

Are Science and Practice Commensurable?

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The scientist-practitioner model assumes a commensurability between science and practice that often is absent in academic and practice settings. One approach to T. S. Kuhn's (1962) concept of paradigm involves communities of social discourse, and within this approach, science and practice may be incommensurable. Current political forces appear to be increasing the distance between the 2 areas. However, another approach to paradigm sees it as a disciplinary matrix of shared understanding, and within this approach, science and practice can be complementary and synergistic. Theories such as behaviorism and psychoanalysis may be incommensurable, but science and practice are each components of both theories. However, for science and practice to be commensurable, the same theoretical matrix must generate both science and practice activities. Psychotherapy integration and the local clinical scientist model are seen as 2 developments that reduce the barriers between theories and activities and contribute to the commensurability of science and practice.

The farmer and the cowman should be friends.

—Oscar Hammerstein II, *Oklahoma*

The vaunted linkage between science and practice in psychology training and practice (Raimy, 1950) has been an area of continuing dispute (Stricker & Cummings, 1992). The intensity of the conflict seems to reach well beyond intellectual differences and has produced ad hominem characterizations that underline the difficulty in finding a necessary resolution. For example, Meehl (1954), in a cathartic exercise, listed the many ways in which the two poles view each other. In the following quotation, substitute *scientist* for *statistical method* and *practitioner* for *clinical method*, and the point will be clear:

The statistical method is often called operational, communicable, verifiable, public, objective, reliable, behavioral, testable, rigorous, scientific, precise, careful, trustworthy, experimental, quantitative, down-to-earth, hardheaded, empirical, mathematical, and sound. Those who dislike the method consider it mechanical, atomistic, additive, cut and dried, artificial, unreal, arbitrary, incomplete, dead, pedantic, fractionated, trivial, forced, static, superficial, rigid, sterile, academic, oversimplified, pseudoscientific, and blind. The clinical method, on the other hand, is labeled by its proponents as dynamic, global, meaningful, holistic, subtle, sympathetic, configural, patterned, organized, rich, deep, genuine, sensitive, sophisticated, real, living, concrete, natural, true to life, and understanding. The

critics of the clinical method are likely to view it as mystical, transcendent, metaphysical, super-mundane, vague, hazy, subjective, unscientific, unreliable, crude, private, unverifiable, qualitative, primitive, prescientific, sloppy, uncontrolled, careless, verbalistic, intuitive, and muddleheaded. (Meehl, 1954, p. 4)

This polarization between scientists and clinicians is in sharp contradistinction to the model of the scientist-practitioner that gives clinical psychology its identity and uniqueness among the helping professions in mental and behavioral health. It raises the question of whether polarization and antagonism are necessary, or if there is a way of achieving a needed synthesis between the two essential components of clinical psychology.

The Boulder Model

The Boulder model (Raimy, 1950) is the foundation on which contemporary training in clinical practice is built. Whether programs embody or reject the model, it is the point of departure for all training models (Stricker & Trierweiler, 1995). The conferees at Boulder endorsed a training model combining the scientific foundation of psychology with its practice applications so that clinical psychologists were to be trained to be both scientists and practitioners (Raimy, 1950). It is unusual for individual psychologists to contribute in both research and practice venues because some choose academic or research careers and most choose practice careers. They often continue to pledge allegiance to the model but often are unable to achieve it in their careers.

The Boulder report recognized that some universities would "strongly emphasize one area to the relative neglect of the other" (Raimy, 1950, p. 81). In the time period immediately following the report, the pattern was for programs to emphasize the scientific aspects of training, either ignoring or giving little attention to the con-

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cerns of practice. This imbalance added to the notion of incompatibility between the two joint concerns of clinical psychologists. The absence of many role models on university faculties for scientist-practitioner functioning probably contributed to the difficulty in transmission of anything but the abstract model within training programs. The focus on science and the failure to develop a continuum of approaches—with some emphasizing science, others emphasizing practice, and many truly integrating the two—gave rise to the professional school movement (Bourg et al., 1987), a converse image of the initial imbalance of traditional programs in that there was an emphasis on practice that often was achieved at the expense of science (Peterson, 1991; Stricker & Cummings, 1992). If the intent of the Boulder model (Raimy, 1950) had been recognized and actualized, the polarization that exists today would not have occurred and would not have been considered acceptable.

