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Expanding the Lens of Evidence-Based Practice in Psychotherapy: A Common Factors Perspective

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In this article, we examine the science and policy implications of the common factors perspective (CF; Frank & Frank, 1993; Wampold, 2007). As the empirically supported treatment (EST) approach, grounded in randomized controlled trials (RCTs), is the received view (see Baker, McFall, & Shoham, 2008; McHugh & Barlow, 2012), we make the case for the CF perspective as an additional evidence-based approach for understanding how therapy works, but also as a basis for improving the quality of mental health services. Finally, we argue that it is time to integrate the 2 perspectives, and we challenge the field to do so.

Keywords: empirically supported treatments, evidence-based practice, common factors, outcomes, psychotherapy

> Dr. Brown, the director of an outpatient mental health clinic, is staying late to prepare for tomorrow's annual staff retreat. She is faced with multiple challenges including a growing demand for psychotherapy services and a reduced budget, and because the clinic was recently approved as an APA-accredited internship site, she also faces the challenge of how to train the next generation of psychologists in "evidence-based practice (EBP)." She decides to pose this question to her staff: what does the research evidence say about providing effective psychotherapy and, specifically, how can we cost effectively apply this knowledge in our setting to improve the quality of care?

In the example above, Dr. Brown asks her staff to think critically about providing EBP. A reasonable request, but what exactly is EBP? It appears that on this matter there is much confusion for students, psychologists, and consumers of psychotherapy alike. For example, in a recent survey of clinical psychology graduate students, the majority identified EBP as synonymous with empirically supported treatments (ESTs) (Luebbe, Radcliffe, Callands, Green, & Thorn, 2007). Similar results conflating EST and EBP have been found among practicing psychologists (American Psychological Association Presidential Task Force on Evidence-Based Practice, 2006; Pagoto et al., 2007; Wachtel, 2010; Westen, Novotny, & Thompson-Brenner, 2005). The same mistake has also appeared in newspaper articles that have portrayed anything other than ESTs as "art," thereby painting the picture that clinicians not providing ESTs are resistant to EBP (Brown, 2013, *The New York Times*, p. D4). Clearly *some* clinicians are ambivalent toward *some* aspects of EBP (see Lilienfeld, Ritschel, Lynn, Cautin, & Latzman, 2013). Yet, we believe the prioritization of randomized controlled trials (RCTs) and ESTs over the last few decades has unintentionally limited the scope of EBP and may have ironically worked against the central purpose of psychotherapy research, that is, the improvement of practice, by limiting the variety of evidence deemed relevant to such an aim.

In this article, we argue that a primary reason EBP has been conflated with EST, and therefore has been somewhat limited in application, is that the scientific exploration of therapeutic factors other than treatment methods have been discouraged or labeled as "unscientific." For example, according to Chambless & Crits-Christoph, (2006):

> Of all the aspects of psychotherapy that influence outcome, the treatment method is the *only* aspect in which psychotherapists can be trained, it is the *only* aspect that can be manipulated in a clinical experiment to test its worth, and, if proven valuable, it is the *only* aspect that can be disseminated to other psychotherapists (p. 199, emphasis added).

Or, as Baker et al. (2008) noted:

> Research on nonspecific effects [that is, aspects of the CF approach] provides little support for the current practices of psychology, however. Legitimate and important issues surround nonspecific effects, but the resolution of the debate about nonspecific effects has little potential to validate a science-based practice of clinical psychology . . . .

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Dr. Alan S. Gurman sadly passed away during the completion of this manuscript. We dedicate this work to him.

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> It is important to note the *marginal scientific status* of those constructs (p. 82, emphasis added).

As discussed by philosophers of science, notably Kuhn (1962), restricting the lens through which a phenomenon is examined (in this case, psychotherapy) restricts what can be observed and the manner in which "evidence" is interpreted. In other words, a restricted scientific aperture means less of the evidentiary picture is in focus.

The result is that the application of EBP toward the reduction of mental illness is limited. The primary strategy for quality improvement in mental health today is to implement ESTs in clinical practice (McHugh & Barlow, 2012). A straightforward and persuasive case has been made that ESTs that have been shown to be efficacious in clinical trials should be disseminated widely and used (Baker et al., 2008). We agree that disseminating ESTs into practice settings should be one arm of a multifaceted "portfolio" approach (Kazdin & Blasé, 2011) toward reducing the burden of mental illness. We are *not* arguing that the field should discard the use of ESTs. Yet, substantial evidence from other perspectives of EBP, including the common factors (CF) approach, are frequently marginalized as "unscientific," or are given such low evidentiary prioritization that important data are overlooked. The result is that our ability to maximize practice outcomes has been limited.

Our purpose here is to expand the scientific understanding of the received EST view and broaden the lens through which observations about psychotherapy are interpreted. According to the APA's Presidential Task Force on Evidence-Based Practice (2006):

> ESTs start with a treatment and ask whether it works for a certain disorder or problem under specified circumstances. EBPP starts with the patient and asks what research evidence (including relevant results from RCTs) will assist the psychologist in achieving the best outcome (p. 273).

Notably, the Task Force criteria highlight the importance of clinical expertise and patient characteristics as central factors in EBP, similarly to the definition of evidence-based medicine (Institute of Medicine, 2001), and "builds on the Institute of Medicine definition by deepening the examination of clinical expertise and broadening the consideration of patient characteristics" (p. 273). The Task Force criteria acknowledge these factors exist within "the limits of one's knowledge and skills and attention to the heuristics and biases—both cognitive and affective—that can affect clinical judgment" (p. 284). Yet, some have characterized the Task Force's inclusion of therapist and patient variables as "a striking embrace of a prescientific perspective" (Baker et al., 2008, p. 84). Although it may be "prescientific" for clinicians to uniformly disregard research evidence of any kind (as some small minority do), it is decidedly and unequivocally *not* "prescientific" for clinicians to approach the results of RCTs as only one of several components of research evidence. We argue that this rhetoric that explicitly devalues the role of other variables, including therapist variables and client preference, and that in practice effectively renders other types of evidence (e.g., process-outcome, qualitative, meta-analytic) as tertiary in quality improvement efforts, is not consistent with APA's definition of EBP, and limits the impact of EBP in clinical settings.

We believe the CF perspective is entirely consistent with the APA's outcome-focused approach toward EBP. The CF approach asserts that (a) most therapists achieve commendable and desirable outcomes, (b) improvement will flow from managing outcomes, (c) *any* variable shown to influence outcome is scientifically important, and therefore (d) data-gathering efforts should focus on changing any therapist behaviors that influence outcome. We begin with a brief review of the core predictions of both the EST and CF approaches. Next, we review the evidence in context, paying particular attention to some overlooked aspects of the EST approach. Finally, we address how both programs approach quality improvement and argue that the two perspectives should be integrated.

