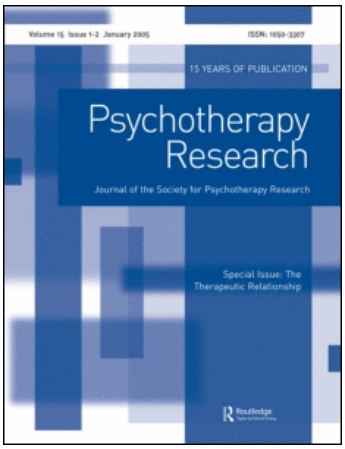

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Toward a working through of some core conflicts in psychotherapy research

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PRESIDENTIAL ADDRESS

Toward a working through of some core conflicts in psychotherapy research

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Abstract
The author discusses the evidence for six basic statements that many, but not all, psychotherapy researchers adhere to: (1) The therapeutic alliance has a causal role in outcome, (2) therapeutic techniques explain patients' outcome, (3) therapists determine outcome, (4) patients determine therapy outcome, (5) randomized controlled trials (RCTs) provide valuable data, (6) data from RCTs are almost worthless. These "truths" combine to form three core conflicts: Is psychotherapy about the alliance or techniques? Does the patient or therapist determine the outcome? Are RCTs a blessing or a curse? After showing that these statements oversimplify the research of the therapeutic process, the author recommends keeping both sides of the conflict in awareness and endorses a pluralistic methodological approach for the study of both efficacy and the mechanisms of psychotherapy.

Keywords: alliance; aptitude–treatment interaction research; brief psychotherapy; cognitive–behavioral therapy; depression; long-term psychotherapy; outcome research; personality disorders; philosophical theoretical issues in therapy research; process research

The goal of this article is to review some major disagreements in the field of psychotherapy research. There are many such conflicts, and it is clearly an impossible task to address all of them. However, one way to approach this matter is to explore the core beliefs that may be responsible for these disagreements. Specifically, I address six key statements, viewed as true by some and false by others, with a number of researchers' and clinicians' opinions falling somewhere between these two extremes.

The first of these statements is that the therapeutic alliance has a causal role in outcome. Second, therapeutic techniques are important in explaining patients' outcome. Third, the therapist determines psychotherapy outcome. Fourth, the patient determines therapy outcome. Fifth, randomized controlled trials (RCTs) provide valuable data and should be considered the gold standard of research methodology. Sixth, data from RCTs are almost worthless. Clearly, some of these statements appear to conflict with one another and, at least on the surface, could be considered mutually exclusive. These "truths," which some researchers take to be self-evident, can be compiled into three core conflicts as follows: (1) Is good therapeutic outcome a result of the alliance or of techniques? (2) Does the patient or therapist determine the outcome? (3) Are RCTs a blessing or a curse? In other words, are RCTs the main gateway to knowledge about the efficacy of psychotherapy or is the information they provide misleading?

Although one could also charge that these are "straw man" positions, I have seen exemplars of all of these statements made in various forms at different presentations or conferences, and it is perhaps not surprising that Society of Psychotherapy Research conventions generate such lively discussions. Further, it is not rare to see articles addressing

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only one side of the conflict. For example, in comparison to the large number of studies examining the relation between alliance and outcome, there are relatively few examining the impact of the Alliance $\times$Technique interaction on outcome (e.g.., Crits-Christoph & Connolly, 1999), as is shown later. Be this as it may, the focus of this article is not to thoroughly cover the large, often relevant body of existing literature addressing these three core conflicts but rather (1) to present some of the research that I have conducted that is relevant to these issues and (2) to convey some of my thoughts on how compromises can be created among these conflicting viewpoints. In addition, I delineate several research questions and paths that can be used to explore these ideas further.

It is also important to note that many other "big issues" or conflicts in psychotherapy research are not addressed in this article. For example, one important issue is the increasing use of drugs for psychological ailments. In the current health care climate, it is conceivable that at some point in the near future a researcher will not be able to conduct a psychotherapy study on depressed patients who have not been on medication or are not currently on medication, because such patients would be rare. Although there is some evidence that certain patients prefer a combined treatment and that many clinicians believe that combined treatments are the treatment of choice, the actual data supporting the use of combined treatment are meager for depression (e.g., Thase et al., 1997) and controversial for the anxiety disorders. Even in cases where patients who received combined treatment improved more than those who received monotherapy (e.g., Keller et al., 2000), Kocsis et al. (in press) have shown that patients who received their preferred modality of monotherapy (i.e., either the antidepressant or psychotherapy) did nearly as well as those who were randomly assigned to combined therapy.

