my methods were and how I made that observation. (p. 32)

Miller then offered this bold statement: "And this is not *my* definition of science. It is *the* definition. I think science might be the closest thing we have on this planet to a universal culture, and these rules apply everywhere" (as cited in Slack, 2007, p. 32). Such boldness is not questioned in the natural sciences, because the scientific methods he described do, in fact, appear to be perfectly suited to the pursuit of knowledge regarding the material world. As Slife and Williams noted, these scientific methods seemed so failsafe and beyond reproach that early

psychological scientists determined there was no need to go through a process of analyzing the philosophical assumptions upon which they were based and what the implications of those assumptions might be if they were applied to humans. These early researchers had, instead, a goal of intentionally distancing their discipline from the fields of philosophy and theology and thus determined that those things that could not be observed and tested through the scientific method simply would not be a part of psychological inquiry. Rather than setting out to come to as complete and thorough an understanding of the human mind as possible, the primary goal was to be scientific and to be considered a legitimate science by the surrounding scientific community. In 1950, Boring noted, "Even today, psychologists have not ceased to be self-conscious about the scientific nature of psychology" (p. 320). Nearly 65 years later, this same self-consciousness—this willingness to elevate being considered scientific by one's peers as a higher priority than developing a unique form of science appropriately equipped to analyze concepts such as compassion, guilt, respect, deceit, or altruism—continues to act as a primary influence on the course current researchers pursue in the development of research methodologies for psychology.

The Practitioner

The path of psychologists who practice psychotherapy has been very different from that of academic psychologists, and the scientist–practitioner gap has been evident from the very beginning. Sigmund Freud is generally accepted as the first person to engage in psychotherapy and to attempt to develop a psychological theory based on his clinical experience. Whereas Wundt relied on an empirical epistemology to develop a theory based on careful observation and experimentation through the scientific method, Freud emphasized a rationalist epistemology, developing his theory using his own reasoning, making assumptions, through the use of careful

logic, about those things he could not observe. Freud took notes as his clients shared their thoughts and feelings, and he began to develop a conceptualization of what he believed must be going on in the mind on both a conscious and an unconscious level. Although the id, the ego, and the superego could never be observed, Freud was careful and methodical as he attempted to draw a map of the mind that represented what he was hearing from his clients (Rychlak, 1981). This was nothing that could be replicated; it was not based on objective observations and there was certainly no way to prove or disprove his theories. Instead, their merit was in their ability to guide the practitioner, who then must use intuition and judgment to apply the theory in a way that could help the client. By Kenneth Miller's definition (Slack, 2007), this was not science. However, Miller's definition rests firmly on the assumption that empiricism is not *a* philosophy of knowing, it is *the* philosophy of knowing.

Whereas Locke is credited as the founder of the empirical tradition, the rationalist tradition has two different points of origin with philosophers Immanual Kant and René Descartes (Slife & Williams, 1995). Kant (1781/1998) proposed a theory in direct contrast to Locke's *tabula rasa*. He believed that humans are not born as blank slates, but that all come into the world with certain pre-programmed categories for understanding and interpreting the environment: what he called *a priori* knowledge. He postulated that all people seek to make meaning out of their experiences by intuitively comparing and categorizing them according to this a priori knowledge, and he further theorized that interpretation is unique to each individual. In other words, all people are seeing the same world, but they are looking at it through their own individualized lenses, and thus they may each look at (or listen to, or think about, etc.) the same external stimuli but interpret those stimuli differently.

To explain his theory further, Kant (1781/1998) used the term *noumena* to refer to the real world as it truly exists, independent of interpretation, and the term *phenomena* to refer to each individual's unique experience of the world, interpreted through his or her own lens of understanding. He asserted that although external, independent realities—the noumena—exist, human beings can never access the noumena, as their observations and experiences are always filtered through their interpretive lenses. Thus, they experience phenomena, not noumena, and they are responsible to use their own reasoning and logic to create meaning and develop understanding based on those phenomena. The process of knowing involves the careful interplay of observations and reasoning, pairing a priori knowledge with experiences in order to come as close as possible to understanding external truth (Rychlak, 1981).

Descartes' argument against empiricism began with his observation that our senses could be fooled, such as with optical illusions or magician's tricks (Gentile & Miller, 2009). He reasoned that sensory observation could not be relied upon as our primary way of gaining knowledge if those senses are subject to such obvious misinterpretation. He determined that a better course was directly to confront all observations and logically determine if there could be any possibility that they were false. If he could reason that an observation could be untrue, then he must dismiss it as an uncertainty on the basis of this reasoning, regardless of what his senses might be indicating. As Descartes applied this process of reasoning and doubting to various topics, he eventually noted that the only thing he could not doubt was the fact that he was doubting. He therefore determined that the only undoubtable truth was his own reasoning, and he is thus considered by many to be the founder of rationalism (Rychlak, 1981).

These three systems—Lockean empiricism, Kantian rationalism, and Cartesian rationalism—were the primary epistemologies upon which early psychological theories

developed. Behavioral scientists such as Wundt, Watson, and Skinner followed the empirical tradition as humanistic clinicians such as Adler, Rogers, and Yalom followed the rational tradition, all of them trying to build on or improve on the work of their predecessors and each seeking to develop a theory for understanding human beings that would yield the greatest contribution to improving mental health. The gap between the two groups began with the choice to follow these different epistemological assumptions. The gap grew as those assumptions faded into the background, and the empirical researchers accused the clinicians of being unscientific while the rationalist practitioners accused the scientists of failing in their attempts to operationalize human experience and in their attempts to provide adequate conceptualizations that were useful for real clients.

Accusations and name calling are strong indicators that morality plays a key role in the scientist–practitioner gap. Whereas values and moral implications may be relatively unimportant in observing and manipulating the physical world, when psychologists follow Wundt's course and turn their focus of scientific inquiry on human beings, *why* suddenly becomes salient in an entirely new way. For example, empirical science has led to the invention of smart phones, which allow one to communicate with others, access media, and even navigate through the world using global positioning satellites. Science has answered the *how* in order to develop this amazing technology. One doesn't ask if it is morally right for the technology to do what it does. *Why* smart phones work is so irrelevant that the question almost seems nonsensical, as though surely when I ask *why* they work, what I really mean to ask is *how* they work. All one needs to know is *how* in order to keep producing them, as well as to take that technology and build on it as engineers and others find increasingly novel applications. In psychology, empirical science has yielded treatments that alleviate the symptoms of depression, whether through psychotherapy

or through medication. Neuroscientists understand how neurotransmitters regulate neural activity that correlates with emotions. If I have uncommonly low levels of serotonin in my synaptic gaps, I am more likely to feel lethargic and apathetic. If I take a medication that prevents the reuptake of serotonin into the neurons, thus increasing the amount available in the synaptic gaps, the lethargy and apathy are ameliorated. Just as it has in the physical world, empirical science has proved successful in providing the *how*.

Many psychotherapists, however, insist that *why* is of equal importance when seeking to help human beings improve their quality of life (Adams, 2005; Christopher, 1996). Why was my client feeling lethargic and apathetic, and why might it be better for him to not have such feelings? Is it morally right for me to help alleviate those symptoms? In most cases the answer seems obvious, but if my client is feeling lethargic and apathetic after years of physically abusing partners, suddenly *why* becomes a more complex issue than simply *how* to alleviate his symptoms of depression. Perhaps giving him the energy and motivation he lacks will allow him to break out of his current isolation, find a new partner, and return to his previous lifestyle of abuse. I know how to reduce his symptoms, but now I find myself wondering if it matters *why* I might reduce them. I would likely determine that there is a preferable course of action with this client, one that is morally superior to the goal of reducing his presenting symptoms. Based on my personal beliefs and values, I may determine that working with this client to help him begin to question his behavior and break down his beliefs about interpersonal relationships will lead to a better life, although it is likely that doing so will initially cause him to feel even more depressed than before. I will leave the realm of neurotransmitters and symptom checklists in order to focus on beliefs, values, respect, compassion, joy, and meaningful interpersonal relationships.

Such concepts, although well outside the breadth of study for traditional empiricist science, were firmly situated within the study of human behavior in past cultures. The central tenet of Aristotle's (1953/1976) philosophy was that the ultimate goal in life is happiness, and that happiness comes through living a virtuous life. According to Aristotle's reasoning, happiness is achieved as one engages in "a virtuous activity of the soul . . . or if there are more virtues than one, in accordance with the best and most perfect kind" (pp. 75–76). As Greek philosophers attempted to deepen their understanding of human behavior, their focus was on human experiences, and they developed methods to analyze and evaluate experiences (precisely the reverse of the process Slife and Williams, 1995, described occurring at the inception of modern psychological theory).

To further explain the practitioner's argument, I will describe a very truncated example of developing an empirically supported treatment for depression. In order to study depression empirically, a researcher must first operationalize it into external (observable) attributes. He or she must then select a sample to study in which he isolates this operationalized definition of depression, so that when he applies different treatments his results are not confounded by interference from other disorders. He carefully does so, running randomized clinical trials until he has established which treatment is most effective. He then provides these results to the clinician, indicating that this is the best treatment available. He becomes confused when the clinician gives him a blank stare as though he were speaking a foreign language. The clinician then points out that (a) no matter how reliable the researcher's checklist for depression may be, there is no way to demonstrate its validity. The list of symptoms and attributes is based entirely on either self-report or on observations made by someone who is not experiencing the depression. The self-reports rest on the assumption that someone who is depressed can be relied

upon to give an accurate and objective assessment of his mental state, and the observations rest on the assumption that others can objectively discern someone else's feelings; (b) when the researcher excluded anyone from the study who had any comorbid disorders, he effectively determined that this experiment would be applicable for approximately no one who ever walks into the clinician's office; and (c) the clinician has been trained in an entirely different theoretical model, and does not have the time or money to attend the training that would be required to administer the treatment in the way the researcher has indicated, especially given points a and b. The researcher accuses the clinician of being unethical for refusing to utilize the most effective empirically supported treatment, and the clinician accuses the researcher of being clueless about what treatment even is, because his experiments bear no resemblance whatsoever to the real life experiences of the people who seek psychotherapy. At this point the two would likely choose to sever their relationship completely, except for the shared context that keeps these two groups together: compensation for services.

The Motivation to Communicate

As the field of psychology developed through the 20th century, it tended toward research models already familiar to third-party reimbursement providers: those used by the medical field. The ability to provide data generated from randomized clinical trials was reassuring to such providers, as experimental designs yielding empirical demonstrations of relative efficacy fit nicely into established formulae for determining reimbursement schedules. Researchers in the field of psychotherapy outcome honed the ability to standardize treatment and the measurement of its effects. As the diagnostic manual expanded, mental illnesses were reduced to easily observed behaviors on checklists that increased in reliability, if not validity.

This simultaneous expansion of diagnoses and reduction of subjectivity was celebrated by some while decried by others. Utilizing the methodology of the traditional *hard sciences* earned psychotherapy researchers a seat at the scientific table, as they began to produce data that appeared very similar to those produced in the field of medicine, as mentioned above. For those who had worked so hard to overcome the label of *soft science*, this was a fulfilling development. Others, however, raised questions about what might be sacrificed in the choice to sit at such a table (Slife & Williams, 1995).

Psychologists, as the etymology indicates, originally sought to study the psyche, or soul. The field of neuroscience emerged as the subject of study transitioned from the soul to the mind to the brain (Cauce, 2011). Those who embrace traditional empirical science have abandoned the study of something so unobservable as the soul, while others have expressed a need to examine more closely the philosophy of science before allowing a predetermined theory to restrict what psychologists can and cannot study. The simple decision to choose to adopt a well-established methodology from another discipline carried with it enormous implications for the development of the field and has played a significant role in psychology's ongoing fragmentation (Slife, 2000).

Researchers extolling the value of bias-free, empirical data shake their heads in confused frustration at those who value intuition or other unreplicable methods of seeking truth (e.g., Goddard, 2009), while clinicians touting the need to expand available paradigms in the social sciences express exhaustion with those who refuse to see empiricism as one epistemological theory among many (e.g., Riger, 1992). B.E. Wolfe (2012) added a level of complexity by acknowledging that he is both a researcher and a clinician and entertaining his own internal paradigm war in a recent special section of the journal *Psychotherapy* dedicated to the research–

practice gap. All of this methodological development has led to the reason the clinician and the researcher in the vignette above find themselves unable to sever their tenuous relationship. Clinicians want to be paid for their services, and know they will not get paid unless they can demonstrate that what they are doing is somehow different from a mystic healing ritual. They know they need the researchers to validate their work as scientific, and the researchers know they need the clinicians to apply their findings and show them to be effective in practice if they are ever to get another grant for more research. And so a researcher and a clinician work out an uneasy compromise as each attempts to make some small adjustments in their course based on the other's feedback.

The Gap

What I am proposing, however, is that the clinician and researcher are in fact speaking different languages. These attempts to make a compromise, to attempt to communicate and negotiate how each can better meet the other's needs, or even to re-envision dramatically the whole enterprise will continue to fail in the absence of the acknowledgement that each side represents a culture with completely unique values, based on different epistemologies and making different ontological assumptions about humanity itself. Communication across such a divide will require a methodology developed specifically for facilitating understanding across divergent cultures, contexts, and perspectives. Before proposing such a solution, I will briefly examine some of the previous attempts at bridging the gap, with the hope that an understanding of how other solutions have been unsuccessful will lead to a better vision of what a successful solution might require.

Previous Attempts at a Solution

Training Scientist–Practitioners

The first large-scale effort to bridge the gap between researchers and clinicians was the Boulder Conference on Graduate Education in Clinical Psychology, held in Boulder, CO in 1949. The aim of the conference was to develop a new training model for clinical psychologists, such that clinicians would be well educated in research methods and procedures and could fluently transition back and forth as both scientists and practitioners (Raimy, 1950). The well-trained graduates of this newly formed scientist–practitioner training model, also frequently called the Boulder model, would be able to make clinical decisions based on current scientific findings, to provide clients with the best scientifically validated tools and techniques, and to conduct their own research based on their ongoing clinical practice. The proponents of the Boulder model recognized that it was tremendously ambitious, thus they presented it as an ideal to which training programs should aspire, while maintaining the belief that a diversity of different models would allow for "the continued possibility of experimentation with new methods of education to the end that quality and vitality are not sacrificed for uniformity" (Raimy, 1950, p. 30).

In the three decades following the Boulder Conference, many psychologists argued that the scientist–practitioner training model was far too idealistic and that it oversimplified both research and practice in a naïve way that was doomed to fail from the start. One of the primary arguments is that the fields of scientific research and clinical psychology are so vastly different that it is incredibly rare to find someone who is sincerely interested in both. Meehl (as cited in Clark, 1967) noted that "the correlation between scientific interests and 'helping' interests is at best negligible and may actually be negative" (p. 55). In 1979, Cohen conducted a survey that

indicated psychotherapists not only were not interested in doing scientific research but also were not even particularly interested in reading scientific research and placed far more value on personal communication with fellow practitioners than on reading journals or other scientific publications. George Frank (1984) explained,

> It can be assumed that a clinical psychologist of any vocational persuasion—a research worker, teacher, or clinician—is interested in the study of human beings. What would appear to differentiate one psychologist from another is that one (the scientist) wants to find generalities and universalities in behavior, whereas the other (the humanist) wants to discover information about *a* person that can be used to help that person. (p. 420)

These observations are but a brief representation of the myriad negative responses to the Boulder model based on the impracticality of training one individual to be both scientist and practitioner (e.g., Albee, 1970; Albee & Loeffler, 1971; Clark, 1957; S. W. Cook, 1958; Hughes, 1952; Levy, 1962; Meehl, 1971; D. R. Peterson, 1968, 1971, 1976; Raush, 1974; Strupp, 1976, 1982; Tryon, 1963).

Another primary objection to the Boulder model is that it ignores important philosophical differences embedded in the goals of researchers and clinicians. Albee (2000) asserted that the primary downfall of the model is not in the idea that clinicians should be well trained in research and science but rather in the "uncritical acceptance of the medical model" (p. 247). Albee took a hard socio-political stance against the medical model, arguing,

> There are major political differences between a medical/organic/brain-defect model to explain mental disorders and a social-learning, stress-related model. The former is supported by the ruling class because it does not require social change and major readjustments to the status quo. The social model, on the other hand, seeks to end or to

> reduce poverty with all its associated stresses, as well as discrimination, exploitation, and prejudices as other major sources of stress leading to emotional problems. By aligning itself with the conservative view of causation, clinical psychology has joined the forces that perpetuate social injustice. (p. 248)

Albee took an aggressive position in the argument based on his rejection of one of the primary tenets of empirical science: atomistic reductionism. Locke's conceptualization of the self as a contained, isolated blank slate was the building block for treating human conditions from a reductionist perspective. From this perspective, mental illness can be reduced down to basic causal factors, such as chemical reactions within and among the neurons in the brain. These reactions form the beginning links of a causal chain and ultimately manifest as psychological disorders. In order to cure the disorder, one must apply empirical science toward the discovery of the atomistic building blocks, then learn how to repair the broken parts of the chain in order for the human machine to be restored to health. The medical model provides the perfect blueprint. When a patient is in pain, a doctor simply has to discover the organic cause of the pain. Through science, researchers discovered how to assess and treat a bacterial infection, for example, at a microbiological level. The assumption against which Albee is arguing is that psychological distress can and should be treated the same way. It is interesting to note that the resistance to this reductionistic approach to psychology has even extended beyond the social sciences. As Pollan (2006) observed,

> The problem is that once science has reduced a complex phenomenon to a couple of variables, however important they may be, the natural tendency is to overlook everything else, to assume that what you can measure is all there is, or at least all that really matters. When we mistake what we can know for all there is to know, a healthy appreciation of

> one's ignorance in the face of a mystery like soil fertility gives way to the hubris that we can treat nature as a machine. Once that leap has been made, one input follows another, so that when the synthetic nitrogen fed to plants makes them more attractive to insects and vulnerable to disease, as we have discovered, the farmer turns to chemical pesticides to fix his broken machine. (pp. 147–148)

Albee's argument is that we are not broken machines, but rather we are interconnected organic components of a broken system, and that we will undoubtedly fail in the goal of improving mental health if we continue to look at the problem as though there is a broken piece within the individual, rather than looking for ways to equip clients with the tools they need to cope with and ultimately fix the social influences that are the true source of mental distress. This fundamental disagreement about the very existence of mental illness is one of many manifestations of the distinct philosophical assumptions guiding the paradigm wars. If scientists and practitioners do not agree on such basic conceptualizations as the definition of mental health, it will be challenging, to put it lightly, to train potential psychologists to be skillful producers in both fields.

