

AUTHOR RESPONSE TO COMMENTARY

Ten Things to Remember About Common Factor Theory

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Recently, we (Laska, Gurman, & Wampold, 2014, pp. 467–481) discussed the implications of taking a common factor approach for practice and policy. In this response to the commentary on our article, we reiterate 10 things that need to be remembered about common factor theory.

Keywords: common factors, evidence-based practice, psychotherapy, outcomes

It would be impossible, in the space provided, to address each claim made by the responders to Laska, Gurman, and Wampold (2014, pp. 467–481). Instead, we outline 10 important points that inform the discussion and address most of the issues raised by the respondents Asnaani and Foa (2014, pp. 487–490), Baker and McFall (2014, pp. 482–486), Beutler (2014, pp. 496–499), Constantino and Bernecker (2014, pp. 505–509), Crits-Christoph, Chambless, and Markell (2014, pp. 491–495), Hofmann and Barlow (2014, pp. 510–513), Lambert and Ogles (2014, pp. 500–504), and Weinberger (2014, pp. 514–518).

1. Common Factors Are Imbedded in a Scientific Theory

The primary criticism of the common factor (CF) approach is that it is tautological, untestable, and therefore not subject to the same scientific rules as empirically supported treatment (ESTs). For example, “Part of this tension is due to the fact that, at present, proponents of the EST movement rely on empirical evidence to justify their preferences and views, whereas proponents of CF primarily rely on repudiation of scientific evidence” (Asnaani & Foa, 2014, p. 489), “Our view is that the ‘CF perspective’ should be subject to the same sorts of empirical investigations as any other ‘perspective’ on behavior change” (Crits-Christoph et al., 2014, p. 491), “We are concerned that the CF approach will not make rapid progress because it appears to rely on reverse engineering . . . 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” (Baker & McFall, 2014, p. 484).

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In our article, we focused on the scientific theory of common factors, relying on Jerome Frank’s work (Frank & Frank, 1993) as well as our extension of it (Wampold & Budge, 2012; Wampold & Imel, in press). Frank’s theory of change was not simply a listing of common factors randomly collected together, or “reversed engineered,” but a coherent scientific explanation for how people change in psychotherapy (see also Frank, Hoehn-Saric, Imber, Liberman, & Stone, 1978). Frank’s theory and our expanded view are based on the science of how people heal in social contexts and describe specific factors that yield conjectures about what should be observed under various conditions (see Wampold & Budge, 2012; Wampold & Imel, in press).

2. The Mechanisms of Change in ESTs Are Ill Specified

We presented evidence that meta-analyses have shown few if any differences among treatments. Although several commentators agreed with this conclusion, others strenuously objected, claiming that we had missed some critical study or research synthesis that proved otherwise (Asnaani & Foa, 2014). Imbedded in this disagreement about the evidence are some critical issues that need to be pursued to their logical conclusion. Those who emphasize differential efficacy have various explanations that might explain the equivalence of treatments:

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 Finally, two treatments might affect outcomes via multiple mechanisms that are not wholly orthogonal. (Baker & McFall, 2014, p. 484)

The “common” mechanism was emphasized by Asnaani and Foa (2014):

It is not surprising, therefore, that treatments that share mechanisms of change will also produce similar outcomes. Accordingly, cognitive processing therapy (Resick & Schnicke, 1992), which shares the mechanisms of changing in inaccurate cognitions with PE, and eye

movement desensitization retraining (Seidler & Wagner, 2006), which shares exposure to distressing stimuli (activation of the fear structure) with PE, will all yield similar outcomes. But the similarity in outcome is due to shared mechanisms, not to the shared nonspecific factors or CF. (p. 488)

Basically, the consequence of these arguments is that any result of a clinical trial is immunized from providing evidence related to a mechanism of change. Equivalence of treatments might be due to incorrect theory or shared mechanisms of change, or both. This leads to some strange bedfellows, as noted by [Asnaani and Foa \(2014\)](#): Prolonged exposure (PE), cognitive processing therapy (CPT), and eye movement desensitization and reprocessing (EMDR) share common mechanisms of change, an unproven assertion. What is troubling is that, according to this view, it is permissible to say “common mechanism of change” to refer to some specific mechanism of change (usually of the author’s theory of change) that is purported to be therapeutic in several diverse treatments, but suggesting “common factor” of change is impermissible and unscientific. Epistemologically, why is “common mechanism” of change allowable but “common factors” impermissible?

