

A critical review of Empirically Supported Treatments (ESTs) and Common Factors perspective in Psychotherapy

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Abstract

In the last years the field of research and practice in psychotherapy has been deeply influenced by two different approaches: the so-called Empirically Supported Treatments (ESTs) movement, due to the development of Evidence Based Medicine (EBM) attitude in the mental health field too, and the Common Factors perspective, supported by who sustain the “Dodo Bird Verdict” and the importance of common features in the explanation of therapeutic process. The major aim of this paper is to provide a critical review of these two different approaches pointing out possible criticisms from epistemological and methodological perspectives not avoiding to consider economic and practical issues. Possible solutions to go beyond this debate are indicated in the last part of the article.

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Introduction

In order to build a bridge between research and practice in the clinical psychology, psychotherapy researchers have increasingly focused their attention on studies about efficacy and effectiveness of different therapeutic treatments. In this investigation about mental health, two different approaches are now the most common and popular ones between clinical practitioners and researchers. On the one hand it is possible to note the development of the so-called Empirically Supported Treatments tested with randomized controlled clinical trials (RCTs) (Herbert, 2003; Morrison, Bradley, & Westen, 2003); on the other hand we underline the growth of studies about the “Common Factors” perspective according to the confirmation of the so-called “Dodo Bird Verdict” (Luborsky et al., 2004; Luborsky & Singer, 1975). The distinction between these two so different approaches is also related to completely distinct epistemological and philosophical attitudes towards psychotherapy and the relative research.

These two different approaches are well described by Tracey (2003): “one view is that each model of therapy has distinctive effects on clients. This assumption of distinctiveness leads naturally to the inference that some models are more effective than others with particular client types ... A competing and perhaps complementary view is that different theoretical approaches are comparably effective” (p. 401).

In the next paragraphs we would like to stress these two different positions indicating also possible alternative solutions to go beyond this current and deep debate.

The Empirically Supported Treatments (ESTs): the state of the art

A factor that have historically contributed to amplify the science-practice gap is “the popularity of theoretical paradigms that embrace epistemologies based on personal experience rather than controlled data” (p. 415, Herbert, 2003). For example the psychoanalytic approach has traditionally been based on the assumption that theoretical principles can be developed only with studies of individual cases in uncontrolled clinical settings (Safran, 2001). On the other hand, also existential and humanistic therapies perceive RCTs as a dehumanizing procedure unable to capture the critical factors responsible of change such as the critical therapist-client relationship (Bohart, O'Hara, & Leitner, 1998).

To understand this negative aptitude towards empirical research, it is important to consider “the recent growth of postmodernism in both academic and clinical settings, which eschews the methods of natural science in favour of subjective narrative truths” (Herbert, 2003).

As indicated by Sanderson, “the emergence of managed care and similar reimbursement systems that require greater accountability and the development of clinical practice guidelines have increased the importance of evidence-based clinical practice. As a result of these two trends, it appears as though the future of psychotherapy will require clinicians to deliver a psychological intervention that is supported by research” (p. 290, Sanderson, 2003).

The American Psychological Association Society of Clinical Psychology's Committee on Science and Practice has emphasized the importance of identifying, studying and disseminating empirically supported therapies (Sanderson, 2003). The official starting date

of this approach in psychotherapy was the 1995, when the Task Force on Promotion and Dissemination of Psychological Procedures of Division 12 (Clinical Psychology) of the American Psychological Association (APA) identified “a number of psychological interventions as empirically validated treatments, later called *empirically supported treatments (ESTs)*” (Chambless & Ollendick, 2001; Herbert, 2003) or *empirically validated treatments (EVTs)*. This interest in promoting the importance of ESTs probably finds its origin in the broader movement, born in United Kingdom, initially called *evidence-based medicine (EBM)* (Sackett, 1997). In the 1995 report, the Task Force indicated the criteria to select an EST and reported a very preliminary list of 25 selected treatments that reached the amount of 71 in 1998 (Chambless & Ollendick, 2001).

According to these APA guidelines, many projects have been developed to disseminate the evidence based approach in mental health. One important example is represented by the *Evidence-Based Practices (EBPs) Project*, designed to increase access for people with SMI to empirically supported interventions (Mueser, Torrey, Lynde, Singer, & Drake, 2003). In particular “the EBP Project aims to improve access through development of standardized implementation packages, created in collaboration with different stakeholders, including clinicians, consumers, family members, clinical supervisors, program leaders, and mental health authorities” (p. 387, (Mueser et al., 2003).

