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Running Head: THE MANUAL IS NOT THE TERRITORY

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Treatment Manuals Do Not Improve Outcomes

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Treatment Manuals Do Not Improve Outcomes

Barry L. Duncan and Scott D. Miller

You can't do cognitive therapy from a manual any more than you can do surgery from a manual.

Aaron T. Beck, New York Times

Although manuals date back to the 1960's (Lang & Lasovik, 1963), the trend toward describing, researching, teaching, practicing, and regulating psychotherapy in the terms of the medical model began much earlier. Albee (2000) suggests that psychology made a Faustian deal with the medical model when it uncritically accepted the call to provide psychiatric services to returning veterans of World War II—and perhaps permanently inscribed it at the historic Boulder conference in 1949, under protest by many, when the scientist-practitioner model incorporated medical language and the concept of “mental disease.”

Later, with the passing of freedom of choice legislation guaranteeing reimbursement parity with psychiatrists, psychologists learned to collect from third-party payers by providing a psychiatric diagnosis. Soon thereafter, the National Institute of Mental Health (NIMH) decided to apply the same methodology used in drug research to evaluate psychotherapy—the randomized clinical trial (RCT). It meant that a study must include manualized therapies and *Diagnostic and Statistical Manual* defined disorders to be eligible for a NIMH-sponsored grant (Goldfried & Wolfe, 1998).

Manualization, however, reached its zenith with the advent of evidence-based practice. Following the trend in medicine toward Diagnostic Related Groups, in 1993, the American Psychiatric Association first developed practice guidelines for Major Depression and Eating Disorders, and followed with many other diagnoses. Psychiatry's imprimatur gave an aura of

scientific legitimacy to what was primarily an agreement among psychiatrists about their preferred practices, with an emphasis on biological treatment (Duncan, 2001).

Arguing that clients have a right to empirically validated treatments, a task force of the APA's Division of Clinical Psychology derided psychiatry's practice guidelines as medically biased and unrepresentative of the literature and set forth its decision rules about what constituted scientifically valid treatments (Task Force, 1993). Instead of clinical consensus, the task force adopted decision rules that favored manualized therapies and research demonstrations that a particular treatment has proven beneficial for clients in RCTs. An explosion of manualized therapies ensued: Drawing on 8 of the 12 overlapping lists of empirically supported therapies, Chambless and Ollendick (2001) noted that 108 different manualized treatments have met the specific criteria of empirical support—a daunting number for any clinician to consider.

Although the move to manualize psychotherapy emerges from its increasing medicalization, this position paper seeks not to demonize manuals as the “evil accomplice” of the medical model. Manuals have a positive role to play. They enhance the internal validity of comparative outcome studies, facilitate treatment integrity and therapists' technical competence, ensure the possibility of replication, and provide a systematic way of training and supervising therapists in specific models (Lambert & Ogles, 2004). Rather, this position paper focuses on two critical disadvantages: manuals provide an inadequate map of the psychotherapy territory, and their use does not improve the outcome of psychotherapy. Manuals emphasize specific technical operations in the face of evidence that psychotherapies demonstrate few, if any, specific effects and very little differential efficacy. Moreover, in direct contrast to the move to transfer manualized therapies to clinical settings, manuals have demonstrated little relationship to outcome, and perhaps detract from positive results. In fact, manualizing psychological

interventions as if they were independent of those administering and receiving them, does not reflect what is known about psychotherapy outcome.

Manuals and Specific Effects

The great tragedy of science—the slaying of a beautiful hypothesis by an ugly fact.

Thomas Henry Huxley, Presidential Address to the British Association for the Advancement of Science

One probable assumption that underlies the manualization of psychotherapy is that specific technical operations are largely responsible for client improvement—that active (unique) ingredients of a given approach produce different effects with different disorders. In effect, this assumption likens psychotherapy to a pill, with discernable unique ingredients that can be shown to have more potency than other active ingredients of other drugs.

