

Evidence based approach in psychotherapy: the limitations of current Empirically Supported Treatments paradigms and of similar theoretical approaches as regards establishing efficient and effective treatments in psychotherapy

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Abstract

In recent times the use of evidence based paradigms which are already very commonly used in medicine has also become common in the field of psychotherapy research. As a result, formal lists of therapies (Empirically Supported Treatments) 'which work' have been drawn up, designed to establish a body of efficient and effective therapies and over time reject therapies whose effectiveness cannot be demonstrated. In this manner criterion for 'well-established', and 'probably efficacious' treatments have arisen. The development of these kinds of paradigms was motivated by the emergence of a 'managerial' approach and related systems for remuneration as regards even the treatment of mental patients in particular, where a higher level of accountability is necessary, not only in the field of science and theory, but also for health providers and for insurance companies. As a result as this tendency it would seem that the future of psychotherapy would require clinics which prescribe psychological interventions which are backed up by research. In this article we will briefly consider the various approaches which are thought of as being evidence supported and then focus on the criteria for selection and in particular on the drawbacks of many approaches believed effective by clinics turn out to be ineffective when examined from an evidence-based point of view. For example in a 1998 list of Empirically Supported Treatments (Chambless et al.) for the treatment of anxiety and stress, only treatments in the behavioural cognitive field are listed, to the exclusion of other therapies (systemic, strategic etc.). We will conclude this article by listing potential criteria and situations compatible with the evidence-based approach which lead to valid effective and efficient psychotherapy.

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Introduction

For a number of years one of the fundamental questions for psychotherapy researchers has concerned the real effectiveness of psychotherapeutic treatments. The reply to this problem has been largely positive (Lambert & Bergin, 1994; Smith, Glass, & Miller, 1980) with some notable exceptions (Eysenck, 1952, 1961). It is worth mentioning comparisons made between the various kinds of control and psychotherapy (Smith & Glass, 1977).

In recent years we have witnessed a transition from a general demonstration of the effectiveness of therapy, to the particular examination, identification and classification of specific treatments which have been shown to be effective in experiments for generally recognised problems for psychology. The quest has increasingly focused on a valid reply to the question: ‘which treatment, prescribed by whom, and in which circumstances, is the most effective for this particular individual with this specific problem?’ (Paul, 1967, p.111)

At this point in time, two approaches are most commonly used and prevail amongst researchers and clinicians seeking efficiency and effectiveness in psychotherapy. On the one hand there are studies related to ‘common factors perspective’, theories confirming the so-called ‘Dodo Solution’ (Luborsky et al., 2004; Luborsky & Singer, 1975) (for a more in-depth analysis of this approach see Castelnuovo, Faccio, Molinari, Nardone, & Salvini, 2004).

On the other hand we note the development of so-called “Empirically Supported Treatments”, which are based on both random and controlled clinical trials (RCT’s) (Herbert, 2003, Morrison, Bradley, & Westen, 2003)

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The validation of treatments which have proved effective (Empirically Supported Treatments, EST) has gained both power and credibility as a result of a convergence of political economic and professional forces: ‘Managed Health Care, developments in biological psychiatry, the setting up of a task force for promoting and spreading the protocols of the American Psychological Association (APA), a change in the guidelines for accreditation within doctorate programmes in clinical psychology’ (Lyddon & Chatkoff, 2002, p.149).

In particular, Managed Health Care groups have pushed for the introduction of Diagnostic Related Groups (DRGs) which prescribe a diagnosis-based, pre-established fixed cost treatment plan. Furthermore, the

National Institute of Mental Health (NIMH), an important source of finance for psychotherapeutic research, has decided to apply the methodology used in pharmaceutical research to evaluate psychotherapy, resulting in the development of random and controlled clinical trials (RCT's) (Duncan, 2002).

In the same spirit Division 12 (Clinical Psychology) of the APA has set up a task force for the promotion and diffusion of psychotherapeutic treatment protocols which are proven experimentally effective (Task force on Promotion and Dissemination of Psychological Procedures, 1995). The work of this task force has resulted in the drawing up a minimum standards level of criterion required to validate as 'effective' a given physiotherapy, with the result that there exists a list of experimentally proven treatments which have been approved or rejected (Chambless et al., 1998; Chambless et al., 1996).

As Lyddon & Chatkoff, (2002) rightly emphasise: 'it is important to underline that the task force has explicitly stated that it set up these criteria to evaluate the experimental effectiveness of a treatment or the validity of experimental results themselves (which in ideal conditions is proven when controlled research demonstrates that a given therapy works). Thus criteria established by the task force have nothing to do with the clinical effectiveness of a treatment, which in turn is based on the external validity of a therapy, but instead concerns itself with proof that a given therapy works in a clinical setting outside of a controlled experimental context.' (Lyddon & Chatkoff, 2002).

