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Let's Face Facts: Common Factors Are More Potent Than Specific Therapy Ingredients

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Luborsky et al.'s findings of a nonsignificant effect size between the outcome of different therapies reinforces earlier meta-analyses demonstrating equivalence of bonafide treatments. Such results cast doubt on the power of the medical model of psychotherapy, which posits specific treatment effects for patients with specific diagnoses. Furthermore, studies of other features of this model—such as component (dismantling) approaches, adherence to a manual, or theoretically relevant interaction effects—have shown little support for it. The preponderance of evidence points to the widespread operation of common factors such as therapist-client alliance, therapist allegiance to a theoretical orientation, and other therapist effects in determining treatment outcome. This commentary draws out the implications of these findings for psychotherapy research, practice, and policy.

Key words: common factors, specific ingredients, medical model. [*Clin Psychol Sci Prac* 9:21–25, 2002]

The effect size resulting from a meta-analysis that compares different therapies gives us greater confidence in a true difference existing between them than does any single study. By the same token, all the greater is our confidence in the results of a meta-analysis of meta-analyses, particularly if the results converge with previous estimates of the relative efficacy of various therapies. Luborsky et al. (this issue) have compared active treatments and found a nonsignificant effect size of .20 based on 17 meta-analyses, which shrank further to .12 when corrected for researcher allegiance. It is of considerable interest that these results closely parallel those of Grissom (1996) who meta-analyzed 32 meta-analyses of comparative treatments, reporting an effect size of .23. In a recent meta-

analysis also comparing active treatments, Wampold et al. (1997) found an effect size identical to that of Luborsky et al., namely, .20.

Even though these are small, nonsignificant effects, it should be noted that they are upper estimates or even overestimates of what is likely to be the true difference between pairs of active therapies. We believe this to be so for two reasons: First, these estimates are based on the absolute values of the differences between pairs of therapy, although some of the effect sizes are actually negative. To illustrate, consider therapies A and B, whose true efficacy is identical. Suppose one study shows, due to sampling error, that A has a slight edge (say a nonsignificant difference equivalent to an effect size of $d = .20$) and another study finds, due to sampling error, that B has a slight edge (again, nonsignificant and an effect size $d = -.20$). Averaging the absolute values produces an effect size of .20, when the true effect size is zero. Absolute values do not take account of the sampling error that typically produces these small but opposite effects, which one would expect to occur even if the true difference were zero.

A second reason we regard the Luborsky et al. figure of .20 as an overestimate is that many of their comparisons are of cognitive or behavioral therapies with treatments described vaguely as “verbal therapies,” “nonspecific therapies,” or “nonpsychiatric treatment.” Many of these treatments used as comparisons for cognitive or behavioral treatments were not meant to be therapeutic in the sense of an active treatment backed by theory, research, or clinical experience. That is, these treatments are not bonafide. At best, they were used to control for common factors, and at worst, they were foils to establish the efficacy of a particular treatment. Wampold, Minami, Baskin, and Tierney (in press) meta-analyzed therapies for depression and found cognitive-behavioral therapy (CBT) to be superior to the noncognitive and nonbehavioral therapies until they separated these therapies into two groups: those that were bonafide treatments (i.e., treatments supported by psychological theory and with a specified protocol) and those that were not (such as “supportive counseling” with no theoretical framework). It was shown that the superiority of CBT to these other therapies was an artifact of including non-bonafide therapies in the comparisons: CBT was not significantly more beneficial than noncognitive and nonbehavioral treatments that were intended to be therapeutic. Similarly, we take issue with the treatment

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groups presumably controlling for “common factors” in the studies of comparative psychotherapy effects meta-analyzed by Stevens, Hyman, and Allen (2000).

Study after study, meta-analysis after meta-analysis, and Luborsky et al.’s meta-meta-analysis have produced the same small or nonexistent difference among therapies. Despite this record going back almost 25 years to the meta-analysis of therapy outcomes performed by Smith and Glass (1977), Luborsky et al. (this issue), Crits-Christoph (1997), and others continue to hold out the possibility or even the likelihood of finding significant differences among the therapies. In a similar vein, Howard, Krause, Saunders, and Kopta (1997) suggested that therapies should be ordered along an efficacy continuum. Luborsky et al.’s meta-meta-analysis places us no closer to either goal than have the previous meta-analyses, precisely because the evidence points to all active therapies being equally beneficial.

