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ABSTRACT

An emphasis on therapy research and empirically supported treatments (ESTs) is currently suggested as the answer to many therapists' and counselors' problems with behavioral healthcare. Those eager to protect psychologists' share of behavioral care in an era of managed care are utilizing ESTs and process research to demonstrate the effectiveness of their clinical interventions. At an increasing rate, therapists are being told that their future in delivering behavioral healthcare depends upon their ability to apply the knowledge gained from several decades of psychotherapy research. In trying to prepare students for their professional lives as counselors in an era dominated by managed care, counseling education programs are increasingly emphasizing the treatment techniques supported by empirical research. This paper reviews the limitations of the extant efficacy research. It discusses the challenges of providing managed care psychotherapy and the limitations of ESTs. It debates the issue of research therapy versus clinical therapy and warns against an oversimplification of treatment standards. In conclusion, the paper suggests that integrating the findings from naturalistic studies can foster more accurate understanding of the everyday challenges confronting clinicians in the field. (Contains 24 references.) (Author/JDM)

Challenges Confronting Counseling Education Programs:

Is therapy research truly representative?

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Abstract

Emphasis on therapy research and empirically supported treatments (ESTs) is currently being touted as the answer to many therapists' and counselors' in the current behavioral healthcare marketplace. Utilizing treatment outcome and process research to demonstrate the effectiveness of clinical interventions is being encouraged by those eager to protect psychologists' share of behavioral healthcare in the managed care era. Far more so than was previously the case, therapists are being told that their future in successfully delivering behavioral healthcare depends upon their ability to apply the knowledge gained from several decades of psychotherapy research. In trying to prepare students for their professional lives as counselors and therapists in a managed care dominated world, counseling education programs increasingly emphasize the treatment techniques supported by empirical research. This paper reviews the limitations of the extant efficacy research.

The Challenges of Providing Managed Care Psychotherapy

Generally, psychotherapy outcome research is consistent with the possibility of providing effective therapy within a managed care framework that emphasizes brief, circumscribed therapy. However, wildly varying utilization and quality control practices subsumed by the label "managed care" make it challenging to generalize about new practice requirements.

Nonetheless, providers have been encouraged to prepare themselves by mastering certain essential skills. Competence in delivering ESTs (Division 12 Task Force on Promotion and Dissemination of Psychological Procedures, 1995; Chambless et al., 1996) for specific disorders is being encouraged by training programs, continuing education workshops, and professional

organizations. However, before it is assumed that students should learn only the empirically validated therapeutic approaches, it is important to examine the limitations of the current empirical research and to consider how these limitations constrain the generalizability of many of the available therapy experiments.

Limitations of ESTs

While the author strongly supports the growing emphasis on empirical justification of clinical practices, it is also important not to overlook the limited representativeness of the randomized control trial (RCT) paradigm that provides the basis for the current listing of ESTs. Efficacy studies often generalize poorly to real life therapeutic situations, yet they are increasingly used to constrain and curtail real life provision of treatment. In an indictment of the current emphasis on RCTs over more naturalistic studies, Seligman and Levant (1998) argue that "...efficacy studies can by their very nature "validate" only brief, simple, and inexpensive treatment. Efficacy studies cannot test longer and more complicated modalities much less "validate" them...Efficacy researchers have become the unwitting vehicle for short-changing patients in need of more than brief therapy -- on a massive scale."(p.212)

This article summarizes some of the major limitations of efficacy studies. It also explores how these problems inherently limit our ability to rely exclusively on ESTs as a means of operationalizing best clinical practices.

The Irony of Psychotherapy Outcome Research: The "Best" Controlled Research is Often Least Representative

Debate about the generalizability of therapy research has a long history, beginning with questions about the usefulness of therapy analogue research, in which volunteers who would not normally seek professional help are placed in therapy-like situations. In comparison with actual clinical contexts, analogue studies generally address less intense and disruptive problems, among patients who are less disturbed, using therapists who are less experienced (Kazdin & Wilcoxon, 1976; Kazdin, 1978, 1986; Marks, 1978; Rosen, 1975).

Analogue research permits experimental control of variables, but its artificiality limits its findings' real world relevance. Similarly, many have challenged the generalizability of clinical efficacy studies due to their lack of realism, even though they do use actual patient populations. Crits-Christoph (1992) argued that the generalizability of outcome results from studies where therapists adhered to fairly rigid protocols to the efficacy of eclectic psychotherapy as it is ordinarily practiced is not clear. Seligman and Levant (1998) share this concern, citing discrepancies between the conclusions of efficacy and effectiveness studies.