Another challenge that I do not address is the lack of funding for psychotherapy research. It is interesting to note that when issues such as posttraumatic stress disorder (PTSD) in military personnel (resulting from the recent conflicts in Iraq and Afghanistan) rise to prominence, there is a quick increase in attention to psychotherapy as a first-line solution for these disorders. However, as these problems lose their salience, they also lose the interest of politicians and the funding agencies (this pattern of interest followed by subsequent "amnesia" has been thoughtfully noted in van der Kolk, Herron, & Hostetler, 1994). Although these and other issues are important to address, they are not discussed further in this article.

First Core Conflict: Is Psychotherapy about the Alliance or about Techniques?

The Role of the Therapeutic Alliance

One of the most commonly studied psychotherapy constructs is the therapeutic alliance. Many readers of this journal have studied this phenomenon and have demonstrated that the alliance is somehow associated with outcome. A meta-analysis of the topic (which included 79 studies) found that the average correlation between alliance and outcome is approximately .22 (Martin, Garske, & Davis, 2000). Although alliance has a reliable effect, it unfortunately does not explain much outcome variance. An important question to ask is whether the therapeutic alliance has a causal role in outcome. The problem with some of the existing research, as pointed out by DeRubeis and Feeley (1990) among others, is that many researchers have examined the correlation between the alliance measured at session X and change in some outcome measure from intake to termination. Therefore, as discussed in Barber, Connolly, Crits-Christoph, Gladis, and Siqueland (2000), many of these correlations may reflect the fact that the alliance itself could come out of earlier changes in symptoms.

In one examination of the causal role of alliance, Barber et al. (2000) used data from four pilot studies with varied patient populations that had been conducted at the Penn Center for Psychotherapy Research ($N = 88$). Eleven patients were diagnosed with either major depressive disorder or dysthymia and major depressive disorder of 2 years duration, 44 were diagnosed with generalized anxiety disorder, 19 with avoidant personality disorder, and 14 with obsessive–compulsive personality disorder. We found that early change in symptoms predicted alliance level at session 5, and that alliance at session 5 predicted change in depressive symptoms from session 5 to the end of treatment. Furthermore, alliance predicted subsequent change in depressive symptoms even after partialing out the impact of early change of symptoms on the alliance. Thus, this study showed that, although initial change in symptoms predicted alliance, alliance itself still predicted or induced subsequent change in symptoms.

The study mentioned previously was an initial foray into this question, but additional data are required to increase confidence that the alliance has a causal role in outcome. Only a limited number of studies have examined whether the therapeutic alliance predicted subsequent symptom change. Table I summarizes all the studies I am aware of that have examined this question. As can be seen, only one study (Klein et al., 2003) besides the one previously mentioned found that alliance predicted

Table I. Predicting Subsequent Outcome from Alliance, Taking into Consideration the Temporal Sequence

Study	*n*	*r*	Significance
DeRubeis & Feeley (1990)	25	.10	No
Feeley, DeRubeis, & Gelfand (1999)	25	−.27	No
Barber et al. (1999)	252	.01[a]	No
Barber et al. (2000)	88	.30[a]	Yes
Barber et al. (2001)	291	.01[a]	No
Klein et al. (2003)	367	.14	Yes
Strunk, Brotman, & DeRubeis (2009)	60	.15	No

Note. Adapted from Strunk, Brotman, and DeRubeis (2008).
[a]Represents the average of more than one correlation.

subsequent change in symptoms. Furthermore, the correlation obtained by Klein et al. was small. In summary, it seems that there is some small association between alliance and outcome. However, when reviewing only those studies that have carefully examined the sequence of alliance assessment and outcome, it seems there is not much support for the conclusion that the therapeutic alliance causes further improvement in symptoms. Because the studies from our group and from Klein et al. were based on therapies possessing a strong interpersonal emphasis, one could speculate that the alliance may have more of a causal role in those therapies. If alliance is not causally related to outcome, perhaps it could be associated with good outcome in the sense that if the alliance is high, then the therapy is going well. In fact, supervisors often tell their therapist trainees that if their alliance is not going well, it will be difficult to conduct therapy. If the alliance is going well, the prognosis is not so clear, but it is typically considered a good thing.

