

The Data Is Still the Thing: A Reply to Gaynor and Hayes

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I have different replies to the two responses to my recent paper (Corrigan, 2001). Thanks to Gaynor (2002) for providing single-subject evidence about Functional Analytic Psychotherapy (FAP). My goal was not to dismiss FAP or any of the other therapies as ineffective. Rather, I wanted to encourage researchers to look at the data, much as Gaynor has done here. It also seems that Gaynor and I agree that "going beyond the data in promoting and disseminating new treatments" should be a matter of concern to behavior therapists. We seem to part company in considering when specific therapies fall in this error. Gaynor seems to view behavior therapy more liberally, suggesting the dissemination of FAP before its empirical findings are obtained serves the purpose of promoting discussion and research. I have a more conservative view. Therapies and data are co-synchronous; one should not precede the other.

I have two responses to Hayes (2002). First is whether there was any utility in my literature analysis. The original paper acknowledged that this kind of meta-analysis has its limitations, as methodologists have discussed elsewhere (Wolf, 1986). Despite these limits, the paper conservatively pointed out some of the reasonable implications for which behavior therapists need to be vigilant. Hayes

focused on its limitations by noting that a review of 20 recent references generated by a library database would yield unreasonable assumptions about such important scientific concepts as relativity and biological evolution. According to his analysis, less than a quarter of the articles are empirical and so, applying my rationale, these ideas are ahead of their data. But there is a major difference in Hayes's review and my effort to track where four behavior therapies fell in the literature. Hayes examined only 20 papers about these topics that generate thousands of hits in reference databases. I reviewed all of the papers generated by the dominant databases in mental health services. Hayes's argument should not obscure what is intensely sobering about the simple findings of my earlier analysis. Behavior therapies as prominent as DBT and ACT rest on empirical studies where 56 and 11 patients, respectively, received the treatment.

Of more concern is Hayes's view that my critique represents "vita" assault. Somehow the argument turned away from the strengths and weaknesses of individual therapeutic systems to perceived attacks on the researchers who developed them. This is by no means my goal and, in fact, would be a terrible regression for behavior therapy. Prior to the rise of behavioral interventions, psy-

chotherapy was dominated by the systems and theories of charismatic figures and their devotees. We had Freudian, Jungian, Adlerian, Rogerian, Ericksonian, Perlsian, and other therapeutic systems from which practitioners might choose based on its wisdom and rationale. With the introduction of behavior therapy, the nature of evidence changed from authoritarian epistemologies to empirical method, from clinical wisdom to observable data. This metamorphosis is clearly evident within our discipline. We do not participate in Wolpean therapy, but, instead, discuss systematic desensitization. We are not Beckians but cognitive therapists. And when critics question the assumptions of systematic desensitization or cognitive therapy, they are not attacking Wolpe and Beck. They are challenging the evidence-based theory that these two researchers had a part in creating.

Behavior therapists need to continue to be vigilant about whether individual therapies get ahead of the data, whether dissemination of a treatment is prominently displayed prior to the completion of careful research.

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The Dissemination of Novel Psychotherapies: A Commentary on Corrigan, Gaynor, and Hayes

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When it comes to the evaluation of the effectiveness and the dissemination of psychotherapy, things often turn out to be far more complicated than they first appear. So it is with the provocative essay by Corrigan (2001). At first glance, Corrigan seems to make a point so obvious that all behavior therapists would readily embrace it: Our assessment and intervention methods should be tied as much as possible to science, and dissemination of these technologies should be kept in step with empirical

developments. It's hard to argue with these general principles. But like so many general principles, the devil is in the details. For example, what kind of science should underlie our technologies? How much research is enough before dissemination? And on what basis do we judge the appropriateness of specific dissemination efforts?