Bearing this difference between clinical and experimental effectiveness in mind, currently a clinical workplace can use drug therapy (in order to satisfy pharmaceutical protocols) and treat DSM-IV disorders (in order to receive sponsorship from the NIMH) with the result that funds for studying non-specific disorders have been drastically reduced to the order of 200% (Duncan, 2002).

Criteria for empirically validated psychotherapies

Before we begin to highlight the drawbacks and paradoxes of the Evidence Based Model let us consider the criteria for selection of empirically validated psychotherapies (Table 1). These comprehensive criteria are freely available on the Internet (for example on the APA's official website www.apa.org/divisions/div12/est/97report.pdf)

**Table 1. Criteria for Empirically-Validated Treatments
(quoted by Chambless et al., 1998)**

Criteria for Empirically-Validated Treatments	
Well-Established Treatments	
I.	At least two good between group design experiments demonstrating efficacy in one or more of the following ways: <ul style="list-style-type: none"> A. Superior to pill or psychological placebo or to another treatment. B. Equivalent to an already established treatment in experiments with adequate statistical power (about 30 per group; cf. Kazdin & Bass, 1989). OR
II.	A large series of single case design experiments ($n \geq 9$) demonstrating efficacy. These experiments must have: <ul style="list-style-type: none"> A. Used good experimental designs and B. Compared the intervention to another treatment as in I.A. FURTHER CRITERIA FOR BOTH I AND II:
III.	Experiments must be conducted with treatment manuals.
IV.	Characteristics of the client samples must be clearly specified.
V.	Effects must have been demonstrated by at least two different investigators or investigatory teams.
Probably Efficacious Treatments	
I.	Two experiments showing the treatment is more effective than a waiting-list control group. OR
II.	One or more experiments meeting the Well-Established Treatment Criteria I, III, and IV, but not V. OR
III.	A small series of single case design experiments ($n \geq 3$) otherwise meeting Well-Established Treatment Criteria II, III, and IV.

List of well-established and probably efficacious treatments

A list of well-established and probably efficacious treatments (which accord with the criteria outlined in the previous paragraph and table) is reported in Table 2 and 3. Official updates stopped in 1998, (Chambless et al., 1998) and treatments in the systemic-strategic field are very limited, if not completely absent.

Paradoxes inherent in the Evidence Based Model

Imposing RCTs methodology upon physiotherapy is tantamount to tyrannical behaviour lacking in any kind of scientific reason (Duncan, 2002). The methodology of random and controlled trials (RCTs) at first solicited a strong consensus of approval amongst researchers, above all because of its inherent promise; however many critics have moved on from an initial period

Table 2. Well-Established Treatments (quoted by (Chambless et al., 1998))

<u>Well-Established Treatments</u>	<u>Citation for Efficacy Evidence</u>
ANXIETY AND STRESS:	
Cognitive behavior therapy for panic disorder with and without agoraphobia	Barlow et al. (1989); Clark et al. (1994)
Cognitive behavior therapy for generalized anxiety disorder.....	Butler et al. (1991); Borkovec et al. (1987)
Group cognitive behavioral therapy for social phobia.....	Heimberg et al. (1990); Mattick & Peters (1988)
*Exposure treatment for agoraphobia.....	Trull et al. (1988)
*Exposure treatment for social phobia.....	Feske & Chambless (1995)
Exposure and response prevention for obsessive-compulsive disorder.....	Balkom et al. (1994)
*Stress Inoculation Training for Coping with Stressors.....	Saunders et al. (in press)
Systematic desensitization for simple phobia	Kazdin & Wilcoxon (1976)
DEPRESSION:	
Cognitive therapy for depression	Dobson (1989); DiMascio et al. (1979)
Interpersonal therapy for depression.....	Ekin et al. (1989)
HEALTH PROBLEMS:	
*Behavior therapy for headache.....	Blanchard et al. (1987); Holroyd & Penzien (1990)
*Cognitive behavior therapy for irritable bowel syndrome.....	Blanchard et al. (1980); Lynch & Zamble (1989)
*Cognitive behavior therapy for chronic pain	Keefe et al. (1992); Turner & Clancy (1988)
*Cognitive-behavior therapy for bulimia.....	Agras et al. (1989); Thackwray et al. (1993)
Interpersonal therapy for bulimia.....	Fairburn et al. (1993); Wilfley et al. (1993)
PROBLEMS OF CHILDHOOD:	
*Behavior modification for enuresis.....	Houts et al. (1994)
Parent training programs for children with oppositional behavior	Walter & Gilmore (1973); Wells & Egan (1988)
MARITAL DISCORD:	
Behavioral marital therapy.....	Azrin, Bersalel et al. (1980); Jacobson & Follette (1985)
SEXUAL DYSFUNCTION:	
Behavior therapy for female orgasmic dysfunction and male erectile dysfunction.....	LoPiccolo & Stock (1986); Auerbach & Kilmann (1977)
OTHER:	
Family education programs for schizophrenia	Hogarty et al. (1986); Falloon et al. (1985)
Behavior modification for developmentally disabled individuals	Scotti et al. (1991)
Token economy programs	Kazdin (1977); Liberman (1972)