There are, of course, some who continue to defend the Boulder model, specifically because of the very spirit in which it was presented, i.e., it presents an ideal in which clinicians are scientifically savvy and capable but allows for diversity in the way that ideal is realized according to an ever-changing body of knowledge in the fields of psychology, pedagogy, and research (Raimy, 1950). Recently, psychologists such as C. Peterson and Park (2005) and Stricker and Trierweiler (2006) have proposed conceptualizations in which the spirit of the Boulder model is maintained and embodied in novel training models, utilizing the knowledge and experience amassed over more than half a century since the 1949 conference. What remains

absent in these proposals, however, is any attempt to remediate or even to address the problem of the dramatically different worldviews, values, and goals of scientists and practitioners. I therefore agree with Albee's (2000) assertion that the Boulder model, and its accompanying goal of training psychologists to be both scientists and practitioners, is fatally flawed.

Learning to Translate

A second category of proposed solutions is that of acknowledging the great divide between scientists and clinicians and therefore seeking to bridge the gap through improved communication. This approach is manifest in various different forms that I have grouped into four categories: (a) models and conceptualizations for how researchers and clinicians might collaborate more effectively in general (e.g., Beutler, Williams, Wakefield, & Entwistle, 1995; Kanfer, 1990; Vivian et al., 2012; Wolfe, 2012); (b) researchers attempting to make their empirically supported tools more user friendly for clinicians (e.g., Adelson & Owen, 2012; Lambert, 2012; Lochman et al., 2012; Youn, Kraus, & Castonguay, 2012); (c) clinicians providing examples of complexities that arise in the practice setting to researchers, so that scientists can study problems specifically relevant to applied practice (e.g., Price & Anderson, 2012; Stricker & Trierweiler, 2006; Walling, Suvak, Howard, Taft, & Murphy, 2012; Westra, Aviram, Connors, Kertes, & Ahmed, 2012); and (d) researchers seeking to develop improved training models for graduate study in psychology (e.g., Hershenberg, Drabick, & Vivian, 2012; Snyder & Elliot, 2005). I will provide a more thorough analysis and critique of these various attempts to communicate later, but here I will simply provide a very general observation that all of them rest on the assumption that the traditional empirical tradition is correct and that the goal is to see how one can fold alternative science traditions into it so that they can be studied empirically. Evident throughout is the presence of epistemological privilege, with traditional

scientists being sensitive to and tolerant of participants of alternative cultures of science, opening their doors to others, but only to help teach them their ways and integrate them into their systems. I suggest this is not a collaboration at all but rather an invitation to engage insofar as those who are different are willing to adapt their practices to fit within the dominant philosophy.

Starting from Scratch

Within the phenomenological tradition, there have been many small steps taken toward developing the science of psychology from scratch, that is to say reversing the process such that researchers first look at the human mind and formulate questions and determine what it is they are trying to study and then develop methodologies and methods appropriate for discovering those answers. Two significant works to that end are Giorgi's (1970) *Psychology as a Human Science: A Phenomenologically Based Approach*, and Richardson, Fowers, and Guignon's (1999) *Re-envisioning Psychology*: *Moral Dimensions of Theory and Practice*.

In Giorgi's (1970) *Psychology as a Human Science*, he clearly stated his intention to work out a solution to the scientist–practitioner gap:

> both the term "human" and the term "science" are important to us. We would insist upon the relevance of the term *human* to those who want to build a psychology of the human person according to the conception of science as developed by the natural sciences and who adhere rigidly to that concept despite changes in subject matter. We would insist upon the relevance of *science* for those who want to study the humanistic aspects of man without any concern for method or rigor whatsoever. . . . As I see it, the solution lies precisely in extending and deepening the very concept of science itself so that science is not committed to only one set of philosophical presuppositions. (pp. vi–vii)

Giorgi set about this task by outlining key differences between the natural sciences and what he envisioned for a human science. What he hoped to achieve was simply to carve out the space needed for human science to develop and establish its legitimacy based on the discoveries such an unencumbered science might make. He argued that the broad fields of philosophy and natural science had both laid claim on the subject matter of human beings but that both were ill-suited to investigate fully the subject. He repeatedly acknowledged throughout that he did not yet have all the answers and did not pretend to have discovered a solution but was instead arguing for a unique approach that he hoped would be better equipped to yield answers and solutions than either relying exclusively on natural sciences, or abandoning the aims of science altogether.

Thirty years later, Giorgi (2000) revisited his conceptualization of a human science and noted that

> a big stumbling block for psychology is the success that the sciences of nature have had. It seemed obvious that all psychology had to do was imitate the approach and practices of the natural sciences for it to achieve the same success. We have done that throughout the modern era, but the same extraordinary success has not followed; only partial successes ensued. (p. 63)

His frustration centered on the fact that the natural sciences can and do produce important information about human beings and yet they do so inefficiently and incompletely, owing to the simple fact that they were not designed to address human phenomena. Humans, of course, are made of chemicals, engage with the principles of physics, and employ biological constructs and therefore the sciences of chemistry, physics, and biology, including their many subfields, will have important things to say about certain properties of being human. He insisted, however, that until psychology is clearly defined and established as its own unique human science, its attempts

to interact and collaborate with the natural sciences would continue to be obfuscated by misunderstandings about subject matter and domain.

Re-envisioning Psychology (Richardson et al., 1999) explores one of the primary human phenomena for which the natural sciences have proven woefully inadequate: morality. Richardson et al. (1999) brought to the forefront of conversation an element of social responsibility that lies peculiarly well hidden in much of psychological theory and practice, given that psychologists are advising their clients to choose certain ways of living over others. Such advice clearly presupposes that those ways of living are better for their clients and for society in general. Thus, psychology is inherently a moral enterprise in which psychotherapists are tasked with guiding their clients toward good choices. These authors began their analysis of the morality of psychology by looking at how empiricism (the underlying epistemology of the natural sciences) and social constructionism (a popular alternative to empiricism in the social sciences) both make certain assumptions about morality that, when elucidated and made explicit, yield unsatisfactory frameworks for an enterprise that is so dependent on a solid moral foundation.

As noted previously, empirical science is based on the assumption that the human self is an isolated blank slate and that the acquisition of knowledge happens objectively, through careful observation using the external senses (sight, sound, touch, taste, and smell). Empirical science focuses exclusively on the *how* of truth, to the deliberate exclusion of the more subjective and unobservable *why*. Whereas the scientific method can produce massive amounts of information, it cannot be used to assess the worth of that information (Richardson et al., 1999). Empirical science, by its very definition and through careful design, eschews value judgments and seeks to provide objective, value-free data about the world. If psychology, then, were to be based on

empirical science, it could provide purely objective data, but it could not provide answers for how to apply the data in a moral way. Everyday questions faced by clinicians and their clients such as "Why is suffering bad?" "Why should I put myself first given these circumstances?" or "Why should I take this medication?" cannot be evaluated empirically, despite data that might provide information regarding the possible consequences of different choices in each case. Empiricism deals with universal laws and generalizations, and thus fails to provide guidelines for addressing the novel context and specific nuances within each situation that might sway the client to choose one path over another.

The appeal of social constructionism lies in its proposed solution to this lack of contextual morality. Social constructionists hold that universal truths and laws are illusory, and that all truth and meaning is constructed by humans given their own unique, culturally embedded interpretations (Richardson et al., 1999). Thus there is no universally correct answer to the question of why one should choose one behavior over another. What may be immoral in one culture could be perfectly acceptable in another, based on the shared beliefs and attitudes within each culture. Unfortunately, the contextual morality of constructionism begins to unravel as one explores the boundaries of the definition of culture. Among the growing number of humans who value liberal individualism, the intersection of multiple cultural influences within any given individual quickly leads, most often, to a culture of a single, isolated self. If we are to accept the value neutrality proposed by social constructionists (ignoring, for the sake of this argument, the logical incongruence of placing value on value neutrality), relying instead on cultural contexts to guide moral decisions, and given that every unique individual ultimately embodies a distinct culture of one, morality becomes completely relative and meaningless (Adams, 2005; Buber, 1970; Christopher, 1996; Cushman, 1990).

Re-envisioning Psychology (Richardson et al., 1999) presented both of these frameworks for morality as untenable bases for theory and practice in psychology and proposed ontological hermeneutics as a philosophical option for escaping both the absolutism of empiricism and the relativism of constructionism. The hermeneutic approach to dialogue is one in which two parties who have unique perspectives, contexts, and historical worldviews can seek understanding through a genuine engagement with one another. Adams (2005) and Richardson et al. (1999) presented hermeneutic dialogue as a means of addressing morality in psychology without becoming either dogmatic about one particular set of values or relativistic and dismissive about the power and influence of one's values. In doing so they, along with Giorgi (1970), called for a new approach to psychology, urging psychologists to consider their arguments and abandon an epistemological foundation for the field that they perceived to be ineffective at best and, at worst, socially irresponsible and destructive.

The goal of starting from scratch and creating a unique form of science better suited to producing knowledge about the human psyche has grown in momentum and inertia for several years. Those supporting this cause see empirical science as holding epistemological privilege in the field and see themselves as fighting for equity and justice in a manner paralleling the fight for civil rights by any other oppressed group (Guba & Lincoln, 2005; Keeley, Shemberg, & Zaynor, 1988). As such, their arguments are compelling and it can be easy for those who encounter this movement to take one of the steps that human beings generally take when they encounter oppression. That is to say, dismiss it as relatively unimportant and try to avoid acknowledging the problem or take sides and fight with varying levels of passion to persuade others to see why their position on the matter is superior. Others seek to communicate and negotiate a peaceful coexistence. What almost never happens in such confrontations is a genuine engagement

between sides, in which the goal of the interaction is to deepen one's understanding of and compassion for the other. On the part of the oppressed group, such an approach can seem offensive. One would feel extraordinarily uncomfortable encouraging an enslaved people, for example, to seek a deeper understanding of and compassion for those who enslave them. On the part of the privileged group, the approach of genuine engagement is far more appealing on the surface, but when it becomes clear that the culmination of such engagement is likely to include the disintegration of the power differential and thus end the existence of the privilege this group enjoys, the motivation to stay engaged in such a complicated and challenging endeavor is nearly impossible to sustain. As I have briefly indicated, and will explain in more depth in subsequent chapters, attempts at communication and collaboration between opposing sides in psychology's paradigm wars have not taken the form of a genuine engagement, which is why the field remains mired in a pre-paradigmatic (Kuhn, 1962) and fragmented state (Slife, 2000).

Statement of the Problem

I have presented a brief summary of numerous arguments that have been made over the past several decades to address the paradigm wars in the field of psychology. The arguments have taken many forms, including explaining the benefits of selecting one philosophy of science over another as the foundation for psychological science, suggesting how one group might better inform the other of their perspective, or persuading doctoral training programs to better prepare psychologists to understand both the science and the practice of psychology. I have briefly explained the development of competing philosophies regarding how one comes to know truth (epistemologies) in the context of the field of psychology. Because the most basic aim of science is to discover truth, it can be somewhat frustrating to acknowledge that there is no way to prove scientifically that one epistemology is correct. To provide scientific evidence, one must

adopt an epistemology that guides the selection of methods. It is therefore impossible to scientifically investigate an epistemology without first assuming that the epistemology guiding the investigation is valid. This bootstrap problem, or the inability to validate one's position without first assuming the position is true, is well known among philosophers of science (Sellars, 1963). What is left, then, is the responsibility to adopt an epistemological approach to science without the ability to rely on science to guide that selection. Epistemologies must be accepted on faith, intuition, instinct, and other such scientifically abhorrent means. Thus the paradigm wars are ultimately wars between competing faiths, and yet, in the field of psychology, they have been fought on the assumption that they are competing sciences. I propose that, as faith-based assumptions, no paradigm is capable of being proven right or wrong, and therefore the goal of demonstrating the superiority of one epistemology over another, or trying to prove that one's faith is true, will continue to be a fruitless enterprise. I propose that, instead, psychology would benefit from turning to methodologies and methods that have been developed with the explicit purpose of communicating and connecting across disparate cultures, contexts, time periods, and worldviews. I recommend utilizing Gadamer's (1960/1989) methodological hermeneutics to analyze and contextualize the philosophical differences exhibited by the different stakeholders in psychology and to demonstrate how this analysis might provide the tools for a novel solution to the paradigm wars and would allow psychologists successfully to bridge the scientist–practitioner gap.

Purpose of the Dissertation

The purpose of this dissertation is to provide a unique solution to the problem of fragmentation in psychology in general and more specifically to the ongoing divide between researchers and clinicians in our field. In order to develop this solution, I will first perform a

historical analysis of the inception and growth of the scientist–practitioner gap over the course of psychology's development as a field of study, with a critical evaluation of the proposed solutions to date. I will then conduct a theoretical investigation of the philosophy of methodological hermeneutics, exploring its potential application to the paradigm wars in psychology. Finally, I will provide examples of engaged hermeneutic dialogue as applied to the scientist–practitioner gap, demonstrating the difference between this approach and all previous suggestions for unifying the field of psychology.

In Chapter 2, I will expand on the literature introduced in Chapter 1 regarding the unique worldviews of researchers and clinicians in psychology. I will provide a thorough presentation of the most recently attempted approaches to addressing the scientist–practitioner gap. I will conclude Chapter 2 by reviewing the literature regarding the political context in psychology that has led to heightened discord and competition among those who embrace different philosophies of science in psychology, leading those who enjoy epistemological privilege to attempt to subjugate others both politically and economically.

In Chapter 3, I will introduce Gadamer's (1960/1989) philosophy as presented in his magnum opus *Truth and Method*. I will provide a theoretical analysis of this philosophy, drawing on contemporary evaluations and investigations exploring its applicability to the field of psychology. I will build on this analysis as I transition into Chapters 4 and 5, in which I will demonstrate how this philosophy can provide a resolution by going beyond epistemology, or in philosophical structure going *beneath* epistemology, to provide a connection that is human in its most fundamental, ontological sense. I will investigate the concept of engaged dialogue from a hermeneutic and phenomenological foundation, explicating how it is unique from traditional attempts at communication. Finally, I will summarize the analytical investigation and provide

some specific examples of hermeneutic dialogue from the scientist perspective and from the practitioner perspective.

It is my desire that the solution I propose in this dissertation will allow psychologists to resolve the paradigm wars not by establishing, once and for all, what is the correct paradigm for the field of psychological inquiry but rather by recognizing that epistemological battles cannot be won. The competing paradigms are based, ultimately, on the faith of those who embrace them. Further, because there are political and economic benefits to espousing the dominant paradigm, attempts to collaborate have traditionally followed the path of similar conflicts between privileged and oppressed groups, effectively preventing a truly engaged dialogue from occurring. By acknowledging and making this context explicit, I hope to provide a novel and useful contribution to the ongoing quest to establish psychology not only as a legitimate enterprise but also as one that benefits humanity.

CHAPTER 2

ADDITIONAL REVIEW OF THE LITERATURE

Because the scientist–practitioner gap has been thoroughly explored for several decades, there is far more literature than can be adequately reviewed in this space. Nonetheless, I will attempt to present a balanced overview of the important historical arguments and the most current suggestions and approaches. I noted previously that the Boulder Conference on Graduate Education in Clinical Psychology in 1949 was the first large-scale effort to address the gap. This conference was held because the gap had been unsatisfactorily addressed for many years. Indeed, since the very inception of psychology as a field of study to the present time, there have been two distinct modes of interest: the nomothetic (a focus on universal and generalizable laws) and the idiographic (a focus on what is unique between and within specific individuals; Frank, 1984). The nomothetic approach to psychology is typically traced back to the opening of Wundt's laboratory in 1879 (Boring, 1950) and the idiographic can be traced back to research done by Seguin (1866) and Galton (1869) around the same time period.

Brief Political History

The divide between those focused on either the idiographic or the nomothetic ideals can be seen from the founding of the American Psychological Association in 1892, when membership was limited to psychologists who focused on scholarly activities, and publishing was a mandatory requirement (Fernberger, 1932). This led to the founding of a separate

organization in 1917 by clinical psychologists: the American Association of Clinical Psychologists. In 1919 the APA invited the AACP to join as a separate section, maintaining the distinction between researchers and clinicians. This arrangement lasted until 1937, when clinicians again decided to withdraw from the APA and form yet another organization, the American Association of Applied Psychologists (English, 1938). At this time there was also a third group of psychologists who wished to focus on the role of psychology in social issues, breaking away to form the Society for the Psychological Study of Social Issues (D. Wolfe, 1946). Finally, in 1945, the three groups chose to merge once again, with the new constitution of the APA acknowledging the unique pursuits of its diverse membership: "The object of the American Psychological Association shall be to advance psychology as a science, as a profession, and as a means of promoting human welfare" (D. Wolfe, 1946, p. 3). This decision to join as one organization while maintaining unique goals led to confusion as to what, specifically, linked psychologists together. As different psychologists were engaged in completely unique activities with goals arising from divergent guiding philosophies, they felt that they needed to define what it was that linked them all under the term *psychologist*. This time period in the 1940s was a critical one, owing not only to the merger of different groups into a reformulated APA but also to the burden placed on psychiatrists due to veterans returning from World War II, which led psychiatrists to seek the assistance of psychologists (Frank, 1984). Psychiatrists, however, were reluctant to consider psychologists equal partners and required that psychologists practice psychotherapy only under the supervision of psychiatrists. It was this combination of seeking their own identity and desiring independence and autonomy from psychiatry that led psychologists to feel the need to come up with a universal training model that would establish them once and for all as doctoral-level professionals with specialized skills.

Unfortunately, there was still significant disagreement as to how that training model should or could be established. Brotemarkle (1947) argued that research psychology was the foundation for applied and social psychology. Krech (1946) contended that applied psychologists had developed their techniques independently and that the work of academic psychologists at that time was of little value to clinicians. The APA thus formed a committee to seek a resolution, and the solution presented by the committee to the APA was to train clinical psychologists not just to understand research but to actually do research, thus the unifying element for psychologists would be a Ph.D. that included training in current psychological research methodology (Shakow et al., 1947). Their recommendation was accepted and presented nationally at the Boulder Conference.

The Boulder model was accepted and presented as the solution that would give psychologists a unified identity along with independence from and professional equity with psychiatrists but it was never intended to resolve the differences among the growing number of factions within psychology. The goal of the model was to provide a universal training model while still allowing psychologists to pursue specialized training in whatever area of psychology they desired (Raimy, 1950). Those who disagreed with the model pointed out, however, that the very philosophical justification for that training ideal presupposed certain assumptions about science and research that were not held in common among psychologists (Hayes, 1986). As criticism mounted, those in favor of a doctoral program that focused on clinical training, while teaching students to be educated consumers of research but not necessarily producers of research, led to the Vail Conference in 1973 that presented the *scholar–practitioner* model, in contrast to the Boulder Conference's scientist–practitioner model (Ellis, 1992). The 1994 Indiana Conference presented a third option, the *clinical scientist* model, for those who were following

McFall's (1991) manifesto and calling for psychologists to be scientists above all else. What is absent in all of these developments is any attempt at collaboration or communication between the nomothetic and idiographic camps. Instead, these training models are political in nature, with different groups wanting autonomy and authority to conduct psychology as they see fit. The political debates are still strong today regarding which types of programs should receive APA accreditation and which should not (Sayette, Norcross, & Dimoff, 2011; Stricker, 2011). However, once the time arrives for professional psychologists to apply their training—be it as scientist-practitioners, scholar-practitioners, clinical scientists, or any other model—there comes the realization that the researchers and the clinicians depend upon one another for their livelihood, and thus communication and collaboration are vital (Snyder & Elliott, 2005).