Epistemological, methodological and economic criticisms about ESTs

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

Nicholas Maurice Arthus (1862–1945)

Despite of the emphasis supported by APA, such empirically supported psychotherapies are not used widely by front-line practicing clinicians. Herbert noted that “this effort has been met with criticism, however, by both practitioners, on one hand, and psychotherapy researchers on the other” (Herbert, 2003). Moreover a consensus on the criteria for determining what is empirically supported has not yet been reached (Beutler, Clarkin, & Bongar, 2000).

Many criticisms have been underlined in ESTs approach. First of all a general consideration could note that the development of the Evidence-based medicine has become a real “social movement”: “EBM has acquired a powerful role of making “verdicts” about what is good treatment and what is not” (p. 278, Starcevic, 2003). It is important to take into account that the standard EBM procedure are often not applicable in the field of psychotherapy, considering that evidence procedures are based on observation and investigation that are never theory-free and value-free. But the basic assumption of EBM is the scientific realism, “which postulates that there is an independently existing reality and that truth about such reality is attainable through scientific investigation” (p. 279, Starcevic, 2003).

Some authors have underlined that in the clinical field there is “a rejection of empirical epistemologies in general, and randomized clinical trials in particular, as legitimate methods for evaluating psychotherapies” (p. 413, Herbert, 2003). Is scientific the status of the specific procedures to identify ESTs? The ESTs approach is built on the foundation of

the medical model where the procedure towards symptom amelioration is composed by diagnosis and prescriptive treatment (Bohart et al., 1998). According to Albee (1998), 50 years ago psychology made an heavy mistake because it uncritically accepted the call to provide psychiatric services to returning veterans using a permanently stamped medical model. Moreover “the National Institute of Mental Health (NIMH), the leading source of research funding for psychotherapy, decided to apply the same methodology used in drug research to evaluate psychotherapy, the randomized clinical trial (RCT)” (p. 45, Duncan, 2002). Also Goldfried and Wolfe (1996) suggested the psychotherapy outcome researchers have become mainly dependent on the “clinical trials” method to determine the best treatments. “In addition to condoning the medicalization of psychotherapy, psychotherapy researchers may unwittingly be playing into the hands of third-party payers in placing unwarranted emphasis on the putative fixed efficacy of specific interventions” (p. 1007, M. R. Goldfried & Wolfe, 1996).

In this scenario, adopting the RCT methodology for evaluating psychotherapy, negative effects have been obtained. A bright description of these consequences is reported in Duncan (2002): “it meant that a study must include manualized therapies (to approximate drug protocols) and *Diagnostic and Statistical Manual of Mental Disorders* (4th ed., *DSM-IV*; American Psychiatric Association, 1994) defined disorders to be eligible for a NIMH-sponsored research grant ... The result: Funding for studies not related to specific disorders dropped nearly 200% from the late 1980s to 1990 ... In addition to these limiting effects, force fitting the RCT on psychotherapy research is empirical tyranny and bereft of scientific reasoning” (p.45). The cognitive-behavioural approach took advantages producing ascendancy on the ESTs lists. The strict EVT criteria could limit the wide application of qualified treatments so “those that could be easily specified through treatment manuals and treatment protocols geared toward very specific disorders or client populations” (p. 266, Brunswik, 1952).

Another negative effect, beside the omission of many treatments not cognitive-behavioural oriented, is the limitation on clinical innovation in psychotherapy practice and research (Chwalisz, 2001). Also Jacobson (1996) underlined this limitation: “Moreover, randomized clinical trials can evaluate only currently existing treatments; they cannot develop new and better ones” (p. 1038, (Jacobson & Christensen, 1996).

According to current criteria, to be indicated in the list as “probably efficacious”, a treatment needs only show that is superior in comparison with no-treatment condition (e.g. a waiting-list condition). Many authors have criticized this approach (Herbert, 2000; Klein, 2002). Using this dangerous criterion “virtually any intervention is superior to no treatment, especially for mood and anxiety disorders” (p. 417, Herbert, 2003), and prayer and placebo could be included in the ESTs list.

Another methodological critical issue is that a treatment could be considered as EST only with the minimal standard of 2 supportive studies, “even if 50 other studies failed to find effects” (p.418, Herbert, 2003). “To make the EVT list, an approach need only demonstrate its efficacy, or superiority over placebo, in two studies” (p. 44, Duncan, 2002). So, as indicated many years ago by (R. Rosenthal, 1979), there is a bias against publication of null results.