Our argument is that there is much greater support in the literature for the efficacy of common factors as conceptualized by Rosenzweig (1936), Garfield (1995), and especially Frank (Frank & Frank, 1991) than there is for specific treatment effects on which, for example, the empirically supported treatment (EST) movement depends. In the remainder of this commentary, we briefly summarize the findings that, in our opinion, are a strong endorsement of the common factors view and constitute an indictment of the specific ingredients approach. For more comprehensive coverage of this evidence, see Wampold (2001) and for the argument against overreliance on ESTs, see Messer (2001).

SPECIFIC EFFECTS

The medical model (on which ESTs are based) proposes that the specific ingredients characteristic of a theoretical approach are, in and of themselves, the important sources of psychotherapeutic effects. Although a common factors or “contextual” approach (Frank & Frank, 1991; Wampold, 2001) also regards specific ingredients as necessary to the conduct of therapy, the purpose of such interventions is viewed quite differently within this model. In the common factor or contextual model, the purpose of specific ingredients is to construct a coherent treatment that therapists believe in, and this provides a convincing rationale to clients. Furthermore, these ingredients cannot be studied independently of the healing context and atmosphere in which they occur.

Component Studies

Within the medical model, component studies are considered to be one of the most scientific designs for isolating factors critical to the success of psychotherapy. Ahn and Wampold (2001) conducted a meta-analysis of component studies that appeared in the literature between 1970 and 1998. An effect size was calculated by comparing the outcomes of treatment with the purported active component versus treatment without the component. The aggregate effect size across the 27 studies was $-.20$, indicating a trend in favor of the treatment without the component, but which was not statistically different from zero. In other words, presumably effective components were not needed to produce the benefit of psychotherapeutic treatments, as the medical model would predict.

Adherence to a Manual

Within the medical model, adherence to the manual is crucial because it assures that the specific ingredients described in the manual, and which are purportedly critical to the success of the treatment, are being delivered faithfully. Consequently, one should expect treatment delivered with manuals to be more beneficial to clients than treatments delivered without them. The meta-analytic evidence suggests that the use of manuals does not increase the benefits of psychotherapy. Moreover, treatments administered in clinically representative contexts are not inferior to treatments delivered in strictly controlled trials where adherence to treatment protocols is expected. Although the evidence regarding adherence to treatment protocols and outcome is mixed, it appears that it is the structuring aspect of adherence rather than adherence to core theoretical ingredients that predicted outcome (Wampold, 2001). Moreover, the slavish adherence to treatment protocols appears to result in deterioration of the therapeutic relationship (e.g., Henry, Strupp, Butler, Schacht, & Binder, 1993).

Interaction Effects

Interactions between treatments and characteristics of the clients that support the specificity of treatments have long been a cornerstone of the medical model of psychotherapy. However, in his review, Wampold (2001) could not find one interaction effect theoretically derived from hypothesized client deficits (such as a biologically versus psychologically caused depression), casting further doubt on the specificity hypothesis. Although some interaction

effects have been found in psychotherapy (e.g., Beutler & Clarkin, 1990), they are related to general personality or demographic features—not interactions that would be predicted by the specific ingredients of the treatment (e.g., medication versus cognitive therapy). From a common factors viewpoint, interactions are more likely to occur based on clients' belief in the rationale of treatment or from the consistency of clients' culture or worldview with those represented in the treatment. It would predict, for example, that a client who believes that unconscious factors play a large role in human behavior is more likely to do well in psychoanalytic therapy than someone who views social influence as paramount.

We now turn to the evidence related to the question of whether commonalities of treatment are responsible for outcomes.

GENERAL EFFECTS

The Alliance–Outcome Relationship

The therapist–client alliance is probably the most familiar of the common factors and is truly pan-theoretical. There have been two meta-analyses of the alliance–outcome relationship. In the first, Horvath and Symonds (1991) reviewed 20 studies published between 1978 and 1990 and found a statistically significant correlation of .26 between alliance and outcome. This Pearson correlation is equivalent to a Cohen's *d* of .54, considered a medium-sized effect. Thus, 7% of the variance in outcome is associated with alliance (compared, for example, to 1% [*d* of .20] for differences among treatments). In the second meta-analysis, Martin, Garske, and Davis (2000) found a correlation of .22, equivalent to a *d* of .45. This is also a medium-sized effect, accounting for 5% of the variance in outcomes. Clearly, the relationship accounts for dramatically more variability in outcome than specific ingredients (which account for roughly 0%).

