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A REEXAMINATION OF THE EMPIRICALLY SUPPORTED TREATMENTS CRITIQUES

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Half a dozen special sections and a number of independent articles in psychotherapy journals have recently addressed and debated empirically supported treatments (ESTs). Serious arguments have been presented in favor of the EST movement, while significant criticisms have also appeared. While acknowledging the importance of criticism on the *process* of developing the EST lists, this commentary reexamines several critiques related to the *goals* of the EST movement. By offering a positive and comprehensive view of EST aspects that may have initially appeared as weaknesses, this article reaffirms the strengths and potential benefits of the EST project and discusses ways to achieve them.

Several years have passed since psychotherapy demonstrated its value and effectiveness as a mental health treatment, with many schools of therapy reaching a compromised decision that they are more or less equally effective (i.e., the Dodo bird verdict). Recently, an old antagonist (biological psychiatry) with the help of a new ally (managed health care) has created a new challenge, that of cost-effectiveness. A new horse race has begun, with cognitive-behavioral therapies leading the competition. While cognitive-behavioral treatments try to compete with drug therapies, psychodynamic and humanistic treatments have just started, screaming "unfair!"

A gloves-off fight has erupted again in psychotherapy. In the empirically supported treatments (EST) debate over the last ten years, the majority of opposition comes from psychodynamic and humanistic researchers and practitioners who refuse to play with the unfair rules that their rivals made in some sort of agreement (based on the medical model) with their major antagonist (psychiatry). Accusations of violation, politics, and conflicts of interest regarding ESTs have proliferated. Indeed, most of the opposition, repeatedly expressed through the years, is largely justified (for a short review see Elliott, 1998).

Nevertheless, calmer voices have called for the avoidance of extremities (Elliott, 1998; Fonagy & Target, 1996; Waechler, 1998). Psychodynamic and experiential researchers have started to develop manuals and empirically support their therapies (for examples see Gallagher-Thompson & Steffen, 1994; Goldman, Greenberg, & Angus, 1999; Greenberg & Watson, 1998; Johnson & Greenberg, 1985; Watson & Stermac, 1999). In some countries, psychodynamic and humanistic therapies are al-

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ready considered empirically supported and enjoy equal status with cognitive-behavioral therapies (e.g., Germany; Strauss & Kaechele, 1998).

To enhance progress in this positive direction, this paper considers recent critiques of ESTs and attempts to pinpoint positive aspects of ESTs that their critics may have overlooked. Specifically, this article is a response to the 1998 special section of *Psychotherapy Research* on the topic of ESTs (Elliott, 1998), in which substantial supportive arguments for the EST project were missing. The article also discusses similar criticism on the ESTs that has recently appeared in the literature. My hope is that psychodynamic and experiential researchers will get more actively involved in the EST movement, fighting for appropriate changes they have already identified, and conducting research to support their psychotherapies.

Although the EST project (and the Diagnostic and Statistical Manual [DSM] taxonomy on which ESTs are based) originated in the United States and are not necessarily followed by the rest of the world, the issues discussed in this paper go beyond specific diagnostic systems and treatment lists. Beyond any potential uses that EST-related research may have for clinicians and researchers who follow other diagnostic and treatment systems, it is the underlying methodological and research principles of the EST project that are most important. These principles have universal value and can be generalized to any alternative systems, lists, and projects around the world.

This article will not focus on criticisms related to possible dangers from the pressure of third-party payers and possible misuse of the movement's findings. Nor will I discuss problems related to the criteria used to determine the ESTs. All of these concerns are important, and recommendations for improvement are promising (see Bohart, O'Hara, & Leitner, 1998; Elliott, 1998; Henry, 1998; Waechler, 1998).

Most of the justified criticism focuses on the *process* of determining the ESTs (e.g., inconsistent, invalid, strict, or lenient criteria, lack of statistical power, nonconsideration of effect sizes and clinical significance, inadequate outcome measures and targeted areas of improvement, limited length of research, lack of long-term follow-up data, high relapse rates, inadequate control for protocol adherence and investigator allegiance, discrimination against specific therapies and populations, and paucity of external validity). But it is difficult to argue against the *goals* of the EST movement (e.g., adequately controlled empirical research, evidence-based treatment, explicit guidance in clinical practice, replicable practice and research, dissemination of empirical findings, improvement of therapy training). Independent of the technical problems in the process, it is important that we should not miss the ESTs' essence, as well as the benefits that they have to offer in the areas of treatment selection, psychotherapy research, and the training of the psychotherapists. What follows is a discussion of some major points of criticism concerning the goals of the EST movement and an attempt to show how these perceived weaknesses might actually be strengths under certain conditions.

