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# The problem of validation of psychotherapy

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**Abstract:** *Some issues concerning the problem of efficacy of psychotherapy are reviewed and discussed. The difference between clinical research and empirical research is examined, also with some philosophical considerations. Clinical research and empirical research are very different: the former is conducted by the psychotherapist in the daily work with patients, while the latter is performed in the experimental laboratory. It is then discussed whether replicability, which is a central characteristic of the scientific method, is possible in psychotherapy. An overview of the history of the psychotherapy research movement is then traced: a first phase has been mainly dedicated to outcome research, while a second phase concerns mostly process research. Finally, some recent research on the efficacy of psychoanalysis is summarized, where it increasingly emerges that psychoanalysis and dynamic psychotherapy are not inferior, but sometimes even superior, to cognitive-behavioral therapy.*

**Key words:** *Efficacy of psychotherapy, Replicability, Psychotherapy and science, Research in psychotherapy, History of the psychotherapy research movement, Efficacy of psychoanalysis.*

What practical use do the results of experimental research in psychotherapy have for the daily work of the clinician? And, vice versa, what scientific value do they have and how generalizable are the observations made on single cases? Is there an impossible gap to fill, or can these two types of knowledge, so different, somehow interface and contribute to building a unitary science? Let us first clarify what is generally meant by research in psychotherapy, with references to some philosophical implications, and let's briefly review the history of the psychotherapy research movement, also referring to previous works (Migone, 1995, 1996, 1998, 2006, 2021, 2024).

Research in psychotherapy does not mean “clinical research”, a term that refers to a type of observations and hypotheses made by the therapist within the clinical situation. The therapist can share his observations with colleagues or in study groups, he can also make predictions and look for possible confirmations. He can write articles on these hypotheses, and some of them can be subjected to experimental validation in studies conducted with different methodologies, which belong precisely to what is commonly called “psychotherapy research”, also called “empirical” or “experimental” research. Some of its main features are the following.

First of all, with a term that has come into common use after Grünbaum's (1984) philosophical critique of psychoanalysis, these methodologies are “extra-clinical”, not just “intra-clinical”. This means that they rely on technical tools and on the observations of independent judges, external to the therapy and sometimes “blind” to the method used, who can use rating scales based for example on videotaped sessions. These rating scales, which are standardized, can concern various aspects: diagnosis (and there are different diagnostic methods; Migone, 2011, 2013), size of change, with the possibility that it may have been caused by events external to therapy, degree of “adherence” to the psychotherapy manual that the therapist had committed himself to follow, and so on. Not only that, but these external researchers can also conduct epidemiological studies to seek confirmation of certain hypotheses. Finally, and this last is one of the most important aspects, the data must always be subjected to an investigation of statistical significance.

“Extra-clinical” research (i.e., empirical or experimental) is therefore very different from clinical research because it uses sophisticated technologies that clinicians do not use, and performs quantitative assessments, generally on samples of many subjects, who are then subjected to statistical analysis. These assessments are not intuitive or “subjective” but, as we can say, “objective” (a term that, in a way, is inappropriate because it does not refer to a supposed “truth” or to a greater adherence to reality, but to the degree of agreement between multiple observers – as Freud himself said, reality as such is unknowable, we only see what our observation tools allow us to see).

Having clarified how these two types of research are different, we would be fooling ourselves if we thought we had solved all the problems. Indeed, it can be said that the real problems begin right here. In fact, we could ask ourselves: how do these two types of research relate to each other? Are they placed on irreconcilable levels or are they conceivable within a single hierarchical system? And furthermore: why is there a need to do extra-clinical or empirical research? Couldn't traditional clinical research be enough to guide the therapist, especially in a field as complex and not "objectivable" as psychotherapy?

