

COMMENTARY

The Promise of Science-Based Training and Application in Psychological Clinical Science

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Baker, McFall, and Shoham (2008) analyzed and critiqued the state of training in clinical psychology, asserting that much of this training is not sufficiently influenced by science. They asserted that the emergent demands of health care, with its attendant costs and resource constraints, require that mental and behavioral health care become increasingly efficient, effective, and cost-effective. Baker et al. (2008) then offered examples of how science-based evidence and methods could influence training in clinical psychology to achieve those goals. Laska, Gurman, and Wampold (2014, pp. 467–481) critiqued aspects of the Baker et al. (2008) paper. In the current paper, we argue that Laska et al. (2014) misconstrued points made in the early Baker paper. We also assert that evidence of common factors in psychological interventions is in no way antithetical or problematic to a science-based approach to clinical training and application. Further, we argue for a multidimensional approach to evaluating intervention performance, one that involves an evaluation of efficacy, effectiveness, cost-effectiveness, translation potential, and so on. Finally, we discuss how researchers can most efficiently develop intervention methods and delivery systems that are superior to the induction of common factors per se.

Keywords: psychological clinical science, common factors, psychotherapy effectiveness

Laska, Gurman, and Wampold (2014, pp. 467–481) attempt to accomplish several interrelated tasks in the target article. Specifically, they attempt to: (a) demonstrate the existence and meaningfulness of common factors (CFs) that exert influence across many types of psychological interventions, (b) rebut several claims and positions made in our earlier paper (Baker, McFall, & Shoham, 2008), and (c) identify the defining, core factors of psychological interventions (induced by CFs) and how these factors should be researched. We believe they do a good job of demonstrating that there are nonspecific or CF effects in psychotherapy and that such factors are related to outcomes to a meaningful degree. However, despite such findings, we think researchers should continue to try to develop and identify interventions that yield effects greater, and more specific, than those associated with CFs. We also have concerns about how well Laska et al. (2014) address the other two goals listed earlier.

Critique of Baker et al. (2008)

Laska et al. (2014) argue that Baker et al. (2008) overemphasized the importance and validity of empirically supported psy-

chological treatments (ESTs). They argue that the EST approach is based on several (at least) erroneous perspectives or beliefs—and that our article (Baker et al., 2008) exemplifies these faulty perspectives. We believe that the criticisms lodged by Laska et al. (2014) are largely misguided because they misconstrued key positions in the Baker et al. (2008) paper.

One critique was that we devalued or ignored factors that should, in fact, play a significant role in treatment selection (e.g., “clinician variables”). We confess that we found it difficult to guess the sorts of variables that Laska et al. (2014) were discussing here, but it seems that these would include clinician intuitions, patient preferences, and perhaps unknown clinician features that aid therapeutic processes (e.g., contribute to the therapeutic alliance). Relatedly, Laska et al. (2014) argued that clinicians should use more than one form of evidence to guide the selection and application of clinical interventions. We agree, in principle. For instance, we believe that decisions about the application of psychological interventions should be guided by evidence from cost-effectiveness studies, quasi-experimental designs, factorial experiments, adaptive trials, training/dissemination studies, meta-analyses, pragmatic trials, and so on (Riley, Glasgow, Etheredge, & Abernethy, 2013). In sum, we believe that scientifically trained, doctoral-level clinical psychologists (psychological clinical scientists) should be trained to synthesize diverse sorts of relevant research evidence that permit strong inference. If there is disagreement on this issue, it concerns how much weight to give nonresearch evidence. We would argue that the clinician should more strongly weight relevant research evidence, when it exists, over intuitions or informal syntheses of past experience. We would

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certainly support the application of research evidence that identifies trainable clinician behaviors that lead to enhanced CF effects. However, in our view, the necessary research evidence is simply not yet available to do so (Crits-Christoph et al., 2006; Huppert et al., 2014).

Laska et al. (2014) also seem to imply that the presence of common therapeutic factors (CFs) undercuts the likelihood that significant treatment-specific benefit will be found. While it is difficult to establish the magnitude and clinical importance of CF effects with certainty, we believe they are not of sufficient magnitude so as to curtail residual variance seriously (perhaps 5%–8% of outcome variance; Horvath, Del Re, Flückiger, & Symonds, 2011). In other words, there is still much room to improve the effectiveness of psychological interventions (even though much residual variance may be refractory to change). In fact, we believe that clinicians should be extremely disappointed if the magnitude of effects associated with CFs is the best we can do. A major goal of intervention researchers should be to specify and “demystify” CFs, transforming them from “nonspecific” factors into trainable components of well-defined interventions: that is, empirically or evidence-supported interventions (Crits-Christoph et al., 2006).