THE DODO BIRD VERDICT AND EMPIRICALLY INVALID TREATMENTS

One major concern is that efficacy research might be abandoned in the name of the Dodo bird verdict. The Dodo bird verdict on the equivalency of therapies and the common factors hypothesis is responsible for a great deal of the criticism on ESTs. Although common factors might explain the largest part of the rough equivalence of therapies, they are not the only explanation. While acknowledging the potency of common factors, Norcross (1995) has summarized reasons to critically question the Dodo verdict and to support, in some instances, differential effects of the psychotherapies.

Two good examples of such differential treatment effects can be found in the areas of the anxiety disorders and the sexual dysfunctions (see review by Emmelkamp, 1994) and are useful in the deconstruction of a universal outcome equivalence belief (Elliott, Stiles, & Shapiro, 1993). In addition to differences found in main effects of treatments, aptitude-treatment interaction (ATI) research also occasionally shows differential therapeutic outcomes (particularly when aptitude is a clinically meaningful psychological variable; see research by Beutler et al., 1991; Piper, McCallum, Joyce, Azim, & Ogrodniczuk, 1999; Shoham-Salomon, Avner, & Neeman, 1989).

Although the degree of real outcome equivalence between therapies cannot be easily determined, a few important limitations of the verdict to keep in mind are: (a) It is based on horse-race outcome studies, almost half of which lack adequate statistical power to detect differential effects (Kazdin & Bass, 1989); (b) The outcome equivalence finding involves at best only a few types of therapy (i.e., those that have been tested, usually well-developed ones), but not all therapeutic practices (Crits-Christoph, 1997; Norcross, 1995); (c) Equivalent outcomes are usually based on self-reported symptom reduction out of many possible areas of change—that is, outcome measures might be insensitive or unsuitable to detect some meaningful differences (Luborsky, 1995; Norcross, 1995); (d) In therapeutic practice, "the Dodo bird verdict is intuitively and clinically wrong . . . and defies clinical reality" (Norcross, 1995, p. 502); Elliott et al. (1993) "seriously doubt that any psychotherapy researcher (or clinician) ever seriously believed in universal equivalence" (p. 462). (For detailed critical reviews of the outcome equivalence phenomenon, see Elliott et al., 1993; Howard, Krause, Saunders, & Kopta, 1997; Fisher, 1995; Kiesler, 1995; Luborsky, 1995; Norcross, 1995; Shadish & Sweeney, 1991).

Two recent major studies that support the Dodo bird verdict (Wampold et al., 1997; Shapiro et al., 1994) were also immediately subject to similar and multiple criticisms (Crits-Christoph, 1997; Howard et al., 1997; Norcross & Rossi, 1994). For example, the former study confirmed the Dodo bird verdict mainly between cognitive and behavioral therapies or, at best, other "bona fide" treatments that are either included in the ESTs list or could be in the future. The latter study was criticized for lack of adequate power and appropriate client variables to be tested for ATI effects. All these methodological critiques should not allow therapists to rest exclusively on the therapeutic relationship, powerful placebos, and other ill-defined common factors, which seems to be a major assumption shared by most of the EST opponents (see Bohart & Tallman, 1999; Garfield, 1996; Henry, 1998; Wampold et al., 1997). In concluding their sophisticated review of the Dodo bird verdict, Elliott et al. (1993, p. 476) clearly advocated "the need for continued disorder- or problem-focused treatment outcome research, coupled with more intensive investigation of client and therapist individual differences and in-therapy processes."

