

The Next Step in the Evidence-Based Practice Movement

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Nearly everyone agrees that psychological practice should be informed by evidence (Westen & Bradley, 2005, p. 266; Norcross, Beutler, & Levant, 2006, p. 7). However, there is considerable disagreement about what qualifies as evidence (e.g., Reed, 2006; Kihlstrom, 2006; Messer, 2006; Westen, 2006; Stirman & DeRubeis, 2006). This disagreement is not a simple scientific dispute to be resolved in the laboratory, but rather a “culture war” between different worldviews (Messer, 2004, p. 580). As Carol Tavris (2003) put it, this “war” involves “deeply held beliefs, political passions, views of human nature and the nature of knowledge, and—as all wars ultimately involve—money, territory, and livelihoods” (as qtd. in Norcross et al., p. 8).

How does one address a cultural battle of deeply held worldviews and political passions? We believe the approaches that have tried to address it so far in psychology have been well-intended and even headed in the right direction, but are ultimately and fundamentally inadequate. We will first describe what we consider the two major steps in this regard, beginning with the empirically supported treatment (EST) movement, which still has considerable energy in the discipline, and then moving to the “common factors” approach, which recently culminated in a policy regarding evidence-based practice (EBP) in psychology from the American Psychological Association (APA, 2006). We specifically focus on the latter, extolling its goals, but noting their distinct lack of fulfillment. We then offer what seems to us the logical extension of these first

two steps – what could be called “objective methodological pluralism” in the spirit of one of our discipline’s founding parents, William James (1902/1985; 1907/1975).

The First Step: The EST Movement

Psychology’s first step in addressing this evidence controversy involved a succession of APA Division 12 (Clinical) task forces. Beginning in 1993, these task forces have “constructed and elaborated a list of empirically supported, manualized psychological interventions for specific disorders” (Norcross et al., 2006, p. 5).” In other words, this first step assumed that the battle of worldviews would be resolved through rigorous scientific evidence. “Rigorous evidence,” in this case, was idealized as the randomized clinical (or controlled) trial (RCT), widely esteemed as the gold standard of evidence in medicine. The advantages of this step were obvious. Third-party payers were familiar with this gold standard from medicine, and many psychologists believed that an EST list would provide a clear-cut index of “proven” treatments, not to mention greater respect from medicine.

Unfortunately, this seemingly rigorous, clear-cut approach has manifested more than a few problems (Westen & Bradley, 2005; Messer, 2004). Much like the testing movement in education, where teachers found themselves “teaching to the test,” psychologists found their practices being shaped by the RCT “test.” The critics of the RCT showed how professional practices were conforming, consciously or unconsciously, to the RCT worldview in order to make the EST list. In other words, the practices being studied tended to accommodate the particular RCT perspective on treatments, therapists, and patients.

With regard to treatments, this medical-model worldview of the RCT is biased toward “packaged” treatments for well-defined, compartmentalized disorders (e.g., Bohart, O’Hara, & Leitner, 1998). This model of treatment took its cues from the pharmaceutical industry, where “one must specify the treatment and make sure it is being applied correctly” (p. 143). According to this model, every patient would receive the same thing, and it is this thing, not the therapist or patient, that is considered the agent of change. Critics have argued that this view of treatment undermined many types of therapy, such as humanistic or psychodynamic therapies, in which “treatment” does not entail a manualized set of principles (e.g., Bohart et al.; Safran, 2001).

A related argument against this packaged view of treatment concerned the role of therapists. The assumptions or worldview of the RCT, these critics contended, turned the therapist into an interchangeable part, discounting the importance of the therapist’s distinctive personality, practical wisdom, and unique relationship with the patient. Many researchers have worried, to use the words of Allen Bergin (1997), that the RCT manualization of treatments turned therapists into “cookie cutters” and researchers into “mechanotropes” (pp. 85-86). This worry has been validated by research suggesting that manualization often hinders important therapeutic factors, such as the therapeutic alliance and the therapist’s genuineness, creativity, motivation, and emotional involvement (Duncan & Miller, 2006; Piper & Ogrodniczuk, 1999).