There are three empirical arguments that cast doubt upon this assumption. First is the dodo bird verdict, which colorfully summarizes the robust finding that specific therapy approaches do not show specific effects or relative efficacy. In 1936, Saul Rosenzweig first invoked the dodo's words from *Alice's Adventures in Wonderland*, "Everybody has won and all must have prizes," to illustrate his observation of the equivalent success of diverse psychotherapies. Almost 40 years later, Luborsky, Singer, and Luborsky (1975) empirically validated Rosenzweig's conclusion in their now classic review of comparative clinical trials. The dodo bird verdict has since become perhaps the most replicated finding in the psychological literature, encompassing a broad array of research designs, problems, and clinical settings.

A meta-analysis, designed specifically to test the dodo bird verdict (Wampold et al., 1997), included some 277 studies conducted from 1970 to 1995. This analysis verified that no approach has reliably demonstrated superiority over any other. At most, the effect size (ES) of

treatment differences was a weak .2. “Why,” Wampold et al. ask, “[do] researchers persist in attempts to find treatment differences, when they know that these effects are small?” (p. 211). Finally, an enormous real-world study conducted by Human Affairs International of over 2000 therapists and 20,000 clients revealed no differences in outcome among thirteen approaches, including medication, as well as family therapy approaches (Brown, Dreis, & Nace, 1999).

Although Lambert and Ogles (2004) conclude that decades of research have not produced support for one superior treatment or set of techniques, Lambert (2004) suggests that some specific and superior effects can be attributed to cognitive and behavioral methods for problems of greater severity. To address the severity issue, Wampold, Mondin, Moody, and Ahn (1997) re-analyzed the 1997 data and separated out the studies addressing severe disorders. The dodo bird verdict remained the best description of the data. The preponderance of the data, therefore, indicate a lack of specific effects and refute any claim of superiority when two or more bona fide treatments fully intended to be therapeutic are compared. If there are no specific technical operations that can be reliably shown to produce a specific effect, then manualizing psychotherapy seems to make little sense.

The second argument shining a light on the empirical pitfalls of manuals emerges from estimates regarding the impact of specific technique on outcome. After an extensive, but non-statistical analysis of decades of outcome research, Lambert (1992) suggests that model/technique factors account for about 15% of outcome variance. An even smaller role for specific technical operations of various psychotherapy approaches is proposed by Wampold (2001). His meta-analysis assigns only a 13% contribution to the impact of therapy, both general and specific factors combined. Of that 13%, a mere 8% is portioned to the contribution of model effects. Of the total variance of change, only 1% can be assigned to specific technique. This

surprising low number is derived from the 1997 meta-analytic study, in which the most liberally defined effect size for treatment differences was .2—indicating that only 1% of the variance in outcomes can be attributed to specific treatment factors. A consideration of Lambert's and Wampold's estimates of variance reveals that manuals arise from factors that do not account for 85% and 99%, respectively, of the variance of outcome. Manuals, because of the limited amount of variance accounted for by specific therapist technical operations, simply do not map enough of the landscape to make them worthwhile guides to the psychotherapy territory.

Finally, component studies, which dismantle approaches to tease out unique ingredients, have similarly found little evidence to support any specific effects of therapy. A prototypic component study can be found in an investigation by Jacobson et al. (1996) of cognitive behavioral therapy (CBT) and depression. Clients were randomly assigned to (1) behavioral activation treatment, (2) behavioral activation treatment plus coping skills related to automatic thoughts, or (3) the complete cognitive treatment (the above two conditions plus identification and modification of core dysfunctional schemas). Results generally indicated no differences at termination and follow-up. Perhaps putting this issue to rest, a recent meta-analytic investigation of component studies (Ahn & Wampold, 2001) located 27 comparisons in the literature between 1970 and 1998 that tested an approach against that same approach without a specific component. The results revealed no differences. These studies have shown that it doesn't matter what component you leave out—the approach still works as well as the treatment containing all of its parts. When taken in total, comparative clinical trials, meta-analytic investigations, and component studies point in the same direction. There are no unique ingredients to therapy approaches and little empirical justification for manualizing psychotherapies for clinical use.

Manuals, Transportability, and Outcome

Seek facts and classify them and you will be the workmen of science. Conceive or accept theories and you will be their politicians.