Some have argued that searching for ESTs is a case of blanket validation of psychotherapy per se, since EST inclusion criteria are too lenient and since almost every therapy could be validated (Henry, 1998; Strauss & Kaechele, 1998; Wampold, 1997). Instead, they suggest that a list of empirically invalid treatments would be more useful (Henry, 1998). Although the lenient criteria of ESTs makes it quite possible that they will lead to another form of Dodo bird verdict, this is certainly beneficial, since (a) the first one is obviously limited, and (b) the second Dodo bird verdict will be more problem-specific this time (and increasingly become more specific with the progressive development and validation of ESTs for comorbid disorders and other client characteristics). As Howard et al. (1997) pointed out, even in the medical field there is often more than one intervention that produces equivalent results in the treat-

ment of an illness, but this does not mean that all kinds of medical treatments are effective for a specific disorder. The same should be assumed to be true in psychotherapy. Even if only a few treatments were proved invalid and nonefficacious, this would be an important gain for the field and the protection of the public. We should not forget that there are 400 forms of therapies out there and that only seven or eight of them have been tested and contributed to the Dodo verdict in the first place (Norcross, 1995). There are serious reasons to believe that not all of the existing treatments will finally make the EST list (especially since it is disorder-specific), while some specific treatments will probably never be tested for specific problems, due to the fact that they lack anecdotal or preliminary effectiveness. That is, an empirically invalid treatments list (official or unofficial) could be potentially devised parallel to the EST list.

DO ESTs IMPEDE MEANINGFUL RESEARCH?

SETTING THE CONTEXT FOR THE NEXT GENERATION OF PSYCHOTHERAPY RESEARCH

A major criticism of ESTs is that efficacy research will stifle other meaningful kinds of psychotherapy research. However, a considerable part of high-quality process, process-outcome, and ATI research (and other advanced process research designs; Stiles, Honos-Webb, & Knobloch, 1999) has been conducted in the context of manualized efficacy outcome research, such as the Treatment of Depression Collaborative Research Program and the Second Sheffield Psychotherapy Research Project (e.g., Barber & Muenz, 1996; Crits-Christoph et al., 1999; Hardy et al., 1998; Stiles, Shankland, Wright, & Field, 1997). While conducting comparative outcome research on the efficacy of process-experiential therapy for depression (Watson & Stermac, 1999), Watson (1999) recently confirmed what psychotherapy researchers know about this type of research: It is a great opportunity to do process and process-outcome research which might actually be more important than the comparative outcome trial itself (e.g., Geller, Greenberg, & Watson, 1999). Thus, rather than being an obstacle, the delineation of some empirically supported, specific, and manualized therapies may be a helpful condition for more meaningful process and outcome research. Beside their usefulness in testing the internal validity of ESTs, randomized controlled trials (RCTs) are important and necessary steps for meaningful research to follow, whereas more powerful designs will be used to identify the active ingredients of ESTs.

The reasons behind the preference for manualized ESTs as an ideal research context are multiple (Lampropoulos, 2000). These include: (a) ensuring that the in-session events and processes under study are related to demonstrated robust therapeutic outcomes; (b) explaining the equivalent outcomes of two or more equally effective treatments for the same problem; (c) generating standard and replicable language of therapist operations provided by treatment manuals; (d) providing some controls given the complexity and the variability of the therapeutic endeavor, like the ones available in manualized ESTs; and (e) providing an ideally documented case of theory-based therapies (i.e., they are measured for adherence to treatment manuals, while even master therapists may depart from their theories in everyday clinical practice).

Indeed, comparative process research, which investigates similarities and differences between therapies on the theoretical level as well as the way they are prac-

ticed in naturalistic settings or by expert therapists, is limited: the first because it examines only theory and not practice, and the latter two because the actual practices of therapists of different orientations (even the expert ones) are often eclectic and inconsistent with their theories. For all the foregoing reasons, manualized ESTs appear to be an ideal research environment for meaningful process and process-outcome research.

Dismantling, additive, and parametric research (Borkovec & Castonguay, 1998; Kazdin, 1994) are very promising experimental designs that can identify the active ingredients of ESTs. One such example is Jacobson et al.'s (1996) work in testing the components of Beck, Rush, Shaw, and Emery's (1979) empirically supported cognitive therapy (CT) for depression. Current ESTs could be further specified and dismantled from their unproven components. Another example is the additive research currently being conducted by Newman, Castonguay, and Borkovec (1999), where variables such as interpersonal focus and emotional deepening are added into ESTs (i.e., Cognitive-Behavioral Therapy for Generalized Anxiety Disorder) to enhance treatment effectiveness.