The EST Approach

The EST approach proposes that psychotherapeutic treatments contain specific techniques that are purported to remediate identifiable deficits that form the diathesis of a given mental disorder (Barlow, 2004; Chambless & Hollon, 1998; Chambless et al., 1996). As described by Barlow (2004), ESTs also contain a variety of components common to all psychotherapies, such as "the therapeutic alliance, the induction of positive expectancy of change, and remoralization," but contain important "specific psychological procedures targeted at the psychopathology at hand" (p. 873). Similarly, Baker et al. (2008), in their comments on clinical science, constrained the scientific focus to these specific ingredients:

> Scientific plausibility refers to the extent to which an intervention makes sense on substantive bases and whether there is formal evidence regarding its mechanisms . . . . However, the absence of a demonstrated or plausible *specific mechanism* of action, especially for a psychosocial intervention, leaves open the possibility that the intervention may merely be capitalizing on nonspecific credible ritual, or placebo effects (emphasis added, p. 72).

Each EST posits a specific mechanism of change based on a given scientific theory. For example, prolonged exposure (PE) for PTSD (Foa, Hembree, & Rothbaum, 2007) is conceptually derived from emotional processing theory (Foa & Kozak, 1986), and the specific ingredients of PE (viz., imaginal and in vivo exposure) (a) activate the "fear network," (b) whereby clients habituate to their fears, and thus, (c) extinguish the fear response. On the other hand, interpersonal therapy (IPT) for PTSD (Markowitz, Milrod, Bleiberg, & Marshall, 2009) is derived from interpersonal and attachment theory (Bowlby, 1973; Sullivan, 1953) and "focuses on current social and interpersonal functioning rather than exposure" (Bleiberg & Markowitz, 2005, p. 181). Alternatively, when comparing Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 2012) and Cognitive Behavioral Therapy (CBT; Beck, Rush, Shaw, & Emery, 1979), two common ESTs for depression, experts in these treatments described these therapies as "distinct models" (Hayes, Levin, Plumb, Villatte, & Pistorello, 2011, p. 16) that "show substantial differences in their philosophical foundations" (Hofmann & Asmundson, 2008, p. 12). Different ESTs are based on different theories of change, and purport different mechanisms of action.

More recently, researchers have identified common symptoms within a class of disorders (e.g., mood and anxiety), and have begun to develop various "unified treatment protocols" (Moses & Barlow, 2006). We believe transdiagnostic treatments are a posi-

tive development, and in fact, overlap with various aspects of the CF approach. Yet, the specifications of methodologies—that is, rules that dictate *how* science is conducted—remain within the EST paradigm, for example, RCTs of treatment packages based on identifying specific ingredients are given top evidentiary prioritization.

There are two core EST predictions: (a) treatment specificity and (b) disorder specificity. In terms of treatment specificity, because two treatments target different mechanisms of change, the conjecture is that one treatment will be more efficacious than the other. That is, under the correct conditions, there will be sufficient evidence to reject the null hypothesis of no differences. Alternatively, some researchers may conduct equivalence trials to show that a new treatment is as effective as a standard treatment (e.g., Arch et al., 2012; Resick, Nishith, Weaver, Astin, & Feuer, 2002). Regarding disorder specificity, the conjecture is that one treatment will be more effective than another for treating a particular symptom of a disorder, e.g., "In this case, a treatment T1 may be more efficacious than T2 for treating symptoms S1 but not for treating symptoms S2." (Hofmann & Lohr, 2010, p. 14).

The CF Approach

The CF approach (Frank & Frank, 1993; Wampold, 2001) conceptualizes psychotherapy as a socially constructed and mediated healing practice. The CF model focuses on factors that are necessary and sufficient for change: (a) an emotionally charged bond between the therapist and patient, (b) a confiding healing setting in which therapy takes place, (c) a therapist who provides a psychologically derived and culturally embedded explanation for emotional distress, (d) an explanation that is adaptive (i.e., provides viable and believable options for overcoming specific difficulties) and is accepted by the patient, and (e) a set of procedures or rituals engaged by the patient and therapist that leads the patient to enact something that is positive, helpful, or adaptive. Whereas EST places primacy on different theories of change, a CF approach states that the adoption of a credible theory is only one aspect of many necessary common factors that contribute to behavior change.

There are a few core predictions of the CF approach. The first is that any therapy that contains *all* of the ingredients of the CF approach (as outlined above) will be efficacious for the presenting problem being treated. As Lambert recently noted, ". . . studies comparing two bona fide therapeutic approaches that find significantly different outcomes may be more unusual than not" (2013a, p. 196). A second prediction is that relationship factors such as empathy, goal consensus and collaboration, the therapeutic alliance, and positive regard, should predict the outcome of psychotherapy. A corollary of this second prediction is that there will be differences among therapists, that is, more effective therapists will more skillfully provide these factors. A third prediction is that treatments intended to be therapeutic will be superior to "supportive control" or psychological placebo conditions.

We wish to briefly identify five common misunderstandings of the CF approach. First, it is important to note that the CF perspective *does not* suggest that a mere "relationship" with a therapist is sufficient, as some have (mis)interpreted. As noted above, the therapeutic alliance should predict outcome, but the alliance is only one factor of several that are necessary from a CF perspective. Second, "supportive counseling" is not synonymous with CF. Typically, these treatments, elsewhere referred to as "intent-to-fail" treatments (Westen, Novotny, & Thompson-Brenner, 2004), are used to control for the common factors in RCTs. Many "supportive counseling" treatments contain no psychologically derived rationale or proscribe therapists from actions that most therapists would normally use, particularly when the therapists in these conditions know that the treatment is a sham (Budge, Baardseth, Wampold, & Flückiger, 2010). For example, in a trial comparing CBT and supportive psychotherapy for PTSD related to motor vehicle accidents (Blanchard et al., 2003), therapists delivering supportive therapy provided "little advice" and "care was taken not to encourage any driving. If the participant asked directly about a specific travel behavior, he or she was told to listen to his or her body and be guided by how he or she felt" (p. 86). Third, the term "bona fide," as theoretically derived from CF theory and operationalized by Wampold et al. (1997), has been largely (mis)understood and incorrectly applied. For example, Hofmann and Smits (2008), included supportive counseling, relaxation treatments, and anxiety management as bona fide therapies in their meta-analysis of adult anxiety disorders, although these treatments were not bona fide treatments (i.e., did not meet the operationalized criteria outlined in Wampold et al., 1997). Similarly, Ehlers et al., (2010), critiqued the results of Benish, Imel, and Wampold (2007) based on incorrect assumptions of bona fide therapies (see Wampold et al., 2010 for response to Ehlers). Fourth, a "common factors treatment" that some have argued is necessary for a valid test of specific versus common factors (Barlow, 2010; Foa, 2013) overlooks the important point that the CF approach states that *any* therapy with *all* CF ingredients will be efficacious. Lastly, under the CF approach, therapists cannot simply do whatever they want, however they want, and for as long as they want. The CF approach is focused on improving practice outcomes and therapist competence. Achieving this aim requires a variety of evidence-based techniques, which we address later in this article, that encourage therapists to adjust their practices in specific ways and *not* practice without purpose.