Third, critics have noted that the biases of RCTs shaped one’s view of the patient, assuming that researchers and clinicians work with pure patient pathologies only. According to this argument, RCTs are limited to patients with textbook symptoms of a single DSM disorder; thus, their results “may apply only to a narrow and homogeneous

group of patients” (Butcher, Mineka, & Hooley, 2004, p. 563). This limitation is no small problem, critics have warned, because the vast majority of U.S. patients are not pathologically “pure” in this narrow RCT sense. Rather, they are co- or “multi”-morbid in the sense that they are an amalgam of disorders (Morrison, Bradley, & Westen, 2003; Westen & Bradley, 2005). The prevalence of these “messy” patients is corroborated by the 35%-70% exclusion rates of RCTs for major disorders (Morrison et al., p. 110).

The common theme behind the above criticisms is that the biases of the EST movement stem from its narrow framework for validating evidence. Thus, it is not mere coincidence, critics have argued, that therapies that exemplify this type of treatment (e.g., behavioral or cognitive-behavioral treatments) are the most frequently listed as ESTs (Messer, 2004). The exclusion of other types of therapy (e.g., humanistic and psychodynamic therapies) has prompted critics to contend that the EST movement constitutes a methodological bias toward behavioral and cognitive-behavioral therapies (e.g., Slife, Wiggins, & Graham; Messer, 2004). If this first step has taught psychologists anything, it has taught that what the evidence seems to say has a great deal to do with what one considers evidence.

The Second Step: The Common Factors Movement

The second step – the common factors movement – was, in part, an attempt to learn from the shortcomings of the EST movement. Common factors advocates have argued that a focus on specific, “packaged” treatments for specific disorders is a narrow way of conceptualizing psychological research and practice (e.g., Westen & Bradley, 2005; Bohart et al., 1998). An alternative approach is to discover and validate factors of therapeutic change that are common across treatments. In this way, responsibility for

change is not just attributed to the treatment, as in ESTs. Change is considered the result of a dynamic relationship among the “common factors” of therapy, which include the therapist, patient, and technique (APA, 2006, p. 275).

A common factors approach is especially appealing to the majority of practitioners, who consider themselves eclectics or integrationists. Its popularity has helped it to play a significant role in shaping APA’s (2006) new policy statement on evidence-based practice. For this policy statement, evidence was liberalized not only to include studies of therapist and patient variables but also to include other methods than RCTs for conducting these studies (pp. 274-75). The main guideposts for selecting these methods, according to the underlying rationale of the APA policy, were their objectivity and their diversity. Methods should be *objective* to prevent the intrusion of human error and bias that would distort the findings (p. 276), and they should be *diverse* to prevent the shaping of practice that a focus on only one method might produce, such as the problems created by RCTs (pp. 272-74).

The problem, from our perspective, is that the APA culmination of this common factors approach is not objective and diverse enough. In other words, we applaud the goals but criticize the implementation. The APA policy is a clear step forward, in our view, but its conceptions of objectivity and diversity are inadequate. As we will attempt to show, this inadequacy means that the lessons of the EST movement have not been sufficiently learned. Recall that this first step restricted itself to a single ideal of evidence, the RCT, and thus disallowed any true diversity of methods. Recall also that several biases resulted from this restriction, obviating objectivity and shaping practice

even before investigation. As we will argue, this same lack of diversity and objectivity has continued into the second approach to the evidence controversy.

Our basic criticism is this: Just as an EST framework uncritically restricts acceptable evidence to a *single method* ideal (the RCT), so does the APA policy uncritically restrict acceptable evidence to a *single epistemology*. By “epistemology” we mean the philosophy of knowing that provides the logic and guides the conduct of a group of methods (Slife & Williams, 1995). Although the EST framework is biased toward a certain *method*, the common factors framework is biased toward a certain *methodology* – a narrow brand of *empiricism*.