Corrigan's solution to these issues centers on an index composed of the number of empirical studies of an intervention in relation to the number of nonempirical

publications in which the intervention is described or discussed in the literature. Papers of the former type are held to represent the scientific support of a therapy, whereas those of the latter type are thought to represent its dissemination. The idea is that the lower the index, the less empirical support an intervention has relative to the degree of effort put into its dissemination. Using this metric, Corrigan compares four innovative therapies that have been featured in several AABT programs over the past decade: Eye Movement Desensitization and Reprocessing (EMDR; Shapiro, 1995), Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999), Dialectical Behavior Therapy (DBT; Linehan, 1993), and Functional Analytic Psychotherapy (FAP; Kohlenberg & Tsai, 1991). He notes that EMDR has been widely criticized for "getting ahead of the

data," but observes that its score on his proposed index is higher (indicating a higher percentage of empirical to nonempirical papers) relative to the other three therapies, which have not been subject to the criticism and heated controversy surrounding EMDR. On this basis, Corrigan suggests that the dissemination of novel therapies such as ACT, DBT, and FAP has proceeded beyond what is justified by the empirical literature.

Problems With Corrigan's Index

Both Gaynor (2002), speaking from the perspective of FAP, and Hayes (2002), as the founder of ACT, raise serious questions about the utility of Corrigan's index. Most fundamentally, both Gaynor and Hayes argue that the number of descriptive papers on a technique cannot serve as a proxy for the specific claims made in those papers. In order to assess the "evidential warrant" (McNally, in press) of a scientific claim, one must begin by directly examining the claim itself. It is not enough to count the number of papers that have been published on a topic, since this reveals nothing whatsoever about the nature of the claims made about it. Thus, Corrigan's index is uninformative with respect to the evidential warrant of any specific claims made on behalf of a therapy.

Gaynor (2002) makes an important distinction between dissemination to the professional (and especially research) community and dissemination directly to the public. He notes that the former is a necessary condition for independent research on a treatment, whereas the latter should be approached with greater caution until a sufficient research base has accumulated. Corrigan's (2002) discussion of dissemination misses this critical distinction. In fact, as elaborated below, the distinction between these two types of dissemination is one of the reasons EMDR has been criticized so much more than ACT, DBT, or FAP.

Hayes (2002) discusses several disturbing implications of Corrigan's index. He notes that therapies that are highly innovative, philosophically or theoretically oriented, and/or seminal are likely to be cited frequently, which will deflate the empirical index. To maintain a high index of empirical to nonempirical papers, it follows that one should do lots of treatment outcome research, but should not write about it too much. Moreover, one should hope that others likewise refrain from writing about it. Hayes points out the absurdity of this position by illustrating that the literature on several well-established scientific topics (e.g., the theory of relativity, the speed of light) is composed of much higher percentages of nonempir-

ical articles than is the literature on any of the psychotherapies discussed by Corrigan. This is a natural consequence of a highly influential scientific theory, principle, or application.

Hayes (2002) also criticizes Corrigan's (2002) exclusive reliance on randomized clinical trials (RCTs) as the index of empirical support for an intervention. Hayes obviously places greater importance on the basic scientific research underlying a technology and the link between that research and its applications than does Corrigan, who emphasizes that the question of a treatment's efficacy and the validity of the theory underlying it are distinct issues. Reasonable scholars can disagree on this point. Hayes notes that one of the unique features in the history of behavior therapy is the link between basic and applied research, and that this link is completely ignored by Corrigan's index. Corrigan writes, "it is an error in logic to assume that the body of data underlying an intervention supports efficacy claims of that intervention" (p. 191). Although this difference in emphasis is well within the bounds of scholarly debate, Corrigan carries his admiration of RCTs too far when he suggests that the proponents of ACT and FAP (among others) "have posed some eloquent arguments against the scientific method" (p. 192). There are two serious problems with this statement. First, it naïvely equates RCTs with "science." Second, it suggests that the proponents of DBT, ACT, and FAP flatly reject RCTs. Both Gaynor and Hayes clearly disavow this characterization on behalf of FAP and ACT, and my understanding is that the same could be said of the developer of DBT. An appreciation of the important role of basic theoretical research, well-controlled single-case designs, and even certain quasi-experimental designs does not diminish the critical importance of RCTs. Despite their importance, one problem with relying exclusively on RCTs to identify empirically supported treatments is that a new RCT would be required for every minor technical innovation, thereby slowing down the field enormously. Along with certain critical RCTs, grounding treatments in basic and analogue research is likely to lead to more efficient development of empirically supported technologies than an endless series of RCTs.