The Contributions of Kuhn

The title of this article contains an allusion to the works of Thomas Kuhn in that it refers to commensurability and, implicitly, the notion of paradigms. Kuhn, who is referred to as both a philosopher and a historian of science, has an interesting role in scientific thought in that his landmark volume, *The Structure of Scientific Revolutions* (1962, 1970), which was intended for the philosophical and natural science communities, has been soundly criticized in both communities but has found life and acceptance in the social sciences (Gholson & Barker, 1985), where it does not really apply. The result of the ensuing confusion led Gholson and Barker to separate “Kuhn’s ideas” from “Kuhnian ideas.” The former refer to positions that correctly can be attributed to Kuhn, whereas the latter involve popular distortions that have become rife. Perhaps the central reason for this widespread misinterpretation derives from a different reading by various audiences, which itself may be indicative of the very difference in paradigms that Kuhn described. Kuhn (1970) felt it necessary to deny specifically charges of relativism and irrationality, qualities that are an anathema to those who seek to know reality as it is and who use logical means of inquiry to reach immutable truths. At the same time, this putative irrationality is heartening to those who believe in a constructed world where the nature of reality is inseparable from the characteristics of the observer.

The central concept in Kuhn’s (1962, 1970) presentation, from which other key elements follow, is that of a paradigm. Kuhn (1970) himself noted that his original presentation (Kuhn, 1962) may have used the word *paradigm* in at least 22 different ways, giving easy rise to the different interpretations it has been given. One sense of the term *paradigm* has been to view it in the context of a scientific community, which consists of people who have been socialized similarly in terms of such matters as their education, the societies to which they belong, the meetings they attend, and the journals they read and

contribute to. They share a social discourse that is integral to their pattern of communication with each other.

Another sense is to view a paradigm in terms of shared examples or exemplars. This involves not only a shared theory and rules of inquiry but also a recognition as to how these rules apply to a variety of disciplinary problems. Thus, it is not only knowledge but also a mode of knowing that is shared in the paradigm. Thus, a paradigm is more than simply a theory. It encompasses the whole disciplinary matrix that surrounds a theory, including an epistemological framework, a corpus of knowledge, a means of generating and understanding that information, a set of values, and possibly even a worldview.

This expanded understanding of paradigm leads to the concept of commensurability. Paradigms can be commensurable when they speak the same language. In incommensurable paradigms, the same words may be used, but they have different meanings within the rules of discourse of the paradigm. Competing paradigms, therefore, cannot be subject to tests of evidence because they cannot even agree as to what constitutes a valid test. It is this inability to find an objective means to choose between paradigms that leads to the charges of relativism that Kuhn (1970) denied.

Are science and practice competing paradigms, and if so, are they commensurable? My belief, as I attempt to demonstrate, is that they can be commensurable, but there are significant obstacles that have to be identified and combated. The answer to the critical question of commensurability depends on the sense of *paradigm* that is used.

Are Science and Practice Commensurable?

Science and Practice: Incommensurable Paradigms

If paradigm is considered in its sense as a community of scholars with similar socialization, the likelihood that science and practice can be considered to be paradigms, and incommensurable ones, is high. Kanfer (1990) discussed how scientists and practitioners differ in their orientation to problems, reducing the possibility of communication between them. Peterson (1991) also commented about areas of incompatibility between science and practice, based on different approaches to training. The scientist is interested in knowledge for its own sake and emphasizes academic freedom in training programs. The professional is interested in knowledge as it can be applied in public service. Academic freedom requires free inquiry with a minimum of requirements, whereas service requires disciplined inquiry with clearly specified requirements.