ATI research could be another promising development, based on earlier efficacy research with ESTs, especially the case with "little interventions." This case employs effective techniques dismantled from larger manualized therapies, like the behavioral activation (BA) component in Beck et al.'s (1979) CT for depression (see also Shoham & Rohrbaugh, 1996). ATI research philosophy has been embraced by the Task Force of the American Psychological Association's (APA) Division of Clinical Psychology (Chambless et al., 1996) and is also echoed in the first of the EST principles recently proposed by the special task group of the APA's Division of Counseling Psychology (i.e., levels of specificity; Wampold, 1998). In these levels of specificity, level IV epitomizes the meaning of ATI research: "Specific approaches in specific areas for specific populations. Example: Well specified prevention program A for persons with risk factor B and cultural characteristics C" (Wampold, 1998, p. 1).

Subtypes of DSM Axis I disorders based on etiology, symptom profiles, preexisting deficits, personality characteristics, stages of change, comorbidity, and other therapy variables are particularly suitable for theory-driven, meaningful ATI research. A good example is the case of depression, where preliminary empirical findings already support the differential effectiveness of treatments (Addis & Jacobson, 1996; Barber & Muenz, 1996; Beutler, Clarkin, & Bongar, 2000; Dance & Neufeld, 1988; Piper et al., 1999; Rodriguez-Naranjo & Godoy, 1998; Stiles et al., 1997). This type of research could also gradually answer the demands for clinical utility, as specific ESTs will be validated as treatments of choice for representative situations such as comorbid disorders. For example, CT (or its equally effective component, behavioral activation) and Interpersonal therapy (IPT) for depression can be tested against each other, matched to depressed clients' comorbid personality disorders, such as Avoidant Personality Disorder and Obsessive-Compulsive Personality Disorder, respectively (see also relevant findings by Barber and Muenz, 1996, that suggest such differential outcomes between these two ESTs). Many similar ATI hypotheses (using a variety of variables as aptitude) can be made for all Axis I disorders, where at least two meaningfully different ESTs exist (e.g., Cognitive-behavior therapy [CBT] and IPT for bulimia).

However, important limitations of ATI research include the issue of adequate statistical power and the need for a clinically meaningful, theory-driven hypothesis. Some widely publicized failures of ATI research such as project MATCH (Project MATCH Research Group, 1997) could be interpreted as invalidating the specific

matches tested, rather than ATI methodology as a whole. ATI studies with alternative research questions and a refined design (i.e., small and distinguishable treatment units, suitable and sensitive outcome measures) still hold promise, particularly with heterogeneous clinical problems such as depression. Excellent accounts on the strengths, weaknesses, and appropriate methodological designs of ATI research are available in Shoham-Salomon (1991) and Shoham and Rohrbaugh (1995).

Based on the aforementioned, it is quite possible that a large part of the next generation of psychotherapy research will be based on and take place inside or in relation to the EST movement. The value of effectiveness research and efficacy research lie precisely in their complementarity, not their exclusivity. In fact, it is my belief that the EST project will remain incomplete and unsuccessful if it stays at the present level and is not complemented through process, qualitative, and clinical utility research. Although the majority of experimental efficacy research will continue to be conducted by research centers and academicians, there are a variety of methodologies suitable for clinicians, groups of clinicians, or small research teams (Arnkoff, Glass, Opazo, Caspar, & Lampropoulos, 2000; Goldfried, Borkovec, Clarkin, Johnson, & Parry, 1999; Hayes, Barlow, & Nelson-Gray, 1999; Lampropoulos et al., 2000). These range from pre- and quasiexperimental research to experimental research, from case studies or small samples to medium and large *N* studies, and from ATI, parametric, additive, dismantling, and qualitative designs to naturalistic outcome research.

Lastly, several of the methodological problems of RCTs can be solved inside the EST movement. For example, outcome measures could be added or modified to capture important areas of human experience and functioning, long-term follow-ups could be included in trials, and allegiance effects could be controlled (for suggestions on the latter, see Luborsky et al., 1999). I would go further to suggest here that even those treatments that oppose the psychopathology-based orientation of therapy could and should be evaluated with RCTs and be included in future EST lists. For example, the process and techniques (but not the content) of solution-focused therapy can be manualized, and pre-post outcome evaluation can include the measurement of both problems and their solutions.