This series of arguments highlights the important issue that various treatments contain multiple ingredients, some common and some specific (see, [Wampold et al., 2010](#) for a list of 17 possible efficacious ingredients in treatments for PTSD). Many of the ingredients are shared by very different treatments (e.g., PE and EMDR apparently), rendering specification of what constitutes a given treatment ambiguous (see [Baardseth et al., 2013](#)). For instance, according to [Hofmann, Asnaani, Vonk, Sawyer, and Fang \(2012\)](#), “Modern cognitive behavioral therapy (CBT) refers to a family of interventions that combine a variety of cognitive, behavioral, and emotion-focused techniques . . . [and] although these strategies emphasize cognitive factors, physiological, emotional, and behavioral components are also recognized for the role they play in the maintenance of the disorder” (p. 428). [Tolin \(2010\)](#) classified a treatment as CBT if it contained *any* of the following components: relaxation training (including progressive muscle relaxation, meditation, or breathing retraining), exposure therapy (imaginal or in vivo exposure, including flooding and implosive therapy), behavior rehearsal (behavioral training in social skills, habit reversal, or problem solving), cognitive restructuring (including direct strategies to identify and alter maladaptive thought processes), or operant procedures (systematic manipulation of reinforcers or punishers for behavior, including behavioral activation). Thus, CBTs can contain diverse ingredients, and it is entirely possible, even likely, that two CBT treatments might contain aspects of [Hofmann et al.’s](#) or [Tolin’s](#) therapeutic ingredients, but have no ingredients in common, rendering the category of CBT unable to explain the mechanisms of change (see [Baardseth et al., 2013](#)).

3. Common Factor Models Are Not a Closed System

We presented one variant of a common factor model. The goal of common factor models generally is to identify the factors that make psychotherapy effective—perhaps the name “common factors” is misleading. As [Weinberger \(2014, p. 514\)](#) eloquently argued:

I would prefer to say that some important factors may have not been operationalized well enough to be studied empirically; they have not yet been specified. Thus, *they are nonspecified, not nonspecific*. Contrary to the views of those questioning their scientific bona fides ([Baker et al., 2009](#)), so-called nonspecific effects are not ontologically nonspecific. They are capable of being empirically specified. They are therefore amenable to scientific scrutiny.

We have attempted to do exactly this—specify factors that make psychotherapy effective. However, there are other attempts to specify such factors and both [Constantino and Bernecker \(2014, pp. 505–509\)](#) and [Weinberger \(2014, pp. 514–518\)](#) commendably have done so. We are not proposing a closed set of factors; we presented some factors and the evidence that supports these factors. There will be others and some will not pass scrutiny, but the goal is to identify the factors that make psychotherapy work, whatever their status. Just as theories such as cognitive theory are a “maturing scientific discipline” and “will continue to change as basic research on psychopathology progresses” ([Hofmann, Asmundsen, & Beck, 2013, p. 206](#)), so too will common factor theories.

4. There Is No Such Thing as a “Common Factor” Treatment—And the Issue of Structure

We need to reiterate that there is no such thing as a “common factor” treatment. One of the aspects of all treatments is that the patients are provided an explanation for their disorder and that there are treatment actions consistent with that explanation. That is, the psychotherapy offered to the patient must contain a cogent explanation for the patient’s distress and a plan for overcoming his or her problems. Of course, explanations and treatments differ widely, but a therapy without *any* explanation—simply a relationship with an empathic therapist—is not sufficient, a point emphasized by [Jerome Frank](#). Consequently, one cannot conduct a randomized clinical trial (RCT) comparing a specific treatment and a “common factor” treatment. There have been over the years comparisons between established treatments and “psychological placebos,” which are treatments that contain no treatment rationale and no treatment actions (i.e., they have no treatment structure and no cogent rationale for how they work). Although many meta-analyses of such comparisons have demonstrated that a treatment with a cogent rationale and treatment actions outperforms such “psychological placebos” ([Lambert & Bergin, 1994](#); [Stevens, Hynan, & Allen, 2000](#); [Wampold, 2001](#)), what is surprising is how effective the “psychological placebos” are—often as effective or nearly as effective as evidence-based treatments to which they are compared ([Baskin, Tierney, Minami, & Wampold, 2003](#); [Cuijpers et al., 2012](#); [Honyashiki et al., 2014](#); [Markowitz, Manber, & Rosen, 2008](#)).