Future Challenges for Research on the Alliance

Although a number of authors in the field have presented their views of the future of the therapeutic alliance (e.g., Castonguay, Constantino, & Grosse Holtforth, 2006). I would like to end this section by focusing on what I feel may be five fruitful lines for additional research:

1. Is the alliance a cause for change in different forms of psychotherapy (cognitive–behavioral therapy [CBT] vs. dynamic), or is it more akin to a thermometer? As previously mentioned, there is some evidence that alliance may be causally related to outcome in interpersonal-dynamic therapies but not in CBT (DeRubeis & Feeley, 1990)
2. Does the patient's main problem make a difference? The Barber et al. (1999, 2001) studies did not find support for the causal role of the alliance in the treatment of cocaine-dependent patients. However, Barber et al. (2000) showed that alliance was causally related to outcome in depressed, anxious, and personality disorder patients in dynamic therapy. Thus, more systematic data are needed on the role of the alliance not only in different therapies but also with different kinds of patients (see DeRubeis, Brotman, & Gibbons, 2005). In other words, are there kinds of patients (e.g., patients with substance dependence) for whom developing an alliance is not as important in predicting their treatment outcome as it is for other patients (e.g., depressives)?
3. Is there a ceiling effect with current measures of the alliance? Examination of the mean alliance scores from many studies shows that patients completing these measures usually rate their therapists very highly (e.g., Barber et al., 1999, 2001). This is not likely due to the patients' concern about hurting the therapists' feelings, because most studies keep therapists blind to results. It is also possible that most patients who have low alliance ratings drop out of therapy, leaving the rest in treatment. However, more data addressing this issue are needed.
4. What participant qualities are associated with good alliance? We know that good alliance is created very early in treatment. In fact, we have found that it is high before some patients even meet their therapists (Iacoviello et al., 2007). However, more research is required to demonstrate which specific participant qualities help create a good therapeutic alliance (see, e.g., Connolly-Gibbons et al., 2003).
5. How much does the patient or the therapist contribute to the strength of the therapeutic alliance? There are very few data on this issue, with the exception of the findings of Baldwin, Wampold, and Imel (2007), who showed that therapists are mainly responsible for the relation between alliance and outcome (this issue is covered in more detail later).

The Role of Technique in Psychotherapy Research

A cursory review of the literature may convey the impression that many therapists feel that the alliance is the most important aspect of psychotherapy. Undoubtedly, creating a good working relationship is an important therapeutic task and possibly a prerequisite. However, when training young therapists, most instructors do not ask the trainees to focus only on the alliance, and most also train their students in the skillful implementation of therapeutic techniques.

For the last 20 years, I have been studying the role of techniques and alliance and their impact on outcome. The tools I have used to study techniques have primarily been measures of adherence and competence. Adherence is typically defined as the extent to which the therapist used prescribed techniques and avoided proscribed techniques as dictated by treatment manuals. Competence is the degree of skillfulness, nuance, and responsiveness with which the therapist delivers these interventions (Sharpless & Barber, 2009). Although my focus has been on the use of adherence-competence scales, it is important to acknowledge that there are a range of other methods for examining therapist techniques in psychotherapy, and these other methods have been useful in understanding the process of both naturalistic and manual-based psychotherapies (e.g., Hill, 2005; Stiles, Honos-Webb, & Surko, 1998).

Thus, an important question involves how techniques are related to outcome. There is an implicit belief that the more therapists do something prescribed by the treatment protocol, the better the outcome will be. Thus, for example, more interpretation of underlying conflicts and defenses will lead to more change. For behavioral therapists, more exposure is better than less exposure. But what is the evidence that greater adherence to a particular manual leads to a better treatment outcome? In the Treatment of Depression Collaborative Research Program, Elkin (1988) did not find a relation between adherence and outcome for interpersonal therapy or CBT. Similarly, no such relation was found for techniques in supportive-expressive therapy (Barber, Crits-Christoph, & Luborsky, 1996). DeRubeis and Feeley (1990), however, found a relation between the use of concrete cognitive therapy (CT) techniques and outcome in CT for depression. Quite clearly and perhaps surprisingly, there appears to be no consistent evidence for a strong relation between adherence and outcome (e.g., see the review by Barber, Triffleman, & Marmar, 2007; Beutler, et al. 2004). If adherence is not directly related to outcome, then how are the two related? Like many clinicians, Barber et al. (2006) suggested that the relation between adherence and outcome may be curvilinear. They examined a series of hypotheses relating technical and relational variables with outcome using data from the National Institute on Drug Abuse Collaborative Cocaine Treatment Study (CCTS; Crits-Christoph et al., 1999). The CCTS included a large number of cocaine-dependent patients ($N = 487$) who received individual drug counseling (IDC), CT, supportive-expressive therapy (SET), or group drug counseling. The individual treatments were supplemented with group drug counseling, and all therapists and counselors were extensively trained. Contrary to expectations, this study demonstrated that IDC was more effective than the other treatments (Crits-Christoph et al., 1999).