of unconditional faith in the experimental method to a position of cautious scepticism, to the point where some maintain, paradoxically that ‘the more rigorous and provable the research is, following the criteria of ‘the classic scientific method’ the less it becomes valid significant and classifiable from the point of view of day-to-day practice’ (Lo Verso et al., 2005, pag. 16; Fava and Masserini, 2002, Aveline and Shapiro, 1995). The first major problem, which is fully recognised and highlighted by many critics, concerns itself with the paradox whereby sticking faithfully to the criteria of the experimental method, which is designed to sustain the inherent validity of the method itself, at the same time threatens its adherent validity in terms of applying its results to realistic clinical situations.

The reply to the question: ‘is research good for clinical practice?’ should be ‘provided that the clinical reality to which it is applied should not be misrepresented to satisfy the demands of research’.

Table 3. Probably Efficacious Treatments (quoted by (Chambless et al., 1998))

<u>Probably Efficacious Treatments</u>	<u>Citation for Efficacy Evidence</u>
ANXIETY:	
Applied relaxation for panic disorder	Öst (1988)
*Applied relaxation for generalized anxiety disorder.....	Barlow et al., (1992); Borkovec & Costello, (1993)
*Exposure treatment for PTSD	Foa et al. (1991); Keane et al. (1989)
*Exposure treatment for simple phobia	Leitenberg & Callahan (1973); Öst et al. (1991)
*Stress Inoculation Training for PTSD	Foa et al. (1991)
*Group exposure and response prevention for obsessive-compulsive disorder ..	Fals-Stewart et al. (1993)
*Relapse prevention program for obsessive-compulsive disorder.....	Hiss et al. (1994)
CHEMICAL ABUSE AND DEPENDENCE:	
*Behavior therapy for cocaine abuse	Higgins et al. (1993)
*Brief dynamic therapy for opiate dependence.....	Woody et al. (1990)
*Cognitive therapy for opiate dependence.....	Woody et al. (1990)
*Cognitive-behavior therapy for benzodiazepine withdrawal in panic disorder patients	Otto et al. (1994); Spiegel et al. (1993)
DEPRESSION:	
*Brief dynamic therapy	Gallagher-Thompson & Steffen(1994)
*Cognitive therapy for geriatric patients.....	Scogin & McElreath (1994)
*Psychoeducational treatment	Lewinsohn et al. (1989)
*Reminiscence therapy for geriatric patients	Arean et al. (1993); Scogin & McElreath (1994)
*Self-control therapy	Fuchs & Rehm (1977); Rehm et al. (1979)
HEALTH PROBLEMS:	
*Behavior therapy for childhood obesity	Epstein et al. (1994); Wheeler & Hess (1976)
*Group cognitive-behavior therapy for bulimia	Mitchell et al. (1990)
MARITAL DISCORD:	
Emotionally focused couples therapy	Johnson & Greenberg (1985)
Insight-oriented marital therapy	Snyder et al. (1989, 1991)
PROBLEMS OF CHILDHOOD:	
*Behavior modification of encopresis	O'Brien et al. (1986)
*Family anxiety management training for anxiety disorders.....	Barrett et al. (in press)
OTHER:	
Behavior modification for sex offenders.....	Marshall et al. (1991)
Dialectical behavior therapy for borderline personality disorder.....	Linehan et al. (1991)
Habit reversal and control techniques	Azrin, Nunn & Frantz (1980)
	Azrin, Nunn & Frantz-Renshaw (1980)

In short there exists the risk that research, rather than contributing to an understanding of clinical practice, helps sustain a constant divide between real therapies, as they are applied in the private and public field alike, and theoretical and experimental models artificially created in a laboratory, which are so very unlike each another (Di Nuovo, Lo Verso, 2004). This is a further instance where the researcher, like the clinician, faces 'the dilemma of whether it is best to sacrifice the quest for full knowledge to the thoroughness of the research method or vice versa' (Salvini 1998, p.36).