Corrigan's Reply

Corrigan (2002) offers distinct replies to Gaynor and to Hayes. To Gaynor, he reasserts his view that dissemination of novel therapies such as FAP should await further data. But he does not clarify how much data would be enough, nor does he

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address Gaynor's important distinction between different kinds of dissemination.

Corrigan takes issue with Hayes's review of established scientific topics on the grounds that Hayes examined only a representative sample of the literature on these topics, whereas Corrigan reviewed all of the available papers in his review and in the calculation of his indices. It is not at all clear, however, why this should matter. Corrigan's implication is that had Hayes examined every available paper on, say, biological evolution (a truly Herculean task!), a far greater number of empirical papers relative to descriptive papers would have been located, and the index would have been quite different. This conclusion is inconsistent with basic sampling theory, and Corrigan offers no explanation for why an exhaustive literature search would make a difference. Although it might be argued that Hayes's sample was not truly random, there is no reason to expect that it was biased in favor of nonempirical publications. Corrigan's reply does not address Hayes's other concerns with his proposed index.

Corrigan does make it clear that his concerns about the dissemination of novel therapies should not be construed as ad hominem attacks on the developers of these therapies: "Somehow the argument turned away from the strengths and weaknesses of individual therapeutic systems to perceived attacks on the researchers who developed them" (p. 140). Interestingly, I did not perceive ad hominem arguments in either Corrigan's original essay (2001), or in Gaynor's (2002) or Hayes's (2002) responses. Although the debate was spirited, it did not strike me as personal. I suspect that Corrigan's perception might have resulted from the title of Hayes's essay ("On Being Visited by the Vita Police"), which might be taken to suggest a personal assault on the work of an individual researcher.

Finally, in his reply Corrigan (2002) seems to move away from his proposed index in favor of the absolute number of subjects treated:

Hayes's argument should not obscure what is intensely sobering about the simple findings of my earlier analysis. Behavior therapies as prominent as DBT and ACT rest on empirical studies where 56 and 11 patients, respectively, received the treatment. (p. 140)

Setting aside, for the sake of argument, the accuracy of these numbers, this raises a different issue. In the original essay, Corrigan (2001) was clear that his concern was with the ratio of empirical to descriptive papers, rather than the raw number of empirical studies, and it was this ratio that both Gaynor and Hayes criticized. In his

reply, Corrigan (2002) moves away from the ratio in favor of the absolute number of participants treated in controlled studies. As discussed above, one problem with this approach is Corrigan's equating "empirical studies" exclusively with RCTs—an idea foreign to behavior therapy. Moreover, Corrigan offers no guidelines as to how many participants should be treated, and how successful that treatment should be, before dissemination takes place.

Other Problems With Counting Studies

There are two additional related problems with Corrigan's proposed metric that neither Gaynor nor Hayes discussed directly. The number of empirical studies supporting a treatment reveals nothing about the methodological strength of those studies, nor the results obtained. A hundred studies of a new intervention may appear impressive, but if they are all seriously flawed they will be much less informative than a handful of well-controlled studies. Moreover, simply counting studies is not informative with respect to the direction or magnitude of the results. Issues of effect sizes, clinical significance, and external validity are ignored. Any serious review of a body of empirical research must address these issues.

So What About EMDR?

By contrasting ACT, DBT, and FAP on the one hand with EMDR on the other, one senses a slightly subversive theme to Corrigan's paper. ACT, DBT, and FAP have not been especially controversial to date, whereas EMDR has attracted widespread criticism within the scientific community, and yet, according to Corrigan's index, EMDR seems to fare better. Although Corrigan states that he is not rendering judgments on the efficacy of any individual therapy, the clear implication is that many of the concerns that have been raised about EMDR should be applied equally, or even more forcefully, to ACT, DBT, and FAP. So why has EMDR borne the brunt of scientific criticism? Are the critics somehow biased against EMDR relative to other innovative therapies?