Recent Attempts at Bridges

As indicated in chapter one, there have been many attempts to resolve the scientist–practitioner gap by indicating a path toward improved communication between the two camps. I will here revisit the four categories of *learning to translate* that I created previously.

General Collaboration

Kanfer (1990) acknowledged that since the Boulder Conference in 1949, the understanding of the definition of science has changed dramatically. He asserted that, although it was generally accepted in 1949 that traditional natural science was the only route to knowledge that could be trusted, in 1990 that was no longer the case. He believed that the ideal of training true scientist–practitioners had never been achieved and suggested that it would therefore be beneficial to accept that some psychologists would be trained specifically for clinical practice, others would be trained specifically for careers in research, and a third group could be trained as

> "translators" who (a) devote systematic attention to research and dissemination of practical implications and methods derived from various domains of the social sciences and/or (2) [*sic*] formulate professional problems in "basic science" language and collaborate with (or act as) scientists whose expertise encompasses the domain in which these researchable questions are phrased. (p. 265)

Kanfer (1990) made some interesting observations regarding the different perspectives of those in applied and experimental settings. He noted that clinicians must attend to all incoming information (both from the client and the reactions that come from within the therapist), rather than being able to select beforehand what information to attend to and what to dismiss. In an experimental setting there is a plan and a goal from the start, and the plan is typically carried through to completion before assessing the results and determining a new goal and forming a new plan. In applied settings, goals and plans are highly flexible and must be adapted as information is gathered and assessed in a dynamic process. He further described differences in the criteria for success, the use of language, the nature and size of data utilized, and the ethical principles that guide the scientist and the practitioner. It is with these differences in mind that he proposed the training of *translators* who focus specifically on understanding these unique contexts and whose job is to provide the bridge between the two.

Beutler et al. (1995) argued that the science–practice split is not unique to psychology, citing examples from journals in dentistry, nursing, and surgical practice and referring to similar divides in the fields of physics, chemistry, computer science, education, "and even the construction industry" (p. 985). After analyzing some examples of successful practitioners, they suggested that perhaps there are "other roads to knowledge that might be as valuable or even more valuable as means of discovery than the scientific method" (p. 987). They conducted a

survey to investigate the breakdown in communication between scientists and clinicians. Their interpretation of the results is that clinicians do in fact value scientific findings but that their preferred means for reading and discussing such findings is through conferences and clinical newspapers/newsletters, rather than scientific journals. They further inferred from their data that although practitioners "may be much more interested in research than academics conventionally believe" (p. 989), scientists are less inclined to hear the clinicians' side of the discussion. Given their findings, Beutler et al. proposed various steps to improved collaboration between researchers and therapists. Primary among these steps is that scientists should place greater value on the important discoveries and clues uncovered through clinical practice rather than dismissing them based on the unstructured methods that produce them. They suggested that by so doing, researchers and clinicians fall into natural roles, in which clinicians discover knowledge and researchers confirm or disconfirm clinical findings. Finally, they proposed the adoption of trade journals to address the fact that scientists and practitioners tend to communicate through unique fora.

Scientists Reach Out to Practitioners

Within this category of attempts to bridge the gap are articles in which researchers attempt to provide tools and explanations to clinicians so as to facilitate understanding and increase utilization of evidence-based treatments. Adelson and Owen (2012) provided a breakdown of multilevel modeling with the goal of helping those less familiar with complex statistical procedures to understand and evaluate reports. They included a basic explanation of what multilevel modeling is and when (and when not) to use it, as well as explanations for how it can provide information that other statistical methods might not reveal, and the impacts of different sample sizes at different levels. Adeleson and Owen showed self-awareness as they

made jokes that nobody likes to read articles about statistics and stated their goal of making this information as accessible as possible to their target audience of clinicians. They included light-hearted headings (e.g., "How Random Is Your Psychotherapist?" and "That's Not All, Folks") and generally seemed to understand that there is a significant challenge involved in attempting to write an article about statistics for those who have little interest in reading it (Cohen, 1979).

Attempting to take collaboration a step further, Lochman and Wells (1996) created the Coping Power program. They described it as "an evidence-based cognitive-behavioral intervention developed for aggressive at-risk preadolescent children in school-based prevention studies, and for children diagnosed with Oppositional Defiant Disorder and Conduct Disorder who are participating in treatment studies" (Lochman et al., 2012, p. 135). Lochman et al. (2012) went on to emphasize that one of the distinguishing features of the program was the collaboration between the researchers who developed the program and the clinicians who implemented it, resulting in "a systematic series of adaptations in the program" (p. 135). The adaptations included varying the number of sessions, targeting different groups (children, teachers, parents), adjusting the setting (school, home, inpatient, outpatient), changing the delivery model (individual, group, face-to-face, web-based), and others. Lochman et al. expressed their determination to study the program continuously as clinicians implemented it to ensure that adaptations were made with confidence due to the ongoing research on efficacy. Although some adaptations were dismissed due to their reduction of efficacy (such as including unstructured "booster sessions"), others were supported due to the statistical indications that the adaptations sustained the positive outcome effects of the original program. This collaboration suggested that researchers and clinicians, working as a team to adapt and implement a specific

treatment, might find success in bridging the scientist–practitioner gap. In Chapter 4 I will provide further analysis of this collaboration and its potential for addressing the gap.

Lambert (2012) presented another example of collaboration, in which he urged clinicians to take research into their own hands through the use of his Outcome Questionnaire-Analyst (OQ-A). Lambert described the OQ-A as "a computer-based feedback and progress tracking system designed to help increase psychotherapy treatment effectiveness" (p. 109). He reported that clinicians regularly overestimate their effectiveness and continue to provide treatments that may be empirically supported but that will not yield significant results with a given client, because they lack real time feedback from session to session. The OQ-A provides clinicians with tools so that they can continuously measure change in their clients and adjust treatment early on with clients who are identified as likely to have a negative outcome if treated as usual. Lambert provided compelling statistical evidence for the importance of this intervention, indicating that, when clinicians use the OQ-A, the percentage of clients who became worse during treatment dropped from 20.1% to 5.5%, and the number of clients who significantly improved increased from 22.3% to 52.5%. He further emphasized the value of the instrument by explaining that it takes clients only about 5 minutes to complete the measure each session and a matter of seconds for clinicians to access the data and determine whether or not the clients are improving.

Lambert (2012) thus attempted to bridge the scientist–practitioner gap by placing the research tools in the clinicians' hands and encouraging them to increase their own effectiveness by utilizing them regularly. He further explained that the data generated by clinicians through the regular administration of the OQ-A could be used to study numerous phenomena in real world settings, such as the impact of various ethnicity and gender pairings between client and

therapist, the phenomenon of sudden gains (an immediate jump in functioning following a specific session, as opposed to gradual, continuous change across the course of treatment), and countless other occurrences that become evident upon regularly measuring client functioning throughout the therapeutic process. He concluded that "there are ways for science to strengthen the art of psychotherapy and that these new methods provide an excellent example of productive blending of seemingly separate worlds" (Lambert, 2012, p. 113). I will address Lambert's article further in Chapter 4 as I analyze his contribution to resolving the gap between clinicians and researchers.

Youn et al. (2012) offered a similar suggestion with their Treatment Outcome Package (TOP), a "standardized measure . . . that has been developed to make outcome data collection not only friendly for clinicians, but also beneficial for both the clinician and his or her clients" (p. 115). The element of the scientist–practitioner gap that Youn et al. addressed is the belief that experimental data or empirical findings are not applicable in real world settings. To address this, the TOP was developed by clinicians and designed for pretreatment, midtreatment, and posttreatment application with clients. Its data collection was performed in mental health clinics and university counseling centers. Youn et al. stated that the most popular road to the practice of psychotherapy is doctoral programs in psychology, despite the fact they are not the least competitive or most remunerative, and asserted that this must be because those pursuing this path value a scientific approach to treatment. Their belief was that clinicians should therefore be "excited by data and the chance to collect it" (Youn et al., 2012, p. 115). Such a statement seems to imply that psychologists are eager to participate in research but simply lack the training or tools to do so. According to most assessments of the scientist–practitioner gap, this is a poor understanding of what is at the heart of the gap. It is likely that there are many practitioners who

would, indeed, be happy to know that they could be more involved in research through tools such as the TOP or the OQ-A but these are not the clinicians who are on the other side of the gap from the researchers. The gap is due to profound philosophical differences regarding the nature of science, knowledge, and the definition of mental health. Researchers such as Lambert (2012) or Youn et al. (2012) have expressed an eagerness to involve clinicians in the research process, believing it would solve the problem, as though the gap has resulted because clinicians do not know how to perform proper science. I will further address these assumptions in Chapter 4.

Practitioners Reach Out to Scientists

Another subcategory of attempts to bridge the gap within the learning-to-translate approach is that of clinicians providing information to researchers regarding the real world challenges that arise in clinical practice, such that the researchers might attempt to develop studies that are less sterile and more applicable to actual clients. Price and Anderson (2012) provided an example of this by studying the impact of outcome expectancy on treatment response to cognitive behavioral therapy (CBT) for public speaking fears with clients who suffer from social anxiety disorder. They were looking from a clinician's perspective at a specific problem within a unique population, hoping to provide information to researchers with the goal of improving the design of their studies to account for this problem. Walling et al. (2012) took a similar approach by studying how race and ethnicity could be predictive factors of the working alliance between clients and therapists when CBT was applied to perpetrators of intimate partner violence. They pointed out the importance of studying this interaction, as current literature addresses working alliance and race/ethnicity impacts on treatment separately but not together, despite the fact that the two are inseparably linked in many applied situations. Westra et al. (2012) provided a third example of this approach in their study addressing the relationship

between therapists' emotional response to clients and clients' resistance in session. They suggested that a therapist's emotional response to the client could significantly impact the effectiveness of treatment, regardless of the therapist's general competence in delivering CBT. Even though all three of the approaches described here took an important step toward looking at highly specific challenges that arise in the real world setting of service delivery, I suggest that, in fact, they do very little to address the scientist–practitioner gap. Each of the studies, despite being driven by clinician concerns, was built on traditional empiricist scientific methodology. Thus, rather than attempting to bridge the gap in the sense of communicating across philosophical differences, they are examples of applying the same philosophy in increasingly specific situations.

Stricker and Trierweiler (2006) took a more radical step in their presentation of a model they called the local clinical scientist model. In it they attempted to restore the ideal of the original Boulder Conference by taking science out of the hands of researchers and differentiating between contextual knowledge and universal knowledge. They claimed that clinical psychotherapy would ideally be "strictly an applied scientific activity, with practice dictated by a sound body of scientific knowledge" (Stricker & Trierweiler, 2006, p. 39). They explained that in order for clinicians to be effective, they must have a natural scientific attitude, seeking to observe and analyze information from their clients. In describing their local clinical scientist model, they categorized unique types of observation, claiming that each is important for the therapist.

> These types of observation are objective observation (observation from the outside), participant observation (including an understanding of the reciprocal effects of the observer and the observed), subjective observation (empathic observation or intuition),

> and self-observation (self-examination). It is the breadth and depth of these skills, addressed to immediate clinical problems but imbued with the scientific approach and attitude, that constitute the heart of the activity of the local clinical scientist. (Stricker & Trierweiler, 2006, p. 39)

They went on to explain that the term *local* refers to a rejection of the high value placed on generalizability in traditional empiricist philosophy. Because a therapist works with specific individuals, he should both make and apply his scientific observations to those individuals, regardless of the ability to generalize his scientific findings to other groups.

Stricker and Trierweiler (2006) further explained the local clinical scientist model by differentiating it from a traditional applied science, in that it begins with the client, rather than with research hypotheses. They emphasized that theory and experience must influence treatment in addition to data. Thus, they considered their model to encompass a novel "vision of the clinician operating as an active scientist, not simply as an applied scientist, and approaching each clinical interaction as a problem to be solved, much as the scientist approaches problems in the laboratory" (Stricker & Trierweiler, 2006, pp. 42–43).

New Training Models

Snyder and Elliott (2005) attempted to address the gap by presenting an entirely new concept for a training model. Rather than trying to emphasize research, practice, or some combination of the two, they presented a three-dimensional model that focused on teaching psychologists to conceptualize clients in terms of individual strengths and weaknesses (first dimension), the internal and external factors that influence them (second dimension), and the array of social contexts in which the first two dimensions interact (third dimension). They

emphasized the rapidly changing world of health care in general and presented their model as a means for securing a spot for psychologists within that field.

The responses to the matrix model were largely positive, as many contributors agreed that psychology training is in need of reform. Their model was published in the *Journal of Clinical Psychology*, along with 28 invited responses. Of the 28 articles, 18 essentially applauded the efforts of Snyder and Elliott (2005) and chose to write about some element of the new model in which they possessed specific expertise. For example, Shorey (2005) demonstrated how he believed the model would be easily amenable to his own focus on attachment theory, and Neimeyer (2005) presented a similar case for fitting his focus on meaning and constructivism into the matrix training model. Roberts (2005) focused his response on how he might integrate his two primary roles as a psychologist (a training director for graduate students and a practicing child psychologist) in order to implement the many good qualities he identified in the new model. Ponce, Williams, and Allen (2005) focused their praise on Snyder and Elliott's inclusion of specialized training in mentoring for psychologists. Some of the authors among the 16 who were generally supportive took a slightly more critical tone, essentially stating that the model has value but in order to be successfully implemented it would need some minor corrections. For example J. M. Cook and Coyne (2005) cautioned against losing focus on science and research, Cummings (2005) reminded readers that failing to see how one's specialization fits within a broader industry has led to the demise of otherwise successful business ventures, and R. L. Peterson (2005) asserted that Snyder and Elliott did not go far enough in emphasizing the social, economic, and educational context of graduate training for psychologists.

Following a graded path from supportive to critical, another group of authors indicated that Snyder and Elliott (2005) were naïve in believing that they could effect large-scale change

with a single new model. For example, Koocher (2005) asserted that they failed to understand the significance of the economic forces driving the future of psychology. He was highly skeptical that psychologists would simply surrender practice opportunities to BA- and MA-level providers in favor of administrative roles and provided suggestions for what he believed to be more realistic adjustments for psychologists given the economic competition between existing groups. Heesacker (2005) explained that the Snyder and Elliott model (2005) would spread training too thin, essentially relegating psychologists to the "jack of all trades, master of none" (p. 1067) role. Worthington (2005) criticized their conceptualization of clients as the target of interventions and failure to place training focus on the dynamic between therapist and clients. Hayes (2005) expressed concern that the new model was too quick to abandon the Boulder model and presented 11 specific rules that he believed must be followed if one were to develop a new training model, pointing out how Snyder and Elliott fell short on several of his points. Others agreed that the Boulder model must be maintained and similarly provided feedback on how Snyder and Elliott might avoid throwing out the baby with the bathwater (Meichenbaum, 2005; C. Peterson & Park, 2005; Stricker, 2005).

The authors most critical of the model asserted that Snyder and Elliott (2005) had a poor understanding of how their suggestions would impact the field. Tennen (2005) argued that Snyder and Elliott were ignorant regarding the value of traditional theoretical approaches to conceptualization, such as psychoanalysis. Desrochers, Halpern, Tan, and Riggio (2005) claimed that Snyder and Elliott did a poor job of developing their primary arguments, they provided suggestions that did not logically follow from their model, and they gave inadequate attention to the historical debate regarding the scientist–practitioner gap and the Boulder model.

More recently, APA's division 12 (Society of Clinical Psychology) presented a series of articles hoping to present a two-way bridge that would facilitate improved dialogue between clinicians and researchers (Teachman et al., 2012). Their suggestions, published in a special issue of *Psychotherapy*, included recommendations for helping clinicians apply the findings of researchers, helping researchers understand the clinical needs of therapists, and developing training programs that better prepare students to navigate the relationship between research and practice. The central article in this series is Vivian et al.'s (2012) translational model of research–practice integration. In it they proposed a four-level approach to resolving the scientist–practitioner gap by building a two-way bridge between researchers and clinicians. Their four levels included (a) addressing treatment validation studies, (b) focusing on predoctoral training, (c) assessing the clinical utility of research generated models, and (d) integrating data from applied clinical practice into research and communicating among all mental health stakeholders, including researchers, psychotherapists, and clients.

In order to demonstrate the four levels of their model, Vivian et al. (2012) invited articles from authors providing examples of each level. I have discussed these articles in greater detail in previous sections, but in review, Lochman et al. (2012) provided an example of how to address treatment validation studies by showing how they have adapted their Coping Power Program's structure, delivery settings, and clinician training in response to feedback from practitioners. Hershenberg et al. (2012) presented an example of focusing on predoctoral training by providing an example of a new curriculum for training students on the use of evidence-based practices. Lambert (2012) gave one example of assessing the utility of research-generated models by giving advice to clinicians in order to help them implement his OQ-A software for tracking client progress. Youn et al. (2012) described a similar tool, the TOP, that was developed by clinicians

and could inform several aspects of clinical work. Finally, B. E. Wolfe (2012) provided an example of how information from researchers and clinicians might be integrated in a way that includes input from each side, as well as from consumers of mental health services. In order to address the essential philosophical differences that I have repeatedly suggested are absent in these attempts to communicate, I will now review literature in which these concepts are explained.

Incommensurability

As I suggested previously, I posit that communication between researchers and clinicians will continue to fail, even in the rare circumstances when both sides are fully willing to listen to and acknowledge the arguments of the other, because the two sides are coming from unique cultures whose values derive from incommensurate philosophies. Although the foundational concepts regarding competing philosophies of science and the construction of different methodologies are still far from being part of the standard curriculum in training programs, the amount of literature devoted to such explanations has grown tremendously and can be found in mainstream psychology journals (e.g., *American Psychologist, Journal of Counseling Psychology, Review of General Psychology*) as well as in the journals of other social sciences and general research. This increase coincides with a similar increase in the number of qualitative studies published in mainstream journals (Rennie, Watson, & Monteiro, 2002) and a general increase in attention being paid to the discussion of methodological diversity and the requisite philosophical exploration that accompanies it (Atkinson, Coffey, & Delamont, 2001). Therefore, I recognize that there is still some lack of familiarity with these foundational pieces of the argument but I will nevertheless focus on breadth while pointing the reader toward sources where these subarguments can be studied in greater depth.