Evidence

The evidence related to ESTs has been summarized elsewhere (Baker et al., 2008; Barlow, 2008; Foa, 2013; Lilienfield et al., 2013), as has the evidence related to the CF approach (Imel & Wampold, 2008; Wampold, 2001, 2007). In this section, we focus on several aspects of the EST perspective that we believe have been overlooked, as well as evidence for the CF approach that is often criticized by proponents of ESTs.

The Post Hoc Issue

We claim that ESTs researchers resort to inconsistently applied post hoc explanations when observations are not consistent with core EST predictions. The result is not falsification of ESTs, but a reduction in the explanatory power of treatment specificity. We review several examples below, focusing on the issue of treatment adherence.

In a large sample of therapists in the National Health Service in England, Stiles and colleagues (Stiles, Barkham, Mellor-Clark, & Connell, 2008; Stiles, Barkham, Twigg, Mellor-Clark, & Cooper,

2006) found no differences in outcomes among therapists who indicated they were practicing cognitive–behavioral, person-centered, and psychodynamic therapies. However, Clark, Fairburn, and Wessely (2007) criticized the studies on the basis that the therapists who indicated that they were practicing CBT were not actually doing so, at least to the degree necessary to reveal that this treatment was superior. In a 1999 study, Tarrier et al. (1999) compared imaginal exposure (IE) with cognitive therapy (CT) and concluded, "A significantly greater number of patients receiving IE worsened over treatment" (p. 17) compared with CT. However, Devilly and Foa (2001) claimed that Tarrier et al. delivered IE inappropriately, "Did the therapist note 'hot spots' where appropriate and habituate the participants to these?" (p. 115). One does not have to venture far into the literature to find other examples where a result is impugned post hoc regarding issues of adherence. This is what might be called the *adherence hypothesis*: differences will be detected provided the treatment is delivered in the specified manner (Perepletchikova, Treat, & Kazdin, 2007). Indeed, this conjecture is of such importance that Suris, Link-Malcolm, Chard, Ahn, and North (2013) found that one therapist failed to adhere to the treatment protocol and eliminated the data for that therapist from the trial. This issue is problematic for a number of reasons. First, it is often used post hoc to explain the lack of treatment differences, but also to protect particular treatments from being identified as inferior, which then renders the EST prediction as less heuristically powerful. Concordantly, meta-analytic findings suggest that adherence in delivering a treatment is not related to outcome (Webb, DeRubeis, & Barber, 2010), although the adherence-outcome correlation needs further scrutiny (Leichsenring et al., 2011).

In an example of null findings, Ehlers et al. (2010) attributed the lack of differences between CBT and present-centered therapy (PCT) found by McDonagh et al. (2005) and Schnurr et al. (2003) to the fact that the patients in these trials belonged to "difficult to treat multiple trauma populations" (p. 271), an explanation that was not mentioned by the authors of the primary studies and more importantly was not stated a priori. Even when rated adherence to both treatments is sufficient, as it was in a comparison of behavioral marital therapy (BMT) and insight-oriented marital therapy (IOMT) that found that IOMT had significantly fewer divorces after therapy (Snyder, Wills, & Grady-Fletcher, 1991), an advocate for the inferior treatment (BMT) invoked a post hoc explanation. Jacobson (1991) claimed that therapists in the BMT condition did not sufficiently provide empathy and emotional nurturance and did not adequately foster hope, actions that were not specific to the treatment; that is, there was purportedly an inequivalence of the CF. Interestingly, the most rigorous RCT ever conducted (viz., the National Institute of Mental Health Treatment of Depression Collaborative Research Program) in its time could not inoculate the results against claims that a treatment was at a disadvantage for some reason (Elkin, Gibbons, Shea, & Shaw, 1996; Jacobson & Hollon, 1996).

Discussion of these factors should not be taken as criticism ipso facto, as every scientific investigation has some flaws (Cook & Campbell, 1979). All observations naturally come with baggage owing to particular experimental arrangements (Serlin & Lapsley, 1985). Nevertheless, a theory must be able to generate a conjecture that results in certain observations under specified conditions. Therefore, it can be argued that invoking explanations post hoc by a treatment advocate to protect a treatment from being found inferior is simply "partisan politics" and should not be taken seriously as science. However, science is a community project and if such explanations are invoked in scientific journals they become part of the scientific discourse, particularly when it is EST scientists who are invoking such explanations.

We believe CF explains these issues, and in this regard has equal (or possibly greater) heuristic power. The CF approach posits that specific ingredients are not necessary, and therefore post hoc explanations are not needed because null results have been predicted. When treatment differences are found, post hoc explanations are also not needed because CF predicts that over the corpus of results, some treatment differences will be found by chance.

Is One Treatment More Effective Than Another Across All Disorders?

According to the APA Resolution on Psychotherapy Effectiveness, "comparisons of different forms of psychotherapy most often result in relatively nonsignificant difference, and contextual and relationship factors often mediate or moderate outcomes (APA, 2013, p. 103)." We concur with APA's interpretation of the evidence (however, see Hunsley & Diguilio, 2002; Siev, Huppert, & Chambless, 2009 for counterargument). As noted above, EST predicts that some treatments will be more effective than other treatments, which is the logical opposite of CF, which states that all bona fide treatments are equally efficacious. The null hypothesis of the EST prediction is exactly the null tested in meta-analyses of direct comparisons of treatments (see Wampold et al., 1997; Wampold & Serlin, 2014), thus providing a direct test of a crucial aspect of these conjectures. Wampold et al. (1997) conducted such a meta-analysis of nearly 300 direct comparisons of treatments intended to be therapeutic and found insufficient evidence to reject the null hypothesis. Although there were comparisons within this data set that resulted in rejection of the null (i.e., produced statistically significant differences), the number was about equal to what would be expected by chance owing to sampling error. Indeed, the observed aggregate effect was less than what would be expected under the null, suggesting not a hint of an effect (Wampold & Serlin, 2014). This meta-analysis had more than adequate power to detect relatively small differences among a small proportion of comparisons (Wampold & Serlin, 2014) and therefore provides evidence that would seem to be in line with the predictions of the CF perspective.