As Shadish et al. (1997) noted, in therapy experiments there is inevitably a tradeoff between features that enhance internal

validity and those that optimize external validity. "Features of studies that facilitate optimal causal inference (e.g., a population that will accept random assignment) threaten generalization (e.g., patients willing to be randomly assigned may differ from the population of interest)." (p.362) Understanding the distinctions between treatments as delivered in typical controlled efficacy studies and those provided "in the field" can improve our ability to apply these findings appropriately.

Research Therapy vs Clinic Therapy

The differences between "research" and "clinic" therapies emphasized by Weisz, Weiss, and Donenberg (1992) are: (1) In research therapy, participants are actively recruited by the experimenter, while in clinic therapy they are self-referred or referred by others; (2) in research therapy, patients are generally more homogenous than the diverse array of clients treated in clinic therapy; (3) in research therapy, treatment usually addresses one main problem, while clinic therapy typically addresses clients with a blend of difficulties; (4) in research therapy, therapists are usually recently trained in the specific procedure being investigated, which typically does not occur in clinic therapy; (5) in research therapy, the therapist is instructed to exclusively use the protocol under study, while in clinic therapy the therapist is not similarly constrained; and (6) in research therapy, a treatment manual is often used and treatment implementation is monitored, which rarely happens in clinic therapy.

In addition to these distinctions, other aspects of therapy as assessed in efficacy studies may limit our confidence in their applicability to actual practice. The following section explores several other concerns about psychotherapy outcome research.

1. The informed consent procedures required in research may introduce doubts about the treatment that have no direct parallel in real world settings. Typically, informed consent forms alert the patient to the possibility that they are not receiving the "real" treatment, which could be distracting or could detract from the quality of the therapeutic alliance and the patient's faith in their therapist. This could reduce the impact of treatments studied in these ways.
2. The process of random assignment to treatments does not represent the process through which patients usually select a therapist. Actual patients in psychotherapy often get there by actively "shopping", rather than being randomly assigned to a particular therapist or treatment condition. Patients are often free to choose from among a variety of therapists offering different styles of treatment. If the match between therapist personality and patient personality is important for treatment success, the use of random assignment may universally deflate the observed impact of psychotherapy. The credibility of a treatment is important in establishing the patient's expectancy of

improvement (a known contributor to patient progress). When patients are free to choose their treatment, it seems likely that the method has greater credence (if for no other reason than cognitive dissonance: "I wouldn't have picked this therapy if it weren't the best for me"). The Hester et al. (1990) study of alcoholism treatment provided evidence that compatibility of a treatment approach with the client's beliefs before entering therapy can heighten therapy success.

However, in actual practice, most patients are only modestly informed consumers. Many never carefully research their therapists before signing on. In addition, their freedom to choose is increasingly being limited by managed care policies. Therefore, this distinction may be of limited relevance.

3. Extraneous confounds are a problem in many outcome studies, despite randomization procedures. Because of the multitude of uncontrolled variables that potentially influence outcome, the sample size of any particular study is never sufficient to ensure that even with random assignment groups will end up equivalent with regard to all possible confounds. Therefore, even when statistically significant outcome differences between treatment groups are obtained, we often cannot be sure that these differences are due to the treatment variables under study, rather than to inadvertently confounded factors.

Various patient factors can compromise the unambiguous interpretation of clinical studies. In a placebo-control study on the drug Clofibrate (Coronary Drug Project Research Group, 1980), used to treat coronary heart disease, patient compliance proved the best predictor of longevity. Patients who took medication as directed at least 80% of the time had a lower mortality rate than those who were less compliant (15% versus 25% mortality within the five years following initiation of drug treatment), independent of whether the patients were taking the active medication or placebo. This illustrates the need to hold such patient factors constant across experimental and control groups, but often they are not even assessed.

Selective attrition is an especially common and vexing source of confounding in outcome research. In conducting research with patients, it is virtually impossible to avoid missing data, because patients routinely fail to provide complete information and often fail to complete treatment regimens. Missing data compromise random assignment, reducing the controlled clinical trial to the status of a flawed, quasi-experiment.