One of the primary challenges in addressing the problem of unique worldviews in psychology is the fact that the terms utilized in the discussion are developed across time in these disparate communities, and thus the same word may come to have different definitions depending on who is using it. Terms such as *methodology*, *paradigm*, or *phenomenology* can be used very differently by distinct groups, and therefore comparing literature from varied journals and subfields requires assessing the different meanings associated with the terms used. Hence I will address here some of the meanings and implications of key concepts I am utilizing to establish my thesis argument.

Key Terms

Kuhn's (1962) *The Structure of Scientific Revolutions* is frequently cited as a pivotal work in the development of psychology as a science. Sankey (2002) suggested that the term *paradigm* entered the vernacular of psychologists largely thanks to Kuhn's exposition on the contextual historicity of science itself. While the significance of Kuhn's contribution extends throughout several scientific disciplines, Fuller (2002) pointed out the irony that it seems to have had the greatest impact among humanists and social scientists. Kuhn, a trained physicist and professor at the Massachusetts Institute of Technology, intended to build on Einstein's statements about the power of theory to influence the development of methodologies, and he believed he had little to offer to social scientists other than his observation that their field was as yet in a preparadigmatic state. Nevertheless, the social scientist context into which *The Structure of Scientific Revolutions* was received—a field already fully engaged in paradigm wars—was a perfect seedbed for Kuhn's ideas to take root, and it provided the structure for arguments to be made by those who had grown disenchanted with positivism as the necessary foundation of science.

Paradigms are constructed using the building blocks of ontology, epistemology, axiology, rhetorical structure, and methodology (Ponterotto, 2005). Ontology is the study of reality itself. It concerns the exploration of what exists and what we can and cannot come to know about reality. Epistemology is the study of how we can come to know what we know and the relationship between us as seekers of truth and truth itself. To further explain ontology and epistemology, questions about whether there is an external reality and universal truth, or if reality is constructed within our minds and truth is relative, are ontological. If I then begin to wonder *how* I could ever discover whether there are universal truths, I have moved from ontological questions to empirical ones. Axiology is the study of values and their role in a search for truth, rhetorical structure refers to the language and forms used to communicate information about my research, and methodology concerns the development of techniques and procedures I will use as I conduct my search. As I make determinations about what I believe could be discovered (ontology), how I could recognize it if I were to discover it (epistemology), what techniques I might use in the pursuit (methodology), what information is valuable and what is superfluous (axiology), and how I will communicate my findings to others (rhetorical structure), I am building a research paradigm (Ponterotto, 2005). Questions and decisions within any one of these areas will greatly influence the others. Determinations about ontology and epistemology will lead me to value specific types of information over others, which will lead to the selection of methods that are sensitive to this information and not to that and will shape the way I describe the entire process. Research paradigms are thus constructed, ideally, through the careful exploration of each of these foundational philosophical concepts and of the relationships among them (Guba & Lincoln, 2005). Most of the literature regarding current research paradigms in psychology indicates that they can be grouped into four general categories of paradigmatic

thought—positivism, post-positivism, constructionism/interpretivism, and critical theory (e.g., Guba & Lincoln, 2005; Ponterotto, 2005; Wiggins, 2011)—or even more generally into positivist vs. interpretive (e.g., Edwards, 2010; Lincoln, 2002; Morrow, 2005).

The positivist paradigm is built upon the ontological assumption that there is a single, concrete, external reality and that we can continue to come progressively closer to a correct depiction of this external reality as we work to diminish subjectivity in our representations (Viney & King, 2003). This ontological assumption leads to the epistemological approach of empirical science, within which we value objectivity and a clear separation between researcher and research subjects. The cornerstone methodology of positivist science is the experimental design, in which the researcher attempts to isolate one possible explanation for a chosen phenomenon (the independent variable) and manipulate that variable while holding all others constant, thus demonstrating the cause-and-effect relationship between the chosen variable and its outcome (the dependent variable). This information is then reported with a focus on describing how alternative explanations for divergent values in the dependent variable were eliminated, leaving only the manipulation of the independent variable as a plausible explanation. In a popular textbook for research methods in psychology, Schweigert (2006) emphasized the importance of objectivity in this process:

> To avoid being swept away by either unfounded speculations or biased perceptions, scientists tie their beliefs to concrete, observable, physical evidence that both independent observers and skeptics can double-check. Scientists look for *independent* evidence of their claim: objective evidence that does not depend on the scientist's theory or personal viewpoint. (p. 2)

In recent years, statistical analysis has developed rapidly, such that the model I present above in which there is a single independent variable influencing a single dependent variable appears almost comically simplified. Multilevel modeling and other advanced statistical procedures now allow researchers to look at far more complex interactions, but the example I provide remains at the heart of positivist science and demonstrates its goals regardless of added complexity. Although the experimental design is familiar to all who have studied nearly any form of science, the paradigmatic assumptions upon which it depends for its validity are less universally understood (Slife & Williams, 1995). The methods, from beginning to end, reflect the ontological assumption that there is a single, concrete truth regarding the cause and effect relationship between the variables and that said truth exists independently of the meanings and interpretations we might subjectively ascribe to it. This ontological assumption leads to the epistemological assumption that I can observe that universally constant relationship by eliminating (or at least greatly decreasing) any factor that might be impacting the relationship aside from the one I have theorized to be the causal variable (Fishman, 1999). The axiological assumption that objectivity is preferable to subjectivity is developed in conjunction with the other philosophical assumptions guiding the development of methods that will ensure a clear separation between the researcher and the object of study, such that we are left with cold, independent facts, and not opinions or ideas (Stiles, 2009). What is not obvious to many is that several opinions and ideas were necessary in the construction of the positivist paradigm, yet the paradigm elicits faith from its believers that the experimental design yields cold, independent facts (Griffin, 2000). Such terms sound out of place in discussing the tried and true scientific method, but upon examining the philosophical assumptions upon which the paradigm is built, it becomes evident that the core ontological and epistemological tenets upon which positivism rests

must be accepted on faith—there is no way to demonstrate the truth of empiricism empirically (Slife & Melling, 2009).

Power and Privilege

Epistemological privilege has been afforded to positivist science, and this privilege is a critical factor in understanding why the foundational beliefs of traditional empiricism are not questioned or explored in any depth by those who hold them to be true. According to Edwards (2010),

> Research reported within the discourse of positivism seems to exert a kind of mesmeric power over readers in that its embedded assumptions are implicitly presented and accepted as definitive truth, and problems created by the epistemological limitations of positivism are projected on to the phenomena being investigated. (pp. 277–278)

This same observation has been expressed repeatedly by different authors over several decades. Suppe (1977) called positivist science the *received view*, because it is simply received as truth without scrutiny. Mahoney (1976) argued against the object/subject dualism of positivist science, Suppe against the failure to recognize embedded values, and Polkinghorn (1983) against the methodological implications and limitations of positivist science for studying human experiences. Keeley et al. (1988) pointed out positivism's limiting impact on student dissertations, Guba and Lincoln (2005) wrote extensively about the societal consequences of the narrow interpretation of scientific research, and all of these authors, along with many others, have proposed alternatives (e.g., Creswell, 1998; Giorgi, 1970; Kvale, 1996; Miles & Huberman, 1994; Packer & Addison, 1989; Patton, 2002; Reason, 1988; Richardson et al., 1999; Rychlak, 1977; Smith, 2003). Indeed, these references are still but a small smattering of the arguments made over the past 40 years insisting that traditional positivist science must be seen as one

philosophical view among many legitimate models for scientific inquiry, and yet those who espouse the positivist ideals continue to dismiss alternatives as fuzzy, vague, and most damning of all, unscientific (Baker, McFall, & Shoham, 2009; also see Wigney & Parker, 2007). As Miller has stated regarding the positivist view of science "this is not *my* definition of science, it is *the* definition" (as cited in Slack, 2007, p. 32), implying that his authoritative statement is the proverbial end of discussion. Shedler (2011) countered that

> people speaking from a dominant paradigm often assume that they are speaking obvious truths, while people in more marginalized groups tend to experience those in power as self-justifying, self-serving, and blind to important information that does not comport with their own worldview. (p. 154)

There is good reason for positivist thinkers to take such a defiant stance toward alternative definitions of science. Since the emergence of psychology as an area of study, psychologists have found it essential to defend the superiority of their scientific expertise relative to common sense or other explanations for psychological phenomena (Lamont, 2010). Whether debunking mesmerism (Forbes, 1845), spiritualism (Jastrow, 1889), or extrasensory perception (Boring, 1966), psychologists have consistently presented an aggressive case that they alone possess the scientific acumen necessary to protect the public, who may be fooled by seemingly persuasive arguments if not for psychologists' ability to utilize scientific methods to reveal the truth about such phenomena (Lamont, 2010). Within psychology, the history of competition for *most scientific* status has also been consistent, and traditional positivist empiricism has dominated the field for several decades. The adoption of a specific training model was done so that the APA could more easily regulate who can become a psychologist. There is a limited number of available positions for students in accredited doctoral programs, then there is another

bottleneck in the availability of approved predoctoral internship training programs, and, of course, there are limited jobs for those who make it through those initial preparatory steps. There must be some criteria for ensuring that those who make it to the stage of licensure in psychology have been properly trained, and the benchmark enforced since 1949 for accrediting training programs has been adherence to traditional positivist empiricism. Nevertheless, at the insistence of its diverse membership, the APA has made statements declaring support for diverse philosophies of science (APA Presidential Task Force on Evidence-Based Practice, 2006). Tellingly, this trend toward scientific diversity led to the formation of the American Psychological Society in 1988, which changed its name to the Association for Psychological Science (APS) in 2006 (APS, 2012).

The APS has led a politically charged effort over the past few years to restrict accreditation to only doctoral programs that adhere more strictly to traditional positivist science (Baker et al., 2009). Baker et al. (2009) claimed that clinical psychologists continue to base their practice on ascientific information, and they urged the stigmatization of all training programs that fail to use their newly developed Psychological Clinical Science Accreditation System (PCSAS). They also called upon the APA to abandon its previous accreditation standards and adopt the PCSAS. Negative responses to the efforts of APS have been strong. Elkins (2011) asserted that their arguments are nothing more than a "political slogan that, once examined, makes no sense," and that their comparison of psychology to pre-scientific medicine is "patently ridiculous" (para. 6). Stricker (2011) pointed out that studies demonstrating the superiority of APS's preferred training programs are deeply flawed based on selection bias and other methodological strategies that virtually assured the results the authors were seeking. It seems clear that the argument is primarily about securing control over the stamp of scientific

legitimacy, with the APS seeking to leverage epistemological privilege into political and economic advantages over those who support alternative epistemologies. As I have indicated previously, this context in which groups (each of which embraces incompatible philosophies for establishing truth) are competing for limited economic and political resources leaves little room for the optimistic assumption that simply communicating with one another or establishing a new training model will lead to a happy resolution to the scientist–practitioner gap. As Stricker and Trierweiler (2006) argued,

> it is not necessary to take sides in this debate to observe that a great many decisions that have been made have been governed by considerations of political power rather than of sound training, and neither practice nor science benefits from such actions. (p. 38)

CHAPTER 3

HERMENEUTIC METHODS

Gadamer (1960/1989) escaped the dichotomy of dogmatic truth versus relativism by appealing to a quest for understanding (*verstehen*) rather than knowledge or explanation (*erklären*). He depicted the latter as coming to know all that can be known about a fixed object, such that it can no longer surprise the student with any new information. This concept fits well within the realm of the natural sciences, in which physical objects or even abstract concepts can be situated completely outside of human influence and interpreted/interpretive meanings and then studied carefully and patiently until all that can be known about them is known. Assuming, however, that the subject to be studied is dynamic, such that a complete knowledge could never be attained because what is true about it today might not be true tomorrow, Gadamer suggested that a completely different goal would be more appropriate. Rather than seeking knowledge about the subject, a more attainable objective would be to aspire to an understanding of the subject. Understanding, as Gadamer explained it, means to set aside the pursuit of a predetermined goal (such as complete knowledge about whatever I am studying) and instead engage in a willingness to be led on whatever path the subject presents to me, even if that path is unexpected. Taylor (2011) explained that the end of this operation is not control (the reason for knowing in most scientific studies is to use that knowledge to predict and control). He asserted that if control were the end goal of science as pertaining to humans, we would find ourselves

"engaging in a sham designed to manipulate my partner while pretending to negotiate" (Taylor, 2011, p. 25). He stated that in the realm of a Gadamerian human science, "the end is being able in some way to function together with the partner, and this means listening as well as talking, and hence may require that I redefine what I am aiming at" (p. 25). This last point emphasizes one of the most important distinctions Gadamer made between *erklären* and *verstehen*: that knowledge is unilateral and understanding is bilateral. My goal does not just shift away from control (explanation) and toward cooperation (understanding), but in addition, part of understanding must include a recognition that I am not alone in the exchange. Vitally different from the enterprise of studying geology, the object of my study has thoughts about me, interprets me, and actively and dynamically contributes something to the process of understanding.

The point in this explanation of terms regarding the differences between explaining and understanding becomes clear upon examination of the purpose of scientific inquiry. My goal, whether as a scientist seeking explanation or a scientist seeking understanding, is to evaluate truth claims. One way to do so is through the methodology of the natural sciences. The methods here exist to facilitate the removal of the feelings, values, and desires of the researcher and lay bare the independent facts external to him. What Gadamer (1960/1989) presented is a methodology that takes a completely different course, because when researchers attempt to remove the feelings, values, and desires of humans as they try to understand humans, they find that they are studying something other than the psyche. They become biologists, neurologists, or chemists who can discover facts about the physiology of human bodies, but they are not psychologists. Thus, this different sort of science is necessary, but the moment researchers begin to look at how feelings, values, and meanings might be reinserted into their scientific inquiry, they begin running into incompatibilities that cannot be solved without stripping away their

beliefs about the ontology and epistemology upon which their science was built. What Gadamer presented in a hermeneutic methodology is a system for evaluating truth claims that is neither devoid of *humanness*, that is, the "sphere of intertwined lives, shared purposes, moral struggles, and the search for meaning" (Christopher, Richardson, & Christopher, 2003, Naturalism & Objectivism section, para. 2), nor relegated to a relativistic approach in which no truth claim can be accredited with greater validity than any other.

The common path of most who have left the positivistic paradigm with the goal of reinserting meaning into the social sciences has led to the constructivist/interpretive paradigm. The pitfall here is the relativism that is required in order to respect the many different meanings and truths constructed by different individuals and cultures. In order to avoid falling into the same dichotomy of relativism versus absolutism, Gadamer (1960/1989) explained the concept of unique horizons, wherein individuals each come to any interaction with a specific perspective in both time and space. This perspective influences not only what they see but the meaning they make out of what they see. Taylor (2011) explained that there would be dramatically different historical accounts, for example, of Rome provided by those in 17th-century England, 21st-century United States, or 25th-century China. Each group would be providing an account of the same culture but their own place and time would lead them not only to see that culture differently but also to focus on different concepts and emphasize different elements of the culture. The differences here, assuming that each group is attempting to provide the most accurate historical picture they can, stem from the values and systems contained in the culture providing the history. This is a departure from the absolutism of positivist science, in the acceptance that there is not one truth about that culture but many unique truths coming from

different perspectives. It is also a departure from relativistic thinking because there certainly are better and worse accounts of the culture, particularly in the area of comprehensiveness.

In further explaining the importance of a hermeneutic method for scientific inquiry, Gadamer (1960/1989) suggested that different cultures develop systems for problem solving. Inevitably, any given system will be good at solving some problems but will eventually prove inadequate for solving others. It is precisely in these situations when the need for interpretation across cultures becomes essential, for if one begins to adopt the methods of another culture without understanding the traditions, values, beliefs, and systems of that culture, he or she is simply squeezing new methods into the already established system: the system that has already proved inadequate for addressing the problem at hand. Instead, Gadamer explained, there must be a willingness to accept that my system is embedded in a history and a tradition that is but one among many and that I can never simply extract myself from my embeddedness: it will forever shape my perspective. The only solution, therefore, is to engage in a hermeneutic dialogue, meaning that I acknowledge my own biases and traditions as I engage with someone who holds unique biases formed in different traditions. This hermeneutic dialogue, as I have suggested previously, is entirely different from a monologic exchange of ideas such that the ideas of another are simply plugged into my own system. Instead, I must make every effort to understand the historical context for the other's perspective. Although I will never transcend my own experience, this dialogic exchange is the fusion of horizons Gadamer proposed. My own perspective is expanded and developed in a way that can only happen as I humbly seek to understand the ways that unique ontological perspectives could yield vastly divergent systems for deriving truth.

One of the criticisms that Habermas (as cited in Warnke, 1987) raised concerning Gadamer's methodology is that it is too dependent on an ideal setting in which both parties are willing to listen and engage hermeneutically. He asserted that true conflict almost always occurs because there is a power differential between the two participants in the dialogue and that difference will prevent such an ideal fusion of horizons in any Gadamerian sense from occurring.

> The linguistic infrastructure of society is part of a complex that, however symbolically mediated, is also constituted by the constraint of reality—by the constraint of outer nature that enters into procedures for technical mastery and by the constraint of inner nature reflected in the repressive character of social power relations. (Habermas, as cited in Warnke, 1987, p. 112)

Habermas was specifically contending with Gadamer's explanation of language as the necessary medium of hermeneutic experience and arguing that social power, political power, and control play a fundamental role in shaping meaning and understanding within language, such that the privileged culture can manipulate the understanding of the other. Both in anticipation of such criticisms, and later in response to them, Gadamer (1960/1989) explained that hermeneutics is not only able to address such power differentials but also that, in fact, it is exactly the model that most successfully allows the oppressed or less empowered party to reveal the complexities of meaning that maintain the power structure. Essentially, Gadamer conceded that hermeneutics must rely on a degree of idealism, in that there must always be those willing to pursue heightened understanding for the progression of humanity as a whole, even if that means acknowledging faults in our cultures and traditions. He insisted that human history has demonstrated that although there are those who would seek power and control at the expense of understanding and rationality, there have thus far always been others who are willing to sacrifice

power and control in the pursuit of truth and that hermeneutic methodology provides the tools for those who are thus engaged.

Methods

A hermeneutic analysis seeks to illuminate the contextual historicity of an argument. It is a careful exploration of the time and space that leads the various participants in the discussion to see what they see, interpret how they interpret, and value what they value. Among its philosophical and practical applications, hermeneutics is described as the science of interpretation (Grondin, 1994), and I am suggesting that it is the interpretive tool necessary to bridge the scientist–practitioner gap. Chang (2010) noted that the desire to explain and define hermeneutic methodology is likely to be left unfulfilled, as the core of the philosophy upon which it is built eschews universal definitions in favor of case-specific contextual observations. Nevertheless, although there is not a large amount of literature describing the specifics of a hermeneutic methodology, some of the most popular texts and articles examining the current state of psychology serve as examples of hermeneutic analysis applied to specific concerns in the field (e.g., Cushman, 1990; Fowers & Richardson, 1996; Hillman & Ventura, 1993; Martin & Sugarman, 1999; Richardson et al., 1999; Slife & Williams, 1995; Taylor, 1995).