Theoretically, from a common factors perspective, treatments without *any* structure, even if the developers have a rationale in mind, will be less effective than treatments that provide the patient a rationale and a plan to overcome one’s difficulties (i.e., treatment actions consistent with the rationale for treatment; see [Wampold & Budge, 2012](#)). Recently, a well conducted trial found that CBT was superior to a psychoanalytic treatment for bulimia nervosa ([Poulsen et al., 2014](#)), the latter of which “was characterized by a nondirective approach where the patient is invited to talk as freely as possible, a focus on the therapeutic relationship, and involve-

ment of the patient in a mutual reflection on the function of and the circumstances triggering the symptoms of the disorder” (p. 110). However, when psychodynamic treatment focused on eating disorder behaviors, it was as effective as CBT (Zipfel et al., 2014). A primary component of the CF approach is the patient’s acceptance of the rationale of the treatment and the concomitant therapeutic actions (Wampold & Budge, 2012); as well, in Frank’s model, patient attribution that their hard work toward goals is an important therapeutic ingredient (Liberman, 1978; Powers, Smits, Whitley, Bystritsky, & Telch, 2008; Weinberger, 2014). Without any structure, treatments are unlikely to be optimally effective, particularly with regard to symptom relief, a point of convergence with Hofmann and Barlow (2014, pp. 510–513), it seems.

5. Anomalies: Deal With Them

As we discussed previously Laska, Gurman, and Wampold (2014, pp. 467–481), to be a legitimate explanation, theories must predict what should be observed under particular circumstances. Results not in accord with predictions are called anomalies. How theories deal with anomalies tells us much about the viability of theory (Kuhn, 1970; Lakatos, 1970; Serlin & Lapsley, 1985). Unexpected results do not *disprove* a theory and typically the theory is revised to accommodate these results. These ad hoc adjustments can produce a richer theory, in the sense that the revised theory is better able to explain how the phenomenon works (in Lakatos’ terms, the research program is progressive), but the ad hoc adjustments can become burdensome, as the theory becomes laden with amendments, and is found wanting particularly if an alternative theory exists which can explain observations more parsimoniously and can anticipate observations under various conditions.

There are many anomalies for any model that posits that the specific mechanisms of treatments are responsible for the benefits of psychotherapy. None of the commentators adequately addressed more than one of these anomalies. Of course, the equivalence of outcomes of diverse treatments is one such anomaly. In the previous section, “common” mechanisms of change was an auxiliary used to explain this observation. Of course, the amendments must stand up to scrutiny.

Briefly, we reiterate additional anomalies:

- *Adherence.* Adherence to treatment protocols is an important auxiliary, and a sensible one, for specificity of action. Unfortunately, adherence does not seem to be related to outcome (Webb, DeRubeis, & Barber, 2010), must be applied flexibly (Owen & Hilsenroth, 2014), and appears to be a function of the characteristics of the patient (Boswell et al., 2013; Imel, Baer, Martino, Ball, & Carroll, 2011). But the problems go further, if PE and EMDR are effective due to exposure elements, what does that say about the necessity of providing exposure in any particular manner?

- *Accidentally effective treatments.* There are many treatments that appear on lists of established treatments that were initially designed to be control groups, without what were considered active ingredients, or are treatments for which many claim are pseudoscientific (i.e., are based on faulty theory or contain inactive ingredients) including behavioral activation and interpersonal therapy for depression and EMDR and present-centered therapy for PTSD. Present-centered therapy, found to be as effective as established evidence-based treatments for PTSD (Frost, Laska, & Wampold, 2014), is particularly problematic theoretically because it intentionally contains neither exposure nor

cognitive restructuring, the two components that Asnaani and Foa (2014) claim are common mechanisms of effective treatments for PTSD. Moreover, treatments without structure, as mentioned previously, are as effective or nearly as effective as treatments designed to be therapeutic for particular disorders (see, Cuijpers et al., 2012).

