

A Conceptual and Methodological Analysis of the Nonspecifics Argument

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In support of the inference that psychotherapies produce their benefit by nonspecific means, researchers cite two sets of empirical findings: (a) null results from comparative outcome studies of different psychotherapies, and (b) a statistical relation between the therapeutic alliance and outcome. We show that neither kind of evidence provides strong support for the “nonspecifics” hypothesis. We argue that outcome equivalence does not imply that the same mechanisms produce the outcomes. We also present evidence that for four mental disorders there exists a psychotherapy that exerts its effects through theory-driven, specific means. Finally, implications of the relationship between the therapeutic alliance and outcome are carefully examined, with particular attention to design limitations in studies that have been assumed to support causal inferences. We conclude that the specific effects of psychotherapies may be substantially stronger than is widely believed.

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The field of psychotherapy has grown immensely over the past several decades as the number of different types of therapy has mushroomed. A few major schools of thought have spawned hundreds of different variations of psychotherapies, each built upon foundations that are often similar, but at least arguably distinctive. Given the number of options patients now face when choosing a treatment, and the increasing influence of managed care, much attention has been paid to research on the comparative benefits of these different treatments.

One approach to the possibility that therapies that appear to differ in theory and implementation, yet produce similar benefits, is to consider factors common to all psychotherapies. It has been proposed that the main benefit derived from psychotherapy may primarily be the result of such nonspecific factors. In fact, numerous findings have emerged in the last three decades that have been used to support the claim that the benefits of psychotherapies derive primarily from features common to all psychotherapies. Nonspecific elements in psychotherapy refer to elements of treatment that are shared across virtually all therapeutic interventions. They have been said to include a healing setting, education, a treatment rationale, expectations of improvement, a treatment ritual, and the therapeutic relationship. Frank (1961) postulated that all psychotherapies share several nonspecific factors, while each may have its own specific factors. Specific factors refer to the technical maneuvers that therapists engage in based upon their theoretical orientation (Butler & Strupp, 1986).

Two major areas of research have been used as evidence that nonspecific factors are the primary mechanisms of change in psychotherapy. The evidence

of outcome equivalence between two or more psychotherapies has been used to support the inference that therapies work through nonspecific means (Ahn & Wampold, 2001; Castonguay, Goldfried, Wiser, Raue, & Hayes, 1996; Ilardi & Craighead, 1994; Luborsky, Singer, & Luborsky, 1975; Strupp & Hadley, 1979). As the field of psychotherapy has grown, a wealth of literature has accumulated over recent years that speaks to the effectiveness of two or more psychotherapy treatments relative to each other. One influential response to attempts to discern superior treatment methods has been to declare the “Dodo bird’s” verdict: Everyone has won, and all must have prizes (Luborsky et al., 1975). That is, the assertion has been made that all (or at least very many) psychotherapies are about equally effective. Proponents of the nonspecific factors hypothesis assume that such equivalence derives primarily from elements of the therapies that are unrelated to the distinctive features of the theories that guide the respective therapies.

The second line of research used in support of the potency of nonspecific elements in psychotherapy is that of therapeutic alliance. Over the last two decades, the alliance appears to be the most frequently cited nonspecific putative causal factor across psychotherapies (Gaston, 1990; Horvath & Luborsky, 1993; Horvath & Symonds, 1991; Martin, Garske, & Davis, 2000), and many believe the therapist–client bond plays a major role in determining treatment success. Some researchers argue that the alliance–outcome relation supports the nonspecific-factors hypothesis inasmuch as the development of a positive therapeutic relationship is not unique to one form of psychotherapy. That is, since all psychotherapies seek to establish an alliance, and if the alliance is found to predict outcome, then it can be said that treatment outcome is determined, largely, by such nonspecific factors.

The evidence adduced in support of these two types of claims has led many to conclude that nonspecific factors are largely responsible for therapeutic change and that specific therapeutic techniques exert little, if any, impact in the course of successful therapy.