Nicholas Maurice Arthus, De l'Anaphylaxie a l'immunité

When manualized psychotherapy is portrayed in the literature, it is easy to form the impression of technological precision. The illusion is that the manual is like a silver bullet, potent and transferable from research setting to clinical practice. Any therapist need only to load the silver bullet into any psychotherapy revolver, and shoot the psychic werewolf terrorizing the client. Some studies support this perspective. For example, Wade, Treat, and Stuart (1998) examined the “transportability” of manualized CBT for panic disorder with 110 clients in a community mental health center (CMHC). Outcomes were compared with two clinical trials of CBT for panic disorder using a benchmarking strategy. The clients who received manualized therapy in the CMHC improved on every measure comparable to the clinical trials. Confounding any direct conclusions of this study, no control group or measures of treatment integrity were used.

Other more well-controlled studies argue the opposite point. Henry and colleagues (Henry, Schacht et al., 1993; Henry, Strupp, Butler, Schacht, & Binder, 1993) found that therapist interpersonal skills were negatively correlated with the ability to learn a manual in the Vanderbilt II project, which examined the effects of training in Time limited Dynamic Psychotherapy (TLDP) for 16 therapists. These therapists provided services to two clients prior to the training, one client during training, and two clients in the year following training. The treatment was brief (25 sessions) conducted in the therapists usual fashion prior to training and according to the TLDP model following training. During the year of training, therapists participated in weekly group supervision and attended workshops teaching the manualized approach. Evaluation of the training revealed that the therapists learned the manualized protocol

(Henry, Strupp et al. 1993; Henry, Schacht et al. 1993). The extensive training, however, did not result in improved treatment outcomes. Clients prior to their therapists' manualized training were as likely to improve as those seen after training (Bein et al., 2000).

This study and others indicate that manuals can effectively train therapists in a given psychotherapy approach. Notwithstanding, the same research shows no resulting improvement in outcome and the strong possibility of untoward negative consequences (Beutler et al., 2004; Lambert & Ogles, 2004). With regard to the former, researchers Shadish, Matt, Navarro, and Phillips (2000) found non-manualized psychotherapy as effective as manualized in a meta-analysis of 90 studies. Comparing an individualized cognitive therapy to a manualized cognitive therapy, Emmelkamp, Bouman, and Blaauw (1994) found a modest, mean negative effect of manualization at treatment end and follow-up. On the other hand, Schulte, Kunzel, Pepping, and Schulte-Bahrenberg (1992) found small positive effects of manualization. Finally, a mega-analysis of 302 meta-analyses of various forms of psychotherapy and psychoeducation (Lipsey & Wilson, 1993) also revealed very similar outcomes between highly structured research treatments and those applied in naturalistic settings. The consistency of these results suggests few differences in outcome following the use manuals in clinical settings.

Regarding detrimental effects, Addis, Wade, and Hatgis (1999) showed that practitioners believe that manuals negatively impact the quality of the therapeutic relationship, unnecessarily and inadvertently curtail the scope of treatment, and decrease the likelihood of clinical innovation. Clinicians' beliefs appear well-founded: High levels of adherence to specific technical procedures interfere with the development of a good relationship (Henry, Strupp et al., 1993), and with positive outcomes (Castonguay, Goldfried, Wiser, Raue, & Hayes, 1996). In a study of 30 depressed clients, Castonguay and colleagues (1996) compared the impact of a

technique specific to cognitive therapy—the focus on correcting distorted cognitions—with two other non-specific factors: the alliance and the client’s emotional involvement with the therapist. Results revealed that while the two common factors were highly related to progress, the technique unique to cognitive-behavioral therapy—eliminating negative emotions by changing distorted cognitions—was negatively related to successful outcome. In effect, therapists who do therapy by the book develop better relationships with their manuals than with clients and seem to lose the ability to respond creatively. Little evidence, therefore, exists that manualized treatments have any impact on outcome, although there is some indication of negative effects.

Manuals and the Known Sources of Variance

Whoever acquires knowledge and does not practice it resembles him who ploughs his land and leaves it unsown.

Sa’di, Gulistan

There is a certain seductive appeal to the idea of making psychological interventions dummy proof, where the users—the client and the therapist—are basically irrelevant. This product view of therapy is perhaps the most empirically vacuous aspect of manualization because the treatment itself accounts for so little of outcome variance, while the client and the therapist—and their relationship—account for so much.