Another key problem is how to distinguish apparently similar treatments. Now the boundaries between different approaches in psychotherapy listed as ESTs are too established on the superficial appearance (frequently only on the name) and a negative consequence could be that “a potentially infinite number of variations of established

treatments could be added to the list” (p. 419, Herbert, 2003) For example many studies have now shown that the feature that may distinguish EMDR (Eye Movement Desensitization and Reprocessing), included as EST, from imaginal exposure are superfluous (Davidson & Parker, 2001).

Moreover the current criteria used do not include the possibility to remove a treatment from the list even if alternative procedures have demonstrated their superiority. According to the principle of *primum non nocere* (first do not harm), it is also important to avoid the inclusion of potentially harmful treatments. Typical examples are certain adolescent peer-group interventions that may increase delinquent behaviour (Dishion, McCord, & Poulin, 1999).

Another limitation of ESTs approach is the focus on “efficacy” more than on “effectiveness”, for example when hypnosis is involved as indicated by (Amundson, Alladin, & Eamon, 2003): ““efficacy”-based research in clinical practice is contrasted with “effectiveness” -focused research ... the “effectiveness” perspective also serves as a counter point for hypnosis in contrast with the dubious efficacy-based gold standard currently proposed for therapy in general, and hypnosis in particular” (p. 11, Amundson et al., 2003).

Another relevant field to take into account is the economic consequence of defining a list of ESTs. Some authors have underlined the negative effects due to the “further demise of long-term psychotherapy (p. 413, Herbert, 2003) and the excessive focus on symptom reduction rather than self-actualization and perceived improvement in quality of life (Fensterheim & Raw, 1996).

Moreover not only scientifically minded psychologists but also stakeholders, in particular third-party payers, support this kind of approach. “Health resources are allocated on the basis of evidence, so that enterprises that are not sponsored by EBM, such as psychotherapy, are often left without resources” (p. 280, Starcevic, 2003). According to (Beutler, 1998), the frequent consequence of the community standard would be a “court-imposed system” to determine which psychotherapies were acceptable or unacceptable and, consequently, reimbursable or not. The final result is that “managed care increasingly dictates the approach therapists must use with specific diagnoses to receive reimbursement” (p. 47, Duncan, 2002).

Are the RCTs so suitable in the psychotherapy research?

About the use of randomized controlled trials, considered as the best standard in the psychotherapy research, there is a lot of criticisms by clinical practitioners. An interesting description of the main features of this methodological procedure and of the relative critical areas has been carried out by Starcevic (2003) and his interesting analysis is reported in table 1.

According to the criticisms reported in table 1, RCTs can be defined as procedure to test a somewhat artificial treatment in the artificially controlled setting with atypical patients (Ablon & Jones, 2002).

Table 1. Features of RCTs and relative criticisms (modified by Starcevic, 2003)

Features of RCTs	Criticisms of RCTs
<p>“Randomized controlled trials” (RCTs) are a methodological procedure that consists in the comparison of the group of patients in whom the usefulness of treatment is being examined (experimental group) with the group of patients who are receiving no active treatment (e.g. a <i>placebo; control group</i>);</p>	<p>In the psychotherapy studies there is no counterpart to a placebo that is used in the pharmacotherapy studies. The non-specific (and presumably placebo-like) psychological treatments, administered to patients in control groups, are not ‘neutral’ in the way that placebo is pharmacologically ‘neutral’ because they produce psychological effects, regardless of whether these are clinically significant.</p>
<p>RCTs focus on <i>strict diagnostic homogeneity</i> of the groups of patients and give emphasis on diagnostic precision;</p>	<p>Psychiatric diagnosis is usually not the main factor that determines the use of psychotherapy and diagnostic precision is not emphasized in psychotherapy. As a result, psychotherapy patients are not as diagnostically homogeneous as patients in RCTs and often have additional disorders that would exclude them from RCTs.</p>
<p>Another key feature is represented by the <i>randomization</i> into experimental and control groups of patients;</p>	<p>Randomizing patients in the psychotherapy usefulness studies is troublesome because clinical practice is not randomized; also, randomization creates an artificial situation because it ignores the fact that psychotherapy patients actively choose their own treatment.</p>
<p>It is important to carry on <i>double-blind design</i> of research;</p>	<p>A double-blind design is impossible in psychotherapy research. Patients cannot be blind as to what psychological treatment they are receiving because they actively participate in it; likewise, therapists cannot be blind because they know what treatments they administer.</p>
<p>Another “gold standard” of the RCTs procedure is the use of <i>standardization of treatment procedures</i>, so that all patients receive (or are presumed to receive) treatment in the same way.</p>	<p>Psychotherapy is extremely difficult to ‘standardize’ so that its procedures and techniques are used in the same way by all the therapists, regardless of their training and personality. Every encounter between the patient and the therapist has some unique features, with the potential of producing ‘something’ that cannot be predicted and entirely ‘standardized’.</p>