To understand what was helpful about IDC, Barber et al. (2006) rated counseling sessions of 95 IDC patients for whom both audiotaped sessions and at least one outcome assessment were available (representing 79% of the sample). They found that linear adherence was not associated with drug use. Because the authors had access to a large sample, they were able to examine several process research questions and could explore complex hypotheses in ways that would be impossible with smaller samples. Thus, they were able to test the clinically relevant hypothesis that a moderate amount of adherence was more effective than a high or low amount. As expected, they found a moderate effect size ($d = 0.44$) curvilinear relation between adherence and outcome. It should be noted that a moderate amount of adherence may imply therapist flexibility or responsiveness (Stiles et al., 1998); however, it may not necessarily indicate this. Moreover, flexibility was not empirically measured. Instead, the authors were mainly interested in assessing the amount of techniques that were utilized. The issue of how to assess flexibility is interesting. One possible way to gauge flexibility would be to look at standard deviations. If one sees an increase in standard deviations over time, this may be related to an increased flexibility (and possibly responsiveness). However, to the best of my knowledge, this hypothesis has never been tested.

An important question raised by these findings is whether they are specific to drug counseling. I was invited to submit an article to a special issue of *Psychoanalytic Psychology* (Gottdiener, 2008) on dynamic therapy for substance abuse. In that article, Barber et al. (2008) reported on 124 patients randomized to SET who had been treated by 13 therapists (nine women, four men) The authors were able to include 108 patients for whom they had outcome data and adherence ratings made with an adherence-competence scale developed specifically for that project (Barber, Krakauer, Calvo, Badgio, & Faude, 1997). The scale assessed therapist use of both supportive and expressive (interpretative) techniques with an additional emphasis on the SET techniques related to cocaine dependence. An example of a cocaine-related technique is whether the therapist relates the appearance of urges during the session to components of the core conflictual relationship theme.

Barber et al. (2008) also reported a relation between adherence and outcome for SET. However, contrary to predictions, the relation was that the

more a therapist was adherent to SET principles, the worse the patient's outcome was. Similarly, the less competently delivered the dynamic therapy, the lower the patient's drug use. These findings were certainly puzzling, because Crits-Christoph et al. (1999) showed that SET patients improved quite a lot even if they did not improve as much as patients who received IDC. In light of the fact that the greater use of SET techniques was associated with worse outcome, the authors examined whether the dynamic psychotherapists were doing something else in addition to the techniques prescribed by the manual. As part of the general investigation of the integrity of the cocaine study, Barber, Foltz, and Crits-Christoph (2004) used each individual treatment adherence-competence scale to rate each of the three individual therapies (SET, IDC, and CBT). Thus, for example, they had IDC adherence and competence ratings for a subgroup of patients ($n =$ 34) who had received SET. They found a large effect size ($d = 0.81$) curvilinear relation between the use of IDC techniques and outcome in SET patients. More specifically, those SET patients who received a moderate amount of (presumably unintentional) IDC interventions during their treatment improved the most in that condition. Of course, the amount of IDC interventions found in SET was somewhat lower than in IDC. Again, however, the pattern of results reported by Barber et al. (2006) in regard to the IDC group using the IDC scale was replicated among the patients who received dynamic therapy using the IDC scale. Barber et al. (2008) then tested how the combination of dynamic psychotherapy techniques and direct counseling techniques helped patients' outcome. They found that both sets of adherence scales (IDC $d = 1.33$; SET $d = 0.88$) predicted patients' outcome. As shown in Figure 1,

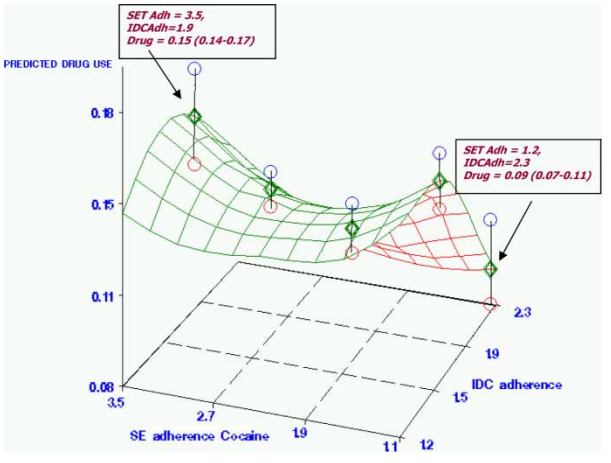

Figure 1. Patients' drug use as a function of adherence (Adh) to dynamic therapy and to individual drug counseling (IDC). (SET = supportive-expressive therapy.)

it is clear that as adherence to IDC increases and adherence to SET decreases, patients' outcomes improve. Focusing on two actual patients depicted in Figure 1, one can see that the patient on the left was still using a moderate amount of drug (predicted Addiction Severity Index Composite Drug Use [ASI] = .14–.17), and his therapy was characterized by relatively high adherence to SET and moderate adherence to IDC. In contrast, the patient on the right of the "saddle" had low adherence to SET but high adherence to IDC, and his predicted ASI Composite Drug indicated decreased drug use.

