

Psychotherapy Research: an Update on the Current State of Affairs

DURING MY SENIOR INTERNSHIP, MY PSYCHOTHERAPY SUPERVISOR OFFERED THE FOLLOWING ADVICE: “LEARN EVERYTHING YOU CAN ABOUT THE HUMAN EXPERIENCE. READ ABOUT PSYCHOPATHOLOGY, GREEK MYTHOLOGY, PSYCHOTHERAPY, AND GOOD LITERATURE. AND THEN, BEFORE YOU GO INTO YOUR SESSIONS, EMPTY ALL OF IT OUT OF YOUR HEAD AND JUST BE WITH YOUR CLIENTS.”

I can't think of advice more at odds with those who advocate a manualized, diagnosis-specific, cognitive-behavioural, evidence-based treatment. What, however, does the psychotherapy outcome research tell us about the best approach to actually helping our clients?

In answering this question, we must begin by acknowledging that the interpretation of scientific research data is far from cut-and-dried. As Stephen Jay Gould opined (Gould, 1981), “Science must be understood as a social phenomenon, a gutsy, human enterprise, not the work of robots programmed to collect pure information.” Psychotherapy researchers Norcross, Beutler, and Levant (2006) emphasize that “defining evidence, deciding what qualifies as evidence, and applying what is privileged as evidence are complicated matters with deep philosophical and huge practical consequences”. After roughly seventy years of psychotherapy research and many thousands of empirical studies, we are still far from a unanimous consensus on how best to help our clients. “What would seem to be common sense—use of evidence-based practice—is intertwined with politics and power” (Sparks and Duncan, 2010).

The recent history of the field has been dominated by advocates of “evidence-based treatments” (EBTs), who have sought to establish which specific treatments have sufficient evidence to be justified for particular diagnoses or conditions. APA's Division 12 (Clinical Psychology) established criteria by which a treatment would be judged as being empirically supported, and listed the specific treatments that were deemed to have met these criteria (Chambless & Hollon, 1998). There was considerable consternation in some quarters at what were perceived to be unilateral proclamations, made without adequate empirical evidence to support them. Other APA Divisions convened their own, competing task forces. This ultimately led to the APA convening a Presidential Task Force on Evidence-Based Practice (APA, 2006). The directives from this Task Force were in marked contrast to what had been previously advocated by Division 12. “Evidence-based practice in psychology (EBPP) is the integration of the best available research with clinical expertise in the context of patient characteristics, culture, and preferences” (p. 273). Further, “Best research evidence refers to scientific results related to intervention strategies, assessment, clinical problems, and patient populations in laboratory and field settings as well as to clinically relevant results of basic research in psychology and related fields” (p. 274).

Most psychotherapists have heard about recent articles in the popular press (e.g. Newsweek's Sharon Begley) condemning most psychotherapists as being unscientific. Those articles were based on a controversial article in the American Psychological Society's *Psychological Science in the Public Interest* (Baker, McFall & Shoham, 2008). The article is critical of psychologists for not adhering to EBTs with their clients, with a very strong advocacy of cognitive-behavioural

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therapy. The issue is not a minor one for the field, and the article advocates an alternative graduate school accreditation system supported by the APS, known as the Psychological Clinical Science Accreditation System. Both the popular press and APS articles have received harsh responses from several psychotherapy researchers. A veteran psychotherapy researcher, Bruce Wampold, protested “[the journal authors] have a very narrow view of what science should be, and if clinicians do not conform to their view of science, then they are being characterized as unscientific” (Price, 2009). How can such radically different points of view be held by people who are each psychologists, psychotherapists, and researchers? It simply depends on how one looks at the research.

Although advocates of EBTs certainly espouse a level playing field when deciding which therapies get the nod of approval, the majority of EBTs, (and arguably of EBT advocates) are cognitive-behavioural. Although I do not know if one exists, I suspect that a tally of all psychotherapy outcome studies would similarly demonstrate that a sizable majority examine the effectiveness of cognitive-behavioural treatment. It might therefore seem a reasonable conclusion that we should go with what we know works, and there is a lot of evidence that CBT works. Along that line of reasoning, however, we psychotherapists should hang up our couches, because there is a lot more research demonstrating the effectiveness of psychopharmaceuticals than of psychotherapy. Predictably, there are also many psychiatrists that now see no use for psychotherapy, given the demonstrated effectiveness of medication. We psychologists protest, of course, that sheer quantity of research is not a good way to judge effectiveness, that we need to look at relative effect sizes, and ideally use head-to-head comparison studies. We

also like to point to the fewer negative side effects of psychotherapy, and to the fact that the benefits continue after the treatment ends. Psychotherapy researchers who are not aligned to any one approach to treatment make the same arguments.