According to this empiricist epistemology, “we can only know, or know best, those aspects of our experience that are sensory” (Slife, Wiggins, & Graham, 2005, p. 84). This narrow conception of empiricism is fairly traditional in psychology. More liberal usages of empiricism differ substantially, such as William James’ radical empiricism. James’ empiricism encompasses “the whole of experience,” including *non-sensory* experiences such as thoughts, emotions, and even spiritual experiences (James, 1902/1985; 1907/1975). Still, psychologists have interpreted the natural sciences to be grounded in the narrow empiricism. Historically, psychologists have wanted to be both rigorously scientific and comparable to medicine, leading them to embrace the narrower empiricism. As we will attempt to show, however, this restriction to a single epistemology is not based on evidence. Analogous to the EST restriction to a single method, the APA policy merely assumes and never justifies empiricism as the only appropriate epistemology for evidence-based practice, in spite of other promising epistemologies.

The reason for this lack of justification seems clear. Throughout much of the history of psychology, empiricism has been mistakenly understood not as a *particular* philosophy of science, but as a *non*-philosophy that makes reality transparent. Analogous to the way in which many EST proponents view RCTs, empiricism is not *a* way to understand evidence, but *the* way. Consequently, nowhere in the APA policy or its underlying report is a rationale provided for a commitment to empirical research, and nowhere is a consideration given for even the possibility of a “non-empirical” contribution to evidence-based practice.

This equation of evidence with empiricism is directly parallel to the EST movement’s equation of evidence with RCT findings. Just as Westen and Bradley (2005) noted that “EBP > EST” (p. 271), we note that EBP > empirical. After all, there is no empirical evidence for empiricism, or for RCTs, for that matter. Both sets of methods spring from the human invention of philosophers and other humanists. Moses did not descend Mt. Sinai with the Ten Commandments in one hand and the principles of science in the other. Moreover, these principles could not have been scientifically derived, because one would need the principles (before their derivation) to conduct the scientific investigations to derive them.

Indeed, the irony of this epistemology’s popularity is that many observers of psychology have long considered empiricism to be deeply problematic for psychological research. Again, the parallel to the dominance of RCTs is striking. Just as the majority of real-world patients, therapists, and treatments were perceived to defy RCT categories, so too the majority of real-world phenomena can be perceived to defy empirical categories. Indeed, many of the common factors for evidence-based practice are not, strictly

speaking, empirical at all. Rather, they are experiences and meanings that are not sensory, and thus not observable, in nature (Slife et al., 2005, p. 88).

Consider, for example, the efforts of APA Division 29 (Psychotherapy) to provide empirical support for therapy relationships, such as therapeutic alliance and group cohesion (Norcross, 2001; APA, 2006, p. 272). Although patients and therapists probably experience this alliance and cohesion, these relationships literally never fall on their retinas. The people involved in these relationships are observable in this sense, to be sure, but the “betweenness” of these relations – the actual alliance or cohesion themselves – never are. Their unobservability means, according to the method requirements of empiricism, that they must be operationalized, or made observable. Thus, it is not surprising, given its commitment to a narrow empiricism, that the APA policy report presumes that operationalization is a requirement of method (p. 274).

The problem with this requirement, however, is that any specified operationalization, such as a patient’s feelings about the relationship (e.g., Norcross, 2002), can occur without the therapeutic alliance, and any such alliance can occur without the specified operationalization. The upshot is that the construct (e.g., alliance) and the operationalization are two different things, yet the operationalization is the only thing studied in traditional research. Moreover, one can never know empirically the relation between the construct and its operationalization because pivotal aspects of this relation – the construct and relation itself – are never observable. Thus, APA’s policy runs the risk of making psychotherapy research a compendium of operationalizations without any knowledge of how they relate to what psychologists want to study.