One genuine difference between the two endeavors is that science is a public and self-correcting enterprise whereas practice is private and therefore more resistant to change. The demands of scientists that practitioners make their work more public is a necessity for adequate research to be done, but it also is seen as contradictory to the confidentiality that is central to clinical intervention.

It is ironic that these two paradigms of training and activity have developed in a competing and incommensurable fashion, because it is precisely this competition that the Boulder model (Raimy, 1950) sought to combat. Unfortunately, the decade following the Boulder conference was marked by an emphasis on traditional scientific training, often to the exclusion of clinical work, and began the trend toward incommensurability (Peterson, 1991; Stricker & Cummings, 1992). Students continued to seek clinical positions after graduation, for which many had been trained poorly, if at all, and the dissatisfaction they expressed culminated in the professional school movement (Stricker & Cummings, 1992) and the Vail conference (Korman, 1976). In recent years, there have been two competing trends. The scientist-practitioner model seems to have experienced a resurgence (Belar & Perry, 1992), encouraging more communication between the unnecessarily competing paradigms. However, political considerations have led to such developments as the formation of the American Psychological Society, an organization whose *raison d'être* is the separation of science from the professional elements it sees as dominant within the American Psychological Association. The more that scientists and practitioners belong to different organizations, attend different meetings, and read different journals, the more separable the paradigms will seem, and the more incommensurable they will become.

Science and Practice: Commensurable Paradigms

However, there is another sense of paradigm, and in this one, a potential solution lies for the commensurability of science and practice. If paradigm is taken to mean shared theory and rules of inquiry, then science and practice are two separate and connected activities. Science is an epistemological undertaking, or a way of knowing the world. Practice is a praxis that seeks to apply the body of knowledge developed in scientific explorations, or a way of acting in the world. Thus, science and practice are not competing paradigms in this definition, in that they do not seek to explain the same aspect of reality. Rather, they are, or should be, complementary and synergistic approaches to that reality. In this view, science and practice are not candidates for commensurability but rather are, or can be, extensions of each other. They are both components within larger paradigms. Why, then, is there so much difficulty with the scientist-practitioner model, and why is it that practitioners frequently eschew reliance on science?

In responding to this question, one must recognize a diversity in theory and philosophy that underlies both the science and the practice of psychology. For the sake of simplicity, I illustrate this point by referencing a behavioral and a psychodynamic way of knowing, but of course, the possibilities are much broader than these two approaches. These theories can be considered paradigms in the second sense of the term because each has rules of inquiry that differ remarkably from each other. For the behaviorist, the emphasis is on an objective, quantitative representation of reality. The philosophical tradition is

that of Locke, Hume, and John Stuart Mill. For the psychodynamicist, the emphasis is on a subjective, qualitative reality. The philosophical tradition is that of Schopenhauer, Kierkegaard, and Nietzsche. These paradigms can be truly incommensurable because they generate a different worldview (Messer & Winokur, 1980). Behaviorists view conflict as rooted in the external world and as addressable by action, whereas psychodynamicists see conflict as internal and subsequent activity focuses on self-reflection.

One test of incommensurability is whether it is possible for a crucial experiment to resolve the differences between the paradigms. This is not possible between behaviorism and psychoanalysis, because what constitutes evidence for one approach would be viewed as irrelevant by the other. Demonstrations of behavior change without insight and structural alteration would be seen by a psychodynamicist as superficial and relatively meaningless. Remarkable insights without accompanying changes in behavior would be dismissed by a behaviorist as empty and obsessional.