Not all researchers seem to appreciate the seriousness of this problem, and report data from studies with substantial differential drop-out across treatment conditions as if this were not germane to the question of relative treatment effectiveness. Differential drop-out should temper our conclusions about particular treatment approaches; the best treatment in the world is of little value if no one is willing to use it. This, in fact, is the major hurdle that many pharmacotherapy methods face; noncompliance rates for psychotropic medications are very high, making even pharmacologically effective drugs a very imperfect

solution.

Selective attrition can skew the average level of patient functioning in some therapy conditions, giving them an unfair advantage. Other researchers seem to overlook how excluding certain cases could inflate the apparent success of a treatment. For example, Birmaher, Holder, Johnson and Kolko (1996) advocate methods they used with adolescents, just mentioning in passing that in addition to a 9% drop out rate, "22% [of the subjects] were removed from the study because they failed to improve or deteriorated." (p.2)

4. Many debate the appropriateness of particular control group conditions. The use of credible, active placebos in double-blind studies (in which a new treatment is compared to a persuasive placebo, and both the patient and doctor are "blind" to which is being administered) is ideal, but rare. Often clinicians are asked to rate their own patients' progress, obviously aware of the treatment received. Waiting-list and delayed treatment control groups provide baseline data about spontaneous remission and the effectiveness of informal methods of problem resolution, but fail to speak to the specific advantages of one particular mode of intervention over another. Use of credible, active placebo treatments is needed to assure that participants are comparably motivated across treatment conditions, because client expectancy for improvement is known to influence outcome.

5. While in research patients are prescreened for one pure diagnosis and excluded if complicating, high-risk factors are present, in clinic therapy, patients are not excluded if they fail to be ideal candidates for the treatment being contemplated. In real practice, the therapist is obliged to seek the sequence of interventions that will reach even the most difficult patient. Actual patients present multiple therapeutic challenges; many clinicians believe that the majority of their clients qualify for Axis II codiagnoses.

Often, the inclusion/exclusion criteria of a study make it difficult for the practitioner to know the extent to which a particular patient in his or her practice resembles a patient who would have qualified for the study. Since few practitioners use structured diagnostic interviews or psychometric instruments to evaluate their patients, a study based on carefully diagnosed patients will have limited generalizability to situations in which diagnoses are not made reliably (Sperry et al., 1996)

6. Research therapy focuses on experimenter-defined outcomes (usually specific symptom reduction), while real therapy emphasizes patient-determined objectives. Non-research psychotherapy is usually aimed at improvement in general functioning, and attainment of some global goals the patient values, rather than reduction of delimited symptoms. Removal of a specific symptom may constitute complete success to the researcher but fail to satisfy the typical psychotherapy consumer.

7. Non-research therapy is not usually of a predetermined, scripted, fixed duration and format (even with managed care,

changes in plan are possible), and is expected to be self-correcting; if one method isn't working, another is quickly tried and assessed. Much of the talent of skilled therapists lies in their capacity to individualize the treatment process, flexibly drawing upon various methods that they tailor to the specific and shifting needs of an individual patient. Real therapists are responsible for avoiding untoward reactions, often without the reassurance of a "safety net", and luckily are not constrained by rigid protocols in so doing.

Standardization of treatment creates artificialities that limit the generalizability of research findings to actual clinical settings. Drug trials allow extrapolations about how patients will generally respond to a proffered treatment, because delivery of a particular dosage of medication varies little across practitioners. In contrast, psychotherapists' personalities define the quality of their treatments; the effectiveness of an interpersonal, educational psychotherapy intervention can be greatly influenced by its manner of presentation. Standardizing treatment in order to reduce this inter-clinician variability can create a stilted process very unlike that which occurs in actual therapy. Reducing the spontaneity of clinicians' responses to an inherently unpredictable process that demands flexible, spontaneous responding probably reduces the power of the treatment process. More structured treatment methods may fare better in these comparative studies in part because less of these methods gets lost in the translation. Attempts to standardize less rigidly structured treatment methods may create a more awkward situation for therapists; the resulting approach may seem more confusing and tentative to clients. This awkwardness may detract from these treatments' measured effectiveness, leading us to underestimate their actual impact when delivered in vivo. In actual practice, therapists familiar with these less rigidly structured techniques may present them with a confidence and flair that is absent in the controlled investigations. Adherence to constraining therapy manuals may therefore make the research version of many therapies less effective than the real thing.