Problems such as these are the reason that alternative philosophies of science, such as qualitative methods, were formulated. Many qualitative methods were specifically formulated to investigate unobservable, but experienced, meanings of the world (Denzin & Lincoln, 2000; Patton, 1990; Slife & Gantt, 1999). The existence of this alternative philosophy of science implies another problem with the unjustified empiricist framework of the APA policy report – it runs roughshod over alternative frameworks, such as qualitative methods. Although the policy includes qualitative research on its list of acceptable methods (APA, p. 274), it fails to understand and value qualitative research as a different philosophy of science.

A clear indication of this failure is the use of the word “subjective” when the report describes the purpose of qualitative research (p. 274). In the midst of a report that extols “objective” inquiry, relegating only qualitative methods to the “subjective” is second-class citizenship, at best. More importantly, this relegation only makes sense within an empiricist framework. In non-empiricist philosophies, such as those underlying many qualitative methods, the notions of “objective” and “subjective” are largely irrelevant because most non-empiricist conceptions of science do not assume the dualism of a subjective and objective realm (Slife, 2005).

The bottom line is that a common factors approach to the evidence controversy is a clear advancement of the EBP project, but it is not an unqualified advance. Indeed, it recapitulates some of the same problems that it is attempting to correct. In both the EST and the common factors approaches, criteria for what is evidence shape not only the studies conducted but also the practices considered supported. Indeed, we would contend there is no method or methodology that is not ultimately biased in this regard. As

philosophers of science have long taught, all methods of investigation must make assumptions about the world *before* it is investigated (Curd & Cover, 1998). The question remains, however, whether there can be a framework for understanding evidence that does not *automatically* shape practice before it is investigated.

Presaging the Next Step: The Ideas of William James

The answer, we believe, is “yes,” and we do not have to reinvent the wheel to formulate this alternative. One of the intellectual parents of our discipline, William James, has already pointed the way. Consequently, we will first briefly describe three of James’ pivotal ideas: his radical empiricism, his pluralism, and his pragmatism. Then, we will apply these ideas to the evidence-based practice issue, deriving our alternative to the current monopoly of empiricism – objective methodological pluralism.

James was actually quite critical of what psychologists consider empirical today. As mentioned above, his radical empiricism embraces the whole of experience, including non-sensory experiences such as thoughts, emotions, and spiritual experiences (James, 1902/1985; 1907/1975). His position implies, as he explicitly recognizes, that there are several epistemologies of investigation (“ways of knowing”) rather than just one. As James (1909/1977) put it, “nothing includes everything” (p. 145). In other words, no philosophy of science is sufficient to understand everything.

Psychology needs, instead, a *pluralism* of such philosophies, which is the second of James’ ideas and an intriguing way to actualize APA’s desire for diversity. In other words, we not only need a diversity of methods, which the APA report (2006) clearly concedes (p. 274), we also need a diversity of *methodologies* or philosophies underlying these methods. It is not coincidental, in this regard, that James (1902/1985) used

qualitative methods to investigate spiritual meanings in his famous work, *Varieties of Religious Experiences*. His pluralism of methods dictated that he should not change or operationalize his phenomena of study to fit the method, but that he should change his method to best illuminate the phenomena – spiritual phenomena, in this case.

This approach to method implies the third of James’s ideas – his pragmatism.

According to James:

Rationalism sticks to logic and . . . empiricism sticks to the external senses.

Pragmatism is willing to take anything, to follow either logic or the senses and to count the humblest and most personal of experiences. [Pragmatism] will count mystical experiences if they have practical consequences. (James, 1907/1975, p. 61)

As James implies, the heart of pragmatism is the notion that one should never approach the study or understanding of anything with fixed schemes and methods. There is too much danger that the method will distort understanding of the phenomena being studied. This is not to say that one can or should approach such phenomena without some method or interpretive framework. Yet this framework does not have to be cast in stone; psychologists should allow the phenomenon itself to guide the methods we choose to study it.