Behind these questions lie age-old problems that have been widely debated throughout the 20th century, which are also reflected in other well-known dichotomies that have characterized the history of philosophy (Migone, 1998). First of all, there is the dichotomy, attributed to Dilthey, of the "two sciences", the natural sciences (*Naturwissenschaften*) and the human sciences (*Geisteswissenschaften*), the latter also called historical (Rickert called them *Kulturwissenschaften*, and Windelband *Geschichte*). Parallel to this dichotomy is the one between "explaining" (*Erklären*) and "understanding" (*Verstehen*), and also between "causes" and "reasons" of a behavior. This issue has been taken up by hermeneutics, which emerged in the psychoanalytic debate in the United States in the 1970s and 1980s (the best-known hermeneutic psychoanalysts were Roy Schafer [1976, 1992] and Donald Spence [1982], in the wake of some European philosophers, essentially Ricoeur and Habermas). Another dichotomy that in some ways follows the previous ones is that between "quantitative" and "qualitative" research, and yet another, perhaps even better known, is that between "nomothetic" and "idiographic" sciences, proposed by Windelband. Nomothetic sciences aim to construct general laws (the Greek term *nomos* means "law"), and therefore to make it possible to predict certain phenomena (typically, nomothetic are the natural sciences), while idiographic sciences study those unique phenomena (*idios* means "particular"), unrepeatable (an example is the study of personality or, indeed, of psychotherapy), for which the methods of nomothetic sciences would not only not find easy application, but would even prevent us from seeing new phenomena, blinded as they are by their own limited methods of observation. In a certain sense, it can be said that nomothetic methods risk discovering only what is already known, while idiographic ones, even if not very reliable, can sometimes lead to unexpected discoveries (think, in this regard, of the difference between objective and projective tests).

If we look closely, the complex question behind these dichotomies concerns the very concept of science: should it be characterized by its method, or by the field of application to which the method must adapt?

In the first case, there would be a single method (called scientific), which could adapt better to certain fields rather than others. As some critics of this concept of

science argue, the classical scientific method (characterized by experimentation, prediction, replicability, etc.) would lend itself more to the study of “natural” phenomena (for example physical) than to subjectivity where the complex variables at play make it difficult to use the traditional scientific method; furthermore, psychotherapy sometimes employs methods that are not easy to investigate experimentally, such as introspection or intuition. If – some critics continue – we force the observation of the data of psychotherapy through the lenses of this method, we risk distorting them or losing something that perhaps constitutes the very essence of the phenomenon we want to study (an example can be represented by behaviorism, which in fact, even according to many of its own exponents, has shown its limits as a form of psychotherapy). The result would be a reductive or even distorted image of the object studied.

In the second case, that is, if we adopt a conception of science according to which it is the method that must adapt to the field of observation, we would have many scientific methods, many “sciences”, with the result that the dichotomies we were talking about would reappear, and a fragmentation of knowledge would be created, with complex implications, some of which are precisely those we are discussing.

This is certainly not the place to resolve these issues, but only to stimulate some reflections. It can be argued that in recent decades, thanks also to a greater knowledge of the scientific method (even on the part of many philosophers, who had an abstract conception of it and not grounded in the concrete practice of research), a sort of reshuffling of the cards has occurred with respect to these dichotomies, in the sense that they are no longer as clear and sharp as they once were. For example, the belief that the nomothetic/idiographic opposition is a false dichotomy, originating from a romantic reaction to a 19th-century conception of science that is now outdated, is increasingly widespread. This is, for example, the position taken by Robert Holt, a research psychoanalyst, later David Rapaport’s successor and leader of his group, in a work he wrote in 1962 in which he criticized the legitimacy of the idiographic method. At the time, Holt was a student of Gordon Allport, who was a strong supporter of the idiographic method in personality theory. Holt, clashing with his master, came to the conclusion that, as soon as we simply observe and describe a phenomenon idiographically, we cannot help but use certain conceptual categories that no longer belong to this method. Without these categories, we could not even communicate our observations to other colleagues. According to Holt, the idiographic method would therefore have only artistic, not scientific, purposes, since it is limited to understanding and not to prediction and control (see Holt, 1989; Holt *et al.*, 1994).

To return to psychotherapy research, to say that there has been a reshuffling of

the cards between the dichotomies mentioned above does not mean that the difference between clinical and extra-clinical research disappears, or that clinical research should be abandoned in favor of empirical-experimental research only. On the contrary: it would be a mistake to neglect, as many research programs and scientific journals unfortunately do, clinical studies on single cases, because they have enormous heuristic implications, and sometimes even the power to falsify certain hypotheses. Let us not forget that a good part of psychotherapeutic hypotheses originated from a number of clinical cases that can be counted on the fingers of one hand, studied in depth by Freud, Binswanger and others.