Also Garfield (1998) has underlined that the utilization of current ESTs approach could be even deleterious for the development of psychotherapy: ESTs underestimate many variables (for example the therapist allegiance to a specific model), are dependent on “manualized” therapies and do not represent the psychotherapy practice and could induce the imposition of rigid constraints on practitioners’ procedures by the specification of specific treatments for specific diagnoses (Deegear & Lawson, 2003; Garfield, 1998).

Moreover the efficacy shown in RCTs does not correspond to effectiveness in clinical settings; so internal validity does not necessarily ensure external validity (Duncan, 2002; M. Goldfried & Wolfe, 1998). So the RCT is not the most adequate methodological procedure for empirically validating psychotherapy in comparison with the practice in the real world (Seligman, 1995). Duncan (2002) noted that “unlike the RCT, in actual clinical practice, manuals are not used, therapies are not ever purely practiced, clients are not randomly assigned to treatments, and clients rarely, if ever, enter therapy for singular *DSM*-defined disorders or experience success solely as diagnostic symptom reduction” (p. 47, Duncan, 2002).

A last but not least problem related to the RCTs is the possibility to generalize the results from clinical trials where subjects are recruited from advertisements. “Investigators question whether these subjects are representative of the types of individuals who seek treatment in clinical practice” (p. 242, Rapaport et al., 1996). Moreover the use of older anxious community volunteers is reviewed and also critiqued in an investigation about the potential usefulness of cognitive-behavioral treatments (Stanley & Novy, 2000).

Problems related to the use of Placebo Controls and waiting-list/no-treatment conditions

Inside the ESTs approach and the Common Factors perspective too, psychotherapy treatment outcome studies have used the *double-blind randomized placebo control design* to evaluate the effects of various psychotherapeutic factors (M. R. Goldfried & Wolfe, 1996).

Historically, starting from the 1930s, this methodological design was originally developed in US and UK for the medical field (Shapiro & Shapiro, 1997; Wampold, 2001a) for the “purpose of holding constant all factors except the medication’s active ingredient” (p.973, Baskin, Tierney, Minami, & Wampold, 2003). After being adopted in medicine, Rosenthal and Frank (1956) suggested that the randomized double-blind placebo control group design could be used in psychotherapy research. So the consequence was that, for example, if “cognitive-behavioral therapy (CBT) for depression were compared with an adequate placebo control group and found to produce superior outcomes, these results would support the contention that the purported active ingredients in CBT (e.g., altering core schema and challenging irrational thoughts) were responsible for the benefits of the treatment (p. 973, Baskin et al., 2003).

But to suitably apply this design in psychotherapy research, several conditions have to be accomplished. According to Baskin (2003), in the table 2 all these main conditions are reported.

But unfortunately there are some problems that reduce the positive impact of Placebo Controls procedures in psychotherapy. First of all psychotherapy studies cannot be blind in the same sense of placebo controlled medical studies (see, for example, drug research)

Table 2. Conditions to fruitfully apply the double-blind placebo control group design in psychotherapy research (p. 974, Baskin 2003)

<i>1) All factors but those purported to be the active ingredients of that treatment must be made equivalent (i.e., held constant). That is, the placebo control treatment must be structurally indistinguishable from the specific treatment (e.g., same number of sessions and same treatment modality)</i>
<i>2) The participants, study administrators, and study examiners must all be blind to the treatment conditions.</i>
<i>3) Other psychological factors, such as participants' expectations, belief in treatment and the credibility of the treatment, need to be controlled (i.e., made comparable for the active and placebo treatments).</i>

(Baskin et al., 2003). In fact the therapist must know the nature of the selected treatment in order to follow the clinical protocol (Seligman, 1995; Wampold, 2001b).

Moreover the placebo is often distinguishable from the active treatment. "If the study participants are informed that they will be randomly assigned to an active treatment or to a placebo treatment without active ingredients, the apparent differences between the groups would allow the participants to determine that they were assigned to the less desirable treatment. Therefore, the quality of the design is increased by informing the participants that both treatments are equally efficacious, thereby creating a deception" (p. 974, Baskin et al., 2003). But also using a deception, some differences between Placebo Control and active treatment condition remain. For example the rationales for the treatments differ and then the credibility of the active treatment and the related expectations of participants might be different taking into account that a credible rationale of treatment has a powerful healing effect (Baskin et al., 2003; Wampold, 2001a, 2001b). Borkovec and Nau (1972) found that the rationales for control conditions were considered as less credible than the ones for different active conditions.