A critique of the scientific status of EMDR is beyond the scope of this essay; the interested reader is referred to recent reviews by Cahill, Carrigan, and Frueh (1999), Davidson and Parker (2001), Herbert et al. (2000), Lohr, Tolin, and Lilienfeld (1998), McNally (1999), and Rosen, Lohr, McNally, and Herbert (1998). Nevertheless, contrasting EMDR with the other novel therapies Corrigan discussed illustrates the limitations of his proposed index as a grounds for judging

dissemination efforts, and also clarifies why EMDR has been targeted for criticism more than ACT, DBT, or FAP. EMDR differs from the other three therapies in many ways. Of specific relevance to Corrigan's analysis are four issues: (a) the nature of the specific claims made about each therapy, (b) the way in which proponents of each therapy deal with unsupportive data, (c) the scientific status of the theories underlying the therapies, and (d) the specific nature of dissemination efforts.

Claims

The developers of ACT, DBT, and FAP have been relatively modest and conservative in their claims regarding their respective therapies. The quotations offered by both Gaynor (2002) and Hayes (2002) from the texts on FAP and ACT, respectively, illustrate this reserve, and Hayes explicitly describes the empirical status of ACT as "positive but preliminary." This conservative tone has been clearly reflected in statements made at symposia and workshops on ACT, DBT, and FAP at AABT meetings. In contrast, the claims made about EMDR are truly mind-boggling. Promotional brochures published by the EMDR Institute, Inc., feature testimonials describing EMDR as uniquely efficient and effective for trauma-related problems. Workshops are offered describing the application of EMDR to a bewildering array of issues, including traumatic memories, depression, smoking cessation, somatic disorders, menopause, sexual harassment, and "inner child" syndromes, among others. Francine Shapiro, the developer of EMDR, describes the technique as a "paradigm shift" that integrates all of the major schools of psychotherapy (1995). It has been described in the popular press as a "miracle cure" (Stone, 1994).

Unsupportive Data

Science advances through criticism. When data consistently fail to support one's hypotheses, the appropriate course of action is to modify or eventually even abandon the theoretical position from which the hypotheses were derived. The limited research on ACT, DBT, and FAP has not yet yielded data that directly contradict important hypotheses associated with each approach. It is therefore impossible to conclude at this time how their proponents would respond in the face of contradictory data. Nevertheless, it is important to note that each of these therapies is associated with potentially falsifiable hypotheses. For example, Hayes and colleagues (1999) hypothesize that ACT tends to impact the believability of dis-

travelling thoughts, primarily, and their frequency, secondarily, whereas traditional cognitive therapy typically shows the opposite pattern. This hypothesis is testable. If data consistently failed to support it, we would have to see how Hayes responded. In the case of EMDR, data have in fact consistently failed to support central tenets. For example, a body of research has shown that EMDR is no more effective than other established therapies for trauma-related conditions and other anxiety disorders (and may even be less effective, especially over the long term), and that the defining feature of the intervention—eye movements—does not add to the effectiveness of the technique. Yet the proponents of EMDR cling to precisely these hypotheses, in spite of the data (Lohr, Lilienfeld, Tolin, & Herbert, 1999; Rosen, 1999).

Theory

Related to the issue of the role of criticism in advancing science is the scientific status of the theory underlying a psychotherapy. ACT, FAP, and DBT are all based on ideas that are plausible, testable, and consistent with a body of basic psychological research. ACT in particular is closely tied to a body of basic theoretical work that encompasses scores of experimental studies (Hayes, Barnes-Holmes, & Roche, 2001). EMDR, in contrast, is based on a theory that is implausible, overdetermined to the point of being difficult if not impossible to falsify, and poorly linked with basic psychological science (Herbert et al., 2000; McNally, 1996; O'Donohue & Thorp, 1996).