There are many reasons which justify this risk which by implication exists above all where the method chosen involves the use of random and controlled clinical trials (RCTs). It should be self-evident that some of the criteria specific to certain kinds of treatment lend themselves more readily to experimental research than others. Westen, Novotny and Thompson-Brenner (2004) in a very thorough piece of work on this subject, which we recall here, emphasise how certain assumptions implicit to the method of experimentation can conspire to transform the perception of the clinical picture once it becomes evaluated. Indeed as far back as 1986 Minguzzi maintained that scientific *objectivity* aligns itself with research criteria only *after* the problem has been resolved within a given discipline, together with its variables, its methods and control procedures.

Whatever method is chosen cannot be said to be completely objective regarding the subject under investigation. The former involves language choices (linguistic or conceptual) within which the intellectual problem is couched, so that we can say that if the choice of language speaks to the problem, so also the presenting problem is expressed in the same language. The method chosen contributes toward establishing the nature of the problem to which it is applied, and when the former derives from the classic empirical tradition, the psychological problems which it investigates necessarily end up appearing to occur naturally. Reprising one of Bruner's axioms: 'the way we speak ends up becoming the way we describe and define what we talk about' (1986, p. 57). The way we phrase the question determines the nature of the reply. Faced with a number of different modes of rhetoric within which a problem can be expressed, and which will in turn determine the nature of the reply, the problem itself anticipates and shares in the nature of the mechanism investigating it (we might add that if the world always corresponded with man's description of it, and if we agreed on the way it is described, as happens with the Evidence-Based Model, then it would become that much more difficult to regard that same problem with a fresh pair of eyes).

Research on efficacy evaluation goes to the heart of the problem of how we set up the experiment to permit the method to function. The closer the treatment can get to the methodology praxis, the more likely it is that we will obtain positive results. The protocol for experimentation defines as 'appropriate' the intervention type described as: referring to a single Axis I disorder, for which the preliminary screening is carried out in such a way as to maximise a single diagnosis and reduce the a minimum the number of co-existing presenting problems which could increase the response-to-treatment variables. Short-term interventions are preferable (to reduce the effects of

interventional modifications), and between 6 and 16 sessions are recommended, each lasting the prescribed amount of time. The symptoms on which the research focuses determines the evaluation criteria, implying that the definition of efficacy is related to the symptom's disappearance (a criterion to which not all psychotherapeutic approaches are prepared to subscribe to).

Westen, Novotny and Thompson-Brenner (2004) remind us that the way the experimental method is ultimately decided upon implicates another series of assumptions: that the experimental method represents the Gold Standard for identifying truly efficacious treatment plans; that psychotherapy lends itself to a high degree of manipulation, (in other words that changes can appear and disappear over a short time period); that a large number of patients presenting with the same problem can be treated at the same time (which in turn presumes that psychotherapy can effectively treat everyone).

Some of the above observations might be better explained if we present them in list form;

1. First paradox: 'pure' cases with regard to DSM categories

Following the methodology of randomised and controlled trials, people presenting one problem only are considered; indeed the selection of participants advantages those who have a single-form disorder, or better yet those who can 'cover' the greatest number of criteria for a given specific disorder, and don't present symptoms of other disorders. Secondly, we assume that the clinical picture described during the initial screening turns out to be the definitive one on which the objectives of the treatment are based. This also means that research projects because they require financing, must necessarily be based on DSM-IV disorders. Yet as some authors (Howard 1996, Messer 2001) remind us, between one third and one half of people requesting treatment cannot be considered as being within DSM criteria (their problems neither satisfy nor exceed diagnostic thresholds). Inevitably this has contaminated the analytical clinical picture, as well as resulting in more 'simple' cases being given precedence. In the methodology of ESTs, control requirements severely limit the possible range of variables which can occur, the opposite to what generally happens in the clinical situation.

2. Second paradox: criteria for recruitment of patients

People taking part in a treatment evaluation project self-refer by replying to an advertisement or are referred on by a GP. Candidates are given a short initial screening (often over the telephone), to evaluate their suitability as regards treatment protocol (the first part of the selection process). There

follows a structured interview to establish their suitability for treatment, after which the person receives an invitation from the researcher/therapist and the treatment begins. Furthermore, the diagnosis is carried out by a 'technician', who is not the same person who implements the treatment plan. The person beginning the treatment does not undergo a further diagnostic evaluation which implicates having to rely unquestioningly on the first diagnosis, but above all leads to a 'production line' system whereby the person intervening does not have access to those elements of accountability which are generally central to the planning of the whole treatment plan.