A more radical and serious criticism is the one that questions the reliability, validity, and meaningfulness of DSM in diagnosis and treatment (Follette, 1995; Henry, 1998). These are recurrent arguments for a theory-based or empirically based diagnostic system that could replace DSM in clinical practice (see Henry, 1998; Horowitz, 1994; Millon, 1996). Of course, such an alternative system needs to prove its empirical value and comprehensiveness to replace the DSM and stand alone in clinical practice. Further, such etiologic systems also must prove to be homogenous enough in terms of the clients' symptoms and characteristics in order not to be equally limited with DSM in guiding treatment and research.

In the meantime, the simultaneous consideration of personality traits or personality disorders with the primary Axis I diagnoses of DSM may be useful in psychotherapy research. This would allow researchers and clinicians to capitalize on existing research findings based on current DSM diagnoses. I concur with the view that "there are enough regularities in even complex clinical formulations" (Henry, 1998, p. 136) to justify a clinical formulation system somewhere between broad diagnostic categories and a totally idiographic case formulation. Nevertheless, such a system can be developed inside and in relation to the DSM system through dismantling, ATI, and process research (see also Kiesler, 1995). This economy-driven approach has the benefit of capitalizing on existing DSM-based ESTs.

ARE ESTs INHUMANE AND INSENSITIVE IN CLINICAL PRACTICE?

DEVELOPING EMPIRICAL TREATMENT SELECTION

ESTs represent one form of eclectic practice, that is, specific treatments for specific disorders. In this respect, arguments that ESTs are insensitive to client variability (Garfield, 1996) seem somewhat unjustified. Empirically supported treatment is more humane and sensitive than any procrustean application of theoretically pure therapies that claim to be equally effective for all clients and all problems (see also Norcross & Newman, 1992), or "anything goes" practices based on the therapeutic equivalence paradox. Symptoms and problems are very specific, meaningful, and important client characteristics, among many other client variables. Symptoms and problems are undeniably a serious aspect of clients' realities and a central characteristic of their case formulation. That is, ESTs in their present form are at least partially sensitive to client individual differences.

However, the insensitivity criticism has a sound basis when it comes to problem subtypes, comorbidity, and nondiagnostic personality variables. Two proposals to deal with these important concerns follow.

A gradual demonstration of ESTs for complex clinical realities. Using ATI research designs, ESTs could be determined for more complex clinical situations, client characteristics and comorbid disorders. Findings from personality-matched treatments might also be very helpful here (Anderson, 1998; Beutler, Zetzer, & Williams, 1996). However, official practice guidelines may be somewhat premature at this point, which explains why the EST Task Force has not issued more specific eclectic recommendations yet (see Chambless, 1996; Chambless et al., 1996). It is likely that specific differential EST recommendations might be issued in the future for client variables like those studied by Beutler and associates (1991, 1996). My integrative hope is that in the distant future, through painstaking research in empirically dismantling, adding, comparing, and matching specific techniques with individual realities, we will be able to create a database of empirically supported components from different orientations to be used in different situations in building optimally effective individualized case formulations.

A flexible and individualized application of manualized ESTs, especially for complex cases (*Chambless, 1996; Persons, 2000; Shoham & Rohrbaugh, 1996*). In addition to identifying and matching ESTs to comorbid disorders and client personality variables, flexible applications of these ESTs will individualize them even further. That is, we should rely on empirical ATI research to advance eclectic practice as far as possible in terms of optimal therapies for meaningful clinical subpopulations (i.e., the big decisions) and leave clinical wisdom and expertise to complete the prescriptive matching task (i.e., the small decisions). The combination of these two proposals will make clinical practice as empirically individualized as possible, resembling the ideal individualized practice described in Stiles' *responsiveness theory* (Stiles, Honos-Webb, & Surko, 1998).

The slowly but steadily accumulated EST-based ATI findings will allow therapists to treat clients more humanely (i.e., sensitively but also empirically supported). This empirically supported approach to client individuality might be necessary, since early research shows that clinicians' individualized case formulations, compared with manualized treatments, may actually reduce therapeutic effectiveness on average

(Schulte, Kunzel, Pepping, & Schulte-Bahrenberg, 1992). This is particularly applicable with trainees and inexperienced therapists who may believe that their clinical experience is adequate to identify important client variables and match them with the appropriate intervention.