In addition, Laska et al. (2014) missed the main point of the argument made by Baker et al. (Baker et al., 2008) with regard to CFs. Laska et al. (2014) offer quotes from Baker et al. (2008) to support their contention that we dismissed CF effects as “unscientific” or not meaningful: for example, “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” (Laska et al. 2014, p. 471).

First, we did not label CF effects as meaningless or as “unscientific.” In fact, we noted in our paper that, “The critics are correct that nonspecific or general factors such as features of the clinician and the nature of the patient—clinician relationship are meaningfully related to outcomes . . .” (Baker et al., 2008, p. 82).

We tried to convey two chief points in Baker et al. (2008) regarding CFs. First, that the current epistemological status of CF effects (and clinician features as well) is not suitable to guide current application and training. We know little about how to manipulate such effects via training, we do not know much about their principal ingredients, and we do not know why they produce benefit—when they do produce benefit. For instance, to the extent that the therapeutic alliance yields CF effects, we do not know which particular therapist behaviors influence alliance and whether such influences are owing to trainable behaviors versus dispositional influences (Constantino, Morrison, MacEwan, & Boswell, 2013; Nissen-Lie, Havik, Høglend, Monsen, & Rønnestad, 2013). Second, we wished to convey that *at present*, the existence of CF effects, at least as they are currently conceptualized (Laska et al., 2014), provides little basis for the *practice* of psychological clinical science per se. We noted that, “At present, there is little basis for assuming that the induction of nonspecific effects will constitute a special province of scientifically trained psychologists . . . However, it may constitute a basis of practice of low-cost providers who do not need intensive training or a complex skill set.” Indeed, there is little or no evidence, at present, that a PhD or science-based training is needed to mobilize such effects. Thus, the Baker et al. (2008) article was aimed at analyzing how psycho-

logical clinical scientists should be trained and how they should spend their time, not at whether CF effects exist. If it is indeed the case that CFs occur via multiple methods, with one being as good as another, do not require adjustment (e.g., for disorder type), and so on (Laska et al., 2014)—we would then see no reason that their induction is a “special province” of psychological clinical scientists (vs. less highly trained and costly professionals). Thus, the situation would be one that is fairly common in health care, in which care is delivered via rational assignment according to training, knowledge, costs, and skills sets. However, if it were discovered that the optimal induction of CF effects requires use of particular techniques and modulation as a function of disorder type, disorder severity, patient features, level of stress, genetic factors, and so on, then we would change our view. Interestingly, recent research suggests that therapeutic alliance *may* show differential treatment effects (Arnow et al., 2013; Ormhaug, Jensen, Wentzel-Larsen, & Shirk, 2014; Shirk, Karver, & Brown, 2011). If this were true, it would mean that “common factors” would really constitute uncommon factors and much of this disagreement would be moot.

Thus, we do not believe that psychological clinical scientists should, at present, view the *induction* of CF effects as an important personal activity and goal. To be clear, though, we are not imposing, and cannot impose, this future on the field: the need for efficiency and cost savings in health care delivery is already doing so. In sum, Laska et al. (2014) misinterpreted our message and spent a good deal of time countering a position with which we do not agree.

There are, however, very real differences between Laska et al.’s (2014) position and ours with regard to criteria for treatment selection. They suggest that because different types of psychological interventions may produce similar effects on a clinical outcome measure (the CF effect), that this constitutes strong evidence of *equivalence* of the interventions. However, in the Baker et al. (2008) article, we were careful to note that from a public health perspective, this constitutes a very limited index of intervention status. We argued (Baker et al., 2008, pp. 69–72) that the scientifically trained clinician should support the use of interventions that are: (1) especially effective across multiple types of outcome measures, (2) can be implemented and disseminated easily (are relatively simple to learn/train), (3) are relatively cost-effective and cost-beneficial, (4) can be delivered by relatively low-cost providers, and so on. Thus, in our view, a comprehensive and methodologically principled appraisal of intervention effectiveness goes well beyond the sort of evidence that Laska et al. (2014) reviewed. We are quite confident that one can distinguish between many different types of psychological interventions on the amount and convincingness of evidence with regard to these multiple dimensions (although integrating and interpreting such evidence is certainly challenging; worthy no doubt of PhD level training). For many types of interventions, such evidence is largely missing in one or more dimensions, making the selection process somewhat easier. Importantly, if all available interventions are similarly efficacious, then the clinician should support the use of the intervention that stacks up best on other dimensions; *one cannot assume that interventions are equivalent or interchangeable just because they are similarly efficacious*. In our view, supporting evidence is not binary and clinical science training should involve

methods to synthesize evidence across multiple relevant domains (Collins et al., 2011).