The history of both medicine and psychiatry presents countless examples of how the method based on “clinical experience” and single cases has led to errors. We know that, for example, at the end of the 19th century most medical interventions were useless if not harmful, and yet they continued to be practiced with a rich series of self-confirmations and with respect coming from the “scientific” community of the time (Migone, 1995, ch. 6). A striking example in this regard is psychosurgery, whose successes were boasted by a generation of psychiatrists and neurologists, as well as by nurses and patients’ families, all convinced of its extraordinary efficacy, so much so that in 1949 its inventor, Egas Moniz, was awarded the Nobel Prize. But the scientific community was greatly embarrassed when the progress of research made psychosurgery disappear overnight: it emerged that the vaunted therapeutic successes were self-deceptions, based on observations without independent control (Pressman, 1998). This is not surprising, after all, if one considers that the first published randomized controlled trial (RCT), i.e., controlled by placebo in a double-blind condition, was the English study on streptomycin for tuberculosis in 1948, so in recent times. In certain cultures, harmful or useless methods have continued to be used for centuries or millennia (just think of blood-letting), and it is not clear why this cannot also happen for psychotherapy. It was only with the generalization of the advances of scientific revolution that a dramatic improvement was made in the identification of the most effective therapies, leading to the eradication of many diseases and saving entire populations from epidemics. The scientific method, based on double-blind controlled studies and on specific extra-clinical methodologies, allows us to break the daily self-deception that is always lurking in front of the clinician. And to the extent that the experimental method allows us to circumvent, at least partially, this constant self-deception fostered by our unconscious expectations, in this respect it is reminiscent, in some ways, of the psychoanalytic method, because it helps us to combat our false consciousness, to see what we sometimes defensively do not want to see.

In this light, Freud’s statement in a 1934 letter to the psychologist Saul Rosenzweig, who had sent him the results of his experimental studies in favor of

the theory of repression, is surprising: «Dear Dr. Rosenzweig, I have examined with interest your experimental studies on the scientific validity of psychoanalytic assertions. I cannot attach much value to these confirmations because the abundance of reliable observations on which these assertions rest makes them independent of empirical verification. However, they cannot do any harm» (cited in: Wallerstein & Fonagy, 1999, p. 91). On another occasion Freud stated, with ill-concealed sarcasm: «These critics who limit their studies to methodological investigations remind me of those who spend their time cleaning their spectacles rather than wearing them to look» (cited in: Jacoby, 1983). But perhaps Freud underestimated the self-deceptions mentioned above, and the possibility that research could invalidate a given theory (he himself, moreover, consistently modified his own theories in the light of new clinical experiences).

Here the underlying philosophical problem is that of inductivism, that is, the possibility of inducing, starting from particular observations, general laws that – through deduction – then allow us to make predictions about other cases. Needless to say, there has been a heated debate in this regard that has seen opposing positions, a debate that cannot be said to have completely died down. I am referring, for example, to the harsh attack by Grünbaum (1984) on Popper (1957) – who never responded – which cannot be summarized here (see Migone, 1995, ch. 11).

The challenge, in short, is to see if it is possible in some way to bridge, as mentioned before, the gap between clinical and experimental research, a gap – the “great divide”, as some have called it (see Carere-Comes & Migone, 2001-03) – that unfortunately still separates the world of clinicians from that of researchers. In fact, clinicians often shy away from empirical research, saying that they don’t need it, that it doesn’t interest them, that for them research is only clinical research, based on their “experience”, or that psychotherapy is an art. The difficult coexistence of these two worlds has been debated countless times (for a debate that lasted several years, which ended with disagreements, see Carere-Comes & Migone, 2001-03; see Luyten *et al.*, 2006). This great divide should be bridged in some way, following, for example, the research path of David Rapaport’s group (Blatt, 2004, 2006, 2008; Holt, 1989; Migone, 1998, 2015; Rapaport, 1942-60; Rubinstein, 1952-83). In fact, both research methods should be used because, in a complementary way, they contribute to the progress of knowledge, and it is not correct to see the two methods in dichotomous terms, but as a *continuum* of ways of knowing. These methods have different purposes, and correspond to different types of cognitive functioning, different ways in which our mind processes information, and as such both should be valued. It could be argued that the clinical method can correspond to a first phase of research, of an inductive type, in which it is essential to formulate hypotheses that can then be tested with the experimental method, which however does not

lead to the “truth”, which by definition should never belong to science (if anything, the problem of truth concerns philosophy); science, even for its limited horizons of investigation, is characterized by great modesty and awareness of its own ignorance, and this has always been its strength.