Another key consideration is that well-designed Placebo Controls (according to the criteria above indicated) are more similar to active treatments than control conditions. So Placebo, to be used in psychotherapy outcome studies, has to become a real therapeutic treatment. In fact, according to Baskin (2003), "when placebo controls are better designed, the present meta-analysis found that the benefits produced by such treatments were not substantially different from the active treatments to which they were compared" (p. 976).

Another limitation to the use of Placebo Controls in clinical studies is provided by Wampold (2001b) that noted the flaws of placebo procedure in psychotherapy. As reported by Baskin (2003), Wampold stressed this issue stating that "the superiority of bona fide treatments vis-a-vis placebos is not sufficient evidence to establish specific effects" (p. 977).

Another criticism more related to waiting-list/no-treatment conditions is more focused on ethical issues. Wesley & Waring (1996) in a critical review of marital therapy outcome research, noted that "there is some evidence to indicate that future outcome research should avoid the use of "waiting list control groups", since their use appears to be neither ethical nor humane..." (p. 421).

The real impact of ESTs on clinical practice

Many authors have underlined that the real impact of lists of ESTs on clinical practice has not been so significative. For example, in the case of anxiety disorders, only a minority of patients have received an empirically supported treatment despite a lot of documents, reports and papers supporting the effectiveness of such treatments (Goisman et al., 1993; Goisman, Warshaw, & Keller, 1999). It is also important to take into account that, according to the RCTs approach, “supporters” of EBM consider that efficacy studies are more appropriate in the clinical field, whereas psychotherapists value effectiveness studies more suitable considering them as an accurate reflection of the reality of clinical practice (Starcevic, 2003).

About the application of “manualized” or “free” protocols, the treatment manuals are traditionally considered restrictive, not able to capture the different nuances of each patient and not useful in the most complex cases (Herbert, 2003). Moreover the use of an EST approach tend to support the cognitive and behavioural therapies (15 of the 16 treatments identified as efficacious in 1998 were behavioural or cognitive-behavioural in orientation) and to limit other models that are less amenable to a manualized presentation (Beutler, 1998; Deegear & Lawson, 2003).

It is also important to take into account that treatment manuals are not so flexible in front of the heterogeneity existing in any DSM-based category of disorders. In fact “there remains a wide degree of therapist and intertreatment variability within a given model of treatment, even when a manual is closely followed, and therapist effects are often quite large” (p. 151, Malik, Beutler, Alimohamed, Gallagher-Thompson, & Thompson, 2003).

One possible conclusion of this basic review about the state of the art of ESTs in psychotherapy could be a sentence reported in (Deegear & Lawson, 2003): “Although there are political, societal and monetary enticements to accepting the current rendering of ESTs, sufficient evidence has cast doubt on the movement as it currently exists” (p. 276).

The “Common Factors” perspective: an introduction

Duncan (2002) advised that instead of assuming a medical model of psychopathology, “a call is made for a systematic application of the common factors based on a relation model of client competence” (p. 32, Duncan, 2002). The *Dodo Bird Verdict* from Alice in Wonderland (Lewis Carrol) (“Everybody has won, and all must have prizes”) has become a metaphor to represent the state of psychotherapy outcome research (Luborsky et al., 2002; Luborsky & Singer, 1975) and “a symbol of a raging controversy regarding the privileging of specific approaches for specific disorders based on demonstrated efficacy in randomized clinical trials” (p. 33, Duncan, 2002).

Weimberger (1995) noted that after 1980 writings began to appear on the common factors and many significant proposals about them have appeared (Garfield, 1996; M. R. Goldfried, 1982; Miller, Duncan, & Hubble, 1997; Patterson, 1989; Weinberger, 1993). But perhaps the most relevant contribution about the common factors approach was made by Michael Lambert. This author has carried on important reviews of many years of outcome research. In 1992 Lambert (see Lambert, 1992) identified four therapeutic factors as the main elements to obtain improvement in psychotherapy: *extratherapeutic factors, common ones, expectancy or placebo, techniques*.