In sum, while we do not ignore evidence of CFs, we note that important knowledge gaps preclude their central role in clinical training. We recognize that Laska et al. (2014) are attempting to address these knowledge gaps and we appreciate the importance of their goal. Further, if the induction of CFs is the best clinicians can do, the psychological clinical scientist might make better use of his or her science training by performing program evaluation, supervision, assessment, research, designing intervention delivery systems, and so on. The psychological clinical scientist should be trained to optimize his or her public health impact, not to occupy a particular professional role that does not make good use of his or her training and skills.

When Has a Psychological Treatment Achieved Scientific Support?

A good part of Laska et al.'s (2014) paper is spent critiquing attributes of an EST approach with which we simply do not agree. In our view (and we do not attempt to speak for other researchers in the field), we define an EST as one that has achieved relatively strong scientific support across a range of evaluative dimensions (e.g., dissemination potential, cost-effectiveness—space constraints prevent further discussion of this issue). We do not propose tight constraints for this definition. Laska et al. (2014) identify two core EST predictions: treatment specificity and disorder specificity. The former refers to the fact that because two ESTs *may* have different putative mechanisms of change, the two treatments must be differentially effective for a particular condition. This assumption can be challenged on many grounds. For instance, two ESTs could easily have the same fundamental mechanism of action in reality, despite contrary theory (i.e., the theory is wrong). The fact that a theory of mechanism might be incorrect does not by itself invalidate the worth of the therapy. Also, it simply might be that a disorder can be addressed similarly well via different mechanisms. One treatment might affect outcomes by changing affective information processing structures, whereas another might change coping strategies (observe the very similar overall effects of antidepressants that have very different neuropharmacologic actions). Finally, two treatments might affect outcomes via multiple mechanisms that are not wholly orthogonal.

Similarly, there is no reason that effective treatments must be differentially effective across different disorders (the disorder specificity prediction). Recent research (and as noted by Laska et al., 2014) suggests that many types of psychopathologies share common underlying vulnerabilities or processes (Caspi et al., 2014), suggesting that despite some differential manifestations, disorders share common causal substrata. Therefore, in some sense, different disorders are not truly, different disorders. Moreover, it is the case that scientific researchers should not fall prey to the “error of affirming the consequent.” That is, the mechanism of change may be largely unrelated to the psychological vulnerabilities associated with a disorder. For instance, monetary reinforcement for abstinence can reduce problematic alcohol intake, but it is very unlikely that alcoholism is caused by an absence of aperiodic monetary payments. Thus, a single psychological intervention (such as contingent reinforcement) could ameliorate a host of disorders having different etiologies, and yet could be considered

an evidence-based treatment for each. In sum, the two core EST predictions noted by Laska et al. (2014), in our view, are not essential for the demonstration that a treatment has strong scientific support.

The critical issue, we believe, is to evaluate intervention effectiveness using the best tools of science available. We do not believe that efforts to force alternative views to conform to narrow Procrustean criteria will yield optimal progress.

Promise of the CF Approach

Laska et al. (2014) not only raise concerns about the Baker et al. (2008) paper, but also outline the CF approach to research and argue for its uniqueness and promise. We now turn to that issue. First, it is clear that Laska et al. (2014) are attempting an innovative approach to identifying key elements of efficacious psychological interventions. We will follow their efforts with interest. However, we would not choose their approach, as we think it has some weaknesses.

Laska et al. (2014) describe “factors” or conditions that are “necessary and sufficient” for therapeutic change according to a CF perspective: namely, “(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” (Laska et al. 2014, p. 469).

We have many concerns about such factors. Because they are derived from a highly heterogeneous set of interventions (e.g., those entered into meta-analyses showing a CF), they must necessarily be loose and inexplicit. Thus, at present, these factors are aspirational and not suitable for training. In addition, they are tautological (as stated); the conditions necessary for healing are those that heal. For instance, there must be an “emotionally charged bond,” the explanation must be “adaptive,” and the procedures must lead to something that “is positive, helpful, or adaptive.” Obviously, positive, helpful, and adaptive strategies will be, well . . . positive, helpful, and adaptive. Certainly, Laska and colleagues (2014) will try to associate such aspirational, goal-based outcomes with therapeutic strategies that can be taught and applied, but it is clear that we are a long way from that at present.