Not only that, but if we look closely, the problem of the coexistence of different “sciences” or methods of investigation does not exist only with regard to the relationship between human sciences and natural sciences, since it already exists within the latter, where we have many research methods, each of which – as argued by the Italian philosopher Evandro Agazzi (2006) – produces or “constructs” its own “scientific *object*”, to the point that the problem remains, it only shifts. For example, with regard to the different methodologies of studying the psychotherapy process, we could ask ourselves: how do all these methods relate to each other? Is there a “better” or hierarchically “superior” method, or one that measures the “true” therapeutic process? If by science we do not mean, in a reductive way, only a method that is applied to objects that lend themselves to being investigated with that method, it does not depend on the type of objects it deals with but on the “way” in which it deals with them. As Agazzi (2006) argues, this method can be called “scientific” to the extent that it satisfies certain criteria such as “rigor” (giving reason for what is stated, not necessarily through quantification, measurement, etc., and using a specific language and logic), “testability”, “objectivity”, “protocolarity”, etc. Each approach to knowledge, however, as has been said, constructs its own “scientific object” which is different from those produced by other approaches. Again according to Agazzi, this scientific object should not be confused with a “thing”, in the sense that the same thing can be the “object” of different sciences, so a thing is transformed into a “bundle” of potentially infinite objects: for example, the fact that new methodologies are always being created that study a certain thing does not mean that the number of things in the world has increased, but that new “points of view” on that thing have been identified (for example, the mind can be studied with the methods of neuroscience, with projective tests, with introspection, and so on; in the same way, psychotherapy can be studied with a purely clinical methodology or by quantifying certain variables). Therefore, each scientific approach, understood as a “point of view”, cuts out or reduces reality according to its own methods, building a different *object*. From this comes the fact that, concretely, each approach, that is, each point of view, translates itself into the identification of its own methods of investigation. This is important because – as Agazzi (2006, pp. 64-65) argues – it helps us understand that the dispute between different schools with the mutual accusation of being unscientific is meaningless. This dispute would make sense if opposing methods dealt with the same object, while this is not the case: each method deals with different “scientific objects”, it carves out its own

object, so «adhering to one methodological choice rather than the other simply means deciding to deal with something more or less different or, if you want, to do another psychology» (p. 65). To give an example, it makes no sense for the behaviorist to accuse of methodological incorrectness those who use introspection, in fact the conflict between different methods is «only apparent when it is understood that it translates into a differentiation of objects and is not a fight about how to take possession of a single and identical object» (p. 65). Moreover, this problem is not new nor does it belong only to psychology, but also to the “hard” sciences: think of physics, where once it was believed that there was only Newtonian mechanics, but then it was realized that there is also electromagnetism, quantum physics, etc., all disciplines that deal with the same things but construct different scientific objects; they are “many physics”, even if they coexist within physics as a discipline.

It could be said, therefore, that psychology from an epistemological point of view finds itself in the same condition as physics (which would also go against the division between soft and hard sciences), and the same obviously also applies to psychotherapy, which is an application of psychology: the problems that the epistemologist finds himself having to face when he reflects on the way in which the different psychotherapeutic schools relate to each other would be the same ones he faces when he reflects on the way in which the different “physics” that coexist in physics relate to each other. And the problem of the relationship that the different “scientific objects” of psychotherapy have between them is not easy to solve, and above all it should not be forgotten that each of them does not completely reveal the “truth”, since they are all bearers of partial and reductive knowledge, useful only for the purposes for which these “scientific objects” were “constructed” (for a criticism of Agazzi see, however, Fornaro, 2013).

If we take this point of view to its extreme consequences, not only could the plurality of psychotherapeutic models be a good thing, because each of them legitimately explores an aspect of the patient’s reality, but, one could say, it would also be a good thing to never arrive at a single psychotherapeutic model that is “truer” or “better” than the others, because this could mean that we have come to know the patient’s ultimate reality, when we know that reality by definition is unknowable (as has been said, this was also Freud’s position). In other words, flattening our field with a single model could inhibit the process of knowledge, which is interminable (Migone *et al.*, 2012).