Another means of explaining the lack of a direct, linear relation between adherence and outcome is whether or not the competent delivery of treatment may be responsible for patients' outcome. A few studies have found evidence for the role of competence. For example, Shaw et al. (1999) showed that competent delivery of CT was associated with good outcome among depressed patients. Similarly, Barber et al. (1996) showed that the competent delivery of SET techniques early in treatment predicted subsequent change in symptoms. Using competence ratings made by expert clinicians, they demonstrated that when therapists skillfully delivered interpretative techniques (e.g., interpreted the patients' core conflicts), patients benefited from treatment. However, one needs to keep in mind that at least two other studies showed that increasingly competent delivery of dynamic techniques ended up being associated with poorer outcome (Barber et al., 2008; Svartberg & Stiles, 1994).

Finally, another reason why linear adherence may not be associated with outcome is perhaps explainable by the more global nature of these measures and the obvious fact that they do not focus on the occurrence of a specific technique at a specific point in treatment. Similarly, it could be that examining adherence alone does not allow for the evaluation of the interactive nature of the therapeutic encounter, where therapists are responsive to patients' expressions (e.g., Stiles et al., 1998).

Summary of the Role of Techniques

The research presented here regarding the role of techniques in relation to outcome leads to the following conclusions. First, delivery of therapeutic techniques is related to outcome in a way that is not necessarily linear, and this is in keeping with the impressions of many clinical researchers. Second, the competent delivery of techniques is associated with outcome. Third, as evidenced by the CCTS study, the use of intended interventions may have unintended consequences. Fourth, at times at least, outcome is associated with the use of unintended

interventions or even a combination of both intended and unintended interventions (e.g., Jones & Pulos, 1993). Some of these findings are associated with RCT research, and it is undoubtedly the case that working with RCTs enables one to explore these types of questions. At the same time, the research presented here does not address the all important question relating to how the delivery of one specific intervention (rather than a treatment or a package of interventions) impacts a theoretically relevant construct, which then results in a more distal outcome. For example, Crits-Christoph, Cooper, and Luborsky (1988) showed that accurate interpretations of core conflicts during the early phase of therapy was predictive of patients' improvement. Undoubtedly, more theoretically derived research of the impact of specific interventions on targeted theoretically relevant constructs is needed.

Future Questions Regarding the Study of Techniques

Many questions remain in the domain of study techniques, and I mention just a few here. One of most important involves the need for greater theoretical knowledge and specificity about which techniques really matter and which may better be classified as clinical lore. A second issue is how supportive techniques (including acceptance) complement and possibly interact with more active techniques (such as interpretation) in explaining good outcome. A third question to address is how the use of different therapeutic techniques changes during the course of therapy. A fourth question is to what extent evaluating therapists' competence involves the assessment of therapists' responsiveness (Stiles et al., 1998). Finally, to what extent is outcome due to unintended or even nontheoretically relevant interventions?

Possible Steps Toward a Resolution of the Core Conflict Between Relationship Variables and Therapeutic Interventions

The way I have described my research on alliance and techniques thus far seems to indicate that when I conduct research on technique, I ignore the therapeutic relationship. However, it is quite obvious that the therapeutic relationship and techniques are intertwined and indeed work together (e.g. Elliott, Greenberg, & Lietaer, 2004). Surprisingly, there is relatively little empirical work in this area (for some exceptions, see, e.g., Crits-Christoph & Connolly, 1999; Gaston, Piper, Debbane, & Garant, 1994; Gaston, Thompson, Gallagher, Cournoyer, & Gagnon, 1998). Barber et al. (1996) examined whether the competent delivery of expressive techniques predicted outcome over and above the effect of alliance. In this sample of depressed patients treated with SET (original outcome data were published in Luborsky et al., 1996), the competent delivery of interpretive (expressive) technique was the main predictor of outcome, and not the alliance (Barber et al., 1996). In fact, competent delivery of expressive techniques predicted subsequent change in depressive symptoms over and above both the level of the therapeutic alliance and the earlier change in symptoms.

In the aforementioned study of IDC for cocaine dependence, Barber et al. (2006) explored more complex relations between adherence and alliance and found that alliance interacted with curvilinear adherence in predicting outcome. A significant interaction between alliance and curvilinear adherence was found (Figure 2). The magnitude of the effect size of this interaction suggested that it was moderate in scope ($d = 0.44$). For patients who had a low alliance with their therapists, the curvilinear relation between adherence and outcome was more pronounced. In other words, for those patients a moderate amount of adherence generated a good outcome. For patients with a high alliance, the relation between adherence and outcome was less pronounced. In summary, one could say that a strong therapeutic alliance negated the impact of the counselor's adherence to IDC model, but that adherence was critical to the improvement of patients with low alliance.