Dissemination

Finally, as Gaynor discussed, not all dissemination efforts are alike. Dissemination of an intervention directly to the public, particularly if associated with claims of unique efficacy, should require a high threshold of evidence. In contrast, dissemination to a professional scientific audience is a prerequisite to the generation of just such a research base. (It should be acknowledged, of course, that not all audiences fit neatly into these two categories.) The books and workshops that have been written on ACT, DBT, and FAP are clearly intended for professional audiences. Although proponents of EMDR have written a few scholarly texts (e.g., Lipke, 2000; Shapiro, 1995), far more books have been written directly targeting the general public, with titles such as *EMDR: The Breakthrough Therapy for Overcoming Anxiety, Stress, and Trauma* (Shapiro & Forrest, 2001) and *Emotional Healing at Warp Speed: The Power of EMDR* (Grand, 2001). In addition, Web sites

designed for the public extol the many virtues of EMDR (e.g., www.emdr.com; www.emdria.org; www.emdr-europe.net).

The point of this discussion is that EMDR differs systematically from the other innovative therapies discussed by Corrigan (2001) in several ways that are not captured by his index. Moreover, examination of these differences reveals the source of some of the criticisms of EMDR, and the reasons why ACT, DBT, and FAP have (so far, at least) escaped such criticism. Corrigan's use of EMDR as a foil against which to make a point about the dissemination of novel therapies does not address the many ways in which EMDR differs from the other innovative therapies he discusses.

Conclusion

Corrigan raises a very important (and seemingly uncontroversial) point about not letting dissemination efforts outstrip relevant data. When the discussion becomes more specific, however, things quickly become messy. The metric he proposes to reflect the appropriateness of dissemination efforts relative to the empirical research is highly misleading. The index ignores many critical issues that must be considered in evaluating the dissemination of novel therapies, including (a) the specific claims made about a treatment, (b) the methodological status of the studies that comprise the literature, (c) the actual nature of the results of those studies, (d) the audience to whom the dissemination efforts are directed, and (e) the status of empirical and theoretical work that supports the therapy other than RCTs. Furthermore, Corrigan's use of EMDR as a standard against which to compare other novel therapies ignores the basis of much of the controversy surrounding EMDR.

The appropriateness of various dissemination efforts in relation to the empirical status of novel therapies is an important topic, and Corrigan should be commended for raising it. The issues are complicated, however, and defy simple formulae. Perhaps we can all agree on at least two general points. First, we need more data on innovative behavior therapies. And second, the proponents of new therapies should exercise caution in keeping their specific claims consistent with the empirical literature. Operationalizing these apparently obvious principles will not be as straightforward as it might appear, and will require further healthy discussion and debate.

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Open Forum

Treatment Outcome Assessment Practices of Psychology Training Clinics

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Provider accountability and the empirical validation of treatment procedures are major trends in contemporary mental health care that encourage efforts to assess psychotherapy outcome. However, in the context of training the next generation of service providers, such interests must compete with other concerns. For example, does the prospect of treatment outcome measurement distract neophyte clinicians from client issues or contribute to excessively elevated anxiety levels?

Whether to routinely conduct formal assessments of psychology training clinic treatment outcomes is a question with a lengthy history. The majority of articles that address the question (e.g., Halgin, 1986; Todd, Jacobus, & Boland, 1992) have strongly advocated for the practice. Serifica and Harway (1980) note that support for assessment of training clinic treatment outcomes can be found as early as

1896 in a proposal to the American Psychological Association (APA) by Lightner Witmer, the founder of the first psychological clinic. Advocates for the practice have argued that routine collection of formal treatment outcome data represents a core value of the scientist-practitioner model (Halgin; Todd et al.). Others have similarly noted that routine assessment of clinical outcomes nicely models the integration of research and practice (Messer & Boals, 1981; Serifica & Harway). The potential benefits of formal treatment outcome assessments (FTOAs) in providing both heuristic feedback to trainees and supervisors and quality control of training clinic enterprises could also be cited. Recent efforts at Penn State University (Borkovec, 2001) to develop a national practice research network (PRN) include efforts toward an outcome database from university training clinics. Members of the Counsel of University

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