The Achievement of Commensurability

It seems that the social discourse community definition of a paradigm is more likely to produce sociopolitical difficulties and solutions, whereas the paradigm as a disciplinary matrix is more likely to involve intellectual barriers to communication. Within the definition of a paradigm as a community of like scholars, the achievement of commensurability involves political as well as intellectual obstacles. I already have referred to the existence of separate organizations as a force that drives scientific and professional interests further apart. However, even within the American Psychological Association, the existence of separate directorates, each with its own budget, staff, and priorities, leads to the development of an agenda for science and an agenda for practice, as though the two are different and, at times, even antagonistic. Indeed, they often seem to be, and a structural embodiment of that difference may create a self-fulfilling prophesy. That is why joint projects between science and practice, such as the recent development of a template for evaluating guidelines (Task Force on Psychological Intervention Guidelines, 1995), are such auspicious events and are much more likely to lead to a scientist-practitioner psychology than are the turf wars that have become more characteristic of the organization. This template combines a sophisticated approach to the evaluation of psychotherapy research with a concern for the applicability of that research to the practice of psychotherapy in a field setting. There are clear and well-documented differences in the training requirements of scientists and practitioners (Peterson, 1991). However, despite the difference in the attitudes or identities of science and practice, their activities need not be in opposition to each other, so that the incommensurability may be resolved to the benefit of each.

The intellectual barriers to commensurability of the two communities seem less daunting than the political

ones. The blueprint already has been drawn at Boulder, and it remains for psychologists to read and to follow it. Considering the paradigm as shared theory and rules of inquiry, science and practice become, in theory, two commensurable components of an overall matrix of a theory. In any theory, practice must be developed on a knowledge base, and a sound knowledge base is rooted in relevant evidence. Yet here, where compatibility should exist, is exactly where the greatest barrier to the scientist-practitioner model has occurred. The recent furor over experimentally validated techniques (Task Force on Promotion and Dissemination of Psychological Procedures, 1995) provides an excellent illustration of the gulf between scientists and practitioners in the very area where each should draw strength from the other. Where can one look for a resolution to this pernicious conflict?

In the realm of the disciplinary matrix, the two paradigms that I have identified for my focus are the behavioral and the psychodynamic. Immediately after the Boulder report (Raimy, 1950), the psychodynamic paradigm was paramount in the clinical area, whereas the behavioral paradigm dominated the research area. The seeds of conflict were sown by this division, because the research generated in one paradigm did not speak to the psychotherapy practiced in the other. It is interesting that cognition and learning theory were competing points of view (even paradigms) at that time, but they since have collapsed into a cognitive-behavioral approach that dominates the training scene and is increasingly prevalent in the practice scene. As research has spawned a cognitive-behavioral practice that can rely on the knowledge base that it creates, practice—in the form of psychodynamic approaches—has begun to develop a research base (see, e.g., Luborsky & Crits-Christoph, 1990) that goes beyond the anecdotal.

Incommensurability results from discrepant communication, so that the work of one paradigm is meaningless within another because definitions, concerns, assumptions, and values are at odds with each other. This often is the case with the behavioral and the psychodynamic approach, and the identity of the behavioral with science and the psychodynamic with practice has contributed to the difficulty between science and practice. It is not science and practice that are incommensurable but the theories that undergird them. However, it is important to underline that science and cognitive-behavioral therapy are not synonymous, nor are practice and psychodynamics.

There has been a recent attempt to survey psychotherapy outcome research and to construct a list of treatments that have sufficiently documented support to be considered empirically validated (Task Force on Promotion and Dissemination of Psychological Procedures, 1995). To be included on this list, an intervention must meet several criteria, including demonstration of efficacy in at least two well-conducted between-groups studies and the use of a treatment manual. The list clearly is viewed as an interim statement to be modified as warranted by developing research and probably would have

attracted less acrimony if it, correctly, had referred to empirically supported rather than empirically validated techniques. Consistent with the formulation of the presumed identity of science and cognitive-behavioral therapy, it is interesting to note that approximately 90% of the treatments cited as empirically validated are cognitive-behavioral, so that the resistance of the practice community (i.e., psychodynamic psychotherapists) could have been easy to anticipate. It also is noteworthy that the template created to evaluate guidelines (Task Force on Psychological Intervention Guidelines, 1995), which was a joint product of scientists and practitioners, provides weight to both the evidence of science and the evidence of practice, valuing each and seeing both as necessary for the construction of sound guidelines.

To make science and practice commensurable, it is necessary for the science and the practice to originate in the same disciplinary matrix. Currently, it is more likely that scientist-practitioner programs can develop in a cognitive-behavioral setting, for that is where the research always has been and where practice increasingly is located. For a parallel development to occur in the psychodynamic community, the research will have to speak to the practice, and there is increasing indication that such programs are developing.