8. Individual therapist effects are confounded with treatment effects, because although patients may be randomly assigned to treatment conditions, therapists rarely are. Consequently, outcome findings reflect therapist-by-treatment interactions, however therapist characteristics are usually neither well controlled nor well described. As typically implemented, outcome research design does not permit the attribution of outcome findings to treatment effects alone.

When employed, the strategy of using therapists as their own controls across different therapy modalities also limits external validity, because this practice assures that many therapies in research studies are being delivered by less than fully enthusiastic proponents. This could dilute the observed effectiveness of all therapies across the board, assuming that therapists' loyalties are evenly distributed across the different

schools of therapy.

9. Bias caused by the researcher's commitment to a particular therapeutic approach may also distort findings. Reviews have found that therapies preferred by the investigators tend to yield better outcomes than other treatment modalities being used for comparison (Robinson et al., 1990). More time and energy may be lavished on perfecting the favored treatment method, and training therapists in its use. Frequently the outcome measures used are indirect and subject to experimenter bias. Self reports from clients and therapists' ratings of overall improvement are susceptible to contamination, yet the majority of outcome studies use such measures.

10. Use of inexperienced therapists also plagues many studies, further limiting their generalizability. Arguably, such studies discern the best treatment methods for use by inexperienced therapists. They do not really provide answers about which therapeutic techniques afford the greatest potential for mobilizing change when practiced after years of experience.

Treatments don't work by themselves. Those pushing for universal application of ESTs seem to overlook therapist factors, which seem to be most important in more complex, difficult cases. Particular treatments are not "effective" in and of themselves. They have the potential to be effective when provided sensitively by competent practitioners, who objectively monitor the effects of their work, and revise their approach as feedback indicates. Many believe this "art" component of clinical practice will continue to be important.

The psychotherapy process literature contains many examples showing the difficulty in training even experienced therapists to practice a given therapeutic approach correctly. Adherence to technique or treatment fidelity is a problem that has marred many efficacy investigations. For those more experienced with traditional treatment, conducted over a long period of time, the switch to working with highly focused treatment plans and ESTs is often very difficult.

Research may also underestimate actual therapy's effectiveness because the closely monitored, supervised therapy being provided in many efficacy studies may be marred by the therapist's elevated self consciousness. Whose best treatment moments occur during videotaped training sessions? Many therapists find that close supervision can be distracting for them and detract from their clinical effectiveness. Some even rationalize that doing more poorly during episodes of closely monitored therapy may actually be a sign of a good therapist; anyone immune to this interference in these situations would probably have to be so insensitive to evaluation apprehension that they would be unable to empathize sufficiently with their anxious patients!

11. Various measurement problems probably limit the meaningfulness of outcome research. Use of unreliable, insensitive, or irrelevant outcome measures potentially acts to underestimate treatment effect sizes. On the other hand,

regression effects may produce overestimates of therapy's effectiveness. Since there tends to be movement from improbable states to probable states, if clients begin treatment in extreme distress, the odds are that over time they will show lower levels of distress, and that the worst clients will show the most gains from treatment. When selective attrition unbalances different treatment groups in terms of their initial symptom severity, it is virtually impossible to rule out the contaminating influence of regression effects.

The criterion used universally to assess the efficacy of therapy has been a reduction or cessation of symptoms in the patient. Symptomatic improvement can be discerned through behavioral indicators, patients' self-reported experience, therapists' rating, or assessment of psychological functioning by "blind" observers. Ratings by members of the patient's home environment have been used in studies evaluating treatments for children or hospitalized adults, but only rarely in cases where psychological treatments are applied to outpatient adult samples. While it might be very valuable to get objective information about the patient's functioning at work or in their home community, the requirement of confidentiality precludes the use of such potentially disruptive measures in most cases.

Speed of improvement, unpleasant side effects, and emotional and financial costs should also be considered when comparing psychotherapeutic treatments. Research should focus on efficiency rather than just effectiveness. Howard et al. (1997) recommend future effect size calculations that take into account cost (e.g., rate of improvement) as well as amount of benefits.