THE DODO BIRD’S DILEMMA

Perhaps the most frequently cited evidence for the proposition that therapy specifics are relatively un-

important comes from global meta-analyses, beginning with the work of Smith and Glass (1977), in which questions are of the form: “Is Therapy A superior to Therapy B?” It is now widely agreed that it is a mistake to ask whether any school of therapy is superior to any other school of therapy, on average, or for all disorders (cf. Shapiro & Shapiro, 1983). To ask such a question is akin to asking whether insulin or an antibiotic is better, without knowing the condition for which these treatments are to be given. When such undifferentiated questions are asked, results are necessarily compromised and, in addition, researchers may be led to believe that the “Dodo bird’s” verdict applies to most or all psychotherapies under most or all conditions (see Luborsky et al., 1975). Alternatively, researchers should begin with a problem and ask how treatments compare in their effectiveness for that problem. When the question has been approached in this manner, at least two patterns of findings have emerged from the psychotherapy outcome literature.

When One Psychotherapy Wins the Race Against Another Psychotherapy

In support of the argument that psychotherapies work through nonspecific means, some researchers have asserted that there is little evidence of any superior treatment effects for any form of psychotherapy for any disorder (e.g. Stiles, Shapiro & Elliott, 1986; Strupp & Hadley, 1979). Although evidence of outcome equivalence has been seen in the literature, it is untrue that no superiority has been established. Indeed there are many cases from comparative outcome studies demonstrating that one particular treatment is more effective than another treatment for a specific disorder. There are at least four psychotherapy/disorder pairs in the literature for which such “efficacious and specific” treatments (cf., DeRubeis & Crits-Christoph, 1998) have been identified: exposure and response prevention for obsessive compulsive disorder (OCD); cognitive therapy for panic disorder; exposure therapy for post-traumatic stress disorder (PTSD); and cognitive-behavioral group therapy for social phobia. Insofar as such specificity is found in research on these treatments for these disorders, we can say that researchers and clinicians know of a psychotherapy that is more effective than some alternative, plausible psychotherapy.

It has been demonstrated repeatedly that exposure and response prevention is a specific efficacious treatment for OCD. Fals-Stewart, Marks, and Schafer (1993) found that both a group and an individual version of exposure and response prevention outperformed a progressive muscle relaxation training group in an OCD sample. Relaxation training also served as the comparison condition in Rachman et al.'s (1979) test of exposure and response prevention for OCD. In combination with either clomipramine or a placebo, exposure and response prevention reduced OCD symptoms more than did the relaxation training condition (which also was given in combination with either clomipramine or placebo).

In panic disorder, David Clark's Oxford-based cognitive therapy package (Clark, 1996) has been found to be more effective than applied relaxation (AR) therapy in three separate studies, including one conducted by Öst, the developer of applied relaxation therapy (Arntz & van den Hout, 1996; Clark et al., 1994; Öst & Westling, 1995), a treatment that was designed, as was Clark's treatment, for use with those suffering from panic disorder.

Similarly, exposure therapy for PTSD has been repeatedly shown to be more effective than a range of nonexposure-based treatments, including "general counseling" (see Boudewyns & Hyer, 1990) and "conventional therapy" (see Boudewyns et al., 1990). In a study comparing exposure therapy, stress-inoculation training, and supportive counseling, both exposure therapy and stress-inoculation training were more effective than supportive counseling at the end of treatment. At follow-up, those in the exposure group were experiencing fewer PTSD symptoms than those in the other groups (Foa, Rothbaum, Riggs, & Murdock, 1991). In a replication of these findings (Foa et al., 1999), exposure therapy decreased PTSD severity, depression, and anxiety symptoms to a greater extent than did stress-inoculation training.

Finally, in two different studies of cognitive behavior group therapy (CBGT) versus an educational support group for social phobia, CBGT has been shown to be superior (Heimberg, Dodge, Hope, Kennedy & Zollo, 1990; Heimberg et al., 1998).