However, one also might raise the questions of what science is taken to be and whether different views of science are seen as incommensurable or as complementary. Science is a privileged term because of its dramatic history of success in understanding and conquering the natural world. Historically, the methods of science have extended from simple observation through the experimental method, including data-free approaches such as the thought experiments of modern physics. A paradigm that is restricted to one portion of this epistemological continuum is likely to be limiting and to disregard evidence produced by a different approach to knowing.

It is important to remember that the critical antinomy of the Enlightenment was not between science and practice but between science and authority. Authority, whether based in the royalty or the priesthood, was rejected in favor of learning, whether through rationality or empiricism. Experience was important to both science and authority as a source of evidence. Practice also was important to both science and authority as an application of knowledge. For authority, experience simply took the form of a command to do or believe on the warrant of the king or priest. For science, experience became a source for the development of hypotheses and theories. However, science went beyond simple experience to systematization through data collection in a public manner and subsequent reevaluation of results so that alterations in "knowledge" could be made. Systematizing data collection has led to the privileging of some types of data, some types of collection, and some forms of systematization, so that some methodological approaches are deemed to be more "scientific," and therefore more valuable, than others. The value of the methodological approach is related to the goals of the inquiry. Discounting some

methodologies, rather than appreciating what they can and cannot contribute, conveys a pejorative note about the entire line of inquiry that generated the problem and its methodological solution.

As long as the same matrix generates both research and practice, the two can be viewed as extensions of each other, and the scientist-practitioner model can flourish. For this to occur, research must use the methodology that is appropriate to the problems that are of concern within the paradigm. However, where research originates in one matrix and practice in another, as occurred immediately after the Boulder report (Raimy, 1950), there is a disjunction that leads to incommensurability. Historically, this was followed by an introduction of political considerations arising from warring social communities and produced a setback from which psychology may just be emerging.

Overcoming the Barriers

Under the discourse community definition of paradigm, the primary barrier is sociopolitical, and the obstacles to commensurability require clear intent by both parties and the recognition that the potential gains outweigh the loss of a unique identity. Within the disciplinary matrix definition, the barriers lie in the intellectual realm, and there have been some recent developments that speak to the possibility of overcoming these barriers.

Psychotherapy Integration

I already have commented on the merger of cognitive and behavioral approaches, producing an integrated paradigm that has been very productive, in both research and practice spheres. This integration typifies a much more general movement in this direction (Stricker & Gold, 1993), and any attempt to integrate paradigms will increase the commensurability between theories.

Psychotherapy integration often is seen as having several distinct formats. One of these, theoretical integration, seeks the unification that has been approached by cognitive-behavioral approaches, which, by definition, will serve to overcome the barriers to commensurability. I now turn to the other two approaches to psychotherapy integration.

The first of these is the common factors approach (Weinberger, 1993). This approach transcends the emphasis on specific techniques, such as interpretation or systematic desensitization, that characterize the single-school systems of psychotherapy. Instead, it looks to aspects of treatments that are common to all schools. These commonalities include such dimensions of psychotherapy as the relationship, exposure to the source of difficulty, mastery of problems, new attributions for personal problems, and corrective emotional experiences. Common factors, in that they cut across the boundaries of the single schools, are more likely to lead to communication between therapists and researchers who traditionally have identified with different theories. Indeed, there is a substantial body of research that has developed within the

general psychotherapy integration movement, and it is not simply restricted to common factors (Arnkoff, Victor, & Glass, 1993; Glass, Victor, & Arnkoff, 1993).

A third approach to psychotherapy integration is to achieve integration at the level of technique, so that the techniques that are typical of different systems are combined in a single psychotherapeutic approach. The most sophisticated version of this technical integration is assimilative integration (Messer, 1992), in which a single governing theory is maintained, but technical innovations that ordinarily would not be practiced by that theory are incorporated within the pattern of practice. Assimilative integration directly serves to reduce the barriers between systems, as a practitioner committed to one approach must be open to the contributions of another approach. More importantly, practitioners then may seek to understand why a technique that their theory would not generate can be used and used successfully. In doing so, the boundaries of their own theory must be explored and stretched, which will serve to reduce barriers and make approaches more commensurable.