It should not be forgotten that the experimental method can also lead to errors, because there are various ways of implementing a research and analyzing the results, and further research can also correct previous results. And it must be said that a therapeutic practice guided only by empirical research data is premature because too many data still escape most of research. The fact that something has not



yet been demonstrated does not mean that it cannot be demonstrated in the future, and certain reviews of the literature seem to imply that if a technique has not yet been studied this means that *it has already been proven ineffective*. We must therefore be very careful not to draw hasty conclusions from research, also because there are many biases and a large “halo effect” created by the way in which certain data are publicized by mass media. In this regard, Westen, Novotny & Thompson-Brenner (2004) have made a magisterial criticism of the methodology that produced the lists of Empirically Supported Treatments (EST) (Chambless & Ollendick, 2001), whose logic is derived from the RCTs of Evidence Based Medicine (EBM). Westen and colleagues have shown that the assumptions on which the EST methodology is based are not theoretically neutral but reflect the fundamental assumptions of cognitive-behavioral therapy of the 1960s and 1970s (and these assumptions are no longer accepted today by many exponents of the cognitive-behavioral therapy movement itself), and that these assumptions are empirically testable but, paradoxically, many of them have never been tested; among those tested, some *have been shown to be false* precisely on the basis of empirical research itself. Also important in this regard are the criticisms of Wachtel (2010) and Shedler (2018); see also Migone (2021, ch. 6 and 7).

With regard to the EBM paradigm, it should be noted that to the extent that its explicit statute proposes to eliminate any type of intuition or “clinical experience” but to rely solely on controlled research (De Girolamo, 1997), it seems to commit the same and opposite error of the paradigm it seeks to combat: if on the one hand we have a clinical practice that is only intuitive, on the other it recommends an impersonal and automated practice, without any integration or dialectic of the two poles and therefore with an impoverishment of real clinical practice which by its nature is based on complex cognitive operations (see Sackett *et al.*, 1996).

## The issue of replicability

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Replicability, perhaps more than other aspects, characterizes science. Without wanting to delve deeply into the age-old debate on the “theory of demarcation” between science and non-science, if the word science has any meaning it must refer to a phenomenon that is to a certain extent replicable, experimentally controllable. Curiously, however, as argued by the physicist Bersani (2008), even in the “hard” sciences (such as physics, chemistry, etc., and not only in the soft sciences such as psychotherapy) there is no experiment that is perfectly replicable, due to a thousand variables that continually modify the conditions of the experiment. We would therefore have one more reason to question a clear difference between hard and soft sciences, because these would be experiments that are more or less perfectly

replicable. In certain physical phenomena, it is not possible to predict the movement of individual particles (that would be hard to replicate), but it is possible to predict fairly well the macroscopic phenomena produced by the sum of those same microscopic particles. An example is that of a gas, whose general laws can be studied but the movements of individual particles cannot; another example, which is part of everyday life, is the porcelain cup fallen on the floor that breaks into a thousand pieces – a phenomenon certainly replicable – of which we cannot, however, predict the number, the dimensions or the distribution on the floor.

This brings to mind the well-known “uncertainty principle” developed by Werner Heisenberg in the 1920s, according to which infinitesimal particles do not have a position and a velocity defined simultaneously: the greater the precision with which their position is determined, the less precision is established with which their velocity is established. In other words, the observer influences the observed object, and this is the reason why Heisenberg’s uncertainty principle is often cited by those psychotherapists who sympathize with relational or intersubjective approaches. But these psychotherapists are wrong because Heisenberg’s uncertainty principle does not apply to macroscopic phenomena but only to microscopic ones (to molecular phenomena but not to “molar” ones, one might say; see Pauri, 2000), and this difference seems to be counterintuitive because one might think that macroscopic phenomena are more complex than microscopic ones and therefore less controllable. But here comes into play the “theory of complexity”, according to which in complex and “chaotic” phenomena there are regularities that are not easily explained by the detailed analysis of the individual components.

Apart from this, it should be noted that there are interesting examples of “scientific” discoveries that were later unmasked and demonstrated to be “pseudoscientific” (Bersani, 2008, p. 67). However, if we use the term “pseudo-science”, we automatically put ourselves in the position of those who believe in science as different from something that it is not, so we are back to square one, that is, the question of demarcation arises again. And if we had previously said that perfect replicability does not exist, the problem of establishing what we mean by science arises again.