When one psychotherapy clearly outperforms another in the treatment of a given disorder, as appears to

be true in the four examples just discussed, credit must be given to the developers of the superior treatments. They have likely analyzed the pathology especially well and have described an effective way to treat it. While it still remains a possibility that, in some of these studies, the differential effects are achieved via nonspecific means (e.g., differing levels of patients' expectancies for success, varying competencies of therapists, experimenter bias, etc.), it is unlikely that such replicated findings are due to nonspecific factors. That is, while no study can purport to measure every possible factor potentially predictive of outcome, replications of a superior outcome effect suggest that treatment success is unlikely due to nontheoretically relevant factors such as client expectancies or therapists' dedication. Rather, the examples given above can be viewed as "existence proofs" that techniques can make a difference. In these cases, specifics matter. Therefore, it behooves clinicians to understand that the application of these specific techniques will likely lead to greater therapeutic change than will the application of techniques that have not fared as well in research trials. That is, it can be inferred from these studies that treatments with these procedures produce more benefit than is achieved by the mere provision of a caring therapeutic relationship in conjunction with different and, apparently, less potent techniques.

It has been suggested that if nonspecific factors were the main causal factors in outcome, research findings would be solely of therapeutic equivalence (Butler & Strupp, 1986). Repeated findings of nonequivalence in well-conducted experiments must, therefore, be taken as evidence that some differential factors between the two treatments are at work.

When the Race Between Two Psychotherapies is Declared a "Tie"

Another pattern one finds in the psychotherapy outcome literature is that two or more psychotherapies appear to produce equivalent benefit in a given population. For example, in two separate studies, both Williams and Falbo (1996) and Bouchard et al. (1996) found no difference in the effectiveness of cognitive therapy versus performance-based exposure in the treatment of panic disorder.

Another example comes from the depression literature, where interpersonal therapy (IPT; Klerman,

Weissman, Rounsaville, & Chevron, 1984) and cognitive therapy (CT; Beck, Rush, Shaw, & Emery, 1979) are considered to be effective treatments. Although there has been only one major comparison of these treatments (the outcomes of IPT and CT were not significantly different; Elkin et al., 1989), each has fared well in comparisons with other treatments, so that they are each considered “efficacious” treatments (DeRubeis & Crits-Christoph, 1998). It has been common for literature reviewers to regard the apparent equivalence of two or more therapies in such cases as evidence for the view that the equivalently effective therapies produce change through (the same) nonspecific factors (Ilardi & Craighead, 1994; Strupp & Hadley, 1979). The implication of this view is that when the practitioners of each system engage specific techniques and procedures they have read about in manuals or learned about in workshops or graduate programs, they are merely (and unwittingly) hand waving. That is, treatment developers and clinicians alike are fooled into thinking that the therapeutic strategies they take so seriously are important in the process of change, when in fact they are just filler. Of course, it is possible that this view is correct. When two therapies have been shown to be equivalent in treating the same disorder under similar conditions, outcome might be determined solely or primarily by elements of the healer-patient relationship, patient expectations, or some other factor that is shared between the two treatments.

However, it is at least a logical possibility that the two treatments each work via specific (and different) means to produce symptom change. This would be true if the developers of these psychotherapeutic systems each had a distinct insight into the nature of the pathology they were trying to understand, as well as a distinctive means of correcting it. Differing yet equally effective methods of treatment are frequently seen in medicine, but patients and physicians do not conclude that different medical interventions that are equally effective for the same condition act in the same nonspecific way. One example comes from the medical treatment of depression. Medications that are presumed to act on the norepinephrine system (most of the tricyclic antidepressants, or TCAs) are known to be effective, about equally as effective on average as those that act on the serotonin system (the selective serotonin reuptake inhibitors, or

SSRIs). But it is rarely, if ever, suggested that the TCA imipramine and the SSRI fluoxetine work primarily through nonspecific means to alleviate depression.

An example even closer to home comes from the psychotherapy versus drug comparative outcome literatures. For several diagnostic problems, including social phobia (Heimberg et al., 1998) and panic disorder (Barlow, Gorman, Shear, & Woods, 2000; Clark et al., 1994), both a psychotherapy and a medication have been shown to be effective. If the equivalence in outcomes between two psychotherapies is sufficient to support the hypothesis that they work through (the same) nonspecifics, it would follow that psychotherapies and equivalently effective medications also work through (the same) nonspecific mechanisms. Although it is likely that some of the effects of psychotherapeutic and pharmacotherapeutic therapies derive from nonspecifics (cf. the placebo effect), insofar as a drug outperforms a placebo, the reasonable presumption is that at least some of the medication benefit comes from the specific action of the drugs. Likewise, some of the psychotherapeutic benefit derives from therapeutic activities that are specific to the psychotherapies and not delivered in the medication treatments.