It was claimed that many of the comparisons in the Wampold et al. (1997) meta-analysis were among variations of CBT treatments and therefore not likely to yield differences (Crits-Christoph, 1997), an observation that generates many responses. First, typically researchers who posed the comparisons were attempting to find differences that would demonstrate the importance of specified mechanisms of change. Therefore, under the EST prediction, differences would be expected. Second, Wampold et al. (1997) found that the similarity of the treatments being compared was not related to the size of the effect (e.g., comparisons of treatments that were different did not produce larger effects than did comparisons of treatments that were similar). Third, regardless of the nature of the comparisons, *there is insufficient evidence to reject the null hypothesis of no differences*, a result that albeit tentative, must be retained at this point in time. Fourth, several meta-analyses that

have compared active treatments contain a wide variety of types of treatments and many treatments other than CBT and have found no differences (Benish et al., 2007; Cuijpers, van Straten, Andersson, & van Oppen, 2008; Cuijpers et al., 2012; Imel, Wampold, Miller, & Fleming, 2008; Powers, Halpern, Ferenschak, Gillihan, & Foa, 2010). As well, several meta-analyses have directly compared CBT with other types of treatment and found no differences (Baardseth et al., 2013; Leichsenring & Leibing, 2003; Shedler, 2010; Wampold, Minami, Baskin, & Tierney, 2002). One meta-analysis (Tolin, 2010) did find that CBT was superior to other treatments for depression and anxiety. This meta-analysis is discussed below.

From a CF point of view, which posits that there will be no differences between treatments, Tolin appears to provide a refutation. But if *dozens* of meta-analyses finding no differences have not refuted treatment specificity (as some have argued), then *one* finding of such differences cannot refute the CF perspective.

There is more to be learned from the Tolin meta-analysis. The effect size for depression for the superiority of CBT was small (viz., 0.21) and only for symptom-specific measures (see Baardseth et al., 2013). Tolin's effect for depression contradicts several other meta-analyses that contained many more studies (Cuijpers et al., 2008, 2012; Wampold et al., 2002), and the effect was not present for outcomes other than the symptom-specific measures (Baardseth et al., 2013). The effect for the superiority of CBT for anxiety was moderate (viz., 0.43), but was based on only *four* studies, two of which were published before 1972. What is curious here was that it appears that Tolin defined CBT broadly to contain many treatments that many would argue are not CBT and are not scientific (e.g., eye-movement desensitization and reprocessing, EMDR; see Herbert et al., 2000; McNally, 1999), suggesting an ambiguity with regard to CBT as well as other treatments. Consequently, Baardseth et al. (2013) used an empirical definition of CBT that relied on a consensus of experts from the Association for Behavioral and Cognitive Therapies (ABCT) to determine what is and is not CBT for anxiety disorders, and located 13 studies that directly compared CBT with other treatments (vis-à-vis 4 for Tolin), and found no differences between CBT and non-CBT treatments for anxiety.

Is One Treatment More Effective Than Another for a *Particular* Disorder?

Again we believe the evidence suggests "no." A criticism of the CF approach has been that it makes little sense to examine comparisons across disorders (e.g., DeRubeis, Brotman, & Gibbons, 2005). Ignoring disorder "is akin to asking whether insulin or an antibiotic is better, without knowing the condition for which these treatments are to be given . . . Alternatively, researchers should begin with a problem and ask how treatments compare in their effectiveness for that problem" (p. 175). When treatments are directly compared, most have found no differences, including for depression (Cuijpers et al., 2008, 2012), alcohol use disorders (Imel et al., 2008), PTSD (Benish et al., 2007; Powers et al., 2010), anxiety disorders in general (Baardseth et al., 2013), eating disorders (Spielmans et al., 2013), and childhood disorders (Miller, Wampold, & Varhely, 2008; Spielmans, Pasek, & McFall, 2007). There are notably few meta-analyses of direct comparisons that have rejected the null. We have already reviewed Tolin. Another showed that CBT was superior to relaxation therapy for panic disorder (although the effect was due to one study—see Wampold, Imel, & Miller, 2009).

By way of contrast, for the treatment of a small subset of disorders, the CF model may have less explanatory power. For example, in the case of specific phobia and panic disorder, and possibly social phobia, evidence suggests improvement cannot occur without exposure to the fearful stimulus taking place. Whether exposure is accomplished in vivo, imaginally, via virtual reality procedures or other methods (Antony & Roemer, 2011), and whether exposure occurs intentionally within structured manual-guided treatment sessions provided by a behavior therapist or less systematically by, for example, risk-taking in everyday life, exposure appears to be essential for these disorders (although apparently not for other anxiety disorders, see Baardseth et al., 2013; Frost, Laska, & Wampold, 2014). It is clear experiential avoidance in some form must be addressed in therapy for some anxiety disorders (Barlow et al., 2011; Hayes et al., 2012), as even Jerome Frank, the originator of the CF perspective, agreed (Frank & Frank, 1993). But we would argue that an inclusive EBP approach would dictate that such decisions as with whom, at what stage of change, and how systematically therapeutic exposure might most helpfully take place, be left up to discussions between therapist and client.

How Important Is the Alliance as a Predictor of Outcome?

According to Baker et al. (2008), "In theory, some aspects of nonspecific effects are malleable or teachable: for example, behaviors that contribute to the therapeutic alliance (the therapist–patient relationship). Even these hold *little promise* that they represent special opportunities for clinical psychology, however" (emphasis added, p. 82). Most psychologists, including proponents of ESTs, acknowledge the importance of the alliance. Yet, as exemplified by Baker et al., may disagree on the relative importance of the alliance as a mediating variable. Here, we review the evidence pertaining to the alliance–outcome relationship in hopes to demonstrate the importance of this variable in improving EBP.

The therapeutic alliance is composed of three components: (a) bond between therapist and patient, (b) agreement about the goals of therapy, and (c) agreement about the tasks of therapy (Bordin, 1979) and is a central component of the CF program (Wampold, 2001; Wampold & Budge, 2012). The most recent meta-analysis, based on hundreds of studies, reveals a robust and moderate correlation (between .25 and .30) between the alliance and outcome (Horvath, Del Re, Flückiger, & Symonds, 2011), although this may be an underestimate owing to measurement issues (Crits-Christoph, Gibbons, Hamilton, Ring-Kurtz, & Gallop, 2011). This evidence has been criticized, however, on the basis that it is correlational, and thus, the alliance may not be causal to outcomes, as (a) it may be the patient's contributions to the alliance that are important (i.e., patients with better prognoses are able to form better alliances and have better outcomes), (b) early symptom change creates the alliance, and (c) the alliance may help patients feel better (i.e., general well-being) but will not affect symptomatology (Baker et al., 2008; DeRubeis et al., 2005; Siev et al., 2009). Several studies from a CF approach have been conducted to investigate these hypotheses. Baldwin, Wampold, and Imel (2007)