The Local Clinical Scientist

Psychotherapy integration is a substantive approach to increasing rapprochement between theories. The model of the local clinical scientist (Stricker & Trierweiler, 1995; Trierweiler & Stricker, in press) is a more procedural approach to enabling commensurability and the scientist-practitioner model.

The local clinical scientist model is based on the notion that science incorporates an attitude and a set of values as well as an epistemological framework. The local clinical scientist deals with immediate clinical issues in the office (local psychological phenomena) with the same rigorous, disciplined, and critical thought that characterizes the scientist working in a laboratory. Traditional scientific attitudes of skepticism and curiosity should be prominent for the local clinical scientist. The psychotherapist who is a local clinical scientist functions in a clinical setting, just as a scientist functions in a laboratory, both in drawing on relevant literature and in supplementing this by approaching the solution of a specific local problem with an overarching sense of scientific discipline and attitude. However, in contrast to more traditional science, the local clinical scientist does not seek large and generalizable answers to clinical questions. Rather, the focus is on the local context, with an attempt to solve local problems by applying available knowledge and by meeting the local problem with an attitude of curiosity, inquisitiveness, and skepticism, as befits a scientist-practitioner.

The local clinical scientist model has been viewed as the bridge between science and practice (Stricker & Trierweiler, 1995) and as an attempt to address training issues in a manner that will promote commensurability. The incommensurability of attitudes and values that have divided science and practice is incompatible with an approach that requires active contributions from the demands of practice and the attitudes and data of science, whenever they are applicable.

Science and Practice in Action

An article by Gladwell (1996) recently appeared in the popular press detailing the travails of a victim of a mugging in Central Park. Her medical care resulted in a surprising recovery from a coma. However, her physician insisted that her recovery should not be described as being a medical miracle. Rather, a "miracle" occurs when the care that leads to the recovery is capable of being repeated many times with many patients, so that the apparently miraculous becomes routine. The vehicle for such repetition is the development of a standard protocol for the care of victims with severe head injuries. The physician who was involved in the care of the victim who was mugged also was instrumental in the development of treatment guidelines for brain trauma care. It was particularly interesting that the group that developed the guidelines consistently asked for scientific evidence rather than clinical experience in producing their document.

However, before one becomes too enthralled with the wonders of medicine, there is another story that needs to be told. A good friend of mine recently was diagnosed with a benign but large and invasive brain tumor. She consulted with three neurosurgeons, each of whom recommended a different procedure, explained fully everything that was wrong with the rival recommendations, and cited his clinical experience as the basis for his judgment. In the end, she was left to make a life decision on the basis of these recommendations, with no solid data to guide her. The men she consulted were not charlatans; indeed, each was reputable and even renowned. However, the state of the art does not permit clear guidelines, and there is nothing other than clinical experience on which to rely.

The development of treatment guidelines is an ideal potential intersection between the contributions of science and the exigencies of clinical care. Attempts to arrive at such guidelines in the behavioral health care field have been met with great suspicion (e.g., Kovacs, 1995), perhaps because of fear of managed care or litigation; perhaps because of the great variety of services offered for similar conditions; or perhaps because, as in medicine, the evidence rarely is sufficient for the task. If different practitioners treat similar conditions differently, it is never clear whether one person is being given excessive treatment, if another is being given insufficient treatment, or if perhaps both are being treated appropriately because of local differences in the presenting condition or in the goals. In any case, despite the obstacles to the construction of therapeutic guidelines, they do remain a goal that, with the assistance of appropriate research, can be achieved within both physical medicine and the behavioral health areas. The fear is that Bambi, in the person of the local clinician, is about to meet the Godzilla of managed care (Parloff, 1982), and there is no point in giving additional weapons to a physically superior opponent. However, if parochial goals can be set aside and if the larger goal of providing the best possible assistance to the patient can become paramount, Bambi and God-

zilla can be on the same side rather than being unequal competitors.