The literature to date remains unable to settle among potential explanations of outcome equivalence. It is possible that when two treatments effect the same level of change, this change is the result of nonspecific factors. It is also highly plausible that the change is the result of different specific factors. Further research remains to be done to differentiate between these (and potentially other) hypotheses. In the interim, findings of a lack of difference between treatments should not be used to buttress an argument that they are insufficient to support.

THE THERAPEUTIC ALLIANCE AND SYMPTOM RELIEF

When outcome equivalence is seen between two treatments (and even when it is not), further effort must be made to investigate the source of symptom improvement. One such area of study includes the exploration of the relationship between the client and therapist. In fact, there has been much interest among those who question the importance of therapeutic specificities in the role of this particular nonspecific factor, the “therapeutic alliance.”

The therapeutic alliance, broadly defined as the collaborative bond between the therapist and patient, has been described as a potent nonspecific factor (Krupnick et al., 1996). The analysis of this relationship as therapeutic can be traced back to Freud's early writings emphasizing the importance of clients forming positive attachments to their analysts (Freud, 1912/1958). Greenson (1965) later developed the term "working alliance" as the collaboration between therapist and client. Most empirical work investigating the therapeutic alliance has been examined in the context of psychodynamic (Barber, Connolly, Crits-Christoph, Gladis, & Siqueland, 2000; Raue, Goldfried, & Barkham, 1997; Gaston, Thompson, Gallagher, Cournoyer, & Gagnon, 1998) and interpersonal therapies (e.g., Krupnick et al., 1996).

The results of two meta-analyses have established a relationship between the therapeutic alliance and symptom improvement. Across 24 studies, Horvath and Symonds (1991) found a correlation of .26 between the therapeutic alliance and symptom improvement. A more recent meta-analysis by Martin et al. (2000) found a similar correlation (.22) between the therapeutic relationship and outcome. However, it is important to note that many of the constituent studies included therapists who integrated techniques from more than one orientation. Thus, the relation of the alliance-outcome within a specific form of psychotherapy was not addressed in these studies.

When examined explicitly in the context of specific forms of therapy, such as cognitive therapy, the role of the alliance has been less consistent. While some results have suggested that alliance leads to symptom improvement (Krupnick et al., 1996; Castonguay et al., 1996; Luborsky, McLellan, Woody, O'Brien, & Auerbach, 1985; Raue, Goldfried, & Barkham, 1997), other investigations have not demonstrated such a relationship (DeRubeis & Feeley, 1990; Feeley, DeRubeis, & Gelfand, 1999; Safran & Wallner, 1991). The inconsistent influence of the alliance in various therapies has led some to suggest that this relationship may play differing roles across treatment modalities (Gaston, Thompson, Gallagher, Cournoyer, & Gagnon, 1998; Safran & Wallner, 1991).

What does it mean that the alliance is critical to the progress of therapy, in a way that specific techniques

may not be? It is generally agreed that therapists who are unable to form a good working relationship with clients rarely will be successful in bringing about psychotherapeutic change in their clients. If this assumption is granted, several questions remain: How much of the variation that we see in therapeutic outcomes can be attributed to variation in the therapeutic alliance? And what is the causal implication of any statistical relation that is observed between the alliance and outcome?

To address the first question, an examination of Martin and colleagues' (2000) meta-analytic review of alliance research is appropriate. Across all studies in their review, the mean correlation between alliance and outcome was .22. This small correlation (Cohen, 1988) should make one pause before concluding that there is a strong association between the alliance and outcome, even if the .22 correlation is a substantial underestimate of the relation.

Perhaps a more important question concerns the meaning of a statistical relationship between outcome and the alliance, however weak or strong it may be, as it is vital that psychotherapy-process researchers come to a better understanding of the sources of variation in the alliance. Who (or what) is responsible for variability from case to case in the alliance? Aside from measurement error, there are at least four possible sources: the therapist, the client, their interaction (in the statistical sense), and symptom improvement. The theoretical implications, or our understanding of the alliance, will be very different depending on which of these causes one focuses on.