Judging by the aforementioned studies, it seems quite clear that patient outcome is associated with both alliance and technique. A difficulty for researchers and clinicians is how to be simultaneously mindful of both of these aspects of therapeutic process. The relation among the therapeutic relationship, techniques, and outcome is a complex phenomenon that is constantly changing over time. This complexity is further compounded by the

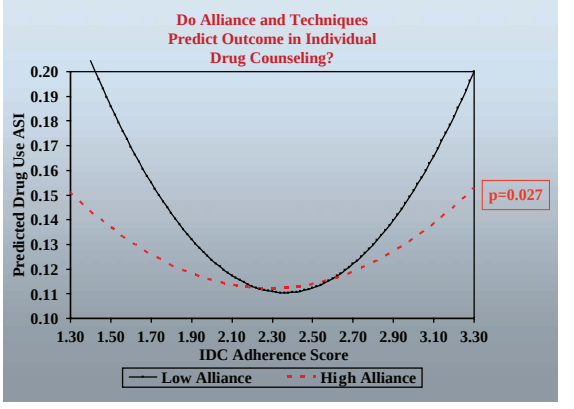

Figure 2. Do alliance and techniques predict outcome in individual drug counseling?

fact that these relations may vary as a function of patients' presenting problem and perhaps as a function of the type of therapy they receive as well. For example, Hill (2005) recommended studying the interplay of alliance and techniques on outcome as mediated by patient involvement. For researchers, however, working through what is perhaps an exaggerated dichotomy between the therapeutic relationship and techniques requires the ongoing confrontation of incorporating these two aspects of the therapeutic encounter in their research.

Future Research on the Alliance and Techniques

An issue that should be addressed in the future is how to pinpoint what the essential techniques are and how these interact with alliance to bring about patient improvement. As techniques and alliance impact on outcome, are they dependent on patients' problems, the specific therapies, or both? Many clinical researchers and clinicians conduct treatment using interventions that they believe are effective, and in many cases patients change. At times, one may conclude that these changes are due to interventions that are believed to have been delivered (whether that intervention is interpretation, exposure, or acceptance). However, there are other possibilities, because it is conceivable that patients change because of (1) something that was done but the therapist did not think much of it, (2) therapists not thoroughly doing something they thought they did, or (3) the fact that techniques outside of their chosen modality were included. In addition, sometimes it might be a combination of both the intended and unintended interventions that induces change. This is an important field of future inquiry that requires researchers to keep an open mind about which interventions they should assess when studying the therapeutic process (e.g., see Barber et al., 2008; Jones & Pulos, 1993). Further, do these processes change with different patients and with different forms of therapy? Creating and using multitheoretical scales to examine therapists' interventions is of great theoretical and practical utility (e.g., McCarthy & Barber, in press). Finally, it is important to gather knowledge about what the therapist and patient respectively contribute to these diverse processes.

Second Core Conflict: Is it all about the Patient or all about the Therapist?

In thinking about the kinds of findings reported previously, one may have the tendency to assume that therapists are responsible for the outcome, and that they have a major role in developing the interaction between adherence and alliance (i.e., that this is what really predicts patient outcome). Barber and Gallop (2008) have shown that in the IDC condition there was a range of outcomes for each therapist and that some therapists, on average, were better than others (see also Brown, Lambert, Jones, & Minami, 2005). Barber and Gallop also demonstrated that therapists differed in their overall level of adherence, with some therapists having overall higher adherence. However, there was a range of adherence scores for each patient that the therapist saw. Furthermore, significant differences between therapists (in terms of the levels of their scores) were found, and this likely reflects the interaction of the alliance with curvilinear adherence. One may infer that therapists were responsible for those differences.

So how can one determine whether these differences are due to the patient or the therapist? Using multilevel modeling, Baldwin et al. (2007) showed that the variance among therapists was responsible for the impact of alliance on outcome. Using the same methods, Barber and Gallop (2008) found that patient variance was responsible for the impact of the interaction of alliance and curvilinear adherence on outcome, and almost no variance was due to differences between therapists. More specifically, they found that patients explained about 24% of the outcome variance, whereas the therapist only explained 4%, and that finding of 4% was not even significant.

Conclusions Regarding Patient and Therapist Contributions to Outcome

Like prior researchers, Barber and Gallop have shown that therapists differ in their efficacy. However, they have also shown that patient factors seem to impact on important process variables such as adherence, the therapeutic alliance, and their interactions, which all have a bearing on outcome. Steps toward working through the conflict of emphasizing patients' variance on one hand or, instead, emphasizing therapists' variance on the other hand require researchers to keep in mind that both patients and therapist variables seem to make a difference. Most importantly, the field now has the tools to study these kinds of questions.