For the purpose of the current study, the methodology took the form of a thorough analysis of the historical, cultural, and philosophical context within which the scientist–practitioner gap arose and developed. This context then served as a foundation on which a dialogue could be engaged and a fusion of horizons could begin wherein the values and beliefs of each side were laid bare. In a dialogue devoid of these hermeneutic techniques, assumptions and taken-for-granted beliefs about the world prevent true engagement between the participants, serving instead as blinders that prevent each side from expanding or fusing horizons with those

who hold different assumptions and take for granted different beliefs about the world. In Chapters 1 and 2 I made explicit many of the assumptions and beliefs pertinent to the gap between researchers and clinicians in psychology and provided what I believe to be important contextual information regarding its historical background. The next step was to engage dialogically the different perspectives on the gap with humility and openness. This suggestion of a linear sequentiality of steps, however, can be misleading. The hermeneutic process is more commonly depicted as a circle in which newly discovered meanings recursively provide new context for both past and future understandings (Chang, 2010).

In the present study I hermeneutically engaged with the texts that present the divergent opinions regarding the directions in which psychology might continue to pursue scientific legitimacy. As I constructed the dialogue in Chapters 4 and 5, I relied upon the information established in Chapters 1 and 2 but attempted to do so such that the dialogue both derived meaning from and provided meaning to the historical context. This dissertation thus serves both as an illumination of past exchanges and a model of future engagements.

Selection of Texts

Although myriad articles have been published addressing the scientist–practitioner gap, I focused on presenting a hermeneutic dialogue primarily involving Snyder and Elliott's (2005) four-level matrix model, Vivian et al.'s (2012) translational model of research–practice integration, and the several voices of support or criticism that have arisen since their presentation. I chose to focus on these articles for two reasons. First, they are recent and as such they represent some of the most current approaches to the solution of the problem. Second, they were both presented with the specific goal of presenting diverse perspectives on the solution to the problem. Snyder and Elliott suggested that their new model essentially did away with the

scientist–practitioner gap by adjusting the focus of psychology away from theoretical points of contention and toward practical application of knowledge that will specifically fit a rapidly evolving health care system. Confident in their novel solution, they invited responses from a vast array of scientists and practitioners with the promise that said responses would be presented without any rebuttal, thus encouraging the authors to be as candid as possible in their evaluation of the model (Elliott & Snyder, 2005). In the issue containing Snyder and Elliott's new training model, the *Journal of Clinical Psychology* published 28 articles representing both support for and criticism of their solution. These articles thus offered an ideal opportunity to analyze an example of an attempt to resolve the fission in psychology and to compare and contrast that attempt with the outcome a hermeneutic solution might provide. Vivian et al. (2012) stated that consensus has not been reached on the best way to reduce the gap between researchers and clinicians, and thus they offered a translational model addressing perspectives provided by several different authors in an issue of the journal *Psychotherapy*. This example included six articles, and their specific intent to demonstrate how we might communicate and collaborate among different factions of psychologists provided another ideal opportunity to compare and contrast these authors' approach with that of hermeneutic dialogue.

Engagement of Data

Step 1. The data in this dissertation are ideas, specifically the ideas that different thinkers have presented in an effort to mold psychology into a viable, trustworthy, and socially beneficial scientific enterprise. What I did with these data was first to demonstrate how they fit into the historical and philosophical perspectives described in Chapters 1 and 2. This demonstration of the implicit values, assumptions, and philosophies that give the data meaning

followed the hermeneutic pattern provided by Gadamer (1960/1989) and many leading hermeneutic scientists (e.g., Cushman, 1990; Martin & Sugarman, 1999; Taylor, 2011).

Step 2. Thus contextually situated, I demonstrated how these data might interact with one another in a unique way through the hermeneutic dialogue presented in the current chapter. I did this by explaining how each idea could be enlarged and expanded by acknowledging its own limitations and the strengths provided by other ideas. Once again, it is critical to point out here that this engagement is integrally unique from a superficial comparison of technique or method differences (according to my argument, this superficial attempt to address differences at the applied level is the substance of the previous attempts at communication), specifically because it goes beyond methodology and addresses differences at the epistemological and ontological level that provide the guiding philosophy for different paradigms. As I have stated previously, the goal of a hermeneutic dialogue is not to determine who has the best solution but rather to engage our proposed solutions at the most fundamental level such that we each leave the dialogue with an expanded understanding of unique ways of thinking, acting, and being (Gadamer, 1960/1989). There can be no scientific proof of an epistemological belief, but there can be a dialogue in which participants discover new ways of understanding, in which they question their own epistemologies and compare them to others, and from which they leave equipped to engage with problems differently from how they have in the past.

Step 3. Having explored how the data can be engaged hermeneutically, I contrasted this dialogic exchange with the previous attempts at solutions to the scientist–practitioner gap in order to provide specific examples of the difference between a hermeneutic dialogue and traditional communication in the scientific community. Utilizing articles from the source journals noted above, I demonstrated the monologic nature of the exchanges between authors.

Step 4. A vital component of hermeneutic science is the recognition that I cannot transcend my own historical context, as that context provides me the only tools I can possess for interpreting and making meaning out of anything. However, although I cannot transcend or escape my perspective, I can expand it (Gadamer, 1960/1989). A hermeneutic work therefore must avoid the dogmatic and monologic assertions it criticizes. I am laying out sequential steps for the sake of clarity in a doctoral dissertation but this step—acknowledging my own limitations and seeking to learn from those with whom I dialogue—permeates each step within a hermeneutic circle of analysis. Hermeneutics without humility can quickly be reduced to another dogmatic set of beliefs. Taylor (2011) reminded us that remaining open to the dynamic nature of human thought is what prevents us from "engaging in a sham designed to manipulate my partner while pretending to negotiate" (p. 25). In Step 4 I thus pointed out my own biases and values and acknowledged the ways that I might need to expand my own perspective in order to contribute effectively to a hermeneutic dialogue.

Step 5. Having historically situated the argument, provided examples of both dialogic and monologic engagement, and acknowledged the limitations to my own perspective, I gave suggestions for how hermeneutic dialogue can be applied in future exchanges.

Conclusion

In conclusion, I employed the methods of hermeneutic dialogue in order to situate historically and philosophically the ideas of Snyder and Elliott (2005), Vivian et al. (2012), and the corresponding responses to their articles. I compared and contrasted these exchanges to a hermeneutic engagement and demonstrated how the authors might enter into a hermeneutic dialogue. I will concluded by suggesting how the present dialogue could inform future engagements, such that those who wish to build upon a traditional empiricist paradigm, those

who wish to build upon an interpretivist paradigm, and those who present alternative paradigms could all work together to advance psychological science. The desire to exercise control over the provision of psychological services will continue to come from many widely divergent sources, and these sources will always have different philosophies regarding what the best criteria are for governing the field. If we continue to seek to validate different approaches by using scientific arguments that cannot be supported philosophically, we are failing to achieve the most basic aspiration of our profession: "to advance the creation, communication and application of psychological knowledge to benefit society and improve people's lives" (APA, 2013, Organization of APA section, para. 4).

CHAPTER 4

HERMENEUTIC ANALYSIS

Snyder and Elliott

Snyder and Elliott (2005) concluded the article in which they presented their four-level matrix model by encouraging others to share their objections freely, because "an in-depth and extensive exchange of ideas is precisely what our field needs right now" (p. 1049). They were calling for widespread and profound changes to the conceptualization of psychology as a science and as a profession in response to the changing needs and demands arising in the 21st century. Their suggestions were presented as the next logical steps as driven by their perception that psychology must change if it is to maintain its potential for having a positive impact on society. They described the various elements of their model one by one, defending their suggestions by identifying the ways in which current ideas and practices would likely fail to meet future needs.

Assumptions Underlying Assertions

In order to engage with authors regarding their suggestions, it is necessary to look at the justifications they offer for the points they make. Snyder and Elliott's (2005) first assertion was that they "no longer perceive that the prevailing 'scientist–practitioner' Boulder model fully educates our clinical psychology graduates for the contemporary marketplace" (Snyder & Elliott, 2005, pp. 1033–1034). They provided a single citation in defense of this assertion: the final report of an APA working group on the implications of changes in the health care delivery

system for the education, training, and continuing professional education of psychologists. The first author of the report, Jean Spruill, is retired after a career as a psychology professor and director of the Psychology Training Clinic at the University of Alabama. The two predominant topics addressed throughout her many publications were intelligence testing and preparing students for professional careers in psychology. Second author Jessica Kohout works as the director of the APA Center for Workforce Studies and has extensively published on the future of the psychology workforce and its adaptation into the broader healthcare field. Third author Sheila Gehlmann has published reports on career placement for various mental health professionals depending on type of degree held. I won't go much further down this path of learning about the careers of each person on Snyder and Elliott's (2005) reference list but I offer it as an example of the importance of going beyond reported data in search of meaning. British empiricist thinking has led to a belief that reported data *speak for themselves* and that they represent pure, objective information that is cleanly divorced from opinions and theories. Following this line of thinking, one might see that Snyder and Elliott have done their due diligence in citing a source for their belief that the Boulder model is no longer adequately preparing graduates for a career in psychology without taking the time or effort to study the source itself. In this case the report they cited appears to be unpublished, but based on what one can learn about the authors, there is a good indication that it likely provided statistics about how the psychology workforce is changing, and ways in which the training models in graduate programs might be adapted to better prepare students for current and future demands on psychologists. This was the justification Snyder and Elliott provided as the basis for their entire argument that the training system needs to be changed in a revolutionary way. In their defense, they offered further sources throughout the article explaining why each change might be a good

one. I will therefore address more of their key suggestions and analyze the philosophical assumptions made therein.

Snyder and Elliott's (2005) model consists of a four-quadrant matrix of concepts that are then assessed on four different levels. The first brick used in building the foundation for their model was the decision that psychology should place a balanced focus on both strengths and weaknesses, comprising the first two quadrants of the matrix. They offered eight different citations to support this assertion, seven of which were authored or co-authored by Snyder. This pattern was established early and persisted throughout the article. Rarely did they explain *why* they believed a certain practice would be better for the profession. Instead they simply stated the change they hoped to see and cited several sources that readers were left to assume offered some reliable explanation as to why this change was warranted and needed in the field. In this case their explanation for the need to shift away from a pathology model to a balanced assessment of both personal strengths and weaknesses was that they "seriously doubt that the general public and influential policymakers will continue to support the previous monolithically negative views presented by clinical psychologists" (Snyder & Elliott, 2005, p. 1034). Again, readers are left to assume that the articles cited offer some support beyond the statement that they "seriously doubt" that everyone else is okay with psychology focusing on pathology. Such ambiguous explanations for suggested changes provide an excellent opportunity to demonstrate how hermeneutic methodology can elucidate and make explicit what may be left unsaid by the authors.

The one source Snyder and Elliott (2005) cited other than Snyder's works was Seligman, a former president of the APA who was originally famous for his learned helplessness studies with dogs but in more recent years has come to be known as the primary author behind the

positive psychology movement. Slife and Williams (1995) explained the need to understand both the assumptions upon which an argument rests and the implications of carrying that argument out to its logical conclusion. They asserted that assumptions and implications are rarely made explicit, with most authors focusing instead on the points that are novel or unique about their arguments. By failing themselves, however, to explore the assumptions and implications of their arguments, authors frequently fall into unseen traps of endorsing beliefs and values with which they may disagree or suggesting future courses of action that were not anticipated but are logical applications of the argument being presented. Snyder and Elliott were likely falling into this trap throughout their article as they expressed significant changes to the training and development of future psychologists without fully exploring either the philosophical assumptions upon which those changes were based or the logical extensions of those changes as applied to various real world settings in psychology.

The assertion that future training models should shift from a focus on mental illness as a pathological condition to a focus on a balanced evaluation of both strengths and weaknesses can only be accepted if one also accepts a philosophy in which pathologizing mental illness is rejected. Explanations of philosophies in which mental illness is not considered pathological can be found throughout interpretivist literature (e.g., Albee, 2000; Cushman, 1990; Guba & Lincoln, 2005; Ladson-Billings & Donnor, 2005; Plummer, 2005); however, Snyder and Elliott (2005) instead cited the work of Seligman and Csikszentmihalyi (2000) in support of their assertion. This introductory article to an entire edition of *American Psychologist* explaining positive psychology was a call to action for psychologists, insisting that the focus on negativity and pathology that had prevailed for the past 50 years was now outdated and needed to be replaced with a focus on the positive human attributes that facilitate full and meaningful lives for all.

What Seligman and Csikszentmihalyi (2000) did not address was the philosophy supporting their assertion. They artfully persuaded readers in an article that appealed to what they identified as the inherent positive human qualities that lead to the good life and then called upon psychologists to employ all of their empirical scientific methods to better understand this positive element of psychology. They stated that empirical science had made tremendous progress in explaining and yielding treatments for mental illness, and thus it was logical to assume that it could also be employed to understand better how to develop positive mental and psychological attributes that would buffer individuals from negative external influences and lead to happier individuals and flourishing societies. They outlined many subfields of psychological research that could be redirected away from studying pathology and toward studying strengths, implying that no change was needed in methodology or philosophy but rather that psychologists simply needed to adjust the focus of their subject matter. Seligman and Csikszentmihalyi extolled the benefits of traditional empirical science while they simultaneously called for a shift to focus on many concepts that are generally considered outside the reach of empirical inquiry, such as virtue, love, faith, insight, hope, and inspiration. The entire article provides an excellent example of the tendency to focus on a pragmatic goal without exploring the philosophical basis for the steps being suggested. Snyder and Elliott's (2005) article, in fact, has a very similar feel to Seligman and Csikszentmihalyi's (2000) article; both articles presented suggestions that have great aesthetic and pragmatic appeal but that were not explored or supported philosophically.

One of the primary philosophical inconsistencies in both articles (Seligman & Csikszentmihalyi, 2000; Snyder & Elliott, 2005) is the failure to acknowledge the key role that reductionism and determinism play in traditional empirical science. Briefly put, these are two of the primary purposes served by empirical methodologies: to reduce a complex phenomenon

down to its smallest building blocks and, by understanding those building blocks, to learn how to predict and control outcomes. Seligman and Csikszentmihalyi (2000) repeatedly mentioned the great strides made in psychology over the past 60 years through the employment of empirical science, specifically that disorders and maladies that were once poorly understood and considered untreatable are now categorized with effective treatments available to ameliorate if not completely cure the disease. Unfortunately, the authors failed to acknowledge the tremendous philosophical leap required to do as they suggested and to take the same scientific process that has been applied to studying disorders and now apply it to positive attributes in order to come to a greater scientific understanding of human strengths. The problem with this step, as many authors have already shown (e.g., Cushman, 1990; Hillman & Ventura, 1993; Martin & Sugarman, 1999; Taylor, 1995) is that such traits seem to defy reductionism and determinism. Trying to break something such as courage down into smaller building blocks has not helped researchers understand courage. Instead, understanding courage seems to require doing just the opposite by studying the context: the bigger picture in which a behavior or thought takes place that leads someone to interpret it as courageous. Furthermore, volition repeatedly rises to the surface as one of the key elements to any positive human characteristic. One finds a specific behavior praiseworthy specifically because it would have been possible to do something less admirable. If we are indeed capable of choosing a *better* path even when some other path is possible and more commonly chosen, then predicting and controlling (the goals of deterministic methodologies) become enormously problematic.

Such philosophical roadblocks are the reason that so many researchers have insisted that developing a human science will require much more than borrowing the methodologies from the natural sciences and applying them to human questions (e.g., Fowers & Richardson, 1996;

Gadamer, 1960/1989; Giorgi, 1970; Taylor, 2011). Snyder and Elliott's (2005) failure to even acknowledge such philosophical encumbrances is a strong indication that they see the scientist–practitioner gap as a disagreement regarding *how* to train psychologists and not as a disagreement regarding philosophies that would suggest *why* to train psychologists a certain way, what training means to begin with, and what the goals of psychology should be. These latter disagreements are often intentionally avoided, because *why* and *should* hold decidedly moral implications, and the strong tradition of divorcing science from morality leads authors such as Snyder and Elliott to focus instead on *how* problems.

The second foundational premise of the matrix is that psychologists should explore both the internal and the external factors that influence mental health. Combining these with the first premise of focusing on both strengths and weaknesses, the four quadrants of the matrix are thus individual strengths, individual weaknesses, environmental strengths, and environmental weaknesses. This second assertion rests on the assumption that environmental and interpersonal factors are of importance equal to biological and intrapsychic factors affecting mental health, and the authors credit this idea to Beatrice Wright (as cited in Snyder & Elliott, 2005). With the four quadrants of the matrix in place, Snyder and Elliott (2005) borrowed from Bronfenbrenner's (1979) ecological developmental model to explain that each quadrant of the matrix should then be assessed on four different levels: the individual level, the interpersonal level, the institutional level, and the societal level. The four-level matrix model thus consists of three dimensions: (a) personal strengths and weaknesses, based on Seligman and Csikszentmihalyi's (2000) positive psychology; (b) individual and environmental factors contributing to mental health, based on Wright's (1991) person–environment model; and (c) the four levels of context, based on Bronfenbrenner's ecological psychology.

Snyder and Elliott (2005) briefly addressed some theoretical and philosophical questions but they did so with a dismissive pragmatism, indicating that there had been a progression in the field from psychodynamic to nondirective to behavioral to cognitive perspectives, and stating that they saw a need to now embrace systemic constructivism as the guiding perspective. They offered no justification for this assertion whatsoever, instead simply implying that change is good: the field has changed over the years; this perspective fits well with our model, so now we should change to this one. This assertion is another example of the approach Snyder and Elliott took throughout their explanation of the matrix model, namely to present intuitively logical ideas and to offer citations to indicate support for the ideas only to then fail to offer any explanation themselves for how or why their suggestions would provide philosophically sound solutions to the profound schisms that have formed within the field of psychology. The authors are seasoned and respected academicians and they presented a carefully organized system for updating the way psychologists are to be trained by combining many popular concepts from different branches of the field. It therefore could be easy simply to accept, for example, their assertion that the field should now adopt systemic constructivism as its guiding theoretical orientation or that we should balance a greater emphasis on client strengths with the traditional emphasis on pathology or that we need to "greatly expand the number of people from other countries who receive their educations in the United States" (Snyder & Elliott, 2005, p. 1049). In searching for explanations, however, their justification throughout the article was that psychology must make these changes in order to remain useful. This reliance on pragmatism as a guiding philosophy has become more and more common as the paradigm wars have intensified. Many psychologists have adopted the attitude that philosophy should be left to the "theory people" and that they will simply "do what works."