The therapist is potentially a major source of variance in the alliance. Large therapist effects would have interesting and important implications. If therapists differ in their ability, or tendency, to participate in good alliances, and if this variation accounts for (or mediates) therapist effects on outcome, this would lead us to identify those behaviors that the good-alliance therapists are engaging in, in the hope that such behavior could be taught to others. Of course, it is possible that these behaviors are not very teachable, in which case it still would be beneficial for clinical administrators, training program directors, and client consumers to use the capacity to form good alliances as a basis for selecting therapy trainees or therapists. Both of the possibilities that follow from the observation that therapist variation

in the outcomes they produce are mediated by the alliance have practical importance, and both of them would support the inference that the alliance is a causal factor in the production of therapeutic gain.

If, on the other hand, the client determines the quality of the alliance, the causal implications of the statistical association between the alliance and outcome are very different. It may simply be that good clients form good alliances and are bound to get better (Connolly Gibbons et al., 2003; Garfield, 1994; Martin et al., 2000). The practical implication in this case is that in order to maximize the benefit of a course of therapy, one would select clients who are likely to form good alliances, and thus improve. The capacity to form a good alliance, then, would be considered in a “triage” process. This is not very helpful for the client who is less able to form an alliance (unless it is also true that the inability to form an alliance predicts good benefit from another type of treatment, such as pharmacotherapy). Empirically, this hypothesis is yet to be tested. Most critically, insofar as clients determine both the alliance and response to psychotherapy, the correlation between alliance and outcome is not relevant to clinical decision making.

Another alternative to the possibilities that therapists or clients account for variability in alliances as well as outcomes is that it is the interaction of the therapist and client that determines the alliance. In practical terms, this means that the interaction of therapist characteristics and client characteristics determines the alliance and outcome. That is, certain types of patients may interact particularly well with certain types of therapists, for various reasons (gender, personality traits, socioeconomic backgrounds, political viewpoints, etc.). It may be that the dyad formed from the two individuals predicts alliance and outcome more strongly than either individual alone. In many ways this is similar to the idea that the client determines the alliance, in that it does not necessarily imply a causal relation between alliance and outcome. Moreover, its most important practical implication concerns the screening of clients for therapy. In this situation, rather than screening clients to decide whether they should be offered therapy at all, one would screen so as to match up a client with the therapist with whom he or she is most likely to form a good alliance and thus would be most likely to help him or her. While this possibility may be the one that most therapists

believe in strongly, it is also the most difficult to research. In fact, we are aware of no successful efforts of this kind.

A final alternative that could explain the alliance/outcome correlation is that the alliance, depending on when and how it is measured, might be a result of good outcome, as opposed to its cause (Glass, 1984). In two studies examining observers' ratings of the therapeutic alliance in cognitive therapy (DeRubeis & Feeley, 1990; Feeley et al., 1999), symptom change was not successfully predicted from alliance scores early in therapy. However, alliance score late in therapy was predicted from the amount of benefit the client had achieved until that point. In other words, the alliance acted like the result of, rather than the cause of, positive therapeutic response. Tang and DeRubeis (1999) found that sudden improvements in symptoms during the course of cognitive therapy (phenomena termed “sudden gains”) were not predictable from the quality of the alliance just before the sudden gain. However, the quality of the alliance was reliably higher in the session following a sudden gain, suggesting that a positive therapeutic alliance follows improvement in depressive symptoms, at least in cognitive therapy.

In the alliance literature, researchers have rarely employed research methods that are capable of ruling out a temporal confound in the search for a causal effect of the alliance on symptom change. Thus, most correlations of alliance and outcome reported to date, including nearly all of those in the Martin et al. (2000) meta-analysis, might well refer to a relatively uninteresting epiphenomenon: that good outcome produces good alliances. In a paper examining brief dynamic therapy, a modest but significant relationship of alliance and outcome was obtained, even after the investigators controlled for prior symptom improvement (Barber et al., 2000). Klein et al. (2003) also found a small but significant alliance-outcome relation within a study of cognitive-behavioral analysis system of psychotherapy (CBASP) with or without medication for the treatment of depression. They found that early alliance predicted improvement over the course of treatment, but early improvement did not predict subsequent alliance. Overall, it is clear that care must be taken in the interpretation of the currently available data and that further examination of these reciprocal effects is warranted.