It seems that we are therefore faced with an impasse, but it is only apparent, since some form of replicability must exist, and not only in the hard sciences, but also in psychotherapy, otherwise it would not be teachable. Certainly, this replicability is never perfect, but to some extent it is there, certainly at a macroscopic level. We can give many examples in which certain patient behaviors are reproduced regularly due to certain traumas as well as certain more or less structured and “replicable” therapeutic interventions, called precisely psychotherapy (for a detailed clinical example in which one can see how in psychotherapy one can make

hypotheses and then test them empirically, see Migone, 2021 pp. 29-34, 2008b pp. 79-83). In short, psychotherapy would be a natural phenomenon, that can be studied just like in medicine. It should also be remembered, as mentioned above, that psychotherapy and medicine are not two sciences but two “applications” of basic sciences, in which complicated factors intervene such as the interpersonal relationship, which is the core of psychotherapy and which makes it more complex in terms of scientific experimentation.

## Notes on the history of the psychotherapy research movement

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Trying to understand which hypothesis is the most probable among rival hypotheses is the aim of the psychotherapy research movement, especially starting from Eysenck’s healthy provocation of 1952 according to which the effect of psychotherapy is irrelevant, useless. Incidentally, Eysenck was careful never to say that psychotherapy is harmful, otherwise he would have been forced to admit that it could be effective. According to Eysenck, improvements occurred only thanks to the “mere passage of time”, that is, by “spontaneous remission” of diseases due to the natural oscillations of their course. This hypothesis implies that therapists, when sooner or later they encounter the positive oscillation of the disease (so the longer a therapy, the better it serves this purpose), take credit for it. According to this hypothesis, one could say that therapy works because of the “streetlight effect”: we put the patient for an hour a week sitting on a bench lit by a streetlight, and it is the streetlight that heals the patient, who, when he feels better, gets up from the bench-couch and goes home, happy to have been healed by the streetlight-analyst.

Well, it was not easy to prove that Eysenck was wrong, thirty years of research were needed, in fact the first data emerged only from the studies made by Smith, Glass & Miller in 1980 (Smith *et al.*, 1980). However, in what is considered the first phase of psychotherapy research, defined as “outcome research”, an embarrassing situation was reached, which was called the “equivalence paradox”: all psychotherapies on average obtained the same results. In other words, «everyone wins and everyone deserves a prize» (Luborsky *et al.*, 1975, 2002; Rosenzweig, 1936) as the Dodo bird said after calling a race in *Alice in Wonderland*. This equivalence is known as the “Dodo verdict”, and it continues to be a specter that haunts researchers, especially those who have a faith in one of the many psychotherapy schools. One of the causes of the Dodo verdict lies in the difficulty of identifying methods capable of measuring change with sufficient precision.

It is because of the Dodo verdict that we have moved on to a second phase in

the history of the psychotherapy research movement, called “process research”, in which research on outcome has been abandoned, considered useless since it was not known which “process” produced the outcome. In other words, we studied what really happens in the therapeutic interaction, since it is not enough for a therapist to say that he does a “psychoanalysis” (or a “cognitive therapy”, etc.) to be certain that he does it; indeed, it has often been shown that at times a therapist does something completely different from what he says he does. Westen, Novotny & Thompson-Brenner (2004), for example, show that sometimes patients improve thanks to interventions that belong to another technique. One study showed not only that both cognitive and psychodynamic therapists used techniques from the other approach, but that in both cases the outcome was correlated with interventions typical of psychodynamic therapy, that is, the fact that cognitive therapists used cognitive techniques was not correlated with outcome (Westen *et al.*, 2004, p. 639, p. 124 in the 2021 edition).

It is for this reason that in the second phase of psychotherapy research, there was a boom in the manualization of various techniques, precisely to ensure that therapists were doing exactly what they claimed to do and not something else. The first psychoanalytic manual for research was Luborsky’s 1984 manual for “supportive-expressive” treatment, and many others followed, such as Inter-Personal Therapy (IPT) by Klerman *et al.* (1984), Transference-Focused Psychotherapy (TFP) by Clarkin, Yeomans & Kernberg (1999), Dialectical Behavior Therapy (DBT) by Marsha Linehan (1993), and so on (the names of the manuals generally refer to specific techniques, not to general theories; in fact, the same theory – for example, psychoanalysis – can inspire different manuals depending on the author who constructs them, the target diagnosis, etc.). The first manuals to be built were naturally those of behavior therapy, because they were simpler (they become almost pre-established algorithms of interventions, the so-called “procedures”). Manuals are built only for research, often *ad hoc* for a specific study, and should not be confused with books on clinical technique: as examples of books on psychoanalytic technique for clinical practice, think of Menninger (1958), Greenson (1967), Etchegoyen (1986), etc. – books on psychoanalytic technique for clinical practice are not very many, and not by chance precisely because of the difficulty in explaining or “prescribing” detailed interventions that are valid for the entire course of treatment (in fact, as Freud [1913, p.123] said, treatment is a bit like a game of chess, in which the opening and closing moves – checkmate – can be described, but it is very difficult to predict the intermediate moves). It is no coincidence that some have defined psychoanalytic technique manuals as “collections of errors” (and the errors can be much greater in research manuals, because they are much more detailed; see Migone, 1986). In this regard, it is quite interesting a beautiful statement by