We are concerned that the CF approach will not make rapid progress because it appears to rely on reverse engineering. That is, it attempts to extract core therapeutic strategies by inferring and inducting them from a heterogeneous set of outcomes gathered across innumerable studies, patient groups, intervention intensities and durations, and so on. This tactic seems less powerful than a priori experimentation that is designed to test specific strategies (designed for implementation) via experimental methods that permit strong inference. Thus, the Laska et al. (2014) strategy appears akin to replacing powerful hypothesis testing with seat-of-the-pants factor extraction. This seems nonoptimal to us because new research methods are being introduced that may increase greatly the efficiency of experimental approaches to treatment development: for example, the multiphasic optimization strategy, the se-

quential multiple assignment strategy, and so on (Collins et al., 2011; Riley et al., 2013). Of course, the CFs offered by Laska et al. (2014) may be used only for hypothesis generation; CF researchers may ultimately resort to mainstream treatment evaluation methods.

Finally, it is important to note that the CF approach shares with EST approaches the goal of identifying intervention strategies that enhance therapeutic outcomes. In fact, the CF therapy approach that Laska et al. (2014) outline could be labeled an *empirically supported nonspecific-therapy approach*. It offers a set of conditions (albeit vague) that must be met if the therapy is to be effective (e.g., a “healing” setting). To the extent that such vague features could be used to extract therapeutic strategies, and the resulting intervention(s) shown to be relatively effective, this therapy then could be considered an EST.

Summary: Core Differences Between Baker et al. (2008) and Laska et al. (2014) Approaches

We believe that Laska et al. (2014) have done a good job in arguing that there are CFs that are likely operative in many psychological interventions. Unfortunately, none of us should be satisfied with the magnitude of the estimated CF effects; moreover, we do not know enough about them to base training on their induction. At present, the occurrence or induction of CFs does not constitute a methodologically principled basis for training in scientifically based application.

We believe that Laska et al. (2014) came up short in the other two tasks they set themselves. First, we believe that they did not do a good job of critiquing Baker et al. (2008). This occurred in part because they misunderstood the points made in that paper, which rendered much of their critique moot. For instance, we did not say that CFs are trivial or nonexistent; instead, we noted that such effects do not constitute a satisfactory basis for the application of a scientifically based psychology that aims to optimize public health impact. Similarly, Laska et al. (2014) missed the point of Baker et al. (2008) that from a scientific and public health perspective, the clinician should give preference to interventions that enjoy the greatest research support when evaluated comprehensively: for example, with regard to efficacy studies, effectiveness studies, ease of training, ease of dissemination, cost-effectiveness, and so on. Treatments differ markedly on the bases of the amount and convincingness of such evidence. We believe that such criteria would constitute a methodologically principled basis of training and application. Finally, we have concerns about the CF approach to treatment research as outlined by Laska et al. (2014). We have concerns about the treatment factors that they identified and about the power and experimental rigor of the methods they outlined. Nevertheless, we wish them well in their work because we see their ultimate goal as similar to ours: to develop more effective psychological intervention strategies.

Finally, we offer what we see as the primary differences between the CF approach, as outlined by Laska et al. (2014), versus the approach to treatment development and evaluation, as outlined by Baker et al. (2008). We see the key differences to be the following:

- We (Baker et al., 2008) believe that the worth of psychological interventions should be evaluated more comprehensively; that

is, using multiple dimensions that address effectiveness, translation potential, application by low-cost providers, and so on.

- We advocate use of an a priori, theory-driven experimental approach to intervention development and evaluation, rather than a reverse engineering approach.

- Laska et al. (2014) believe that one treatment will be optimal for virtually all types of psychological disorders. We agree that many people may benefit from the same intervention components, which is consistent with recent research showing strong commonalities across multiple disorders. However, we believe that *ultimately* research will reveal a need to alter treatment with regard to dose/duration, contents, and delivery systems based on such features as genetic factors, disorder severity and type, costs, cognitive aptitude, and so on. As Laska et al. (2014) note, while progress in this area has not been rapid, there is already evidence that some differentiation is needed.

- Finally, the goal of Baker et al. (2008) differed from that of Laska et al. (2014). We were (and are) not so much concerned with how the individual clinician can justify the choice of the intervention that she/he personally delivers. We are much more concerned with training in *psychological clinical science*, which we believe requires a change in mindset about both training and application. We want to shift the field's focus from justifying individual practice per se to promoting public health benefit by considering the multiple roles that scientifically trained psychologists might play if their professional activities were truly informed by a comprehensive analysis and application of available data.

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