Given the discrepancy between these findings in one study of dynamic psychotherapy; one of CBASP (which the authors postulate focuses on managing the alliance more than traditional cognitive therapies); and results obtained by DeRubeis and Feeley (1990), Feeley et al. (1999), and Tang and DeRubeis (1999) for CT, however, it is tempting to hypothesize that variations in the alliance may be predictive (and importantly causal) in some treatments, but not in others.

The one pattern of data that would be the most interesting in the investigation of the connection between the alliance and outcome is one that would support the possibility that therapists determine, to a substantial degree, the quality of the alliance, and that the alliance mediates therapist effects on outcome. Unfortunately, the data on this point are rather limited. There has been relatively little empirical work on therapist effects on outcome (i.e., do different therapists reliably produce different outcomes?), and none of these studies of therapist effects has included an inquiry into whether such effects are mediated by the alliance. The analysis that is closest to this comes from studies of therapy for opiate addiction, conducted by Luborsky, McLellan, Woody, O'Brien, and Auerbach (1985; see also Luborsky, McLellan, Diguier, Woody, & Seligman, 1997). They reported a significant therapist effect on outcome; that is, therapists differed reliably in the outcomes experienced by their clients. The authors also reported substantial correlations between outcome and clients' reports of the alliance in the 3rd session (upwards of .50, depending on the outcome measure). Unfortunately, they did not report on whether there was a therapist effect on the alliance, and they did not report any mediational analyses (cf. Baron & Kenny, 1986) that might have tied the critical statistical relations together.

A dataset would be consistent with the hypothesis that the alliance mediates (explains) a therapist effect if the following set of statistical relations were obtained in the context of random assignment of clients to therapists: (a) a significant difference among therapists in their mean outcome (a significant *F* test of the "therapist" factor in an analysis of variance, with symptom change as the dependent variable); (b) likewise, a significant difference among therapists in their mean therapeutic alliance score (a significant *F* test of the "therapist" factor in an analysis of

variance, with alliance as the dependent variable); and (c) a significant positive correlation between therapeutic alliance and outcome. Other statistical relations need to hold as well, but the three listed are essential (see Baron & Kenny, 1986, for a discussion of tests of mediation).

One would also want to examine phenomena other than the alliance that might account for the therapist effect in the same study. In fact, Luborsky et al. (1997) found therapist effects not only on the alliance, but also on adherence to the methods of the intended therapy and on positive therapist qualities. These authors suggest that these and other variables should be carefully examined in research on therapist effects. This type of research is now feasible in modern, large-scale efficacy or effectiveness studies, as long as there is random assignment of patients to the therapists in the study, and each therapist has a caseload that is large enough to yield sufficiently precise estimates of therapist effects (likely 10–30 per therapist). The existence and maintenance of tape archives is also crucial, so that variables such as the therapeutic alliance, adherence, and quality of the interventions can be examined.

CONCLUSIONS AND SUMMARY

We presented evidence from four disorders for which there is a psychotherapy that appears to exert its effects through specific means, and we argued that equivalence in therapeutic outcome does not necessarily imply that therapies achieve their benefit through the same non-specific means. Our examination of the statistical relation between nonspecific factors such as the therapeutic alliance and outcome raised serious questions about the available evidence base for the hypothesis that variation in the alliance is a strong causal factor in therapeutic change. In particular, we cautioned therapy researchers about the interpretations of correlational findings that have typically been reported in studies of the alliance. Thus, the "dodo bird" and recent interest in the alliance notwithstanding, we believe it is a mistake for the field to elevate the nonspecific factors of psychotherapies at the expense of specific therapeutic techniques. Clearly, further research must be done to deconstruct successful treatments and to identify potential causal mechanisms of change. While such research has been both suggested and conducted for some time, we believe that recognition of

its importance is imperative in the current climate. For now, the evidence that technique matters is sufficient to justify continued training in specific methods. Our hope is that results will continue to emerge from studies that are designed to test causal hypotheses about psychotherapies, and that new findings will bring greater clarity to the specifics versus nonspecifics debate.

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