Helmut Thomä, which Kernberg, with wisdom and self-irony, wanted to use as an epigraph to his own therapy manual for borderline patients: «This treatment book should be memorized, and then forgotten» (Clarkin *et al.*, 1999, p. V).

Manualization is only one aspect of the phase of process research; there are other aspects, for example the construction of several rating scales, even very sophisticated ones, precisely to measure the process: today the best known are at least twenty (see Dahl *et al.*, 1988; Dazzi *et al.*, 2006; Levy *et al.*, 2012; Migone, 1995, p. 225 note 15 of the 2010 edition) just think of Luborsky's *Core Conflictual Relationship Theme* (CCRT), which is an “operationalization” of transference for research purposes, a sort of bridge between the qualitative and the quantitative (Luborsky & Crits-Christoph, 1990), Wilma Bucci's *Referential Activity* (1997, 2019), the scale to measure the reflective function studied by Fonagy *et al.* (2002), the *Shedler-Westen Assessment Procedure* (SWAP) by Shedler & Westen (2003) which allows a narrative formulation of the clinical case, and so on.

To return to psychotherapy manuals, they have advantages and disadvantages. The main advantage is that they allow one to do research, identifying a phenomenon that can be replicated, while the disadvantages are many: think of the excessive rigidity that can go as far as to distort psychotherapy itself, or the “efficacy” which however is achieved at the price of a low “effectiveness”, that is, poor results are obtained in the phase of “exporting” the technique from the laboratory to real clinical practice (what can also be called “external validity”), in which the patients are not selected and therefore for example present a comorbidity that was not present in the research sample (so much so that, ironically, one can say that research is on a third of the patients while clinicians see the remaining two thirds, the most difficult ones and excluded from the researchers' rigid criteria). In short, there seems to be a curious paradox: the better a research is done, the less useful it is to the clinician, in the sense that the required methodological rigor distances too much from real clinical practice, which necessarily is not very rigorous and is “contaminated” by a thousand factors.

For further information, I refer to the article by Westen, Novotny & Thompson-Brenner (2004), cited above, and to other reviews (see Migone, 1996, 2006). Among the main international reviews on psychotherapy research, we can mention that of Roth & Fonagy (1996), and the “bible” of researchers, the *Bergin and Garfield's Handbook of Psychotherapy and Behavior Change*, now in its seventh edition (Barkham *et al.*, 2021). An important manual is that of Levy, Ablon & Kächele (2012), which has the subtitle “evidence-based practice and practice-based evidence”, which offers an in-depth overview.

## The efficacy of psychoanalysis

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After this overview of some problems in psychotherapy research, one might ask: what is the evidence of the efficacy of psychoanalysis? It is certainly not possible here to carry out a detailed review of the literature, so only some general observations will be made that summarize the trends that emerge from research. From many quarters – in books, journals, conferences, and in the mass media that act as a sounding board – it is often stated that cognitive-behavioral therapy is the most effective, as if this were an incontrovertible fact. But in recent years more and more evidence has accumulated showing that therapies derived from psychoanalysis are effective. The superiority of cognitive-behavioral therapy may have seemed true years ago when there was not yet enough research on psychodynamic therapy – and, as has been said, many made the mistake of believing that if it had not yet been studied it meant that it had been proven ineffective – but when the psychoanalytic movement entered into the arena of empirical research, things soon changed: not only did psychodynamic therapy prove to be not inferior to cognitive-behavioral therapy, but it was shown that at times the results of psychodynamic therapy can increase over time, as if the patient internalizes certain abilities whose effects mature gradually.