12. Patient and therapist expectations may taint many of our estimates of outcome. Interactions between treatment features and outcome measures can introduce considerable distortion into the treatment comparison process. Treatments that foster clearer demand characteristics may appear superior if the dependent measures transparently assess the types of changes the therapist has been explicitly advocating during treatment. Certain treatments may, in effect, give patients the right answers, inflating the treatments' apparent efficacy, independent of the treatments' actual impact on the ensuing quality of the patient's life. For example, when a cognitive therapist indoctrinates the patient in how to think more rationally, and then a researcher inquires about the patient's habits of thought, the possibility exists that the patient is giving the answers they have been taught, without necessarily successfully deploying this new cognitive style on a regular basis.

13. Research often focuses on statistical rather than clinical significance. Statistical significance is a joint function of effect size and sample size; when samples are large enough, trivial group differences can emerge as statistically noteworthy. For example, in comparing two weight loss treatments, a difference between groups of one ounce per month would be practically irrelevant, but might be sufficiently improbable to pass our statistical tests of significance. The statistical

conclusion conveys little or nothing about clinical or "real world" significance. Statistical significance indicates merely that it is likely that an observed trend did not arise on a chance basis.

According to Trull, Nietzel and Main (1988), psychotherapy effectiveness has traditionally been assessed through studies that tend to neglect the issue of clinical significance, or the extent to which clinical outcomes represent a meaningful magnitude of change (Nietzel, Russell, Hemmings, & Gretter, 1987). Clinical significance involves the extent to which therapy has made a desired and meaningful difference in patients' lives.

Those who confuse practical, clinical significance with statistical significance can easily be misled by outcome research. It makes little sense to champion a "statistically superior" treatment if its advantage for the patient is inconsequential (for example, an average score that is 1 point lower on a 100 point self report scale). Jacobson (1995) found that when the results of treatment studies are examined in terms of their clinical significance, the conclusions can be disturbing. In the NIMH TDCRP study (Elkin et al., 1989), the proportion of clients who completed the 12-week, 20-session treatment, recovered from their depressive episode, and stayed nondepressed for 18 months ranged from 19% to 32% across the three active treatments (imipramine, cognitive therapy and interpersonal psychotherapy). Only a minority of patients recovered and stayed recovered for more than a year, and the placebo treatment was comparably effective (20%). Nothing produced lasting recovery for the majority of cases.

Jacobson points out that "the TDCRP is widely considered to have achieved the highest degree of methodological rigor of any large-scale outcome study yet conducted, and thus has produced results that are more believable than those from many other trials of dubious design quality. These findings are not atypical, either for major depression or for other mental health problems. In a series of studies of clinical significance, our research group has examined conduct disorders in adolescents, couples seeking therapy for marital distress, and people with anxiety disorders. We have found the recovered patient (the one who shows few or no signs of symptoms of the initial complaint and believes him- or herself to be "cured") to be the exception rather than the rule for every type of disorder examined and for every type of therapy that we have looked at- psychodynamic, behavioral, cognitive and family therapy. When one considers even more intractable problems, such as addictive behaviors, schizophrenia and personality disorders, the clinical significance data are even more bleak. The only exception we have found thus far to these modest recovery rates is the cognitive behavioral treatment of panic disorder, developed by David Clark at Oxford University and David Barlow at the State University of New York in Albany." (Jacobson, 1995, p.37)

This does not mean that psychotherapy never produces recovery or that therapies are not differentially effective. It

simply indicates that psychotherapy generally yields rather modest recovery rates. There is no particular treatment modality that is uniquely subject to this criticism; most therapies examined seem somewhat deficient in terms of clinical significance.

14. A final problem in applying efficacy research involves the leap clinicians must make from normative findings to their individual work with individual therapy cases. What is true in general may not necessarily be true in particular cases. One of the first things every student of statistics is taught is that you can't generalize from group findings to individual cases if there is substantial within-group variability. Men may on average be taller than women, but this does not hold true in all cases. Yet as practitioner-scientists therapists are routinely asked to draw upon normative findings in shaping treatment for individual cases.