used multilevel models to disentangle therapist and patient contributions to the alliance and found that it was the therapists' contributions to the alliance that predicted outcome rather than the patients' contribution. That is, therapists who were able to form better alliances generally with their patients had better outcomes; however, patients who were able to form a better alliance with a given therapist did not have better outcomes than patients with poorer alliances with the same therapist, a result that has been replicated in primary studies (Dinger, Strack, Leichsenring, Wilmers, & Schauenburg, 2008; Zuroff, Kelly, Leybman, Blatt, & Wampold, 2010) and meta-analytically (Del Re, Flückiger, Horvath, Symonds, & Wampold, 2012). Moreover, there is accumulating evidence that the alliance is predictive of patient change after controlling for early symptom change (e.g., Baldwin et al., 2007; Crits-Christoph et al., 2009; Crits-Christoph et al., 2011; Falkenström, Granström, & Holmqvist, 2013; Webb et al., 2011). Finally, the alliance is not weaker in ESTs, in disorder-specific manualized treatments, for symptoms measures, for cognitive or behavioral treatments, or in RCTs (vs. naturalistic studies) (Flückiger, Del Re, Wampold, Symonds, & Horvath, 2012). Nor is it weaker in family therapy, with one hallmark study (Alexander, Barton, Schiavo, & Parsons, 1976) of the behavioral treatment of delinquent adolescents finding that therapists' relationship skills accounted for 59% of the outcome variance. That is to say, it appears that the alliance is an important factor in psychotherapy outcomes. Therefore, an inclusive EBP approach would not disregard a body of evidence simply because one believes it has "little promise" for clinical psychology.

How Important Is the Individual Therapist as a Predictor of Outcome?

According to the most recent review of the therapist effects literature (Baldwin & Imel, 2013), approximately 5% of outcome variance is attributable to therapists. Given that the treatment method accounts for roughly 1% of total outcome variance (Wampold, 2001; see Table 1 for list of effect sizes for different psychotherapeutic variables), therapist differences certainly hold their own as variables of scientific importance and for the improvement of the quality of care.

One of the conjectures of the CF approach is that therapists are important to the success of the treatment (a conjecture with which many proponents of ESTs also agree) and trials that do not examine therapist effects are consequently biased in that they overestimate treatment effects (Wampold & Serlin, 2000). Moreover, using therapists who have an allegiance to other treatments also biases the outcomes. The CF approach predicts that there will be variation among therapists in terms of their outcomes, as some therapists are more skilled, even if they are delivering the same treatment with adequate adherence. Therapist effects have been observed in both clinical trials and naturalistic settings for individual therapy (see Baldwin & Imel, 2013, for a review), and family/couple therapy (Alexander et al., 1976; Blow, Sprenkle, & Davis, 2007), although these results have been challenged (Crits-Christoph et al., 1991; Elkin, Falconnier, Martinovich, & Mahoney, 2006; Siev et al., 2009). The primary criticism is that the variability among therapists is owing to variability in the manner in which the treatment is delivered: If therapists give the treatment as designed (with adequate adherence and competence), then therapist effects would disappear (Shafran et al., 2009). This appears to be an adherence issue, and yet adherence (as well as rated competence) appears to not be related to outcome (Webb et al., 2010; but see Leichsenring et al., 2011). Importantly, we have begun to identify the actions of more effective therapists and they appear to be related to aspects of the CF approach. More effective therapists, as mentioned above, generally form better alliances with their patients (Baldwin et al., 2007) and have better facilitative interpersonal skills (Anderson, Ogles, Patterson, Lambert, & Vermeersch, 2009), and provide an emotionally activating relationship (Laska, Smith, Wislocki, Minami, & Wampold, 2013), as predicted by the CF perspective.

Are All Components of an EST Necessary?

The EST prediction is that all the ingredients of a treatment are necessary, as they target specific mechanisms of change, whereas

Table 1
Effect Sizes for Common Factors and Specific Ingredients

Factor	Number of studies	Number of patients	Effect size Cohen's *d*	% of variability in outcomes
Common factors				
Alliance[a]	190	2,630	.57	7.5
Empathy[a]	59	3,599	.63	9.0
Goal consensus/collaboration[a]	15	1,302	.72	11.5
Positive regard/affirmation[a]	18	1,067	.56	7.3
Congruence/genuineness[a]	16	863	.49	5.7
Therapists[b]	46	14,519	.46	5.0
Specific ingredients				
Differences between treatments[c]	295	>5,900	<.20	<1.0
Specific ingredients (dismantling)[d]	30	871	.01	0.0
Adherence to protocol[e]	28	1,334	.04	<0.1
Rated competence in delivering particular treatment[e]	18	633	.14	0.5

[a] Norcross and Lambert (2011). [b] Baldwin and Imel, 2013. [c] Wampold et al. (1997); confirmed by various other meta-analyses for specific disorders. [d] Bell et al., 2013 (targeted variables); see also Ahn and Wampold (2001). [e] Webb, DeRubeis, and Barber (2010).

the CF program states the opposite. We examine this conjecture by focusing on treatments for depression and PTSD.

According to the cognitive model, "changes in cognitions causally predict changes in psychopathology" (Hofmann, 2013), although it appears that this treatment may not be effective because of the cognitive components, as demonstrated by a dismantling study (Jacobson et al., 1996) and replicated by Dimidjian et al. (2006) (however, see Hofmann, 2008 for counterargument). The purpose of the Jacobson study was to "provide an experimental test of the theory of change put forth by Beck et al. (1979) to explain the efficacy of cognitive–behavioral therapy (CT) for depression" (p. 295), and it was hypothesized that "according to the cognitive theory of depression, CT should work significantly better than AT [modifying automatic thoughts + behavioral activation], which in turn, should work significantly better than BA [behavioral activation] only" (p. 296). Contrary to expectations, the outcomes of the BA condition were comparable with CT at termination and follow-up, and the authors concluded.

> These findings run contrary to hypotheses generated by the cognitive model of depression put forth by Beck and his associates (1979), who proposed that direct efforts aimed at modifying negative schema are necessary to maximize treatment outcome and prevent relapse (p. 296).

Concordantly, as noted by Dimidjian et al. (2006), "This growing body of research raises questions about the necessity of directly targeting negative thinking to achieve treatment response" (p. 667).

Regarding PTSD, a number of treatments have been found to be efficacious and have been classified as ESTs, including PE and cognitive processing therapy (CPT; Resick & Schnicke, 1992). However, EMDR has also been shown to be as effective as PE and CBT (Seidler & Wagner, 2006), a finding that is somewhat problematic because EMDR has been described as "pseudoscience" and has been compared with Mesmerism (McNally, 1999). To accommodate EMDR as an EST, a distinction between treatments that were "trauma-focused" and those that were not was invoked to maintain a hierarchy among treatments (Ehlers et al., 2010). Yet, trauma-focused treatments have not been found to be more efficacious than nontrauma-focused treatments that were intended to be therapeutic (Wampold et al., 2010). PCT, a treatment developed as a control for PE and CBT that contains no exposure and no cognitive components, and is not trauma-focused, became as effective as other ESTs for PTSD when it was provided by therapists who were trained to deliver it faithfully (Classen et al., 2011; McDonagh et al., 2005; Schnurr et al., 2007), and is now classified by the Society of Clinical Psychology as a research-supported treatment with "strong research support" (Hajcak & Starr, n.d.; Frost, Laska, & Wampold, 2014). Moreover, dismantling studies of ESTs for PTSD reveal that when the purported ingredients of the treatment are removed, the treatment remains efficacious (Bryant et al., 2008; Resick et al., 2008).