Arriving at a definition of appropriate research is an area of controversy itself. This, too, is an area where paradigms come into play, and social groups constituted differently can seek different data and can arrive at very different readings of the same data. For example, the elaborate set of guidelines constructed by a medically oriented panel convened by the Agency for Health Care Policy and Research (1993a, 1993b) were soundly criticized by a group of psychologists who referred to the same data but drew very different conclusions (Muñoz, Hollon, McGrath, Rehm, & VandenBos, 1994).

In addition to the social obstacles that exist, there is a profound methodological difference between clinicians and scientists. Scientists look for large truths, or aggregate findings, and seek to make general statements about reality. Practitioners should heed these findings, but the findings then must be applied in local settings, taking into account individual characteristics of the parties to the therapeutic encounter and the context in which it occurs. Attempts to reconcile these realities are more likely to be productive than are arguments as to which is the correct way to view the problem.

However, despite the profound social and methodological obstacles that exist, both between and within professions, guidelines can be developed and can meet important health care needs. To do so requires attention to both the contributions of research and the contributions of practice. This balancing of complementary (not competing) contributions and needs has been described in a document constructed by a group of scientists and practitioners convened to provide guidance for the evaluation of the proliferating guidelines that currently exist (Task Force on Psychological Intervention Guidelines, 1995). The template produced by this group consists of two dimensions. The first of these dimensions is concerned with treatment efficacy and considers evidence from clinical consensus but also considers it as inferior to randomized controlled trials, which remain the gold standard of laboratory investigations of psychotherapy. However, there also is a second dimension concerned with treatment effectiveness that gauges the extent to which the laboratory findings are transportable to the field setting. A guideline will be seen as sound only if it meets the criteria of both efficacy and effectiveness, if it can be proven both in the laboratory and in the field. Room is left for individual local clinical decisions, but the burden will be on the clinician to demonstrate the reason for a departure from sound general practice if it is possible to describe such practice. If scientists are willing to allow for the experience of clinicians to be weighed appropriately and if clinicians are willing to be guided by the sound findings of science, psychology, as a profession, will move a great deal closer to the scientist-practitioner standard that always has dominated its rhetoric.

Conclusion

The paradigms of science and practice have appeared incommensurable, whether paradigm is considered to be

a community of scholars or a disciplinary matrix. The local clinical scientist approach, in that it highlights the compatibility and complementarity between science and practice, can oppose the social antithesis between communities. Psychotherapy integration, in that it reduces the opposition between theoretical approaches, serves to counter the rigid disciplinary boundaries between theoretical approaches. Both the local clinical scientist model and psychotherapy integration view science and practice as synergistic rather than antagonistic, and both therefore promote the scientist-practitioner model, which requires the commensurability of science and practice.

The key to commensurability was seen as being the consistency between the theoretical basis of the research and the practice within a given theoretical paradigm. When research and practice are each generated by a similar theoretical paradigm, they are not incommensurable with each other and can achieve the synergism that will lead to the highest achievements of the field.

Finally, I can address the question with which this inquiry began: Are science and practice commensurable? It can now be answered, consistent with the spirit of Kuhn (1962), by saying, "It all depends." However, also in keeping with Kuhn, and not with the radical relativistic distortions of his position, I also am able to state what it depends on. It depends on psychologists' willingness to expand the boundaries of their paradigms and epistemologies, to consider evidence from sources that are not suggested by their epistemology, to listen respectfully to different points of view, and to accommodate partially to the position of those operating in different paradigms. It depends on psychologists' willingness to pursue and to embrace the scientist-practitioner model, to consider the potential contributions of psychotherapy integration in their practice, and to function as local clinical scientists. And, it depends, at long last, on psychologists' willingness, in fact and not solely in principle, to accept and to meet the challenges of the Boulder model (Raimy, 1950).

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