Starting with the variance attributed to the alliance—a partnership between the client and therapist to achieve the client’s goals (Bordin, 1979)—researchers repeatedly find that a positive alliance is one of the best predictors of outcome (Horvath & Symonds, 1991; Martin, Garske, & Davis, 2000). Research on the power of the alliance reflects over 1,000 findings, and counting (Orlinsky, Rønnestad, & Willutzki, 2004). For example, Krupnick et al. (1996) analyzed data from the landmark Treatment of Depression Collaborative Research Project (TDCRP) and found

that the alliance was predictive of success for all conditions—the treatment model was not. In another large study of diverse therapies for alcoholism, the alliance was also significantly predictive of success (sobriety), *even* at one year follow-up (Connors, DiClemente, Carroll, Longabaugh, & Donovan, 1997).

Based on the Horvath and Symonds (1991) meta-analysis, Wampold (2001) portions 7% of the overall variance of outcome to the alliance. Putting this into perspective, the amount of change attributable to the alliance is about seven times that of specific model or technique. As another point of comparison, in the TDCRP, mean alliance scores accounted for up to 21% of the variance, while treatment differences accounted for at most 2% of outcome variance (Wampold, 2001), over a ten-fold difference. Recognition of this disparity has led to the creation of a counterbalancing movement by the APA Division of Psychotherapy to identify elements of effective therapy relationships (Norcross, 2001).

Turning to variance attributed to the therapist, the explosion of manuals has not eliminated the influence of the individual therapist on outcomes. Treatment still varies significantly by therapist. Once again, the TDCRP offers a case in point. Blatt, Sanislow, Zurloff, and Pilkonis (1996) reanalyzed the data to determine the characteristics of effective therapists. This is a telling investigation because the TDCRP was well-controlled, used manuals, and employed a nested design in which the therapists were committed to and skilled in the treatments they delivered. A significant variation among the therapists emerged in this study, related not to the type of treatment provided or the therapist's level of experience, but rather to his or her orientation toward a psychological v. biological perspective, and longer term treatment.

There is substantial evidence of differences in effectiveness between clinicians and treatment settings (Miller, Duncan, Brown, Sorrell, & Chalk, in press; Lambert et al., 2003).

Conservative estimates indicate that between 6% (Crits-Christoph et al., 1991) and 9% (Project MATCH Research Group, 1998) of the variance in outcomes is attributable to therapist effects while treatment context accounts for up to 3-4% (Wampold, 2001). These percentages are particularly noteworthy when compared with the variability among treatments (1%).

Finally, the largest source of variance, virtually ignored by the move to manualize, is accounted for by the so-called extratherapeutic factors—those variables associated with the client, including unexplained (and error) variance. These variables are incidental to the treatment model and idiosyncratic to the specific client—factors that are part of the client and his or her environment that aid in recovery regardless of participation in therapy (Lambert, 1992). What clients bring to the process—their attributes, struggles, motivations, and social supports—accounts for 40 percent of the variance (Lambert, 1992); clients are the engine of change (Bohart & Tallman, 1999). Wampold's (2001) meta-analytic perspective assigns an 87% contribution to extratherapeutic factors and unexplained variance.

Among the client variables frequently mentioned are severity of disturbance, motivation, capacity to relate, ego strength, psychological mindedness, and the ability to identify a focal problem (Assay & Lambert, 1999). In the absence of compelling evidence for any of the specific client variables to predict outcome or account for the unexplained variance, this most potent source of variance remains largely uncharted. This suggests that the largest source of variance cannot be generalized because these factors differ with each client. These unpredictable differences can only emerge one client at a time, one alliance at a time, one therapist at a time, and one treatment at a time. Although specific treatments do not have unique ingredients, the data seem to suggest that clients do.

Manualization, neither explains nor capitalizes on the sources of variance known to effect treatment outcome. Indeed, as Wampold (2001) notes, “manuals focus attention toward a wasteland and away from the fertile ground” (p. 212). Given the data, we believe that continuing to invest precious time and resources in the development and dissemination of treatment manuals is misguided. A simpler path to effective, efficient, and accountable intervention exists. Rather than attempting to fit clients into manualized treatments via “evidence-based practice,” we recommend that therapists and systems of care tailor their work to individual clients through “practice-based evidence.”