- *Dismantling effective treatments.* Dismantling designs remove the purportedly effective ingredient from a treatment to demonstrate the purported therapeutic value of the ingredient. Two meta-analyses have shown that removing purportedly active ingredients does not attenuate the effects of the treatment (Ahn & Wampold, 2001; Bell, Marcus, & Goodlad, 2013).

These, and other anomalies (see Wampold & Imel, in press), while not fatal theoretically, should at least make clinical scientists who claim that the effects of psychotherapy are due to particular mechanisms stay up late at night worrying.

6. What Are the Conjectures Underlying EST Theory?

The following claim, mentioned above, needs additional scrutiny: “The fact that a theory of mechanism might be incorrect does not by itself invalidate the worth of the therapy” (Baker and McFall (2014, pp. 482–486). This suggests that theory may take a back seat to pragmatism; that is, we shouldn’t care so much about how a treatment works, as long as it does work (makes one question why EMDR has been scorned as pseudoscientific; Herbert et al., 2000; McNally, 1999). If the pursuit is truly pragmatic then we need only test the efficacy of treatments and not attempt to understand how they work. Such an atheoretical stance would render threats due to anomalies moot; however, it would also make any criticisms of common factors moot as well. But let’s be serious—we all want to understand how psychotherapy works because we are scientists, and more importantly, identifying therapeutic factors is the key to improving the quality of mental health services and to providing effective training.

If we do indeed take theory of change seriously, then theoretical propositions about what should be observed in a given situation must follow from the theory and conjectures should be put to the test. The three of us decided to write our article because we were unclear about what evidence would be sufficient to cast doubt on the specificity of ESTs. As discussed here, it appears that an equivalence of outcomes of theoretically diverse treatments is not sufficient, nor is the fact that treatments devoid of theoretically hypothesized ingredients are effective, or that removing the ingredients fails to attenuate the benefits of the treatments. We ask: What evidence would be sufficient for advocates of particular therapeutic ingredients to abandon their belief that these particular ingredients are remedial and consider an alternative explanation for how psychotherapy works?

7. Common Factors Does Not Imply “One Size Fits All”

One of the criticisms of common factors models is that such models imply that the same treatment could be applied to all patients, regardless of their disorder or other characteristics of patients. This is corollary of the claim that there is a “common factor” treatment discussed above. As emphasized in Laska et al. (2014) and as explicitly stated in common factor models, explanation and treatment relevant to the patient and the patient’s

problems is one of the common factors. Indeed, one of the consequences of taking a common factor approach is that there is flexibility to adapt the treatment to the characteristics of the patient, as emphasized by Beutler (2014, pp. 496–499) in his commentary as well as elsewhere (Beutler, Harwood, Kimpara, Verdirame, & Blau, 2011; Beutler, Harwood, Michelson, Song, & Holman, 2011). As Beutler has indicated, characterologically resistant patients may well benefit more from less structured treatments. As well, it appears that culturally adapted treatments, particularly if the explanation provided to the patient is consistent with cultural beliefs, are more effective than nonadapted treatments (Benish, Quintana, & Wampold, 2011; Huey, Tilley, Jones, & Smith, 2014). Some patients may respond to CBT and some may respond to emotion-focused therapy or dynamic therapy.

8. What Is Omitted Is Important

Often what is not discussed is more important than what is discussed. We find it interesting that other than Crits-Christoph, Chambless, and Markell (2014, pp. 491–495), none of the commentators mentioned the cost of training necessary to disseminate particular treatments, particularly given the paucity of evidence that such training would improve the quality of services. As noted by Weisz, Ng, and Bearman (2014):

One significant challenge is the implementation cliff, a drop in benefit that often occurs when interventions leave laboratory settings. Meta-analyses reveal substantial falloff in effect size when interventions move from research to practice contexts and when ESTs are tested against usual clinical care (UC; Wampold et al., 2011; Weisz, Jensen-Doss, & Hawley, 2006). In fact, one recent meta-analysis (Weisz, Kuppens, et al., 2013) showed that ESTs did not significantly outperform UC among studies using clinically referred youths or youths meeting formal diagnostic criteria (p. 59).