This pragmatism may sound complicated, but it is not significantly different from what good carpenters do at every job – they let the task dictate the tools they use. They have a pluralism of tools or methods, rather than just one, because many tasks cannot be done with just one tool, such as a hammer. Moreover, not every carpentry job can be “operationalized” into a set of “nails.” As Dupre’ (1993) and others (e.g., Feyerabend,

1975; Viney, 2004) have noted, this pragmatism is the informal meta-method of physics, where the object of study is the primary consideration, and the method of studying it is a secondary consideration.

By contrast, APA's version of evidence-based practice is method-driven rather than object-driven. That is to say, psychologists have decided the logic of their investigation before they even consider what they are studying. If the object of study does not fit this logic, they have no choice but to modify it to fit this logic through operationalization. For example, an unobservable feeling, such as sadness, becomes operationalized as an observable behavior, such as crying.

The irony of this familiar research practice is that psychologists are driven more by an unrecognized and unexamined philosophy of science, as manifested through their methods, than by the objects they are studying. Indeed, they are changing their object of study – from sadness to crying – to accommodate this philosophy. We believe that this accommodation is contrary to good science, where everything, including the philosophies that ground one's methods, should be subject to examination and comparison.

The Next Step: Objective Methodological Pluralism

This description of James' three pivotal ideas – his radical empiricism, pluralism, and pragmatism – sets the stage for our proposal on evidence-based practice: “objective methodological pluralism.” First, this pluralism assumes a broader empiricism, in the spirit of James. To value only sensory experiences, as does the conventional empiricist, is to affirm a value that is itself unproven and non-empirical. There simply is no conceptual or empirical necessity to value only the sensory. We recognize that many would claim the success of this value in science, but we also recognize that no scientific

comparison between such philosophical values has occurred. These claims of success, then, are merely opinion, uninformed by scientific findings.

In practical terms, this move from conventional empiricism to radical empiricism means that alternative methods, such as qualitative methods, are no longer second class citizens. They are no longer “subjective” and experimental methods considered “objective,” because all methods ultimately depend on experiences of one sort or another. This creates more of a level playing field for methods – a pluralism – and allows for an even-handed assessment of each method’s advantages and disadvantages.

Unlike the APA policy’s conception, the criteria of this assessment are not already controlled by one, unexamined philosophy of science. They are guided, instead, by the object of one’s study. This is the reason for the term “objective” in our alternative, *objective* methodological pluralism. Methods, we believe, should be driven not by some philosophy of method that is deemed to be correct *before* the object of study has even been considered. Methods should be driven by consideration of the objects themselves.

This consideration is itself evaluated pragmatically, in terms of the practical differences it makes in the lives of patients. As James realized, any evaluation of practical significance begs the question of “significant to what”? In other words, any methodological pluralism requires thoughtful disciplinary discussion of the moral issues of psychology, a discussion that has begun in a limited way in positive psychology (Seligman, 2002) What is the good life for a patient? When is a life truly flourishing? Such questions cannot be derived from the “is” of research; they must be discussed as the “ought” that guides this research and determines what practical significance really means.

Obviously, much remains to be worked out with a Jamesian pluralism. Still, we believe that this particular “working out” is not only possible but also necessary. The monopoly and problems of empiricism – the lessons of our first two steps in the evidence controversy – do not go away with a rejection of this pluralism. This is the reason we titled this article “the next step” – the difficulties with empiricism and APA’s desire for diversity lead us logically, we believe, to this next general step. Admittedly, this kind of pluralism is a challenging prospect. Still, if carpenters can do it in a less complex enterprise, surely psychologists can. In any case, it is high time that psychologists face up to the challenge, because ignoring it will not make it go away.

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