Some argue that global assertions about psychotherapy's efficacy are meaningless, because even though research shows that psychotherapy on average has a positive effect, we cannot infer from it that a particular treatment will work for a particular patient treated by a particular therapist. According to Robyn Dawes (1994), it is important to remember that normative data on psychotherapy's general effectiveness does not offer proof of its effectiveness in every individual case. As Dawes puts it "success in therapy is far from assured, even though it works overall in a statistical sense. Someone who is dissatisfied with their current progress in therapy should not be inhibited about changing therapists or mode of treatment. (The therapist that is abandoned may attribute this decision to the depth of the client's pathology, but so what.)...Statements from professionals that they "know" much better than the client what is "needed" may often best be politely ignored--especially when these statements are made after minimal contact, followed by a standard diagnostic label. If verbal therapy is sought, find someone empathetic. Unfortunately, I have no good advice about how to judge whether someone is empathetic before getting to know that person." (p.73-74)

High variability within treatment groups makes it difficult to know how to apply research findings in individual cases. Data from efficacy studies shows considerable variation among patients assigned to the same group, indicating how differently individual patients respond to the same treatment. When within-group variation exceeds measurement error, there are reliable differences among patients within treatments. Effect sizes are seldom large enough to eliminate overlap between the outcomes of patients in the treatment groups being compared (Howard, Krause, & Vessey, 1994). Accordingly, there are almost always some patients in the inferior treatment that achieve better outcomes than some patients in the superior treatment, and vice versa.

Actual clinical practice is individual, case by case work, with an N of 1. At the same time that we must exercise caution in inappropriately extrapolating from normative research to

individual cases, we must also be wary of the tendency to believe in the necessary superiority of therapists' individualized, intuitive understanding of clients. Research by Dawes, Meehl, and others suggests that the impression that individualized understandings are more accurate than broad generalizations based on established principles is often illusory (Dawes, 1994).

Real therapists must also guard against confirmation bias, or failure to check on the accuracy of our beliefs about patients by obtaining relevant ancillary information, and to realize that patients may also make this type of error. Successful clients may attribute their improvement to treatment, because there is no way for them to evaluate what would have happened without treatment. There is considerable ambiguity in actual clinical practice. Coping with this lack of certainty and applying principles established on a probabilistic basis are challenges all psychotherapists must continually face.

Estimating Clinical Representativeness of ESTs

One way of estimating the magnitude of this generalization problem is to consider the few studies which have explicitly compared the treatment effect sizes of experimental studies conducted in actual clinical settings versus those performed in university contexts. Smith, Glass, and Miller (1980) found that effect sizes from studies conducted in clinical settings (mental health centers, d = .47; other outpatient facilities, d = .79) were smaller than those from college-based studies (d = 1.04). Consistent with this are the findings of the Weisz et al. (1992) review of four meta-analyses based on more than twelve thousand children and adolescents who participated in more than two hundred controlled studies. Weisz et al. (1992) concluded that while overall children and adolescents benefitted from treatment, effects were more modest among the more representative clinic studies. In fact, most of the six studies of referred patients treated in clinics for more general psychopathology they reviewed did not show significant treatment effects.

On the other hand, several researchers have failed to find statistically significant differences between studies conducted in laboratory and clinical settings (Jorm, 1989, Shapiro & Shapiro, 1982; Steinbrueck, Maxwell, & Howard, 1983). In a very ambitious investigation, Shadish et al. (1997) selectively reviewed 56 non-university outcome studies whose conditions most closely mimicked those of actual practice. They found that effect sizes from the more representative clinic therapy studies (d = .54, based on 46 Stage I studies) were generally comparable to those from the studies comprising the original meta-analyses they used (d = .59).

In contrast, an evaluation of stuttering treatment studies (Andrews & Harvey, 1981) found larger effect sizes from clinic studies (outpatient settings: d = .76; inpatient settings: d = 1.00), than from university-based studies (d = .67). These last results, favoring treatment provided in actual clinical settings,

are in accord with the findings of the 1995 Consumer Reports (CR) naturalistic, effectiveness study, which revealed "90% of [psychotherapy] patients doing well, in contrast to efficacy studies which are usually in the 65% range" (Seligman & Levant, 1998, p.212).

Seligman and Levant highlight several conclusions from the CR study: "long term therapy worked much better than short term therapy; no particular modality of therapy or medication exceeded any other for any disorder; and insurance limits on choice and duration of therapy predicted worse outcome. These conclusions are notable because each contradicts what is often found by the efficacy method and suggests that the outcome of therapy "in the field" may be quite different from findings of laboratory efficacy studies." (p.212)

Summary

While the use of ESTs to guide clinical practice is advantageous in many respects, awareness of the limitations of the efficacy studies the EST list is based upon may help to prevent overly simplistic thinking about treatment standards. Balanced consideration of the findings from these studies will hopefully limit inappropriate generalizations. Integrating the findings from naturalistic studies can also foster more accurate understanding of the challenges that confront "real life" clinicians.

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