3. Third paradox: assumptions implicit in hands on approach

The logic of hands on feeds the paradox whereby fulfilling the aim of the research (which is to establish a protocol for treatment), homogenises clinical situations and nullifies the worth of clinical results. From the point of view of experimental methodology, the most authoritative manuals advise the researcher: 'to standardise the dose, the time it is administered and the ingredients contained in each dose' (Westen et al. 2004, p.639). In this way we are sure that each patient receives the same treatment. Thus a good manual should also prescribe the number of sessions, as well as what precisely should happen at each session. The more such considerations are ignored, the less possibility there is to scientifically explain unusual results. Thus it is necessary to design treatment 'packages' which minimise the possibility of surprises. As Westen et al. (2004, p.639) have stated: 'every exercising of clinical judgement represents a threat to the heart of RCTs because they reduce the possibilities of standardising experimental variables and make consequent deductions ambiguous.'

This road leads to a change of perception, because the manual is no longer the *means* (a system used to prove or exemplify a theory) but becomes the *way* in other words a description of the manner in which the treatment itself should be carried out. The patient must change to fit in with the treatment, and not vice versa, and additionally the manual changes from being a description of the way a problem should be tackled, to being an instrument of prescription (to the extent that it even prescribes what the patient should say, think or do). Paradoxically then a good therapist is one who is to assume a procedural and bureaucratic role. (Perhaps some clinicians would become indignant at the idea of sacrificing their creativity on the altar of clinical technique). A good therapist in an efficient surgery would be the one who follows the manual to the letter, who is not put off by any distractions from the patient if they are not covered by the manual, and who doesn't succumb to the temptation to take

a more active part in proceedings. We are therefore describing therapy carried out *on* or prescribed *to* a patient, certainly not a situation where the patient is part of an interactive process.

The hands on process involves dividing up treatment into small segments, each one of which constitutes a package which can be disassembled and tested in minute detail.

The process of choosing one package rather than another, for the researcher, is what the philosopher scientist Karl Popper (1934) would describe as a 'prescientific' process (because it precedes a verification of a hypothesis). The choice of which treatment is to be evaluated heavily conditions the procedures of the 'scientific' process which follow. How do we choose precisely the most effective from amongst a selection of effective criteria to determine which treatment will be the most effective? the choice cannot be anything other than an act of faith... perhaps involving hope a little too much, certainly not very scientific when measured against the scientific expectations it is designed to fulfil, and in per se is an example of a 'clinical prediction' to use a phrase of Paul Meehl's (1954).

4. Fourth paradox: 'prescientific' selection of which treatment is to be 'scientifically' tested.

No employment panel however rigorous and demanding it be, can hire the best candidate if that person has not filed an application. Pre-selection of treatments is the most delicate part of the procedure, since here the possibilities are reduced down to an exclusive practical list. The experimenter's demands for brevity and total control largely excludes the possibility of experimentally verifying long-term treatments, even though the latter are largely preferred by the psychotherapeutic community (we know that the longer a treatment takes the more difficult it becomes to control variables and the more complicated it becomes to manage them, which often leads to the conclusion that these manipulations have invalidated the research). Normally it is the most cherished of the researcher's projection which are put to the test. Thus this lack of scientific care in selecting which treatment to investigate has contributed to the confusion reigning in the literature between a 'not empirically validated' and 'empirically invalid' or 'empirically unconfirmed' treatment (Roth & Fonagy, 1996, Weinberger, 2000, Westen and Morrison, 2003). We find it hard to understand why there are no studies carried out on 'unconfirmed treatments'; perhaps the answer lies in the fact that it is in the scientist's best interests to validate rather invalidate an hypothesis. Some authors understand: 'that patients who have failed to react positively to

validated treatments, will find it increasingly difficult to become effectively treated using other methods', thus legitimising the superiority of an approved treatment compared with all others, even those which have not been evaluated' (Wilson, Fairburn & Agras, 1997, p.85).

5. Fifth paradox: can research improve therapy?

This paradox functions in terms of a reduction in the proposed treatments which can be analysed and shuts the door to new findings (discoveries) since all one's energy becomes invested in testing out what already exists (justification). But would it not be possible to consider the verification phase in isolation to the list of possible generational hypotheses? The importance given to pre-scientific procedures can lead to scientifically weak conclusions, despite all the guarantees which the scientific method promises with regard to hypothesis verification. If it true that the essence of science is to rigorously confirm hypotheses, then the way that these come to light seem to be an extremely subjective proposition.