Table 3. Commonalities Across Therapeutic Factors and Five Superordinate Categories Identified by Grencavage and Norcross (1990) (as reported in Tracey 2003)

Superordinate category	Commonality
1. Client characteristics	1. Positive expectation/hope or faith 2. Distressed or incongruent client 3. Patient actively seeks help
2. Therapist qualities	4. General positive descriptors 5. Cultivates hope/enhances expectancies 6. Warmth/positive regard 7. Empathic understanding 8. Socially sanctioned healer 9. Acceptance
3. Change processes	10. Opportunity for catharsis/ventilation 11. Acquisition and practice of new behaviors 12. Provision of rationale 13. Foster insight/awareness 14. Emotional and interpersonal learning 15. Feedback/reality testing 16. Suggestion 17. Success and mastery experiences 18. Persuasion 19. Placebo effect 20. Identification with the therapist 21. Contingency management 22. Tension reduction 23. Therapist modeling 24. Desensitization 25. Education/information provision
4. Treatment structures	26. Use of techniques/rituals 27. Focus on “inner world”/exploration of emotional issues 28. Adherence to theory 29. A healing setting 30. There are participants/interaction 31. Communication (verbal and nonverbal) 32. Explanation of therapy and participants’ roles
5. Relationship elements	33. Development of alliance/relationship (general) 34. Engagement 35. Transference

A good summary of the main commonalities about therapeutic factors is reported by Tracey (2003) according to the original classification of (Grencavage & Norcross, 1990) (see table 3).

As well summarized by Duncan 2002, "Miller et al. (1997) expanded the use of the term *common factors* from its traditional meaning of non specific or relational factors to include four specific factors: *client* (40%), *relationship* (30%), *placebo* (15%) and *technique* (15%). The percentages of influence indicated in the previous sentence was reported in Assay and Lambert (1999). Even if there is a general concurrence about these four areas, "the specific factors within each of these categories vary considerably in nature and number with "little apparent agreement" (p. 373, Grencavage & Norcross, 1990) among researchers regarding what they are" (p. 402, Tracey et al., 2003).

Considerations about the "Common Factors" perspective

One possible criticism related to this perspective is the limitation of the contribution of the *techniques* in the explanation of improvement in psychotherapy (only 15% of all the common factors (Assay & Lambert, 1999). In fact, in our opinion, a specific technique could characterize and deeply influence the role of the others three factors.

First of all the possible and concrete action of the *client factors* is strictly related with the particular kind of epistemology (and the relative techniques) behind each approach. Thus to allow patients to be the most potent contributor to psychotherapeutic change (Bohart & Tallman, 1999; Duncan, 2002; Duncan & Miller, 2000b), the selected approach has to give them the role of leading actor in the psychotherapeutic process. This issue is possible only with a particular technique that influence the relationship and the communication with patients giving them the idea that they have the worth of their improvement. Not all the approaches transfer this idea to patients, overall if the epistemology behind the treatment is more deterministic than constructivist. So if the medical model characterizes the theoretical groundings of a particular approach, the patient is not more an "heroic client" (Duncan, 2002; Duncan & Miller, 2000b), but he or she is subjected to the action of external factors such as drugs or the ability and expertise of a well-known psychotherapist or "guru".

At the same time to allow *relationship factors* to play a regenerative role in psychotherapy, the therapist has to build up a particular relationship that has the aim to change the dysfunctional situation of the client. The use of direct or indirect communication, one-down or one-up position, open or closed questions, brief aphorisms or long reframings, suggestion or rationality in the prescription of homeworks, etc. could quickly accomplish this task. But to select the best kind of communication and relationship, according to the specific patient and the different steps of psychotherapy, is necessary the use of particular techniques. In fact only an advanced use of them could allow therapists to "tailor" the approach to different patients' features and logics. "Some clients, for instance, will prefer a formal or professional manner to a casual or warmer one. Others might prefer more self-disclosure from their therapist, greater directiveness..." (p. 39, Duncan, 2002).

Also about the *placebo, hope and expectancy factors*, Duncan (2002) noted that the client's awareness of being treated is not the only element that could allow the placebo component to carry on a significant therapeutic improvement: to have a good placebo

effect, it is also necessary “the assessment of the credibility of the therapy’s rationale and related *techniques*” (p. 40, Duncan, 2002).

About the *model/techniques factors*, Assay and Lambert (1999) defined them as beliefs and procedures unique to specific treatments. Rosenzweig (1936; 1940) proposed that the key point of techniques was the formal consistency with which the doctrine used is adhered to, whether the approach is psychoanalysis or Christian Science. Another relevant point of view is that the main contribution of model/techniques to therapy is simply related to the enhancement of other common factors (Hubble, Duncan, & Miller, 1999a). In our opinion *model/techniques factors are not the last (and least) elements of the therapeutic process but they are the most important one* because, as indicated before, *they deeply influence the functional accomplishment of other common factors*. So without a functional technique a therapist cannot easily build a change-oriented relationship, communication, strategy and then therapy.