When we compare effect sizes of different approaches to psychotherapy, we

find that there are no significant differences. This applies whether we are looking at different treatments for adults with particular diagnoses (e.g. Beutler, 2009, Wampold, 2010), for youth (Kelley, Bickman & Norwood, 2010), for couples and family therapy (Sparks & Duncan, 2010), or for alcohol and drug treatment (Mee-Lee, McLellan

& Miller, 2010). While a considerable amount of effort has gone, and continues to go, into investigating what particular therapies work for what particular problems, the research data have compellingly and overwhelmingly demonstrated that this is a fruitless pursuit. “Bluntly put, the existence of specific psychological treatments for specific disorders is a myth” (Hubble et al., 2010, p.28). It has been very compelling for us to believe that particular treatments will work best for specific diagnoses and client characteristics. The research, however, does not support this. Perhaps the largest study of its kind, Project MATCH, cost \$33 million and compared cognitive-behavioural, motivational enhancement, and twelve-step facilitation therapies in a randomized clinical trial for 1,726 alcohol-abusing clients. A variety of client characteristics including personality, pattern of alcohol abuse, and severity of alcohol abuse were all examined, and of the 64 possible matching interactions tested only one proved statistically significant. At ten-year follow-up, no support for differential treatment response was found on any measure (Tonigan et. al, 2003).

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recent decades and particularly in recent years, there are others who point to evidence that is believed to demonstrate the superiority of one type of treatment over others. It is certainly conceded in the literature that some studies find larger effect sizes for one type of treatment over another. How these apparent differences are interpreted, however, is a source of dispute. We psychologists seem to have no difficulty recognizing the bias and research design flaws in studies intending to demonstrate the efficacy of psychopharmaceuticals. Despite hundreds of studies apparently demonstrating the effectiveness of antidepressant medication, we certainly seem to have embraced the latest data indicating no benefit of antidepressant medication over placebo, except for the most severe depression (Fournier et al., 2010). We recognize that the pharmaceutical industry is strongly motivated to demonstrate benefits produced by their products, and that this is often achieved through unfair comparisons, for example through studies that are not truly double-blind because both patients and physicians accurately guess whether placebo or active medication has been administered, or comparisons with medications known to have relatively weak effectiveness (Sparks et. al., 2010). We seem much less willing to subject our own favoured evidence-based treatment to the same critical analysis. The data, however, overwhelmingly point to a similar conclusion. When research is conducted by people with a strong allegiance to a particular psychotherapeutic approach, often using therapists with a similarly strong allegiance, we predictably find superiority for that approach. Similarly, superiority is often found when a particular EBT is compared to “treatments” that are not intended to be therapeutic, or are obviously set up to be inferior to the EBT (e.g. less patient contact time, poorly trained therapists, therapists who don’t believe in the “treatment” they are supposed to be

delivering, or more reactive measures used in some studies than others) (Anderson, Lunnen, & Ogles, 2010, Beutler, 2009). When these biases and research design flaws are factored out of the data, any differences between recognized psychotherapies disappear. Similarly, research involving head-to-head comparisons between different psychotherapies, in which the therapy is conducted by adherents to those psychotherapies, demonstrates no significant benefit of one approach over another. “The notion of requiring clinicians to use empirically-supported treatment or evidence-based treatments simply is not supported by the research evidence” (Wampold, 2010). CBT advocates may be taken aback by the most recent salvo (Shedler, 2010) which presents compelling evidence, from meta-analyses and other research, for the superiority of psychodynamic psychotherapy over CBT, including greater benefits at follow-up and superior benefit when something beyond symptom relief (i.e. psychological health) is the goal. “A rational weighing of the status of current evidence behooves scientists to take another, more careful look at why ESTs have failed to distinguish themselves from other treatments and to use this information in framing a broader approach to psychotherapy research” (Beutler, 2009).

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A related line of research has examined the relationship between supposedly critical components of a particular therapeutic approach and client outcome. A recent critique published in these pages, for example, questioned the validity of practicing EMDR based on deconstruction research which has demonstrated that eye movements are not necessary for the benefits of the therapy. However, these deconstruction (also called component analysis) studies also demonstrate that the cognitive and behavioural components are similarly irrelevant to the benefits of cognitive-