From Evidence-Based Practice to Practice-Based Evidence

The proof of the pudding is in the eating.

Cervantes, Don Quixote

Early treatment benefit has emerged as a robust predictor of eventual outcome (e.g., Brown et al., 1999; Hansen & Lambert, 2003; Howard, Kopte, Krause, & Orlinsky, 1986). In recent years, researchers have been using data about client progress generated *during* treatment to enhance the quality and outcome of care (Howard, Moras, Brill, Martinovich, & Lutz, 1996; Lambert et al., 2001; Whipple et al., 2003). Unlike treatment manuals, such approaches actively utilize the known sources of variance in psychotherapy outcome. For example, in one representative study of 6224 clients, Miller, Duncan, Brown, Sorrell, and Chalk (in press) provided therapists with ongoing, real-time feedback regarding two potent factors affecting outcome: the client’s experience of the alliance and progress in treatment. The availability of this “practice-based evidence” not only resulted in higher retention rates but also doubled the overall effect size of services offered (baseline ES = .37 v. final phase ES = .79; $p < .001$). Germane to the controversy of treatment manuals, the findings were obtained without any attempt to control

the treatment process—clinicians were not trained in any new techniques or diagnostic procedures. Rather, they were completely free to engage their individual clients in the manner they saw fit.

Paradoxically, practice-based evidence—at least when judged on the basis of measurable improvements in outcome alone—may be the most effective evidence-based practice identified to date. Indeed, Lambert et al. (2003, p. 296) point out, “those advocating the use of empirically supported psychotherapies do so on the basis of much smaller treatment effects.” There are other advantages. For example, Miller et al. (in press) showed how practice-based evidence could be used to identify reliable differences in outcome between clinicians. Such differences, it will be recalled, account for several times *more* of the variance in outcomes than method (Wampold, 2001). Ongoing research is currently examining the ways that such information can be used to enhance training, supervision, and quality assurance. Preliminary data from one site document a slow but progressive decrease in the variability of outcomes between clinicians when they are provided with ongoing, real-time feedback regarding their effectiveness as compared to average effectiveness of the agency as a whole (Miller, Duncan, Sorrell, & Chalk, in preparation).

Conclusions: The Manual Is Not the Territory

At bottom every man knows well enough that he is a unique being, only once on this earth; and by no extraordinary chance will such a marvelously picturesque piece of diversity in unity as he is, ever be put together a second time.

Friedrich Nietzsche, Unknown

Manuals provide an empirically incorrect map of the psychotherapy terrain that sends both research and practice in the wrong direction. The assumption that specific therapist technical operations result in client change is not supported by the evidence. Although training in

manualized psychotherapies does enhance therapist learning of and technical competence in a given approach, there is no relationship between such manuals and outcome. Because of the emphasis on specific or unique ingredients, manuals ignore the known sources of variance. The manual, simply, is not the psychotherapy territory.

Manuals equate the client with a *DSM* diagnosis and the therapist with a treatment technology—both interchangeable and insignificant to the procedure at hand. Consequently, manuals lose sight of the idiographic analysis of single cases (Davison, 1998). Given the amount of variance attributed to unidentified client variables and unexplained variance, there is no way to know a priori what factors will emerge as salient for a given client-therapist pairing. Specific treatments are not unique—but clients are. From this perspective, manuals fall flat. Experienced therapists know that the work requires the tailoring of any approach to a particular client's unique circumstances. The nuances and creativity of an actual encounter flows from the moment to moment interaction of the participants—from the client, relational, and therapist idiographic mix—not from step a to step b on page 39. Monitoring the client's progress and view of the alliance—using practice-based evidence—and altering treatment accordingly, is one way to manage the complexity and wonderful uncertainty that accompanies the process of psychotherapy (Duncan, Miller, & Sparks, 2004).

Psychotherapy is not an uninhabited terrain of technical procedures. It is not the sterile, stepwise, process of surgery, nor the predictable path of diagnosis, prescription, and cure. It cannot be described without the client and therapist, co-adventurers in a journey across largely uncharted territory. The psychotherapy landscape is intensely interpersonal, and ultimately, idiographic.

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