Future Research on Patient and Therapist Contributions to Process and Outcome

Research on patient and therapist contributions to the process and outcome of therapy is going to blossom. Until now, the methodological tools (or maybe the interest) to dissect the contributions of

the patient and therapist were not available. Using these tools, researchers will be able to investigate the relative contributions of using a variety of both outcome and process variables across a wide range of disorders and interventions. Finally, one will need to investigate the source of these contributions. For example, although Barber and Gallop (2008) found that patient variance was responsible for the impact of adherence and alliance on outcome, it was not clear what specific patient variables were responsible for these findings. In other words, what is it about the patient and about the therapist that can really make a difference? Hill (2005) suggested patients' involvement, and others have considered motivation for change.

Third Core Conflict: Do Randomized Controlled Trials Provide Valuable Data or Not?

Let me begin by suggesting that if the readers require treatment for a loved one who is ill, they would appreciate having data available from an RCT to help determine the best course of treatment. In other words, I speculate that in these situations many readers would prefer data coming from an RCT to data from a naturalistic trial or clinical lore. RCTs have many advantages. They are considered the epitome of the experimental approach as they attempt to reduce biases and minimize uncontrolled differences between the groups. RCTs increase the likelihood that one knows how results from the experimental group differ from those of the control group. In conducting RCTs, researchers attempt to define both the treatment and the population as much as possible in order to adequately generalize results. Finally, randomization allows for the use of powerful statistical analyses.

Despite these advantages, RCTs also have many shortcomings. In fact, many researchers have written extensive critiques of RCTs (e.g., Kazdin, 2008; Seligman, 1996). Before focusing on criticisms of RCTs that, to the best of my knowledge, have not been often made (yet may be valid), it is important to discuss a common criticism of RCTs that, in my experience, is not valid. One assumption of RCTs is that the patients involved are representative of the patient population at large, but some researchers have questioned this assumption. Critics of RCTs claim that these trials include easy and simple cases only, and that these patients are dissimilar to patients seen in private or community practices. In my experience, this has been the case in the past; however, in recent studies, this has clearly changed (e.g., Vinnars et al., 2007), and most clinicians who participate in RCTs would readily agree. Many practitioners treat clients who pay large sums of money (out of pocket) for psychotherapy. Such fortunate patients, generally, do not often come to psychotherapy RCTs. In the United States today, most of the patients seen in psychotherapy trials are those who cannot afford to pay even $20 per session to see a therapist. With such financial limits, the patients seen in RCTs may not be representative of the patients other clinicians see, but they are certainly not easy, straightforward cases. The patients recruited in RCTs certainly do not seem to have fewer Axis I, Axis II, and medical comorbidities. They also do not less frequently struggle with complex, ongoing psychosocial stressors than patients seen in other settings. Stirman, DeRubeis, Crits-Christoph, and Brody (2003) presented data suggesting that samples in RCTs of psychotherapy are representative of community outpatients, except for the fact that patients with adjustment disorders who are often seen in clinical work are not often studied in RCTs.

I now raise criticisms of RCTs that are rarely made but that appear to be valid. One concerns the belief that there should be only one difference between the treatment group and the control group. In reality, it is rarely the case that there is only one difference between the two groups when one conducts psychotherapy research. For example, most RCTs involve different therapists. By using different therapists, who cannot be randomly assigned, and different patients, there is already more than one difference between the two groups (Borkovec & Castonguay, 1998). Some researchers (e.g., Shapiro et al., 1994) have creatively avoided this problem by using the same therapists in both treatment groups. However, using the same therapist solves some problems but causes others, because there is now the possibility of allegiance effects for one treatment over another. Even if the therapists in these studies have no allegiance to a specific modality, it may not be representative of clinical practice (where therapists tend to be committed to a certain way of conducting therapy).

RCTs also have the implicit assumption that patients with the same diagnoses are similar and that they will respond to the same treatment. However, in light of the high comorbidity of psychological problems and heterogeneity of symptom presentations, it is quite likely that two patients with the same Axis I diagnosis will have different responses. For example, the presence or absence of one or more severe personality disorders (e.g., borderline personality disorder) may lead to a different pattern of response or even a lack of response. In their reanalysis of Elkin et al.'s (1989) study, Barber and Muenz

(1996) showed that depressed patients with avoidant personality disorder benefited more from CT, whereas those with obsessive–compulsive personality disorders benefited more from interpersonal therapy.