Therapist and Researcher Allegiance

Allegiance refers to the degree to which the therapist delivering the treatment or the researcher studying it believes that the therapy is efficacious. Within a medical model, allegiance should not matter because the potency of the techniques is paramount, but it is central to the common factors model espoused by Frank (Frank & Frank, 1991). Compared to the upper bound of *d* = .20 for specific effects, Wampold (2001) found that therapist allegiance effects ranged up to *d* = .65 and Luborsky et

al. (1999) found even larger researcher allegiance effects. Indeed, the latter authors found that almost 70% of the variability in effect sizes of treatment comparisons was due to allegiance. How odd it is, then, that we continue to examine the effect of different treatments (accounting for less than 1% of the variance) when a factor such as the allegiance of the researcher accounts for nearly 70% of the variance!

Therapist Effects

Because the medical model posits that specific ingredients are critical to the outcome of therapy, whether ingredients are administered to and received by the client is considered more important than the therapists who deliver them. By contrast, the contextual or common factors model puts more emphasis on variability in the manner in which therapies are delivered due to therapist skill and personality differences. In effect, the medical model says, "Seek the best treatment for your condition," whereas the contextual model advises, "Seek a good therapist who uses an approach you find compatible." As it turns out, therapists within a given treatment account for a fairly large proportion of the outcome variance (6–9%), lending support to the common factors model. (On the importance of the therapist, see also Bergin [1997] and Luborsky, McClellan, Diguier, Woody, & Seligman [1997].)

To summarize, common factors and therapist variability far outweigh specific ingredients in accounting for the benefits of psychotherapy. The proportion of variance contributed by common factors such as placebo effects, working alliance, therapist allegiance, and therapist competence are much greater than the variance stemming from specific ingredients or effects. The findings presented by Luborsky et al. (in press) have contributed in an important way to the evidence supporting the common factors versus specific ingredients model. We turn now to what follows from this conclusion.

RECOMMENDATIONS

Research

1. Limit clinical trials comparing bonafide therapies because such trials have largely run their course. We know what the outcomes will be.
2. Focus on aspects of treatment that can explain the general effects or the unexplained variance in outcomes. For example, American Psychological Association Division 17 (Counseling Psychology) is in the process of

studying interventions that work that are not tied to a specific diagnosis (Wampold, Lichtenberg, & Waehler, in press), and Division 29 (Psychotherapy) has a task force reviewing therapist–client relationship qualities and therapist stances that move therapy forward (Norcross, 2000).

3. Decrease the emphasis on specific ingredients in manuals and increase the stress on common factors. Most helpful are psychotherapy books that present both general principles of practice and specific examples, rather than manualized techniques that must be adhered to rigidly in clinical trials.

Practice

1. Cease the unwarranted emphasis on ESTs. They are based on the medical model, which has been found wanting and wrongly leads to the discrediting of experiential, dynamic, family, and other such treatments (Messer, 2001). The latter are not likely to differ in outcome from the approved ESTs (most of which are behavioral or cognitive-behavioral).

2. Therapists should realize that specific ingredients are necessary but active only insofar as they are a component of a larger healing context of therapy. It is the meaning that the client gives to the experience of therapy that is important.

3. Because more variance is due to therapists than the nature of treatment, clients should seek the most competent therapist possible (which is often well known within a local community of practitioners), whose theoretical orientation is compatible with their own outlook, rather than choosing a therapist strictly by expertise in ESTs.

Policy

As psychologists, we should try to influence public policy to support psychotherapy outside the medical system. In many respects the kinds of problems we treat and what constitutes healing or growth do not fit well within a medical model. Luborsky et al.'s (in press) results are a reminder of this fact. We do deal with suffering, and plenty of it—be it over loss, failing marriages, disturbed children, addictions, confused identities, and interpersonal or intrapsychic conflicts. Society has a large stake in terms of both public expenditures and quality of life in supporting psychotherapy, which has a proven track record of alleviating human suffering but needs a new institutional framework within which to do its job.

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