Furthermore, two meta-analyses of dismantling studies have showed that removing critical specific ingredients did not attenuate the effects of the treatment. Ahn & Wampold (2001) included 27 studies and found no difference when components were removed or added. More recently, Bell, Marcus, & Goodlad (2013) conducted a meta-analysis of 66 different dismantling studies and replicated the results of Ahn and Wampold. Although tentative at this time, there is insufficient evidence to reject the null hypothesis of no differences.

Conclusions

We claim there is no construction of science that at this time should privilege the EST perspective over the CF approach. Decisions about whether to abandon a research program are made by the scientific community and clearly clinical psychology has not abandoned the EST perspective; indeed, it is the received view at the present time. However, "the problem fever of science is raised by the proliferation of rival theories rather than by counterexamples or anomalies" (Lakatos, 1970, p. 121) and consequently the validity of the EST approach requires that it be examined vis-à-vis rival theories, the most prominent of which is CF. We believe both perspectives, which are not mutually exclusive, provide useful heuristics for testing the mechanisms of change in psychotherapy, and both are needed if we are to make impact on reducing the burden of mental illness for consumers of psychotherapy, a topic to which we now turn.

Implications of the EST and CF Approaches for Policy

In this section, we examine the policy implications for improving the quality of care from both the EST and CF perspectives. Our purpose here is to suggest that policy guided by the EST approach is useful, but that certain limitations have been overlooked.

EST and Quality Improvement

We believe transporting ESTs into practice settings should be one aspect of EBP. Yet, the prioritization of ESTs as the only "ethical" EBP requires some reexamination. For example, as noted by Chambless and Crits-Christoph (2006).

> Thus, in the face of evidence that Tx A works, it is not sufficient for the practitioner who prefers Tx B to rest on the fact that no one has shown that TxB is ineffective. Tx A remains the *ethical* choice until the success of Tx B is documented (emphasis added, p. 193).

However rational this seems, caution must be exercised when adopting this strategy.

EST implementation involves training (and supervising, as we shall see) therapists to deliver ESTs in practice settings, in lieu of the treatments they are currently practicing. The essential premise is that having therapists deliver an EST, rather than the currently delivered treatment, will result in better outcomes. This premise rests on the EST conjecture that some treatments are more effective than others, which as we have argued, is questionable. However, there is evidence that the marginal utility of transporting ESTs may be small. In a large data set of 12,743 patients collected in a managed care setting, therapists treating depressed patients obtained outcomes that were comparable with benchmarks set in clinical trials, and did so in fewer sessions (Minami et al., 2008, see also Saxon & Barkham, 2012). That is to say, practicing therapists appear to be achieving, on average, commendable outcomes, a fact that seems to be generally ignored by many. Moreover, there are few controlled trials in naturalistic settings comparing the delivery of ESTs to treatment-as-usual (TAU) in which the TAU involves comparable doses of psychotherapeutic services.

Most of the comparisons of EST and TAU involve a TAU that is not a legitimate psychotherapy service, such as referral to primary care physician (Wampold et al., 2011; Weisz, Jensen-Doss, & Hawley, 2006). When ESTs are compared with TAU that involves comparable doses of therapy, there is insufficient evidence to conclude that ESTs are superior for depression and anxiety in either adult or youth clients (Spielmans, Gatlin, & McFall, 2010; Wampold et al., 2011). In other words, there is insufficient evidence at this point in time to suggest that implementing ESTs will significantly improve on the quality of mental health services already in place.

When differences are found in effectiveness trials, the positive benefits may have little to do with the specific treatments being transported, and more to do with changing unproductive aspects of systems of care (e.g., identifying and removing iatrogenic treatments, providing less competent therapists with a theoretical rationale, extra training and supervision for therapists in the experimental condition, etc.). For example, in an effectiveness trial of CBT for depression (Simons et al., 2010), therapists in the TAU condition received no workshop, no extra training, and no supervision or consultation, whereas therapists in the CBT condition received a 2-Day 12-hr workshop, and 16 1-hr group telephone consultations for 1 year. Furthermore, claims that ESTs improve care beyond TAU simply because a significant prepost effect was found for the EST are spurious absent any preintervention outcome data.

A second issue is related to therapists. Some have argued that therapist effects can be eliminated through proper training and adequate adherence (Crits-Christoph et al., 1991), yet RCTs of ESTs do not use a random sample of therapists. Rather, therapists in clinical trials typically are selected for their expertise and are removed from the study if they cannot deliver treatment skillfully (see Suris et al., 2013). The assumption is that transporting ESTs to practice settings will be uniformly effective if therapists deliver the protocol with adequate fidelity. Yet, growing evidence suggests otherwise. There is evidence that therapist variability is significant even in practice settings in which therapists receive extensive training and supervision in an EST by a nationally recognized trainer (Laska et al., 2013).

Unfortunately, the assumption of therapist uniformity has persisted for decades. As noted by Donald Kiesler (1966) almost 50 years ago.

> Despite this token admission of therapist differences, the Uniformity Assumption still abounds in much psychotherapy research. Patients are still assigned to "psychotherapy" as if it were a uniform homogeneous treatment, and to psychotherapy with different therapists as if therapist differences were irrelevant . . . If psychotherapy research is to advance, it must first begin to identify and measure these therapist variables so relevant to eventual outcome (personality characteristics, technique factors, relationship variables, role expectancies, and the like) (pp. 112–113).

Although the methodological rigor of psychotherapy research has come a long way over the last several decades, we may have lost our way in asking some of the most important questions. Perhaps there is no more compelling and relevant area for advancing the study of which therapist variables to "identify and measure," than the study of what expert therapists, of any theoretical persuasion, actually do, as urged over 40 years ago by Bergin and Strupp (1972).

Cost under the EST approach, treatments are designed to be disorder-specific. In naturalistic settings, clients typically present with a wide range of comorbid conditions and therapists treat patients with a variety of disorders. Thus, to meet the needs of individuals under the current model, providers would need to be trained in multiple ESTs for various disorders (e.g., ACT for Depression, PE for PTSD, CBT for panic disorder). This concern is highlighted by McHugh and Barlow (2010):

> For example, even at specialty outpatient clinical service settings, clinicians would need to receive training in multiple individual protocols to be able to treat the target patient population using ESTs. A community mental health center that serves a wider variety of clinical presentations would require training in even more protocols. Attempting to maintain fidelity to each of these individual treatments would present an enormous challenge to a clinical care system. Given the cost of didactic (e.g., workshop, written materials) and competence (e.g., supervision and feedback) training, implementing multiple treatments to a facility is often not a feasible consideration (p. 951).