To conclude this section about problems peculiar to research i.e. problems involving both logic and epistemology, let us follow the opening premise about how to safeguard the inherent validity of the method to its ultimate conclusion. This appears a lost cause only if it is considered alongside the equally legitimate researcher's desire to protect their own role as psychotherapist. To reprise one of Bazzi's (1979) sayings: 'Each psychotherapist is also an anthropologist' and we might add also an epistemologist. Behind every therapist there more or less hides the figure of a human being, who has a theory about the mind, about how it functions, about the processes he is investigating, together with a theory about what defines consciousness, and who holds a series of assumptions about what is normal, psychic disorder or deviancy. If we draw up a particular definition of a problem from pragmatic assumptions, the indicators of change cannot be redefined other than within the confines of this same theory. What do we mean here by 'change'? in fact we mean an hypothetical construct, a surface upon which can be read all the inscriptions left there by different theories; choosing one definition, often through direct comparison with another, most of them concurring with the original outline. It is not right to ask a psychotherapist to measure their own effectiveness using a construct which they do not own. When for example an approach works on a symbolic level it is not pertinent to measure its effectiveness on a symptomatic level, even though the

FIRST PARADOX: ‘Pure’ cases with regard to DSM categories

An improvement in the criteria which satisfies the internal validity of a piece of research becomes a threat for its external validity. ‘Paradoxically, the more rigorous and provable the research is the less valid, significant and applicable it becomes to clinical general practice. ‘ (Lo Verso, et al. 2005, p.16)

SECOND PARADOX: Criteria for patient recruitment

The diagnosis is carried out by a ‘technician’ who is a different person to the one who will implement the treatment programme. The latter does not make their own clinical evaluation but ‘trusts’ in the original diagnosis This results in a ‘production line’ system whereby the person intervening does not have access to those elements of accountability, which are generally central to the planning and ultimately to the success of the whole treatment plan. Furthermore we assume that the situation as described at first contact is the correct one, and all the objectives of the treatment arise out of this first contact.

THIRD PARADOX: Assumptions implicit in the hands on approach

‘...the best manual is the one which standardises the dose, the time it is administered, and the specific ingredients contained in each dose...In this way we can be sure that each patient receives the same dose...a good therapist is the one who follows the prescriptions of the manual, who is not put off by any distractions from the patient if it does not fall in with what the manual suggests and doesn’t succumb to the temptation to take a more active part in proceedings” (Westen et al. 2004, p.639). Every exercise of judgement becomes a threat to the integrity of the research because the standardisation process permits fewer experimental variables
 ...the manual is no longer a MEANS, but becomes an END
 ... from a study of PRINCIPLES to an evaluation of PACKAGES
 ...from a DESCRIPTIVE function to a PRESCRIPTIVE one

FOURTH PARADOX: Science and prescience: selecting treatments

What criteria are used to select which treatments require validation?
 The influence of pre-scientific processes cannot lead to anything other than scientifically weak outcomes despite all the guarantees which should be present in hypothesis verification
 Further, treatments which are not tested (or which cannot be tested because they fall outside the guidelines) cannot be validated.
 Confusion between ‘not empirically validated’, ‘empirically invalid’ and ‘empirically unconfirmed’

FIFTH PARADOX: Can research improve therapy?

The essence of science is to VERIFY HYPOTHESES (justification)
 ... but how can NEW HYPOTHESES come to light (discovery), if research is geared towards evaluating what already exists?

possibility of demonstrating change through highlighting a transformation in behaviour would make everything that much more simple. We are aware that “some improvement criteria are more useful in experimental situations than others”, particularly “a reduction of observable symptoms”, often used in behavioural therapies, and are more useful to the researcher than, for example, those used to reconstruct, or re-define a problem’ (Minguzzi, 1986). We should not therefore underestimate in this regard the work of those psychotherapists whose work breaks epistemological laws in order to better empirically evaluate their objectives.

On this subject Dell’Erba (1997, p.80) states: ‘that even regressive evaluation research such as meta-analysis loses its meaning. Many meta-analytical works, even though they produce very consistent results seem largely irrelevant precisely because they are analysed in parallel with different psychotherapeutic disciplines: end of treatment criteria cannot be used because their equivalents do not exist. It is fruitless to try to translate measurements whose values and means of recording don’t lend themselves to alteration. We know full well (according to the like to like theory) that it is impossible to compare objects which belong in different worlds.