What is being advocated here is some form of empirical humanism, as opposed to an idiosyncratic and unsystematic way of approaching clients' multidimensional and complex realities. ESTs are, by definition, empirical means toward humanistic ends; the most respectful way to treat people is effectively and efficiently, and this requires empirical research. Thus, the argument that ESTs are inhumane and insensitive can be reversed and stated in the form that treating clients with empirically supported interventions according to their diagnostic (and other individual) differences is a sublime version of humanistic practice.

A related major point of criticism is that diagnostic specificity dehumanizes clients because it deemphasizes the relationship variables in therapy (Bohart et al., 1998; Henry, 1998; Strauss & Kaechele, 1998). Although clinical diagnosis alone is not adequate for a humane treatment, it is definitely important, and does not preclude an effective therapeutic relationship. Training therapists in techniques does not mean that the therapeutic relationship and important common factors must be neglected. After all, when efficacious ESTs are applied to naturalistic settings, we should expect both the therapeutic alliance and subsequently the clinical utility of ESTs to increase, at least comparing to efficacy research conditions. This is due to the fact that random assignment of clients to therapists in RCTs may compromise the relationship and the final outcome, thus underrepresenting the real effectiveness of an EST in the free market (where clients actively look for the therapist and treatment most suitable for them; Strauss & Kaechele, 1998).

IS TRAINING IN ESTs POTENTIALLY HARMFUL?

THE ART AND THE SCIENCE IN THERAPY

Parts of the scientific community have expressed concerns about the use of manuals in clinical training and the emphasis on techniques (e.g., Henry, 1998). Is therapy a science or an art? Many believe it is both. Whether we like it or not, some of the basic human and relationship qualities preexist training, and might be difficult to be taught (Dobson & Shaw, 1993). That is, therapists might be partly born and partly bred (Greenberg, 1998; Lambert, 1998; Orlinsky, Botermans, & Ronnestad, 1998).

In that case, what is the role of graduate training? Obviously, its role is to take these naturally equipped individuals and teach them the best way to enhance, complement, and transform their interpersonal qualities into practice. This is the technical and scientific part of therapy training. We get the talent and the art, and add scientific training. This suggests that most of our concern with therapists' interpersonal qualities should move to the pretraining level (i.e., the selection process; see also Stein & Lambert, 1995). Of course, training in interpersonal skills might also be useful. For example, Henry (personal communication, April 1997) has been developing a CD-ROM for therapist training in interpersonal behavior, based on the Structural Analysis of Social Behavior (SASB) system.

Admission committees should ensure that they accept "humans," and use manualized and other training to make them scientists. ESTs are manualized, step-by-step explanatory processes of how to do therapy. This might be the best way to teach

therapy, rather than talking in general about principles and theories (for training in ESTs, see Calhoun, Moras, Pilkonis, & Rehm, 1998). Even if trainees do not always follow the manual step by step (and they should not), they have a great opportunity to be exposed to and educated in the way theoretical principles are operationalized in clinical practice.

Some evidence of negative therapeutic outcomes associated with manualized training came from a data set of 16 clinicians trained in brief psychodynamic treatment (Vanderbilt II project; Henry, Schacht, Strupp, Butler, & Binder, 1993; Henry, Strupp, Butler, Schacht, & Binder, 1993). In this study, a subgroup of "therapists with hostile introjects were found to be largely responsible for the posttraining increase in negative and complex interpersonal communications" (Henry, Schacht, et al., 1993, p. 446). However, "it appears that training did not make them more hostile per se. Rather, the greater activity level might simply have provided more opportunities to display baseline rates of problematic interpersonal behavior" (Henry, Strupp et al., 1993, p. 439). This points toward issues of therapist preexisting qualities and trainee selection, as well as additional interpersonal training. Training in techniques through the use of manuals does not mean that interpersonal effectiveness training must be neglected.

In any case, research findings showing that manualization may interfere with therapist's interpersonal skills should receive attention. However, such findings might be transient and reflect trainee difficulties in assimilating new knowledge while using their interpersonal skills effectively at the same time (see also Henry, Strupp, et al., 1993), a problem that may gradually disappear as trainees become more experienced and flexible in applying manualized ESTs. Although manuals are not panacea for psychotherapy training, their value and benefits for novice therapists have been recognized (Levenson & Strupp, 1999; Strupp & Anderson, 1997).