Pragmatism

In addressing the history of pragmatism in psychology, Leary (2009) refered to William James's 1885 argument that "*truth* can only be measured by its *practical issue*, and further, that if an assumption, perception, or claim makes no practical difference, it fails to put us into a closer connection with *reality*" (James, 1885/1975, as cited in Leary, 2009, p. 7). Pragmatism thus plays a prominent role in the earliest development of psychology as a field, and it is typically traced back to James, John Dewey, and Charles Peirce, all of whom were members of the Cambridge Metphysical Club in the 1870s, where philosophical concepts important to the early foundation of psychology as a science were discussed (Leary, 2009). The ontological premise of a pragmatic philosophy leads to the rejection of seeking some accurate representation of reality. Rather than assuming the existence of an external reality and attempting to develop tools and methods for approximating the closest possible representation of that reality, pragmatists focus on problem solving as the central evaluative tool for reality. If a thought or belief or idea proves useful in solving practical problems, it is embraced regardless of any sort of transcendent external conceptualizations of the truth of that idea. Pragmatism has played an ironically strong role in the development of psychology, given its incommensurability with many of the core foundational assumptions of positivism (such as the belief in an objective external reality and a primary goal of representing that reality as accurately as possible).

Those who employ the "just do what works" approach in the practice of psychology are therefore embracing a pragmatic philosophy, whether or not they have fully considered the philosophical assumptions or implications in doing so. If one wished to work within the framework of a pragmatic philosophy, one would need to work out the questions of ontology, epistemology, and axiology within that paradigm, such as Rorty (1979), Bernstein (1986), and

others have done. This would include a definition of what *works* means and a clear articulation of how their plan meets that definition. Unfortunately, no such explanation accompanied Snyder and Elliott's (2005) implicit reliance on pragmatism. An argument could be made that within a pragmatic philosophy no such explanation would be necessary, as long as the model produced the intended results. However, as one explores the hidden assumptions within the matrix, it becomes clear that some of these assumptions are incompatible with pragmatism. In their omission of any sort of discussion regarding the guiding philosophy for their four-level matrix model, Snyder and Elliott (2005) likely fell into the common trap of presenting ideas that seem to fit together logically but which are tied to incommensurate philosophies. By implying—i.e., they presented ideas in a "do what works" attitude without exploring or explaining the implications of that philosophy—pragmatism (such as justifying suggested changes by stating that they are necessary in order to keep psychology relevant and economically viable) while suggesting the utilization of traditional positivist science (such as promoting the use of multilevel-modeling techniques and other advanced statistical procedures to represent better the *true* experiences of clients) and also advocating for an interpretivist philosophy (such as suggesting the need to "move away from simplistic, reductionism views of linear causality," p. 1046), Snyder and Elliott have constructed a model using three incompatible building materials, the very definitions of which often include a specific reference to the rejection of one or more of the other. One might assume that Snyder and Elliott (2005) were attempting to escape this philosophical bind by relying on the pragmatic philosophy that psychologists just need to do what works, but as stated previously, they were not explicit about this. By failing to explore their philosophical assumptions and the implications thereof, they have built a model out of incompatible pieces. Despite several potentially good ideas, it is thus doomed to fail, meaning it

ultimately cannot pass even the test of pragmatism.

Hermeneutics and Humility

I have thus far provided some philosophical criticisms of Snyder and Elliott's (2005) ideas for reform in training psychologists. These critiques are centered on the authors' omission of a discussion acknowledging that the philosophical problems dividing the field of psychology cannot be solved without addressing the philosophical incompatibilities among different ideas. To be consistent with the hermeneutic philosophy I am proposing as a solution, I must here acknowledge my own biases and be explicit about my own contextual background. As outlined in Chapter 2, much of the disagreement surrounding the scientist–practitioner gap arises from disagreements regarding the role of science in guiding the field of psychology and regarding what techniques, methods, methodologies, and philosophies qualify as science.

One of the central arguments of this dissertation is that any type of science is founded upon philosophical building blocks, and thus arguing for one type of science over another must be recognized as arguing for one philosophy over another. However, as Gadamer (1960/1989) presented in *Truth and Method*, the methods one might utilize to establish the truth of an argument are inseparably embedded within the philosophies that produced them. Therefore, if I were to attempt to prove the superiority of one type of science over another using the methods from the science for which I am advocating, I would be attempting, as the metaphor goes, to pull myself up with my own bootstraps. Given this challenge, it is fair to ask whether there could ever be any resolution to philosophical differences or if those who embrace competing philosophies are left to agree to disagree.

What I am acknowledging here is that my entire argument is, of course, based on a philosophy just as unprovable as any other. My position—that science is not inherently valid by

its mere adherence to its own rules and that one must examine the philosophies upon which those rules were established in order to evaluate the quality of the science in question—rests on a hermeneutic philosophy for interpreting reality. Based on hermeneutic philosophy, I assume that truth and meaning are interpreted constructs and that they must be worked out dialogically among all who hold a stake in their interpretation. Agreeing with Kant's (1781/1998) depiction of noumena and phenomena, I do not fully reject the possibility of universal truth or a reality external to humans, but I believe that our perception of that truth is always limited by our own culturally constructed tools for interpreting it. I do reject the radical relativism of some constructivist philosophies, based on my rejection of the applicability of reductionism to human beings. In other words, I do not believe that the human self can be reduced to isolated individuals (Cushman, 1990) but rather that a foundational quality of being human is communication with and responsibility to other humans (Buber, 1970; Gergen, 2009).

These core philosophical assumptions underlie my argument that psychologists must firstly be willing to examine the philosophies beneath their own assumptions about science and practice and secondly be willing to engage in dialogue with those who accept different philosophies. Agreeing to disagree is not, in my opinion, an acceptable position to take when we are claiming that we possess uniquely qualified information and skills that will improve the quality of life of those who seek our services.

Plural Monologues vs. Dialogue

One of my stated goals in this dissertation is to model how hermeneutic engagement is different from monologic exchanges typically seen in the field of psychology. My primary criticism of Snyder and Elliott (2005) is that they presented many ideas that seemed to fit together on the surface but that they failed to examine the underlying philosophical assumptions

upon which their ideas were based. I further argue that once these underlying philosophies are examined, it becomes clear that the pieces of their model cannot fit together without contradicting each other. One cannot practice psychology from a pragmatic philosophy, an empirical philosophy, and an interpretivist philosophy simultaneously because each of those philosophies includes rejections of core tenets of the others. I commend Snyder and Elliott (2005) for eliciting responses from their colleagues: "An in-depth and extensive exchange of ideas is precisely what our field needs right now" (p. 1049). However, in their response to their colleagues' responses, they added that they "assured them that we would not write a rebuttal to their comments" (Elliott & Snyder, 2005, p. 1197). They assumed that such an assurance would improve colleagues' willingness to be candid in their criticisms, but they also effectively ensured that the exchanges would be monologic, "agree to disagree" arguments, rather than dialogic engagements with expectations of collaborative work toward resolutions of differences. To engage dialogically, one must begin to shift one's goals away from the traditional pursuits of knowledge (reduction, prediction, and control) and toward the hermeneutic pursuit of understanding. To demonstrate this shift, I will analyze two representative responses to Snyder and Elliott's (2005) model and discuss how they could be presented dialogically.

Hayes

Hayes (2005) began his criticism of Snyder and Elliott's (2005) model by explaining that the scientist–practitioner model could be reduced to two main points: "clinical psychologists had to be responsible for the development of clinical psychology's scientific base, and practice had to be linked to that knowledge base" (p. 1055). He argued that Snyder and Elliott did nothing to either argue against these points in order to justify a fundamental change to a different model or to argue for them in order to justify a renewal of the original model. Hayes stated that their

suggestions were little more than "common sense solutions to a complex intellectual task" (p. 1056) and countered that updating training models for psychologists would instead require a return to the fundamental core questions of the discipline: "What is psychology? If clinical psychology does not stand alone, how do we best create a progressive discipline of psychology? If clinical psychology is more than an art, how do we best link practice to disciplinary knowledge?" (p. 1056). Hayes then provided 11 rules that he believed would help guide future efforts toward improving the field of psychology and concluded his article by stating that he hoped psychologists would take a more proactive role in that improvement.

As I demonstrate how Hayes (2005) might have approached his engagement with Snyder and Elliott's (2005) article dialogically, I must first acknowledge that journal articles are a poor medium for dialogic engagement. Conferences and face-to-face meetings obviously present a much more natural setting for discussion. However, attempts have been made to demonstrate a back-and-forth dialogue in which authors respond to one another in a series of articles (e.g., *The Journal of Constructivist Psychology, 24*(4); *The Journal of Theoretical and Philosophical Psychology*). Hayes's (2005) 11 rules actually fit nicely into a shift from monologic exchange to dialogic engagement, as each could easily be transformed into specific questions for Snyder and Elliott. Rule 1, "be clear about your philosophical assumptions and be guided by them" (Hayes, 2005, p. 1056) could be reframed thus: It seemed to me that you omitted an important discussion about the philosophical assumptions guiding your suggestions. Was this intentional? Could you speak somewhat regarding the philosophy behind your model? Rule 2, "define psychology clearly and stick to that definition" (Hayes, 2005, p. 1057) might become this: Could you provide your own working definition of psychology? In the exchanges that follow each question, Hayes would have opportunities to explain his own perspective that leads him to value the rule he

created. At the same time, true pursuit of understanding requires that each party genuinely desires to know how the other's perspective differs from her or his own. If two of us see the same problem in different ways, we must each have a sincere hope that those things the other sees that I do not might contain important keys to the solution of the problem. Gadamer (1960/1989) pointed out that we are not simply abandoning our own claims and agreeing with a different perspective because it is different. Only through sincere and thorough dialogue can we hope to expand our own perspectives through others' willingness to share theirs, allowing us to then reevaluate our own positions with gratitude that we have gained a broader perspective. Furthermore, I must be aware that initial exchanges will almost necessarily be superficial, and my inclination, if I am unaccustomed to hermeneutic engagement, will be simply to plug those superficial differences into my own philosophical worldview in order to make sense of them. The longer I can put off this step (though it is perhaps ultimately inevitable, as hermeneutic philosophy asserts that we can never transcend our own interpretive lens), the greater the chance I have of understanding more of the context and the deeper meanings that lead my partner to interpret differently than I do.

For example, Snyder and Elliott might respond to Hayes's (2005) initial question about guiding philosophies by explaining that their intent was not to address psychology at a philosophical level. They might say that they recognize that there are many different philosophical camps in the field but that their model is meant as a blueprint for training psychologists such that they are prepared to face the specific challenges awaiting applied psychologists in real world settings. If psychologists wish to explore philosophical questions, they are free to do as long as they have been trained in the core areas identified in the matrix model (subtext: we are interested in pragmatic solutions, not in exploring the black hole of

philosophy of science). This superficial difference (Hayes believes it is important to explore guiding philosophies and Snyder and Elliott believe it is not) could lead the two parties to acknowledge that they are on very different pages, and that it might be easiest to just agree to disagree. A hermeneutic dialogue, however, would progress beyond this initial difference. Hayes might explain his belief that the goal to address specific challenges and to present solutions to those challenges one by one is based in the philosophy of pragmatism, while certain specific solutions within the model are based in the philosophy of empiricism, and others in the philosophy of constructivism, therefore the model is full of philosophical assumptions regardless of whether those assumptions are made explicit. Failing to acknowledge them is like a fish failing to acknowledge he is swimming in water. Snyder and Elliott might respond by stating that they are willing to concede that their arguments have philosophical components, but that they are choosing instead to focus on the data: psychologists are increasingly unprepared to work within the framework of the managed care health system. We could talk about *why* that is for the next ten years, or we could do something to fix it. The matrix model presents a way to fix it, thus ensuring that psychology remains viable as a profession long enough for those who are interested in philosophy to continue exploring questions about epistemology. This exchange could go on indefinitely, with each side failing to take a step beneath the superficial disagreement.

A hermeneutic step might be for Hayes to express his understanding of Snyder and Elliott's position. He can see that a pragmatic solution would be highly desirable, because he, too, would like to see psychology improve its status as a respected profession among health service providers. He could perhaps give an example of uninterpreted data: a chart of random numbers with no explanation for what any of them represent. Data, he might argue, do not speak

for themselves. They must be interpreted, and those who interpret them must agree on a system for their interpretation (Slife & Williams, 1995). One could interpret the data suggesting that psychologists are poorly prepared for applied settings through an entirely different system that leads to identifying a different root cause of the problem, suggesting a different solution. Therefore, Hayes might argue, they both want to focus on the data, but in order to escape the wild goose chase that has typified the past 60 years of seeking theoretical harmony in psychology, they must start with how they are generating and interpreting the data, rather than pretending that data speak for themselves any more than those random numbers do. "Before we give up and assume we are not talking about the same thing," Hayes might say, "could we find out what we do agree on, and why?" The addition of *why* is a critical step toward a Gadamerian fusion of horizons (1989). The goal of this conversation must shift from *what* and *how* (explanation, or *erklären*) to *why* (understanding, or *verstehen*) if the two sides are to each leave with a better understanding of the other's perspective, such that their own perspectives can grow and they can have any hope of working together to resolve the scientist–practitioner gap.

Ingram

Hayes (2005) and others were primarily critical of Snyder and Elliott's (2005) model and other authors primarily offered praise for the model. Ingram (2005) stated that past discussions about training psychologists have focused too much on achieving the right balance between science and practice, to the exclusion of examining underlying assumptions. He believed that Snyder and Elliott successfully broke that trend by providing a model that not only gave practical suggestions for specific changes but also "acknowledge[d] and question[ed] some long-standing but often unstated assumptions in the training of clinical psychologists" (Ingram, 2005, p. 1155). He identified these as the shift from a pathology-based model to a strength-based model and the

inclusion of training in health psychology for all graduate students. When an author wholeheartedly supports another's perspective, a hermeneutic engagement can take a different course from that of two authors who disagree. Ingram expressed that he shared Snyder and Elliott's fears that psychologists would paint themselves into a corner and become irrelevant if they did not find a way to integrate psychology more fully into the managed health care system. To engage hermeneutically around agreement, Ingram could focus on seeking understanding beyond the superficial agreement. For example, after stating his agreement with the need for training in health psychology, Ingram explained his belief that managed care and its reliance on diagnostic codings was an inevitable part of psychology's future. He might then ask Snyder and Elliott if they agreed or if they could see a path to a complete separation from the medical model in psychology. As stated previously, an impediment to hermeneutic dialogue is the assumption that we are both understanding a concept in the same way, despite our different historical and cultural backgrounds that likely lead us to interpret even similar concepts differently. By engaging on a deeper level, Ingram might discover areas where he possesses some insight that Snyder and Elliott lack and vice versa. Such discoveries are important in order to avoid oversimplifying for the sake of harmony.

Another way to ensure that dialogue proceeds beyond the superficial is for the two agreeing parties to work together in engaging a third party. It is interesting that Ingram (2005) complimented Snyder and Elliott (2005) on their ability to make hidden assumptions explicit and to confront them directly and that Hayes (2005) criticized them for failing to acknowledge their philosophical assumptions. As the articles now stand, there is no resolution to this contradiction. A hermeneutic engagement could begin with a clarification regarding assumptions in general and then going progressively deeper so that each party began to expand their understanding about

what Snyder and Elliott did and didn't accomplish in their article. It is likely that each author was using the term *assumption* to refer to different concepts. A hermeneutic dialogue would allow these groups to begin by explaining the different understandings of the term, but then proceed to an exploration of both shared ideas and differences until each better understands not just the term, but why the ideas behind the term are important to the authors and how they make up key elements to their approach to training psychologists or practicing psychology.

I will not similarly explore all 28 of the responses to Snyder and Elliott (2005), but I have provided these examples as a template for how a hermeneutic engagement could occur. In a monologic exchange, each person or group takes the time to explain his or her perspective, but there is not a chance to engage with one another such that understanding is even sought, let alone achieved. Despite the invitation for different voices to be heard, those voices are being projected into the ethos with no confirmation that they were heard, interpreted, or understood, or that the hypothetical interpretation or understanding did anything to expand the perspectives of those who took part in the exchange.

Vivian, Hershenberg, Teachman, Drabick, Goldfried, and B. Wolfe

In presenting their translational model for research–practice integration (RPI), Vivian et al. (2012) stated that

> it is imperative that psychologists find a way to collaborate, given that the rates and global burden of mental health problems remain remarkably high, and there have been decreases in the use of psychotherapy interventions compared with psychotropic interventions, despite strong evidence of the effectiveness of psychotherapy interventions. (p. 143)

The RPI model included "four levels of translation that lead to a multiway bridge between research and practice" (Vivian et al., 2012, p. 144). The authors specified that this bridge was intended to be bidirectional, such that researchers and clinicians had the means for translating knowledge and for transmitting that knowledge back and forth in a collaborative approach to providing the best care for those who seek psychological services. From the outset of the article, it was clear that the authors were conceptualizing the scientist–practitioner gap as an inability to work together due to a lack of the proper tools and channels. The RPI model is thus a structural solution, outlining a clear organizational chart that Vivian et al. believed would reduce the gap.

> As an overview, broad-based psychological research pertaining to biological, psychological, and social bases of behavior, as well as clinical observations, can lead to empirically testable hypotheses. This research can (a) be exported directly to practice to inform the psychotherapist's treatment implementation, and (b) lead to the development and evaluation of novel treatment studies, which we highlight in our translational model. (Vivian et al., 2012, p. 144)

The RPI solution is therefore based on an unstated assumption that clinicians and researchers are in complete agreement about what constitutes valuable scientific information but simply lack the communication platforms necessary to ensure that information flows back and forth to advance both science and practice ("it is imperative that psychologists find a way to collaborate," Vivian et al., 2012, p. 143). I have consistently argued herein that the reason previous attempts to resolve the scientist–practitioner gap have failed is because the strategies do not address that the two sides have widely divergent ideas about what kinds of information are helpful for improving mental health. Here it is important to be transparent about another of my own biases by acknowledging that there is a tremendous number of practitioners who value

empirical science above all other epistemologies and who would esteem Vivian et al.'s (2012) RPI model as a highly valuable contribution. Conversely, I have repeatedly cited researchers and scientists who devote their careers to arguing that natural scientific methodologies are entirely inappropriate for the field of psychology. However, the assumption that positivist scientific research methodologies, built upon a traditional empirical epistemology, are the "most scientific" and therefore the best path to improving mental health is not supported philosophically, nor can it be supported scientifically due to the bootstrap problem. Hillman and Ventura (1993) argued this point extensively in their book entitled *We've Had 100 Years of Psychotherapy: And the World's Getting Worse*. Although many clinicians have been taught the superiority of empiricism and would have no objections to the underlying assumption in Vivian et al.'s model, those clinicians who do object have a compelling case that naturalistic science has improved only the reliability of labeling disorders while actually slowing the potential progress psychology could make toward improving mental health (Hillman & Ventura, 1993).