The costs are indeed great. Suppose a therapist in practice, who has not been trained in an EST, heeds the advice of Chambless and Crits-Christoph (2006) as stated above. Because this therapist sees many depressed patients, she decides to be trained in CBT for depression, the gold standard of treatments for depression. The cost of participating in a 3-day CBT Level I workshop for depression offered by the Beck Institute for Cognitive Therapy is $1,200 (http://www.beckinstitute.org/cbt-for-depression-and-suicidality/) to which one must add travel costs (airfare, hotel, and meals, say conservatively another $1,200 for someone who does not live near Philadelphia) and opportunity costs (i.e., lost income for at least 3 days, say at $100/hr, or conservatively $1,800), for a total of $4,200. In addition, once therapists have completed the workshop, ongoing supervision (competence training) is required at an additional cost to the individual therapist. Moreover, dissemination experts claim that there will be "drift" in adherence to treatment protocols, and so the practitioner would need to have refresher courses to adequately provide the treatment (Waller, 2009). Keeping in mind that the practitioner would need to learn several, and perhaps many, ESTs to deliver such treatments to most of his or her patients, the time and cost will be great. There might be some cost savings for scale, say for a clinic that might contract for group training. For a system of care, however, the cost could be great—the Department of Veterans Affairs spent over $20 million dollars to roll out ESTs between 2007 and 2010 (Ruzek, Karlin, & Zeiss, 2012). These costs must be considered in comparison to the actual benefit to an individual practitioner, clinic, or system of care, all of which are largely unknown.

Unfortunately, the costs are not limited to practitioners, clinics, and systems of care. Because the EST approach is the received view by many clinical scientists, we were interested in knowing how much is spent comparing two treatments intended to be therapeutic and what the marginal utility of such research is, in terms of knowledge. That is, what have we learned, and at what cost? We conducted a systematic search of several journals and relevant meta-analyses to find published comparative clinical trials that were funded by the NIMH between the years 1992 and 2009 for the treatment of anxiety and depression (Laska, 2012). We

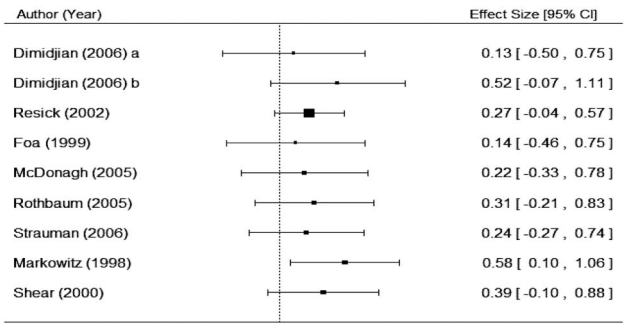

Figure 1. Forest plot of effects of NIMH-funded comparative trials.

found eight such studies, which included 483 patients and nine direct comparisons (see Figure 1), with a total cost of $11,760,874. A meta-analysis revealed that the null hypothesis that the true effects were zero could not be rejected, using the methods developed by Wampold and Serlin (2014). The only study with an effect statistically different from zero was Markowitz et al. (1998), which found IPT to be superior to CBT for depressed HIV patients. That is to say, for over $11 million, one null hypothesis was rejected, the overall effect was zero, and the actionable evidence is questionable: Should practitioners treating HIV depressed patients with CBT be retrained to provide IPT?

The dollar costs of disseminating multiple ESTs for multiple disorders to individual clinicians and health care systems are certainly prohibitive and make such efforts challenging. Yet, there lurks another aspect of the practicability of such aims that is rarely acknowledged. Although the feasibility of providing a clinical intervention usually emphasizes patient acceptability (e.g., APA Presidential Task Force on Evidence-Based Practice, 2006), both the interventions themselves and their supporting empirical base must be acceptable to practitioners. Just as different treatment approaches may "fit" the person of the therapist better than others, even the most compelling treatment research, whether based in the EST or CF tradition, will have little impact on clinical practice if it does not match the "fit" between the therapist's preferred method of therapy and her Self (Gurman, 2011). There is substantial evidence (e.g., Orlinsky, Botermanns & Ronnestad, 2001) that psychotherapy research, including RCT-focused research (Stewart, Stirman, & Chambless, 2012) generally has limited influence on how therapists practice. Rather than viewing the gap between RCTs and practice as "resistance" by clinicians, as some (Lilienfeld et al., 2013) have described the situation, we and our clients may be better served by acknowledging that, as shown here, there are many empirically legitimate ways to both practice and investigate psychotherapy. Skynner's (1980) quip that we need "different thinks for different shrinks" (p. 274) is consistent with the CF approach discussed here.

CF and Quality Improvement

The premises of quality improvement from the CF perspective are consistent with "practice-based evidence" (Barkham, Hardy, & Mellor-Clark, 2010; Duncan, Miller, Wampold, & Hubble, 2010), also aptly referred to as "progress research" (Pinsof & Wynne, 2000). In this approach, patient progress is assessed regularly, and these data are used to improve the quality of care. That is to say, the evidence is derived from actual practice, mirroring best practices in quality improvement in other fields. Our purpose here is not to suggest specific competency standards for therapists or to offer technical protocols for the administration of quality improvement systems. Rather, it is to advance the argument that the CF approach provides a scientifically grounded foundation for practice-based quality improvement that should complement RCT-based approaches to enhancing clinical care. Thus, for example, the possibility that a clinical focus on the five central components of the CF approach described earlier might improve outcomes deserves further investigation, even as technical refinements of RCT-based methods continue.

Feedback. Feedback systems typically consist of one or more self-report measures completed by clients regularly, and these data are then used in real time to provide individually tailored treatment decisions. By way of analogy, feedback systems have been described as a "mental health vital signs lab test" (Lambert, 2013b). Both "rationally derived methods," which set predetermined criteria for clinically significant improvement, and "empirically derived methods," which compare expected versus actual rates of change (see Castonguay, Barkham, Lutz, & McAleavey, 2013), have been adopted in systems of care.