MANUALS AS THE COMMON LANGUAGE FOR TRAINING, PRACTICE, AND RESEARCH

The development of manuals is not just a necessary evil for the validation of treatments; it specifies the content of often abstractly described interventions and gives form and shape to sometimes inconsistent theories. In this sense, manuals may provide a standard language in terms of techniques that can enhance understanding, communication, and comparisons in the field, both between the same theoretical orientation and between orientations. The problem of a common language and standard frames of reference has been long considered as one major obstacle for psychotherapy integration and rapprochement between therapies (Norcross & Newman, 1992). The manualization part of the ESTs movement seems to represent an improvement in this direction. This standard technical language can facilitate the quest for commonalities and differences between therapies, at least at the lower level of techniques.

CONCLUSIONS

To conclude, ESTs seem to have advanced our knowledge about therapy one step further. Slowly but steadily, we move from "does therapy work" to Paul's (1967) "*What* treatment, by *whom*, is most effective for *this* individual with *that* specific problem, and under *which* set of circumstances." These developments have been recently advanced to include the identification of efficacious and cost-effective psychological

ESTs in areas that have been traditionally dominated by pharmacotherapy, such as severe, chronic, and resistant nonpsychotic disorders (DeRubeis, Gelfand, Tang, & Simons, 1999; Guthrie et al., 1999).

In the EST effort and through constructive criticism, we will hopefully be able to detect and overcome weaknesses, improve our research, protect clients' welfare from economic pressures, and capitalize on the products of the EST movement. The identification of empirically supported, manualized treatments seems necessary in order to proceed, and arguments on *how to do it* should not cancel the decision to *do it*, as some may suggest (see Wampold's, 1997, recommendations on suspending comparative trials). On the other hand, premature decisions should be avoided on mandatory practice guidelines, while plenty of time and equal opportunity should be given to therapies to join research trials, especially since RCTs need time and effort to be completed. As we move to the next EST-related generation of psychotherapy research, insurance companies, licensing procedures, accreditation bodies, and professional regulations should be careful and fair.

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Zusammenfassung

Ein halbes Dutzend von Sonderheften sowie zahlreiche Einzelartikel in Zeitschriften zur Psychotherapie haben sich in jüngster Zeit mit der Debatte um die empirisch validierten Behandlungen (EVB) befaßt. Zugunsten der EVB-Bewegung wurden ernste Argumente ins Feld geführt, gegen die sich wiederum kritische Stimmen richten. Die Wichtigkeit der Kritik im Hinblick auf den Prozeß der Entwicklung von EVB-Listen wird in diesem Kommentar anerkannt, gleichzeitig werden verschiedene Kritiken, die sich auf die Ziele der Bewegung beziehen, kritisch analysiert. Bestimmte Aspekte der EVB, die anfänglich als ihre Schwäche erschienen, werden in eine positivere und umfassendere Perspektive gerückt, wodurch in diesem Beitrag die Stärken und der potentielle Nutzen des EVB-Projektes hervorgehoben wird, ebenso wie bestimmte Wege, daraus Nutzen zu erzielen.

Résumé

Une demie douzaine de sections spéciales et un nombre d'articles indépendants dans des journaux de psychothérapie ont récemment traité du débat au sujet des traitements empiriquement fondés (empirically supported treatments, EST). Des arguments sérieux ont été amenés en faveur du mouvement EST, mais aussi des critiques de poids. Tout en reconnaissant l'importance des critiques au sujet du processus d'établissement des listes EST, ce commentaire réexamine quelques critiques concernant les buts du mouvement EST. En proposant une vision positive et globale de certains aspects d'EST qui au départ ont pu apparaître comme des faiblesses, cet article réaffirme à nouveau les forces et les bénéfices potentiels du projet EST et discute des voies pour les réaliser.

Resumen

Media docena de secciones especiales y gran número de artículos independientes en revistas de psicoterapia han tratado y debatido recientemente el tema de las terapias con apoyo empírico (ESTs). Se han presentado serios argumentos a favor del movimiento EST, así también como críticas significativas al mismo. Junto con el reconocimiento de la importancia de la crítica en el proceso de desarrollo de las listas EST, este comentario reexamina varias críticas relacionadas con los objetivos del movimiento EST. Al ofrecer un punto de vista positivo y comprehensivo de los diversos aspectos del EST que inicialmente podían haber aparecido como debilidades, este artículo reafirma la fuerza y los beneficios potenciales del proyecto EST y discute caminos para lograrlo.

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