A second underlying assumption in the RPI model is evident in their statement that "the rates and global burden of mental health problems remain remarkably high, and there have been decreases in the use of psychotherapy interventions compared with psychotropic interventions, despite strong evidence of the effectiveness of psychotherapy interventions" (Vivian et al., 2012, p. 143). This is an appeal to pragmatism: if researchers and clinicians don't learn to work together, we will continue to see mental health problems rise, and those who experience them will seek medical intervention, rather than psychotherapy. This statement avoids some unappetizing questions, such as "Could our approach to psychology actually be *responsible* for the increase in mental health problems?" The authors cited evidence that psychotherapy has proven to be at least as effective as medication in treating mental illness, but that statement rests

on the assumption that reducing the operationalized symptoms of mental illness used in empirical studies equates to improving mental health. That assumption has also been challenged extensively by many researchers (e.g., Albee, 2000; Garza & Fisher Smith, 2009; Leventhal et al., 1997). I am a strong advocate of psychotherapy and I do believe that in many cases it is an intervention preferable to psychopharmacological ones, but I raise questions regarding Vivian et al.'s pragmatic argument. Similarly to Snyder and Elliott (2005), they are attempting to blend a pragmatic philosophy (no discussion about *why* we might carry this out, just *how*, because we are not concerned with objective reality) with an empirical solution (more empirical hypothesis testing and better communication of results, so that we are constantly growing closer to an objective reality). As I argued previously, such philosophical incompatibilities will surface as the idea is carried out and will doom the plan to failure. For example, a pragmatic focus on doing whatever works can potentially lead to a highly effective treatment that cannot be supported empirically. The solution in that case, according to this model, would be to discard that treatment and develop one that could be supported empirically.

Epistemological Privilege

The RPI (Vivian et al., 2012) model explains a clear method for clinicians to communicate their discovery of an effective treatment to scientists, with the implication that the scientists can then generate empirical studies to validate the clinicians' experience. However, to study empirically the complex interpersonal experience that happens in psychotherapy, that experience must be reduced to operationalized and observable components. Sometimes this can be done successfully, and empirical studies can either confirm the efficacy of a treatment or discover important problems that need to be worked out. Other times, a treatment that is in fact highly effective may simply defy the reduction process and fail to be supported empirically. In a

system that reifies the philosophical superiority of empirical knowledge, such as the RPI, the scientists are left with no other option than to inform the clinicians that their treatment is not scientifically valid.

Level 1 of the translational model consists of research on treatment validation. This level includes four different stages of research studies. Stage three, studying mechanisms of change, is itself broken down into five categories: (a) moderator studies, (b) mediator studies, (c) dismantling studies, (d) studies of the therapeutic relationship, and (e) common factors studies. In describing each of these categories, Vivian et al. (2012) emphasized how interventions are broken down into their component pieces in order to study empirically the role that each piece plays in treatment outcome. Level 1 of the RPI model is described in greater detail than any other level, because it contains all of the keys to ensuring that practitioners have tools that are empirically supported. The entire model is based on the unifying goal of the researchers: to ensure that practitioners are provided with empirically supported treatments. For me to suggest there is anything questionable about that goal would sound like ascientific heresy to those who wish to perpetuate the epistemological privilege of positivist science (e.g., Baker et al., 2009). However, the language used throughout the RPI model is typical of oppressive language that indicates the authors are either unaware of or unconcerned about their privileged status. Although a member of a privileged population might state that he or she is open to differences, the use of language that alienates and excludes others, whether used inadvertently or not, erects a powerful barrier to inclusion, open communication, and collaboration (Stoudt, Fox, & Fine, 2012). Vivian et al. (2012) did not explicitly state that the RPI model could not be used for research based in alternative scientific paradigms, but they did emphasize repeatedly that the purpose of the model was to increase the production and application of empirically supported

treatments, e.g., "the robust design of RCTs provides numerous advantages in establishing the internal validity of the intervention(s) evaluated" (p. 145); "we also support efforts to identify and evaluate components of ESTs . . ." (p. 145); "another critical component to enhance our use of ESTs . . ." (p. 145); "an empirically informed curriculum may include basic research . . ." (p.147); "incorporation of empirically supported 'relationships,' and empirically supported 'principles of change'. . ." (p. 147).

Similarly, a counseling center that provides a handout outlining all of its services designed for men, with specific references to ensuring increased masculinity and improved ability to perform traditional male roles, does not need to state specifically that women are not welcome in order to for them to feel excluded. Perhaps this counseling center also promotes itself as having an ideal solution to the breakdown in communication between men and women because it teaches people to focus on clean, masculine behavioral solutions, rather than messy, feminine emotional solutions. Under the assumption that it is always clearly preferable to be masculine, such a counseling center might sound like a great place to go for communication problems. If, however, there were an individual who questioned that assumption, it is not difficult to see why that person might choose a different counseling center, rather than putting effort into engaging with this counseling center to try to work out a collaborative solution to the breakdown in communication between men and women. Although it may appear extreme to compare the RPI model (Vivian et al., 2012) to this imaginary counseling center, this is an example of carrying an assumption out to its logical conclusion. Vivian et al. (2012) suggested that this model would provide a solution to the scientist–practitioner gap by allowing information to flow freely between scientists and practitioners. They went on to consistently define knowledge as empirical data, which ensured that those who reject the epistemological privilege

of positivist science would see this model similarly to how one might see the fictional counseling center. Choosing to not acknowledge those on the other side of the gap does not resolve the gap.

Lochman, Powell, Boxmeyer, Andrade, Stromeyer, and Jimenez-Camargo

Lochman et al. (2012) provided an example of how researchers might help to bridge the gap by working collaboratively with practitioners to adapt their programs for application in practical settings. However, the article documenting this success, written by the researchers who created the program, conspicuously lacks any indication of the clinicians' voices in the process, other than documenting their specific requests for adaptations. The exchange, in short, goes a bit like this: it is difficult to implement such a lengthy program (34 sessions over a two-year period), can we make it shorter? The researchers run some experiments and say yes, the program can be shortened from 34 sessions to 27 if done according to their instructions. That's still quite long; could we get it down to a one-year program, and then perhaps provide a booster session? The researchers run some experiments that indicate that the unstructured booster sessions actually reduce the effectiveness of the program, so no, you can't do that. It's hard to get the parents to attend the parent sessions. Will it still work if we just do the child sessions? The researchers demonstrate empirically that eliminating the parent component does not reduce the positive outcomes at school, but it does reduce the positive outcomes at home, so eliminate it only if willing to accept that loss, and so on through all of the discussed adaptations. Although this certainly does seem to be a collaborative exchange between researchers and clinicians, it is far from a dialogue, at least as presented in Lochman et al.'s (2012) article. Instead, the clinicians are under the complete control of the researchers. In each of the exchanges this is clear, but particularly so in the case of the unstructured booster sessions, which provided an example of the researchers clearly indicating that any variation from the prescriptive treatment they suggested

would actually undo the positive effects. One might wonder how these unstructured booster sessions were any different from teachers or parents reminding the students of and reinforcing what they had learned (an important element of the program, according to the article), yet Lochman et al. (2012) indicated that their findings raised "important cautions about the use of unstructured booster sessions" (p. 137).

The suggestion that this approach is a collaboration, and that it addresses research-practice gaps seems to indicate a poor understanding of the significance of the gap. Lochman et al. (2012) appear to share Vivian et al.'s (2012) assumption that the gap exists merely because researchers and practitioners lack a system for communicating. Although there are many cases in which researchers and clinicians share philosophical assumptions about science, and for which a basic model for improving communication might be all that is required to facilitate a collaborative relationship, the classic dilemma of the scientist–practitioner gap goes far beyond communication. It results from a fundamental difference in beliefs about ontology and epistemology, leading to vastly different definitions of science and to incompatible methodologies built on those definitions. What Lochman et al. presented was not a collaboration in which the philosophical differences between a nomothetic and an idiographic approach to treatment were each carefully considered, but rather the nomothetic philosophy of the researchers was being used as the standard to either grant or deny permission to the idiographic clinicians' requests. The manifestation of a system built on the assumptions shared by these researchers is a system that discredits and silences the voices of those who are on the other side of the gap, rather than building any sort of a bridge to span the gap.

Lambert

It appeared that Lambert (2012) successfully achieved this blending in some areas where

others had failed. He seemed to be speaking directly to the idiographic–nomothetic divide by acknowledging that there needed to be some way to address the "outliers," or those who, for whatever reason, were not going to fit into the group of those who would normally benefit from whatever evidence-based treatment a clinician might use with a given client. In order to persuade clinicians and others to take his argument seriously, Lambert cited studies that indicated that clinicians typically overestimate their effectiveness to a large degree. Whereas Lambert's data indicated that anywhere from 30 to 60% of clients either drop out of treatment or deteriorate during treatment, the clinicians in the survey he cited believed that 85% of their clients improve or recover. Lambert suggested that clinicians will have a clearer picture of their clients' progress if they measure outcomes from session to session, and will therefore be able to adjust treatment for the clients who are not progressing or responding to treatment according to expected norms. Thus, he argued, his outcome-tracking software will allow clinicians to take research into their own hands and provide them with the data they need to adjust on the fly and catch those clients who may fall outside the expected range of response to treatment.

Upon examining the assumptions behind Lambert's (2012) argument, it is clear that he has taken the same approach as the other authors mentioned in this section. Lambert has assumed that because there are many practitioners who accept the philosophy of science behind his research and who are willing to utilize his systems to bring their practice into better compliance with his conceptualizations of improved outcomes, he has successfully bridged the scientist–practitioner gap. By situating his system within a positivist science paradigm, however, Lambert is not addressing the gap at all. Although he implied that he was taking an idiographic approach by providing a system that would catch the outliers—those who don't respond to treatment as usual—a careful reading of his article reveals that he is merely encouraging

practitioners to cast a wider nomothetic net. Whereas a clinician who is not monitoring outcomes as Lambert suggests may help 40 to 70% of clients, Lambert indicated that his outcome questionnaire would help clinicians identify early on those clients who were likely to fall outside that range and adjust treatment accordingly. Again, this approach sounds very much like an idiographic approach to specializing treatment for individual clients. However, the means for identifying the outlying clients is based on empirical data generated by a computer program that analyzes a predetermined set of questions that are based on operationalized indicators of mental health. Using these nomothetic indicators, the computer program flags certain clients so that the clinician can adjust treatment in order to bring those clients back under the normal curve with the rest of her or his caseload.

Lambert's (2012) argument was essentially this: Clinicians all believe that they are helping nearly all of their clients, but positivist science indicates otherwise. Clinicians' judgment is significantly clouded by their desire to believe they are good at what they do. When clinicians simply apply across the board the evidence-based treatments indicated for the presenting concerns of their clients, many clients fail to respond positively. Therefore, clinicians who utilize a computer program to identify which clients are actually improving and which are not can further analyze those clients who do not respond positively in order to increase the specificity of the initial computerized treatment indicators. Eventually, through a continuous feedback loop between clinician and computer program, clients will be grouped into far more specific categories, and empirically supported treatments will be developed for each category so that every client's responses to the intake questionnaire will indicate the correct treatment, and the number of clients who drop out or deteriorate will decrease accordingly. This argument can seem highly convincing and difficult to refute without sounding like I am rejecting science and

technology out of defensiveness regarding my own clinical judgment.

Lambert (2012) made some interesting points at the end of his article regarding clinical judgment. Acknowledging the limitations of a computer-based system, he indicated that clinical judgment would be required to assess situations in which a client might not provide forthright information on the questionnaire. In his concluding paragraph he added, "There are certainly cases for which the information is not clinically useful and in any case the laboratory test data cannot substitute for or replace clinical judgment" (Lambert, 2012, p. 113). This apparently blatant contradiction between his central argument noted in the previous paragraph and his concession that clinicians must ultimately rely on their own judgment might be a perfect starting place to engage in hermeneutic dialogue regarding Lambert's perspective.

Hershenberg, Drabick, and Vivian

In their proposed doctoral training program reform, Hershenberg et al. (2012) suggested that the central focus needed to be on establishing a more complete definition of evidence-based practice (EBP) and then building a training program around adherence to that definition. They proposed that EBP be defined to include three elements: "the best research evidence; clinical expertise; and client characteristics, values, and preferences" (Hershenberg et al., 2012, p. 123). From the outset of their article, these authors emphasized the need to expand on what constitutes evidence. They stated that empirically supported treatments (ESTs) are certainly one important type of evidence but that they must be considered just that: one type of evidence among many. One of the primary differences between their approach and that of some other programs is this desire to be inclusive in their definition of evidence. They went beyond assuming a dichotomy between empirical research and anecdotal practice and instead developed a full spectrum of ways to conceptualize clinical expertise based on demonstrable evidence—empirical or otherwise.

Among all of the approaches to bridging the gap I have analyzed thus far, this is the first to acknowledge that there could be a scientific approach to practice rooted in something other than traditional positivism.

In an effort to expand further the practitioner's argument for legitimacy, Hershenberg et al. (2012) chose to differentiate between clinical experience (which has earned a rather dubious connotation due to the extensive literature questioning both its validity and reliability) and clinical expertise. Expertise, they said, qualifies as one of the three realms of evidence, whereas experience alone does not. They explained that in order to advance from experience to expertise, a clinician must develop skills beyond the selection and application of ESTs. Rather, a clinician must learn the core principles of psychotherapy that will allow her or him to maintain flexibility throughout the treatment process. These include case conceptualization skills, selection of an appropriate intervention based on the conceptualization, construction and assessment of the therapeutic relationship, assessment and appropriate utilization of therapist emotions, development of the qualities (e.g., warmth, flexibility, honesty, alertness) that are considered common factors for therapeutic success across theoretical orientations, and growth through supervision. The authors asserted that mastery of these skills and qualities leads to expertise that qualifies a clinician to provide one type of evidence that his or her treatment approach is scientifically valid.

The core assumption beneath Hershenberg et al.'s (2012) assertions is that empirical evidence is not the only basis for scientific proof. As I have indicated throughout my analysis, during a discussion of the scientist–practitioner gap it is important to recognize that there are scientists who fall on the side of the *practitioner* argument and practitioners who fall on the side of the *scientist* argument. What determines one's side is not one's profession but rather the

philosophical paradigm one espouses in conceptualizing mental health and psychological treatment. The argument of the practitioner is that science is not automatically granted validity through adherence to predetermined scientific rules. Those rules are developed through a careful philosophical process, and if they are applied in a way that defies the philosophy on which they were built, the entire project becomes untenable. Each of the suggestions presented in the recent journal editions as ways to bridge the scientist–practitioner gap fall apart philosophically because they assume certain foundational beliefs to be true only to suggest then solutions that violate those beliefs. Hershenberg et al. (2012) presented the first solution in which the authors so much as acknowledge that the gap is not about being scientific or not being scientific but rather about what qualifies as science. They went on to present a doctoral training program that teaches students to understand and develop skills in different areas, based on these different perspectives regarding science. From my hermeneutic perspective, this article is an unheralded hidden gem among all of the other suggestions. I believe that if their program were implemented as explained it would make a tremendous contribution to reducing the gap. Unfortunately, the authors quickly glossed over the greatest impediment to their program's success: "the adoption of EBPs into training needs to be accepted by all faculty; . . . motivation to change may need to be addressed" (Hershenberg et al., 2012, p. 131). In an article that understated its own contribution to the field, this understatement still stood out. Addressing motivation to change among doctoral faculty is a large task, and it is my hope that this dissertation serves as a valuable tool in doing just that by explaining the significance of the gap and the urgent need to work together if psychologists truly hope to provide a beneficial service to their clients.

Results

Snyder and Elliott (2005) presented a model for completely restructuring the training of psychologists. A philosophical analysis of their model demonstrates that they built it upon a pragmatic belief system. They presented a patchwork of ideas that each seemed to have proven successful on its own and they suggested that a training program should be built upon an amalgam of these successful pieces. They did so without regard for the philosophies upon which each of the individual pieces were built, which is consistent with a pragmatic philosophy: if it works, it should be implemented without becoming bogged down with explanations or questions about why. What they did not state explicitly about their approach is that it is entirely theoretical; there is no evidence that combining all of the elements together will be successful. Theoretical work can be greatly beneficial to the field, but the standard is to judge theory work on its philosophical consistency and logical application. Snyder and Elliott's model seems to appeal to the philosophy of pragmatism in its lack of theoretical explanations, but its pragmatic value is dubious because the pieces it blends are built upon contradictory ontologies and epistemologies regarding truth. There is no evidence to support the belief that ideas that worked well in different contexts will continue to work well when combined together in a new context; thus, Snyder and Elliott's model is not sound pragmatically.

Responses to Snyder and Elliott's (2005) model were universally monologic, regardless of whether they supported or refuted the model. Snyder and Elliott virtually assured this would be the case when they encouraged candor by promising not to offer any rebuttals to any of the comments. Unfortunately, this also ensured that little would be accomplished regarding bridging the scientist–practitioner gap. The gap (differing opinions about the most appropriate philosophy of science for the field of psychology) would not be nearly so problematic if those on opposing

sides understood and valued the approach of the other. Choosing to present ideas monologically, without an attempt to engage and expand one's perspective, is to ignore the gap and instead focus on one's own perspective exclusively.

Vivian et al. (2012) suggested a model for translating information across the gap, thus facilitating the integration of research and practice. The model was built on the assumption that empirical science is the standard for evaluating all claims in psychology. Their understanding of the scientist–practitioner gap appeared to be that those on both sides were in agreement about the need to rely exclusively on positivist science but that they lacked the communication tools to transmit information between one another. Although Vivian et al. presented some valuable communication tools that will surely improve collaboration among scientists and practitioners who are already on the same side of the gap, they did nothing to bridge the gap between those who disagree about philosophies of science.

In conjunction with Vivian et al.'s (2012) RPI model, Lochman et al. (2012) provided an example of researchers who solicited feedback from clinicians in order to develop a scientifically sound program that also addressed the real world needs of clinicians in an applied setting. Upon analyzing the process Lochman et al. presented, it became clear that the practitioner's opinions about philosophy of science were not important. The researchers took requests from the clinicians and either granted or denied the requests based on an empirical epistemology. Lochman et al. thus demonstrated not that they could collaborate across the gap but that they were willing to plug clinicians' ideas into their own philosophy and then let them know which of their ideas they would support.

Lambert (2012) also indicated that he was reaching across the scientist–practitioner gap to enhance the practice of psychotherapy through regularly assessing treatment effects. By

showing how the idiographic concerns of clinicians could be analyzed objectively, Lambert claimed to be placing the research tools in the hands of the practitioner to enhance treatment effectiveness, particularly with clients who do not respond as expected according to nomothetic norms. An analysis of Lambert's argument reveals, however, that he is following the same path as Lochman et al. (2012) in attempting to merge clinician concerns into a positivist framework. Despite language indicating that he values the idiographic perspective and that clinical judgment is vital to the successful use of his outcome questionnaire, Lambert did not address how to reconcile this with his foundational argument that clinical judgment is unreliable.