One area in which feedback systems have the potential to make a significant impact is in regards to the "Lake Wabegon" effect. Studies have shown that therapists, regardless of approach, are poor at self-assessment and too often have a biased picture of treatment progress. For example, in one study, roughly 90% of therapists rated themselves in the top 25% of outcomes (Walfish, McAlister, O'Donnell, & Lambert, 2012). Feedback systems protect against this self-assessment bias and introduce data-driven methods into the therapy process. Simply providing therapists with feedback about the progress of their patients has shown to prevent treatment failures and improve outcomes (Lambert & Shimokawa, 2011; Shimokawa, Lambert, & Smart, 2010). Moreover, providing feedback reduces treatment length for patients making expected progress, but also keeps patients at risk for failure in therapy from dropping out. In one example where the Partners for Change Outcome Management System (PCOMS) was implemented by the Center for Family Services (CFS) in Palm Beach, Flordia, Bohanske and Franczak (2010) note:

> For example, average length of stay decreased more than 40%. Cancellation and no-show rates dropped by 40% and 25%, respectively. Most impressive of all, the percentage of clients in long term treatment that experienced little to no measured improvement fell by 80%! In 1 year, CFS saved nearly $500,000, funds that were used to hire additional staff and provide more services (p. 308).

Indeed, the Substance Abuse and Mental Health Services Administration (SAMHSA) National Registry of Evidence-based Programs and Practices (NREPP) only includes programs and practices that meet strict standards (see http://www.nrepp.samhsa.gov/). Lambert's OQ Analyst, Miller's (PCOMS), and Norcross' Evidence-Based Therapy Relationships are included in NREPP, indicating that aspects of the CF model can be translated into viable evidence-based programs and practices.

Alliance. Real-time feedback systems that include attention to CF factors may have great potential to address the central matter of *how* change happens in therapy by considering a wider range of

potential mediators of change than is typically attended to in RCT-based models. The use of practice-based evidence is progressing importantly toward giving therapists information about various elements in the CF approach, particularly the alliance. Lambert (2009) has developed clinical support tools that assess alliance, readiness for change, and social support. Miller and colleagues (Duncan, 2012) have developed the Session Rating Scale to assess the quality of the session and the alliance, so that therapist can monitor not only the outcomes of therapy but also the progress of therapy. Pinsof et al. (2009) developed an inventory to assess treatment progress and the therapeutic alliance focused on varying therapy systems, that is, couples and families as well as individuals. Given that a strong therapeutic alliance is one of the most robust predictors of outcome, continued data-driven efforts in this area has the potential to greatly improve care.

Therapists. Examination of therapist effects in practice settings indicate that most therapists achieve desirable outcomes, which is to say that they meet the benchmarks for various disorders (Minami et al., 2008; Minami, Wampold, Serlin, Kircher, & Brown, 2007; Wampold & Brown, 2005). What is most important from the CF perspective is investigating those therapists who achieve expected outcomes to identify what it is they are doing. Given that most RCTs typically include less than 12 therapists, rendering examination of therapist effects difficult, quality improvement efforts from a CF perspective should focus on those therapist behaviors in naturalistic settings that contribute to expected outcomes. Why is it that therapist differences have been acknowledged for over 40 years, yet as a field we are not much further in understanding the role of therapist variables than when Kiesler first acknowledged the "uniformity assumption" almost half a century ago? If we continue to disregard the importance of the therapist, a full one half of the clinical dyad, we drastically limit our ability to reduce the burden of mental illness. In order for EBP to achieve the aim of reducing mental illness, greater emphasis must be placed on the therapist, regardless of theoretical orientation or system of care in which he or she is examined.

Additionally, in naturalistic data sets it is clear that the patients of some therapists consistently fail to make expected progress (Minami et al., 2008; Saxon & Barkham, 2012; Wampold & Brown, 2005). The quality of service would be dramatically improved by assisting these therapists to achieve better outcomes. Managers of care could identify these therapists and provide additional training or supervision. However, the training provided should be designed to address particular skill deficits. From our perspective, the training and supervision might well involve a focus on relationship skills, if it is determined that those are lacking. However, in our experience some therapists fail to deliver a clear, cogent, and consistent treatment, which is one of the important aspects of the CF approach (Wampold & Budge, 2012). From the CF perspective, the particular treatment is not inherently crucial for most disorders, and adherence to the protocol is not crucial, but providing a cogent and acceptable explanation for the patient's difficulties and engaging the patient in healthy actions through a treatment structure is crucial. Thus, training in an EST may indeed be helpful, if there is evidence that a particular therapist is not achieving desirable outcomes because he or she is not providing an adequate treatment structure. If all attempts to assist a therapist to meet some consensual minimal outcome standards fail, then the profession, as well as the manager of care, needs to determine whether in the best interest of patients, the therapist should be "counseled" to find another line of work. This would be a controversial action but one that is accord with professional standards and accountability. By way of offering some context for this recommendation, we note that Saxon and Barkham (2012) found that of 119 therapists in practice, 19 had outcomes considered "below average" and if their 1,947 patients had been seen by "average" therapists an additional 265 patients would have recovered (see also Baldwin & Imel, 2013).

Training. The ideal training program, in our view, should contain elements of the both the EST and the CF perspectives. Psychotherapy trainees should be trained to provide ESTs as well as be trained in feedback systems, and how to form and repair strong working alliances, express empathy, and collaborate on treatment goals. We recommend going one step further, however, to provide "competency-based" certification. That is, to be certified, trainees would need to attain outcomes with various types of patients that meet a given standard (e.g., the benchmark for a particular disorder or type of patient). Such a competency-based system would help to attenuate the schism between Baker et al.'s (2008) proposed Psychological Clinical Science Accreditation System (PCSAS) and the American Psychological Association (APA) accreditation. Standards of accountability would dictate that students graduating from PCSAS or APA-accredited programs should have demonstrated that they are competent therapists by achieving commendable outcomes, regardless of the emphasis of their training programs and regardless of treatment approach.

Such a balanced approach to clinical training, open to scientific input from both the CF and EST points of view, would be in full accord with both the standards for EBP set forth by the APA (2006) and the important recently emerging emphasis on transdiagnostic understanding of emotional and behavioral problems (Barlow et al., 2011) and related unifying principles of psychotherapeutic change.

Conclusions—and a Challenge

Increasingly, psychotherapy theory and research of late has focused on how the CF and the specific ingredients work together to produce the benefits of therapy (Hoffart, Borge, Sexton, Clark, & Wampold, 2012; Owen & Hilsenroth, 2011; Pesale, Hilsenroth, & Owen, 2012; Ulvenes et al., 2012; Wampold & Budge, 2012). Much of this research shows that the CF do their work differently in different therapies—but clearly, both the CF and the specific ingredients need to be integrated.

We have presented the science of two research perspectives and suggested some ways to improve the quality of psychotherapeutic care, including the use of feedback systems as a unifying means of establishing accountability for treatment outcomes. In many quarters, the EST approach is the received view. We have attempted to make the case for an alternative and complementary CF approach, which has significant consequences for improving mental health services. Rather than argue about which perspective is more or less scientific, we issue the following challenge: How can we integrate the two models of empirical inquiry in a way that the field can move forward?

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