behavioural therapy (full disclosure: I still use CBT as part of my treatment of every client). Jacobson et al. (1996) found that removing the cognitive component from the therapy had no effect on outcome for depressed patients. Kazdin (2007), having reviewed the empirical literature on mediators and mechanisms of change, concluded that “perhaps we can state more confidently now than before that whatever may be the basis of changes with Cognitive Therapy, it does not seem to be the cognitions as originally proposed” (p. 8). The essence of CBT for PTSD involves behavioural exposure to stimuli associated with the trauma, yet when the exposure was explicitly eliminated from treatment, actively keeping the clients from thinking about or exposing themselves to anything to do with the trauma, outcomes were identical. Indeed, the results in the control group were strong enough to spawn a new approach to treating trauma that is the antithesis of CBT—present-centered therapy. Ahn and Wampold (2001) meta-analytically reviewed all research that examined the effects of removing components of psychotherapeutic treatments, and found no evidence that removing or adding a specific ingredient altered outcomes: “research designs that are able to isolate and establish the relationship between specific ingredients and outcomes should reveal how specific ingredients lead to change ... Decades of psychotherapy research have failed to find a scintilla of evidence that any specific ingredient is necessary for therapeutic change” (Wampold, 2001, p.204). Some, such as David Barlow (2004), have emphasized a distinction between “generic psychotherapy” and “psychological treatments”, which are asserted to be superior in treating a specific “disorder”, particularly anxiety disorders. The data demonstrate, however, that superior outcomes are not achieved by the supposed “psychological treatments”,

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and that the specific ingredients of these treatments are not related to patient benefit (see also Wampold, 2010, and Benish et. al, 2008, in relation to anxiety disorders).

The research data demonstrating equal effectiveness of different approaches to treatment is sometimes interpreted nihilistically, as suggesting that anything goes in psychotherapy. In reality, the data provide some very clear evidence that what goes on in psychotherapy matters very much to the outcome. It is treatments that are intended to be therapeutic, that the therapist believes in and has a compelling rationale for, that work. There are characteristics of psychotherapy that lead to poor or damaging outcomes, and characteristics that predict good outcomes. At the top of the heap, the data indicate that some therapists are consistently better than other therapists, that therapeutic relationship factors account for much of the variability in outcome attributable to therapy, and that a major way that helps therapists achieve the better outcomes is enhancing the therapeutic relationship. Hubble et al. (2010) concluded that “available evidence documents that the therapist is the most robust predictor of outcome of any factor ever studied”. Wampold (2005) concluded that the portion of outcomes attributable to differences between therapists is 8-9%, far outstripping the amount attributable to an empirically supported treatment (0-4%), to the differences between different treatments (0-1%), or even to the therapeutic alliance itself (5%). Consistent with this, psychotherapy clients do not emphasize particular techniques or methods when accounting for their improvement, but instead emphasize the relationship with their therapists (Norcross, 2010).

The fact that some therapists consistently achieve good outcomes with their clients, while some of their peers consistently achieve poor outcomes, is consistent with what is being found in other fields. For example, in education, “the most stunning finding to come out of education research in the past decade: more than any other

variable in education—more than schools or curriculum ... Parents have always worried about where to send their children to school; but the school, statistically speaking, does not matter as much as which adult stands in front of their children. Teacher quality tends to vary more within schools—even supposedly good schools—than among schools—teachers matter” (Ripley, 2010). Similarly, among political experts, some are consistently more accurate than others: “what experts think matters far less than how they think ... We are better off turning to experts who embody the intellectual traits of Isaiah Berlin’s prototypical foxes—those who “know many little things,” drawn from an eclectic array of traditions, and accept ambiguity and contradiction as inevitable features of life—than we are turning to Berlin’s hedgehogs—those who “know one big thing”, toil devotedly within one tradition, and reach for formulaic solutions to ill-defined problems” (Tetlock, 2005).

Through his bestselling books, David Burns has perhaps done more than anyone to promote a cognitive-behavioural approach in the public consciousness. Yet, as many of us know, he now advocates a “tools, not schools” approach, which strongly recommends measuring client progress, and the therapeutic alliance, in each session. The research data strongly suggests that we heed his advice. Although we have accumulated a lot of data demonstrating that some therapists are much better than others, the data also indicate that therapists are very poor at knowing where on the continuum they fall. Like other professionals, we see ourselves as being above average (Leonhardt, 2009). Client outcome is well correlated with the client’s perceptions of the therapeutic relationship, not so well with the therapist’s perceptions of the therapeutic relationship (Horvath & Bedi, 2002). Fortunately, rapidly accumulating data is showing that the use of client outcome and therapeutic alliance measures in each session has a dramatically positive impact on alliance and outcome (Lambert, 2010), and that

some of the poorest therapists have used this data to become amongst the best. This direction is also emphasized by the APA Task Force: “The application of research evidence to a given patient always involves probabilistic inferences. Therefore, ongoing monitoring of patient progress and adjustment of treatment as needed are essential to EBPP [evidence-based practice in psychology]” (APA, 2006, p 280).

This very brief review can only scratch the surface of the extensive research evidence in support of the conclusions presented here. For those interested in examining these issues more deeply, the most extensive and recent review of the literature is, “The Heart & Soul of Change: Delivering What Works in Therapy” (Duncan et al., 2010). 

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