Connected to the earlier discussions of adherence and competence, another problem with RCTs is that different experts might not consistently agree that a specific therapy session was delivered accurately and with finesse (e.g., Jacobson, 1998). If disagreements exist on such a fundamental issue, treatment integrity must be questioned, because it may not be the case that everyone involved possesses the same understanding of what is meant by a specific treatment and clinical skill. Furthermore, it means that there is great latitude in the ways some of the therapies are conducted, even in RCTs. This is especially a problem for the cognitive, humanistic, and dynamic therapies, although possibly less of a challenge in the case of structured behavioral therapies such as prolonged exposure (e.g., for PTSD). More work, both theoretical and empirical (as discussed in Barber, Sharpless, Klostermann, & McCarthy, 2007, and Sharpless & Barber, 2009), could help to remedy this difficult situation.

The final problem I raise in regard to RCTs is more pragmatic. RCTs are enormously expensive and time consuming. There is obviously a real and very finite limit to the number of RCTs that even very productive leaders in the psychotherapy research field can conduct during their lifetimes. Very few researchers will conduct more than five trials in their career. So how many RCTs can all psychotherapy researchers as a group conduct? And at what financial and temporal cost? Keep in mind that there is a large number of possible diagnoses (more than 350 in the *Diagnostic and Statistical Manual of Mental Disorders*, fourth edition, text revision; American Psychiatric Association, 2000) and a very large number of possible comorbidity combinations, and that there are many forms of therapy to remedy these diagnoses and diagnostic combinations. As Parloff (1982) calculated years ago, it is clear that there is no possible way, even in a better financial climate, that we could conduct enough RCTs to cover all disorders and treatments.

In summary, RCTs are crucial and provide us with very important, high-quality data, but they have real limitations when applied to the study of psychotherapy. Even if there were no epistemological or methodological problems with RCTs, it is impossible to conduct enough of them to meet our needs for evidence and accountability. So it is apparent that RCTs must be somehow supplemented with other types of evidence. A question remains, of course, as to how to best accomplish this task.

Toward a Multifaceted Approach to Study Psychotherapy Research

One way of addressing these issues is to conduct naturalistic studies with large samples. One recent example is provided by Stiles, Barkham, Mellor-Clark, & Connell (2008a) using data collected from 33,587 patients seen by 637 therapists at 34 National Health Service primary care counseling services. They inquired as to which treatment participating therapists intended to implement and focused on therapists who delivered CBT, client-centered psychotherapy, and psychodynamic psychotherapy. Using patients' self-report, they found no significant difference in outcome between the three groups. Following the publication of the study, Stiles and colleagues had a lively discussion about the shortcomings and advantages of the study with David Clark (Clark, Fairburn, & Wessely, 2008; Stiles, Barkham, Mellor-Clark, & Connell, 2008a,b). Although I am certainly not suggesting that there are no methodological problems with Stiles et al.'s particular study, future studies could clearly learn from this discussion and be improved in ways that they can better answer Clark et al.'s criticisms, remain fairly naturalistic, and not utilize randomization. A relatively simple way to improve such naturalistic studies would be to assess patients more thoroughly so as to engender confidence in the patients' diagnoses. One would also need to know more about why specific therapists included specific patients in the study and, further, why particular patients approached a particular therapist as opposed to another. Finally, it would be advisable to have a better operationalization of the treatment while maintaining fidelity to practice in order to ensure that the CBT or dynamic therapies that were implemented were indeed acceptable versions.

As stated, psychotherapy RCTs are important and beneficial but very expensive. For the most part, only governmental agencies are funding them, in contrast to pharmacotherapy trials funded by pharmaceutical companies. Therefore, they are difficult to implement in high numbers. These issues related to RCTs are not often mentioned as problems in the field, and it may be that the manifest difficulty of the conflict makes researchers not want to keep them in the foreground. Even if readers do not agree with these points, they are still likely to agree with the appraisal that there are clearly major strengths and weaknesses to both RCTs and the more naturalistic studies of psychotherapy outcome. It seems clear that methodological pluralism could be a fruitful approach for the study of both the efficacy and the mechanisms of psychotherapy.

As a corollary of this, one often considers data from these divergent methods when thinking practically about which types of treatment would benefit our patients. Flexibility is an important quality that most psychotherapists and researchers value in their own lives and the lives of their patients. This is important, because there might not be clear and simple answers that work for all patients, treatments, therapists, and disorders.

In closing, I foresee an interesting future for our field of psychotherapy research, and there appears to be a sufficient number of questions and problems to keep at least several generations of researchers quite busy. I hope more work will be done to understand why some patients change in some treatments and to determine whether there are ways to help more treatment-refractory patients. In light of our subject matter's inherent high level of complexity, it seems reasonable for us to proceed with a measured respect for what has worked, an openness to new approaches, and a healthy skepticism toward methods that promise more than seems reasonable. Looking at the larger picture, it is my hope that what we have learned in the clinics regarding what helps and what impedes change can be used by the next generation of researchers to solve some of the problems facing humanity such as war and intolerance.

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