Where is the overwhelming body of evidence demonstrating that routine care is not effective? Why does it seem that the guiding principle of much treatment research is that therapists in clinical settings are performing poorly and will continue to do so until ESTs are delivered with fidelity? To date there is simply insufficient evidence to suggest that implementing ESTs will significantly improve outcomes, particularly given the extraordinary costs of dissemination (see McHugh & Barlow, 2010). Moreover, none of the commenters discussed the cost of conducting research comparing treatments, which has produced few if any significant results. Laska (2012) discovered that over \$11 million dollars had been spent on psychotherapy comparative clinical trials between 1999 and 2009, without any actionable results, a fact not a single commentator discussed.

We mentioned that providing information to therapists and/or patients themselves appears to improve the quality of services. With the exception of Lambert and Ogles (2014, pp. 500–504), and Crits-Christoph et al. (2014), the commentators did not appear to support such feedback as an alternative to EST dissemination, an augmentation to the delivery of ESTs, or as an accountability and cost-effectiveness tool.¹

9. RCTs Are Not the Only Path to Knowledge

Despite our arguments to the contrary, some of the commentators stated evidence from RCTs should receive the highest prioritiza-

tion among other research designs. We will not repeat our arguments again for brevity, but one should take note that clinical trials do not tell us much about mechanisms of change, a point discussed at length by Kazdin (Kazdin, 2007, 2009). Typically, clinical scientists use some form of correlational methods (structural equation modeling or multilevel models) to examine the relationship between mechanisms and outcome. Finally, it is indeed possible, although difficult ethically and methodologically, to examine the effects of some common factors in RCTs. With regard to empathy, for example, we refer the reader to an RCT that found that interactions with empathic clinicians improves outcomes of patients with irritable bowel syndrome, both in terms of symptoms as well as quality of life (Kaptchuk et al., 2008; Kelley et al., 2009).

10. “Different Thinks for Different Shrinks”

Our collaborator Dr. Al Gurman, who sadly passed away during the publication of this special issue, routinely emphasized the necessity that therapists base their practice on a variety of research evidence that “fits” within their worldview and how they see themselves as human beings. “This is *not* a call to ‘let them do whatever they want,’ but, just as different patients with different problems may need different methods and different therapists, so, too, do therapists need to have available to them different kinds of research that can help to improve outcome within their own preferred theory frame that makes sense to them about human nature” (personal communication, March, 2013). Given the substantial amount of evidence on the importance of therapist and patient factors on outcome (Baldwin & Imel, 2013; Bohart & Wade, 2013; Lambert, 2013), and the emphasis the APA’s Presidential Task Force on Evidence-Based Practice (2006) has placed on these factors, one conclusion that results from the CF approach is a greater emphasis on ideographic methods and individualized evidence-based practice.

Conclusions

Our reflections on the importance of common factors for improving the quality of services generated a range of reactions. Despite the criticisms of our presentation, we observe a recognition of this fact, as noted by Hofmann and Barlow (2014, pp. 510–513), “We do not agree and never have that so called ‘common factors’ are unimportant, nor do other clinical scientists” (p. 511). Rarely does a clinical scientist who wishes to demonstrate the efficacy of his or her treatment use a random sample of therapists—rather they select skilled therapists rather than cold, aloof, self-absorbed therapists. To continue to claim that common factors and therapists do not have an effect, or to simply say the effect is minimal compared with other psychological variables on outcome, is to deny both the broad empirical evidence in the research literature (Baldwin & Imel, 2013) as well as the experience of patients and therapists—we all know these things matter and it is time to truly expand the lens of evidence-based practice in psychotherapy.

¹ We mentioned both the OQ system (OQ Measures, SAMHSA, Salt Lake City, Utah) and the PCOMS (SAMHSA) as empirically tested feedback systems, without citing some studies supporting Duncan and Miller’s PCOMS (Anker, Duncan, & Sparks, 2009; Reese, Duncan, Bohanske, Owen, & Minami, 2014; Reese, Nosworthy, & Rowlands, 2009; Reese, Toland, Slone, & Norwosowrthy, 2010; Schuman, Slone, Reese, & Duncan, 2014).

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