If true that the linguistic formula we use to express the problem ends up becoming the means by which we express ourselves and influences what we talk about, we might think that the question: ‘Who is the most effective psychotherapist?’ can be restated (remaining faithful to its spirit and internal cohesion) thus: ‘Which psychotherapist delivers what they promise?’ As we will see later on, regarding sliding scales, the effectiveness of a psychotherapist could be measured by using the criteria whereby self-defined objectives are met. Thus time and again we run the risk of setting up research programmes which satisfy the criteria of scientific correctness, and excluding those most needed.

Alternatives to the Evidence based approach

Comparison between Treatments

The “Treatment-placebo” method of comparison when used in clinical psychology research can result in a number of paradoxes. Indeed if there was a logical reason for the placebo, if it lacked credibility, patients would soon realize its existence and it would thus lose its effect (here there is a clear difference between its properties and those of the chemical being tested). On the other hand if the control conditions regarding the placebo were to be

believed, then its administration would produce results much more in line with an active treatment than with those of control conditions (Baskin, Tierney, Mine me, & Walpole, 2003). Thus the placebo, much used in psychotherapeutic research, becomes in reality a therapeutic treatment and loses its nature of placebo.

Another reason favouring comparisons between different treatments is that the current evidence-based protocols do not permit a means of treatment being removed from the list of approved protocols even if alternative procedures had proved their superiority when compared to treatments originally included on the approved list: in order to remain on the Evidence based list a treatment simply needs to work better than a placebo. For this reason clinical evidence-based research can be defined as a procedure to test out any given artificial treatment in an artificially controlled clinical context using atypical patients (Abloom & Jones, 2002).

We therefore suggest an approach which is clinically and ecologically legitimate in psychotherapy research wherein patients with similar DSM diagnoses are able to pursue different "active" treatments (without controls, placebos or waiting lists), and their progress is measured using not the traditional tests but rather a so-called 'sliding scale technique' (de Jong & Berg, 2001; Nardone, 1996; Nardone & Portelli, 2005; Nardone & Watzlawick, 1993, 2004) which highlights a satisfaction shared by therapist and patient alike at the final outcomes of therapy, and which yet respects the individual nature of a given approach.

'The sliding scale technique' (de Jong & Berg, 2001; Nardone, 1996; Nardone & Portelli 2005; Nardone & Watzlawick, 1993, 2004) therefore allows each treatment, independently of the chosen approach, to validate the results predicted at the start of the treatment. We are talking about a measure of value, and the intrinsic coherence of each psychotherapeutic intervention respecting its uniqueness and traditions; a measure of the correlation between what every approach promises and its final outcome. Thus we can quantify the quality (of a treatment using a sliding scale) and qualify the quantity (in other words by giving a clinical meaning to the value). To better understand this we quote the sliding scale technique as described by (Nardone & Portelli, 2005): "If you had to mark the improvement reached so far regarding your problem, 0 being the lowest, corresponding to when you came here asking us to help you with your problem, and 10 being the maximum – when you feel you can tell us, "Thank you, doctor(s), but I no longer need your help" – where would you place yourself now?" (p. 173).

It is important to underline that the use of such techniques is closely related to the internal workings of an advanced kind of research where key words are openness, evolution, knowledge through change, knowledge of the problem through its solution, absence of inflexible theories at the beginning. In fact Action-research allows self-corrective protocols, which can be repeated and are predictable to be developed: “the protocols are simple guidelines, which are far from being rigid and preordained. Protocols and strategic interventions are designed in a way that allows self-correction at any point of the therapy, since we are aware that the only way to really get to know a particular problem is through its solution” p. 170, (Nardone & Portelli, 2005). Such a framework differs from the traditional way research is carried out, where the recurrent words are justification, interpretation arising out of strong theories and a priori explanations and therefore and therefore give rise to systems which are self-protecting.

Practice-based evidence

Another alternative to the Evidence based approach is to move from an "evidence-based" practices to a "practice-based" evidence approach (Margison, Barkham, Evans, & al 2000) examining the results of the psychotherapy in natural situations with treatments which vary in length and are carried out with greater flexibility allowing the therapist to exert a greater clinical influence and to have a more active role. An evaluation of the results should not only consider a reduction of the symptoms but also factors such as ability to function, disabilities and quality of life (Margison et al. 2000)

Evidence-based practice and Hermeneutic Single-Case Efficacy Design

Amongst the alternatives to the Evidence based movement the Pragmatic Houses Study Method (Fishman, 2001; Fishman & Messer, 2004; Messer, 2004), is also interesting; this consist of systematic, largely qualitative case studies that focus on practical results. Also worth noting is the Hermeneutic Single-Case Efficacy Design (Elliott, 2001) which comprises the use of quantitative and qualitative information, direct and indirect evidence to create a rich case record.