It should also be considered that cognitive-behavioral therapy, in a way, does not exist, there are different types, in the same way that psychoanalysis does not exist but there are *many psychoanalyses*. It is therefore necessary to be clear about the meaning of the terms. For example, within the cognitive-behavioral therapy movement, the so-called “third wave” is very popular (the importance of “acceptance” of the patient’s emotional states is recognized, the technique of mindfulness and other practices derived from Eastern philosophies have spread, and so on; see Migone, 2008a), a phenomenon that can also be read as the rediscovery of psychodynamic ideas and therefore as a sign of crisis or rethinking in the cognitive-behavioral movement (moreover – as important exponents of cognitive therapy itself have admitted, for example Kazdin (2007, p. 8) – the low efficacy of cognitive therapy had already been recognized, as it focused on cognitions while underestimating emotions). In short, we are witnessing hybridizations and assimilations of ideas and techniques taken from other approaches, often without recognizing their debt.

And if we must specify the terms we use, what is then meant by “psychodynamic therapy”? This term is not intended to be understood in a generic but precise way, and not based only on theoretical but empirical research. In fact, this term refers to a technique based on psychoanalytic principles and defined by “seven distinctive characteristics” as they emerged from empirical studies capable of reliably distinguishing it from other techniques, such as cognitive-

behavioral therapy (Shedler, 2010 pp. 98-100, pp. 48-51 of the 2021 edition). And just as cognitive-behavioral therapy is generally defined with the acronym CBT, the research community has decided to use the acronym PDT for psychodynamic therapy, but in a more precise way than for CBT which, as we have seen, is an “umbrella” term that includes different techniques, some of which also include techniques derived from psychoanalysis (think of Schema Therapy, which is considered a CBT technique but avowedly uses techniques derived from Gestalt therapy, psychoanalysis, etc.).

The now famous review by Shedler (2010), which reports the “effect size” of various types of psychotherapy, shows the efficacy of psychodynamic therapy on the basis of several “meta-analyses”, i.e. “analysis of analyses”. And this research by Shedler was followed by others (see, for example, Cuijpers *et al.*, 2021). Regarding depression, which is one of the most common disorders, it is also interesting to compare it with antidepressant drugs, that on average have a decidedly lower efficacy (Migone, 2005), which implies that a treatment for depression that favors the use of drugs – as unfortunately often happens – is in fact malpractice. Vast sectors of academic culture and many specialty schools of psychiatry, to the extent that they emphasize the use of drugs and neglect the importance not only of psychotherapy but also of the patient/therapist relationship, are complicit in this antiscientific approach, based on ignorance of research. There are many studies on the efficacy of psychodynamic therapies that could be mentioned here (among the many references, see for example Steinert *et al.*, 2017; Leichsenring & Steinert, 2017; Leuzinger-Bohleber & Kächele, 2015; Leuzinger-Bohleber & Target, 2018; Levy *et al.*, 2012; Migone, 2021; etc.). Also very useful is the third edition of the *Open Door Review of the International Psychoanalytic Association* (IPA) (Leuzinger-Bohleber & Kächele, 2015), with introductory interventions in which some epistemological issues of research in psychoanalysis are discussed.

Finally, a mention of psychiatry is necessary. There is strong empirical evidence showing that a psychiatry based solely on a “technological paradigm” (privileged use of drugs, instrumental tests, etc.) is less effective than a psychiatry based instead on careful listening to the patient and understanding the symptoms within his life history and interpersonal relationships. It is a big misunderstanding to think that psychiatry is a medical specialty comparable to high-tech specialties such as anesthesiology or ophthalmology (and moreover it has been demonstrated that also in medicine – even in surgery – attention to the psychological relationship with the patient has an impact on improvement); the “specialty” of psychiatry also consists in the fact that, as Michael Balint (1956) once said – the psychiatrist administers himself as a drug, that is, it is a matter of working carefully on the variables of the interpersonal relationship, because it is from relationships that most mental

disorders originate. In short, thinking that complex psychological or personality problems can improve only with the administration of some pharmacological “poisons” is a myth, well fueled by multiple interests that are intertwined: the identity insecurity of psychiatrists in such a difficult profession, the need for illusion of many patients, and above all the propaganda of pharmaceutical companies that heavily influence the culture of the field (Migone, 2017). There are many contributions that could be mentioned here (American Psychological Association, 2013; Angell, 2011a, 2011b; Bracken *et al.*, 2012; Whitaker, 2010).

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