Before closing this section dedicated to the different common factors and the importance of *model/techniques factor* among them, we would like to stress some issues in order to avoid an excessive emphasis about technique and a possible new kind of reductionism: *technique is important but is not all*. In particular, during the training of a young therapist, the learning of a specific group of techniques (and the following adherence) is a key point, but going through the years of practice, the technique has to modify itself according to the personal style of each therapist. Only with a personal evolution, techniques could be changed and improved allowing a better therapeutic process. We have also to take into account that some steps in psychotherapy are more related to “tailored” applications of personal perceptions and intuitions (also with a percentage of “mystery” and unexplained issues) more than a repetition of well-learned techniques.

Coming back to methodological bias, these ones are not only present in the ESTs studies, but also in the investigation about the common factors in psychotherapy. For example the problem of control conditions is reported in Chambless (2002) about the Luborsky’s studies. In fact Chambless (2002), underlining a significative number of problems with the meta-analysis of the effects of comparative psychotherapy studies Luborsky et al. presented in 2002 (Luborsky et al., 2002), noted that “acceptance of the Dodo bird verdict is dangerous. Despite my great respect for Lester Luborsky and his colleagues, I must disagree with their conclusion that there are no meaningful differences in outcomes of different approaches to psychotherapy” (Chambless, 2002). The most important methodological criticisms about the Luborsky’s “Dodo bird verdict” are well summarized by the same Chambless (2002): “Luborsky et al.’s ... conclusion that there are no meaningful differences in the efficacy of various psychotherapies should be reconsidered for the following reasons: (a) errors in data analysis, (b) exclusion of research on many types of clients (e.g., children and adolescents), (c) faulty generalization to comparisons between therapies that have never been made, and (d) erroneous assumption that the average difference between all sorts of treatments for all sorts of problems can be assumed to represent the difference between any two types of treatment for a given problem. Concern for clients’ welfare demands that psychologists be very wary of accepting the dodo bird verdict” (Chambless, 2002).

Criticisms about the meta-analyses contributions

One of the main methodological resource used within the “common factors” perspective is the meta-analyses. For example a recent large investigation designed to test the dodo verdict, based on the meta-analyses procedure, once again has confirmed the bird’s judgement (Duncan, 2002; Wampold, 1997). Wilson and Lipsey (2001), in their synthesis of 319 meta-analyses of psychological, behavioral, and educational treatment research, underlined that “these results underscore the difficulty of detecting treatment outcomes, the importance of cautiously interpreting findings from a single study, and the importance of meta-analysis in summarizing results across studies” (p. 413).

A good discussion of meta-analyses as tool in psychotherapy is reported in Koch and Ziegler (2000): “in these days, more than one clinical trial is mostly performed to evaluate a new treatment or therapeutic intervention. This necessitates a combined evaluation of their results. An integration of evidence from several trials is also helpful to determine the actual knowledge. These are the main goals of meta-analyses. Since the end of the 80s meta-analyses are widely used in clinical research. At the beginning of a meta-analysis, a protocol has to be developed. Similar to a protocol of a clinical trial, the inclusion and exclusion criteria for trials, the hypotheses and the planned analyses have to be fixed. After a careful localization of trials, a combined statistical analysis is performed. An investigation of heterogeneity, i.e., differences between study results, is indispensable. During the last years, the tool meta-analysis has been criticized. The criticism mainly results from poorly conducted meta-analyses which generated results without pre-specifying hypotheses or which merely combined study results. Well-planned meta-analyses, on the contrary, have an increasing influence in clinical research” (p. 109). The main positive features of meta-analyses procedure are well summarized by Rosenthal (2001): “Advantages of the meta-analytic procedures include seeing the “landscape” of a research domain, keeping statistical significance in perspective, minimizing wasted data, becoming intimate with the data summarized, asking focused research questions, and finding moderator variables” (p.59).

A more critical discussion of this methodological tool considers that “negative trials are often unreported, and hence can be missed by meta-analysts. And how much heterogeneity between trials is acceptable? A recent major criticism is that large randomised trials do not always agree with a prior meta-analysis. Neither individual trials nor meta-analyses, reporting as they do on population effects, tell how to treat the individual patient” (p. 123, Lau, Ioannidis, & Schmid, 1998). O’Rourke (1989) reported an interesting remark about current difficulties in deriving firm conclusions based on meta-analysis, but with a good expectation about its future application in helping clinicians and policy makers answer clinical questions.