Same approach - Different therapist

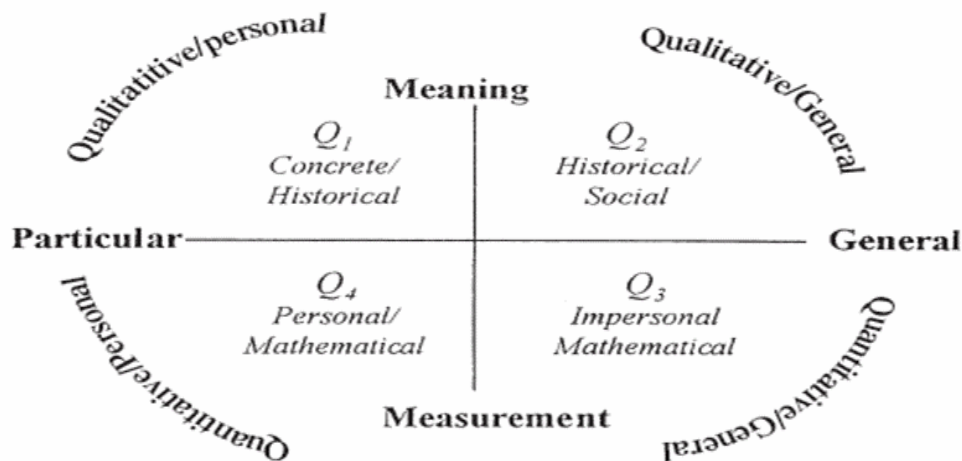
A further alternative is the evaluation of the effectiveness and the efficiency of the same approach-treatment when used by different therapists. A possible

alternative would be to evaluate performance both from the point of view of the patient and of the therapist (Starcevic, 2003).

Conclusions

To conclude this contribution to the Evidence based model we would like to emphasise that the term evidence itself does not just have one single definition. To this end we would like to reprint a recent classification of the different meanings of the term "evidence" depending on the various contexts in which it is used (Upshur, VanDenKerkhof, & Goel, 2001).

Table 5. Shades of the concept of evidence (part. 1) (reported by (Upshur et al. 2001)



Our contribution has not sought to discredit the Evidence based approach but rather to throw light on the limits of such a framework and on the alternatives that can be integrated with and complement such methods of research. Our position is in line with the recent letter to the publisher of (Westen, 2005), where the author reports that "the letter urges clinicians to consider science as a "close ally" rather than as an enemy, because it can be used to demonstrate that psychotherapy works and can help us identify those therapies that do work. But "science" already demonstrated that psychotherapy works, when Smith and Glass (1977) published their landmark meta-analysis, and the data remain just as clear today (Wampold, 2001) ... EBP > EST—that is, evidence based practice includes many forms of evidence other than data from RCTs (Wampold, 2001)" p. 7, (Westen, 2005)

Table 6. Shades of the concept of evidence (part. 2) (reported by (Upshur et al., 2001)

	<i>Illustrations</i>	<i>Evidence type</i>	<i>Reasoning style</i>	<i>Disciplinary manifestation</i>
Q ₁ Qualitative/personal	Attitudes Perceptions Signs and symptoms	Concrete Particular Historical	Narrative	Nursing Clinical medicine Ethnography Humanities
Q ₂ Qualitative/general	Policies Consensus statements Community and social goals	Historical Social	Narrative	Administration Social sciences Epidemiology
Q ₃ Quantitative/general	Traditional evidence Hierarchy	General Mathematical	Quantitative	Clinical epidemiology Bench sciences Statistics
Q ₄ Quantitative/personal	Bayesian Decision theory Quality of life	Particular Mathematical	Quantitative	Economics Political science Statistics

We conclude by remembering that, as well as being in a position to integrate the EST movement with that of other evidence -based methodologies, using the word evidence in its widest sense, we can also improve the EST system itself. Medicinal research, from which the EBM method derives, has a built-in retroactive mechanism by which the clinical effectiveness of a given treatment in the field is controlled. The EST movement, and psychotherapeutic research in, general, ‘do not currently predict this kind of built-in and retroactive mechanism being set up’ p. 258, (Lyddon & Chatkoff, 2002). Therefore "if it is true that the EST movement’s focus on the experimental effectiveness of psychotherapeutic procedures represents an important step forward..., it is also true that only a similar focus on clinical efficacy can lead to the public giving psychotherapy its complete trust’, p. 259, (Lyddon & Chatkoff, 2002).

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