Hershenberg et al. (2012) presented a doctoral psychology training program that was based on teaching students to understand evidence-based practice (EBP) and that incorporated divergent definitions of evidence in order to ensure that psychologists are able to maintain flexibility and address real world challenges without compromising the scientific integrity of the field. Among the articles included in this analysis, this was the only one that appeared to acknowledge the philosophical nature of the scientist–practitioner gap and suggest a solution that addressed the paradigm wars. The primary shortcoming of this article from a hermeneutic perspective was its brevity. Despite acknowledging that there would surely be resistance to the implementation of their training program, Hershenberg et al. neither fully expanded on why their model was so different from other proposed solutions nor provided any suggestions for how to address the inevitable resistance it would face.

Conclusion

Having analyzed the data from Snyder and Elliott (2005) and Vivian et al. (2012), the final step of this dissertation is to explain and demonstrate how a hermeneutic solution to the scientist–practitioner gap can succeed where previously suggested solutions have failed. In the

final chapter I will provide an argument for addressing the gap at the ontological level rather than an epistemological level or a methodological level as has been done in previous attempts. Finally, I will argue that this hermeneutic solution is a novel and valuable contribution to the field of psychology because it addresses the philosophical nature of the scientist–practitioner gap and provides a method for communicating across different philosophies of science.

CHAPTER 5

HERMENEUTIC SOLUTION

> In some circles it is becoming a new orthodoxy that the whole enterprise from Descartes, through Locke and Kant, and pursued by various nineteenth– and twentieth–century succession movements, was a mistake. What is becoming less and less clear, however, is what exactly it means to overcome the epistemological standpoint or to repudiate the enterprise. Just what is one trying to deny? (Taylor, 1995, p. 2).

Epistemology

Western science has a strong empirical tradition, meaning that scientific knowledge is attained through sensory means. A phenomenon must be observed, the methods of observation must be replicable, and explanations for the phenomenon must be consistently demonstrated in order to claim that the phenomenon is known scientifically. This tradition relies on several epistemological assumptions about reality. These epistemological assumptions address philosophical debates regarding truth, beliefs, and justification. The acceptance of certain assumptions about truth, beliefs, and justification leads to further debates regarding evidence and its sources and whether knowledge is structured foundationally or coherently, which spawns questions about virtues, morals, and reason (Steup, 2013). When one uses the term *scientific* in a common setting, the intention is usually to communicate something about the methodology that ensures that the subject has been investigated empirically. The term becomes an efficient

shorthand for an agreed upon set of assumptions. However, I wish to emphasize here that the shorthand is representing a tremendously complex philosophical idea. Thousands of pages are dedicated to arguing the merits of foundationalism or coherentism, of deontological justification or non-deontological justification, of evidentialism, reliabilism, or skepticism. All of these components of epistemology warrant careful exploration, but *answers* are not available. Rather, philosophers provide logical arguments for accepting one assumption over another. I referred earlier to the bootstrap problem: that one cannot investigate the truth of an epistemological concept without using methods based in some epistemology. Pollock (1975) explained,

> To justify a belief one must appeal to a further justified belief. This means that one of two things can be the case. Either there are some epistemologically basic beliefs that we can be justified for holding, without being able to justify them on the basis of any other belief, or else for each justified belief there is an infinite regress of potential justification (the nebula theory). On this theory there is no rock bottom of justification. Justification just meanders in and out through our network of beliefs, stopping nowhere. (p. 21)

Although each of the articles analyzed in Chapter 4 addressed important elements of the scientist–practitioner gap, the essence of the gap is disagreement about the key pieces of epistemology that form a foundation upon which to conduct methodical and systematic inquiry. Because scientists and practitioners (I use these terms loosely, as I have previously noted that many scientists fall on the side of the practitioner philosophically, and many practitioners fall on the side of the scientist philosophically) hold different assumptions about epistemology, bridging the gap requires some way to communicate about truth without relying on one accepted set of assumptions.

Verstehen

To approach this communication among those who accept different assumptions, I return to the concept of pursuing understanding rather than knowledge. This shift in thinking is typically attributed to Simmel (1892/1977) and Weber (1917/1949), each of whom focused on developing an alternative to positivist science. Because positivist science assumes a concrete and universal reality and its methods are designed to discover that reality, it is not useful for those who reject that assumption. Interpretive science assumes instead that reality is a mutually agreed upon concept and that language and meaning are central to the process of agreeing. The goal of interpretive science, therefore, is increased understanding among the participants in the pursuit of reality. This is the philosophy of science upon which constructivist paradigms are built. The primary criticism of constructivism is that it can easily lead to radical relativism, in which no assertion of truth can claim greater authority than any other, given that there is no concrete and universal reality. Rorty (1980) argued against this criticism:

> *Relativism* is the view that every belief on a certain topic, or perhaps about *any* topic, is as good as every other. No one holds this view. Except for the occasional cooperative freshman, one cannot find anybody who says that two incompatible opinions on an important topic are equally good. The philosophers who get called *relativists* are those who say that the grounds for choosing between such opinions are less algorithmic than had been thought. (p. 727)

The pursuit of understanding through interpretivist science rejects the assumption that science must be a pursuit of universal laws and accepts the assumption that truth claims are embedded in systems of language and meaning. As Rorty indicated, this does not mean that all truth claims are equal. It only means that interpretive scientists are engaged in a "less algorithmic" process,

having rejected the pursuit of universal laws for predicting and controlling human behavior and instead seeking methods to communicate and develop understanding in a cooperative effort to improve quality of life for members of societies. This willingness to seek understanding, rather than knowledge, is at the heart of Gadamer's (1960/1989) hermeneutic science, and I suggest that it contains the solution to the scientist–practitioner gap.

The Hermeneutic Bridge

In Chapter 4 I provided dialogic alternatives to some of the monologic exchanges regarding the scientist–practitioner gap. Dialogue, speaking hermeneutically, requires more than simply speaking to one another. A hermeneutic dialogue involves an intentional and determined effort on the part of each member of the exchange to learn from the other. The essence of dialogue becomes acknowledging the limits of my perspective and therefore valuing that which another can offer (Taylor, 2011). Here I wish to clarify the basic steps to bridging the gap hermeneutically.

Erfahrung der Nichtigkeit

Erfahrung der nichtigkeit, meaning *experience of negation*, was of utmost importance to Gadamer (1960/1989) in developing the concept of scientific understanding. He explained that when one truly experiences something, that experience forces her or him to reconsider previously held beliefs about the world. The true experience negates my constructed meanings upon which I made sense of reality, forcing me to alter them in some way to account for the new experience. Gadamer argued that the goal of positivist science was to transcend experience or to reach some point where no experience could negate my fully comprehensive belief system. He proposed that such a point could never be reached, and in fact its pursuit was detrimental to human science, wherein such experiences are a goal in and of themselves. "Experience stands in an

ineluctable opposition to knowledge and to the kind of instruction that follows from general theoretical or technical knowledge" (Gadamer, 1960/1989, p. 353). In human affairs, the infinite possibility for novel experience will require abandoning the pursuit of universal knowledge in favor of the pursuit of understanding. This first step toward a hermeneutic bridging of the gap is therefore to approach the gap with a willingness to experience something new, and to have challenged the philosophical assumptions I have previously accepted. For the interpretivist, this would mean a willingness to challenge his or her rejection of elements of the positivist paradigm and vice versa for the positivist. Openness to the possibility that my perspective is both limited and limiting leads to a willingness to be challenged.

Seeing Me, Therefore Seeing You

Once the participants in a dialogue have accepted that their perspectives might be limiting, they must begin to examine their assumptions. I have previously articulated the myriad assumptions underlying every facet of any paradigm of science. This second step in a hermeneutic bridging of the scientist–practitioner gap is to revisit these assumptions, making them explicit and bringing them to the conscious level for examination and consideration. This step allows each participant in the dialogue to recognize his or her own peculiarity: no longer taking for granted that reality is what she or he previously accepted it to be, but instead noting that such a view of reality is unique to her or him. That view has been constructed and interpreted through several layers of assumptions and is not identical to any other person's view of reality. This awareness of one's own peculiarity brings about a simultaneous awareness that the other is not divergent from an imaginary *normal*, but rather that he or she simply has his or her own peculiarity: his or her construction of reality is equally unique (Taylor, 2011). This step removes the barrier that often prevents crossing the gap because each person assumes some

externally valid *normal* exists. Such an assumption leads to seeking that validation at the expense of seeking to understand the other. From this perspective, beliefs are at their core prejudices. Gadamer explained the importance of acknowledging and challenging all such prejudices.

> If a prejudice becomes questionable . . . this does not mean that it is simply set aside and the text or other person accepted as valid in its place. Rather historical objectivism shows its naiveté in accepting this disregarding of ourselves as what actually happens. In fact our own prejudice is properly brought into play by being put at risk. Only by being given full play is it able to experience the other's claim to truth and make it possible for him to have full play himself. (Gadamer, 1960/1989, p. 299)

Engaging

With prejudices now acknowledged and put into play, the next step toward bridging the scientist–practitioner gap hermeneutically is to engage in the dialogue itself. This step is where Gadamer's (1960/1989) *fusion of horizons* takes place. Gadamer used both the term fusion and the term horizon carefully in this depiction of communication. The term fusion was intended to depict the notion that a horizon is not merely extended through hermeneutic dialogue, it is fundamentally changed: what is seen is interpreted in a different way than it was before. Thus horizons are fused, both becoming something entirely new. The term horizon was meant to convey both the limits on one's perspective and also the fluid nature of that perspective. As one moves, the horizon changes, such that what is included or excluded is constantly fluctuating, requiring that dialogue remains open and dynamic.

A fusion of horizons can appear unrealistic if an assumption of representationalism is made. From a representationalist perspective, there is an external reality or truth, and each

person interpreting that truth might do so in a way that is incomprehensible to another, given cultural traditions, language systems, and conceptualizations of knowledge. This perspective is often what leads to Gadamer being labeled a relativist. If each person interprets from his or her own tradition, without the ability to transcend that tradition, and if there is no way to externally validate one interpretation as preferable to another, then truth becomes either relative or at least completely unattainable. Gadamer was clear in his refutation of such a representationalist epistemology. Rather than falling into relativism, this example demonstrates precisely the value of hermeneutic engagement. The very purpose of dialogue is to increase the comprehensiveness of understanding. Although superficial comparisons of the final depictions of truth among members of the dialogue may seem to yield an impasse (such as the scientist–practitioner gap), those depictions are not static representations of some external reality but rather the manifestation of a complex, dynamic process that is built upon an intricate interplay of beliefs and assumptions, all of which can be scrutinized and brought to play in the dialogue. As this process unfolds, and as the pieces are questioned, what develops is a more comprehensive picture than was previously available to either member of the exchange.

It is for this reason that the previous attempts at bridging the gap have been unsuccessful. They conceptualize the gap on a superficial level and then make the assumption that improved cooperation will resolve the conflict. To truly bridge the gap, it must be recognized rather as a gap between two distinct cultures who are interpreting the field of psychology in ontologically unique ways. The reality of mental health itself—its etiology, its present constitution, and the processes of change that impact it—is not the same for those on opposing sides of the gap. When those ontological differences are skipped over and the focus is placed on therapeutic techniques, research methods, or even training models for psychologists, each side is left

explaining solutions in a language the other does not speak. This problem is compounded, however, by the fact that each side is using words the other understands but to which they attribute different meanings. The hermeneutic solution to the scientist–practitioner gap starts at this ontological level. It provides steps for acknowledging the differences in perceived reality, questioning and challenging one another with a willingness to be questioned and challenged in return, leading to genuine reflection and reconsideration of previously held assumptions and beliefs, and then reinterpreting with an increased understanding and a new perspective.

Responsibility

As a result of the paradigm wars, most social scientists are now familiar with both quantitative and qualitative research methods. This is an important example of using words that each side understands but which hold unique meanings. Among positivist and empirical thinkers, qualitative research refers to a methodology that includes extensive information gathered from single subjects, such as in-depth interviews, ethnographies, observations, and collecting information from pictures, texts, and similar sources. This information is then analyzed to answer important questions that might not be answerable through quantitative, large-sample methods. It is often seen as a precursor to quantitative studies, providing information about what topics might be studied or what quantitative questions might be important to ask (Guba & Lincoln, 2005). For the positivist, qualitative research can be valuable, as long as careful restrictions are implemented to ensure that it remains as objective as possible. For the interpretivist, qualitative research must be understood from an entirely different lens. For the interpretivist, qualitative research has more to do with a moral obligation to improve quality of life on both an individual and a community level.

Many hermeneutic scientists have explored the role of morality in scientific research (e.g., Adams, 2005; Buber, 1970; Christopher, 1996; Levinas, 1985; Richardson et al., 1999). Most relevant to my argument for a hermeneutic solution to the scientist–practitioner gap is the moral obligation that psychologists hold to improve the quality of life of those who seek their services. The typical consumer of psychological services assumes that the term *scientific* is sufficient to describe a process that ensures the assessment and treatment of mental illness is based on valid and reliable evidence. Such consumers cannot be expected to understand the philosophical assumptions and implications behind the science of psychology. A typical expression from a client is "I'm just tired of feeling this way. Can you help?" I propose that it is the responsibility of the psychologist to have a greater understanding than either "this seems to work with many other clients who feel the same way" or "current literature indicates that clients who present with these symptoms will benefit from this intervention." Both statements indicate a limited perspective: one that is confined to understanding one side of the scientist–practitioner gap. I suggest that this limited perspective is unethical and represents a failure to consider responsibly all the therapist can do to improve the quality of life of the client. Thus, the final step in bridging the scientist–practitioner gap is to accept moral responsibility for providing philosophically cogent services to clients, thus engaging researchers and clinicians on the same team and providing an ethical motivation to remain engaged in the hermeneutic circle.

Conclusion

I have argued that the failure to bridge successfully the scientist–practitioner gap is a result of more than a mere failure to communicate or to cooperate. I suggest that it is instead rooted in a lack of recognition that the gap spans incommensurable philosophies of science. I further posit that those philosophies need not be compatible in order to bridge the gap between

researchers and clinicians. The philosophical pieces of different paradigms in psychology can only be accepted through logical reasoning. One cannot scientifically demonstrate the superiority of one philosophical concept over another without first assuming that the philosophies that guided the selection of the methods she or he would use for the demonstration are valid. This concept has been labeled the bootstrap problem or the regress problem (e.g., Pollock, 1975; Sellars, 1963). I suggest that the only solution is for proponents of each paradigm to acknowledge that each other paradigm has equal claim to validity. However, in order to avoid falling into a relativistic trap, these various approaches to psychological science must engage with one another. This endeavor presents a new problem: that members of different cultures, even if speaking the same language, interpret reality in fundamentally unique ways, leading to misunderstandings and distortions in the interpretation of each other's claims. To address this problem I propose the implementation of hermeneutic methods (Gadamer, 1960/1989). These methods provide a process wherein each member of the dialogue brings his or her assumptions into conscious awareness with a willingness to compare and contrast them to those of the other members. In this process, competing prejudices and assumed truths are addressed at an ontological level, allowing the horizons of the participants in the dialogue to fuse with one another. When done authentically, this process allows those who hold divergent worldviews to challenge their own perspective, and in turn to challenge that of others in a sincere search for understanding. This hermeneutic understanding overcomes the epistemological arguments for external universal truths and grounds our social constructions about the subject. There need not be an external, universal truth in order for understanding to increase among participants in a dialogue, and the different perspectives need not be accepted as equal because the very goal of

the exchange is to improve the comprehensiveness of the understanding possessed by each member.

Before concluding, I wish to emphasize a final attribute of this solution that is unique to previous attempts. I wish to be even more explicit at this point that I do believe the philosophical differences to be fully incommensurate among those on different sides of the scientist–practitioner gap. I take seriously the arguments made by philosophers such as Berger and Luckmann (1966), who have carefully demonstrated that any analysis of knowledge rests entirely and exclusively on socially constructed assumptions about reality. Although human understanding of knowledge and observations necessarily entails some social construction, a Gadamerian hermeneutic will also serve methodologically to keep the socially constructed interpretations of the object in question grounded by the object itself. Advantageously, understanding social construction as Berger and Luckmann described also offers a check to arbitrary and idiosyncratic constructions of the object. Gadamer effectively brought the object and social construction together, in a process of knowing that allows the object to derive its meaning simultaneously from both its original intent and its current interpretation (Warnke, 1987). My approach to resolving the gap is not an attempt at tying a pretty bow on top of the problem and pretending that these divergent systems of knowledge will somehow become compatible through the use of hermeneutic dialogue. Interpretivist researchers and clinicians will continue to ask different questions, seek different solutions, and provide different recommendations from those asked, sought, and provided by positivist researchers and clinicians. This is because they conceptualize mental health in ontologically different ways. This proposed solution instead provides a pattern psychologists can follow in order to discover ways in which the field can be infinitely more valuable to its consumers if it is expanded to

include all philosophically sound perspectives rather than fighting an unwinnable fight to prove who is the most correct. Again, this does not mean that either side simply abandons its beliefs and accepts the others. In true hermeneutic fashion, I acknowledge that I fall firmly on the interpretivist side of the gap. I have no intention of resolving the gap by trying to be both interpretivist and positivist. I can, however, become a more effective researcher and clinician if I put sincere effort into understanding positivist claims and then go a step further by acknowledging that those claims may yield valuable knowledge if I analyze them through various philosophical lenses. For example, operant conditioning relies exclusively on traditional positivist empiricism to explain human behavior. An interpretivist might see the extensive empirical support and engage in a dialogue that would allow for multiple philosophical explanations for that support. Perhaps consistent rewards and punishments lead to a deeper sense of trust and commitment between those administering them and those receiving them, and this bond inspires each member to behave differently than he or she would have before this bond was formed. As with the interpretivist, the positivist does not need to accept this position in order for a hermeneutic dialogue to develop. Each side is simply showing a willingness to acknowledge and then engage with the other's perspective so that the field of psychology becomes deeper and more valuable. If each participant in the dialogue ultimately walks away still rejecting the philosophical arguments of the other, she or he still does so having been fundamentally changed if she or he truly engaged in the dialogic process I have outlined. This change ultimately benefits the consumers of psychological services.

I propose that this hermeneutic approach offers a novel and valuable solution to the scientist–practitioner gap and that this solution is the most responsible way to ensure that all members of the psychological community are furthering the mission of the American

Psychological Association "to advance the creation, communication and application of psychological knowledge to benefit society and improve people's lives" (APA, 2013, Organization of APA section, para. 4).

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