Despite of these promising results, such meta-analyses often include studies with methodological shortcomings which might invalidate their results: see for example the analysis of two recent meta-analyses carried on to study child and adolescent psychotherapy effects (Weiss & Weisz, 1990).

Moreover, according to Shadish, Matt, Navarro, & Phillips (2000), “concern has arisen that meta-analyses overestimate the effects of psychological therapies and that those therapies may not work under clinically representative conditions” (p. 512).

Alternative solutions to go beyond the debate: toward a more clinically valid approach in psychotherapy research

Two cultures (scientific and clinical) speaking different theoretical languages reside in psychology. The categorical structure of scientific language is based on sensorially, linearly, and analytically formatted cognitive processes (common sense). The categorical structure of the clinical language is formatted for cognitive processes accommodating non sensory, nonlinear information about purposive personality processes. Clinically and scientifically oriented psychologists have difficulty communicating with one another because of underlying paradigmatic differences in their languages. Unfortunately, both cultures use the same sensory-system-based vocabulary, which leads to the unwitting and confusing assumption that they are speaking the same language.

Zoltan Gross (2001)

Morrison et al. indicated two complementary possibilities in which the clinical research can be carried on: “by starting with efficacy trials and then testing treatments with promising results in the laboratory using broader community samples; or by starting with everyday clinical practice, examining patterns of co-variation between specific interventions and outcomes at clinically meaningful follow-up intervals with diverse and ecologically valid samples and using these data to generate prototypes of treatments that can be used to guide the next generation of experimental studies” (p. 109, Morrison et al., 2003).

Another possibility is the shift from *evidence-based practices* to *practice-based evidences* (Margison, Barkham, Evans, & al., 2000) examining outcomes of psychotherapy in naturalistic settings with psychotherapy treatments of variable duration carried out with more flexibility, allowing the therapists to exercise more clinical influence and an active role (Margison et al., 2000). Outcome measures would take into account not only symptom reduction, but also “constructs such as functioning, impairments and quality of life”.

Another possible exit from the debate is represented by the evaluation of efficacy and effectiveness of the same approach-treatment in the hands of different therapists. According to Starcevic (Starcevic, 2003), one way to address this issue could be “the assessment of the therapist’s performance by both the patient and the therapist” (p. 280, Starcevic, 2003).

Research’s clinical validity has been strongly compromised by the “medicalization of outcome research, use of random assignment of clients without regard to appropriateness of treatment, fixed number of therapy sessions, nature of the therapy manuals, and use of theoretically pure therapies” (p. 143, M. Goldfried & Wolfe, 1998). So from one side “outcome research became clinical trials” and “target problems were replaced by DSM diagnoses” (p. 144, M. Goldfried & Wolfe, 1998); from the other side an extreme “negoziating” and generalist position where “all works with the same efficacy” could stop the improvement of the clinical research and practice.

In a field of research presently characterized by efficacy research and focus on internal validity, “the challenge for effectiveness research ... is to add the component of external validity while still preserving internal validity” (p. 144, M. Goldfried & Wolfe, 1998). We have also to remember the Brunswik’s lesson (1952): *systematic designs* are those that have as their primary consideration tight experimental controls, whereas *representative designs* more accurately sample the universe to which one wishes to generalize.

In our opinion psychotherapy could and must offer an alternative to the “reducing” medical model and to the “diplomatic” common factors perspective for the evaluation of

psychotherapy services. Our proposal is that psychotherapy research has to use only active treatments (“active” vs “active” and not “active” vs “control”) using different kinds of assessment tools according to each approach (not only cognitive-behavioural based and “auto-referred” tests but also video and audio tapes). Efficacy and effectiveness (in particular the number of sessions of the complete therapeutic iter) have to be evaluated not avoiding to collect data with the follow-up too.

So we would like to propose to come back to a more clinically and ecologically valid approach in psychotherapy research where some patients (with the same DSM-based diagnosis) could follow different active treatments (without control, placebo or waiting-list procedures) measuring the different improvements using not only traditional tests (too often CBT based), but also the so-called “scale technique” (de Jong & Berg, 2001; Nardone, 1996; Nardone & Watzlawick, 1993, 2004) for detecting the conjoint satisfaction between therapists and patients about the clinical results. This technique is very simple to administer and consists in giving a numeric mark to the patient’s situation. It can give us clear data about the “convergent” or “divergent” opinions about psychotherapy from the client’s and professional’s points of view (Nardone, 1996; Nardone & Watzlawick, 1993, 2004).

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