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CAGE UP DODO, PLEASE! THE TALE OF ALL PSYCHOTHERAPIES BEING EQUALLY EFFECTIVE

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El veredicto del pájaro Dodo afirma que cuando las psicoterapias son comparadas entre sí ofrecen resultados similares, lo que sería consistente con la visión de que los factores comunes son los ingredientes más potentes para conseguir los beneficios de las psicoterapias. Este trabajo revisa el debate en torno a este asunto, resalta algunas cautelas en las conclusiones que se pueden extraer de él y discute, con el reconocimiento de las debilidades de la investigación en este campo, los motivos por los que el debate sigue vivo. Finalmente, se sugiere cómo esto puede contribuir a la investigación y a la atención a los pacientes.
Palabra clave: Veredicto del pájaro Dodo, Equivalencia de frotamientos, Meta-análisis, Investigación en psicoterapia, Factores comunes.

The Dodo bird verdict states that when the psychotherapies are compared with each other they yield similar outcomes, which is consistent with the view that common factors are the most potent ingredients for producing the benefits of psychotherapy. This paper reviews the debate around the issue, highlights some caveats in the conclusions that can be drawn from it, and discusses, with recognition of the weaknesses of the research in this field, the reasons for the debate still being alive today. Finally, a number of suggestions are offered with regards to how this can contribute to research and to patient care.

Key words: Dodo bird verdict, Treatment equivalence, Meta-analysis, Psychotherapy research, Common factors.

Dodo is the most quoted bird in the field of psychology. This Alice-in-Wonderland character solves a race with a senseless approach (each participant beginning where and when they like, and stopping when they felt like it) with the statement "Everybody has won, and all must have prizes". In psychology, the Dodo bird verdict represents metaphorically the idea that all of the psychotherapies obtain equivalent results. Thus it has been postulated that, in the efficacy of treatments, the prevailing factors are common factors relating to the patient, the therapist, the relationship between the two, the structure of the therapy or the therapeutic process itself, rather than the specific components of each therapeutic approach (Wampold, 2007).

This article aims to review the history of this debate, highlight some of the limitations that must be contained in the conclusions which can be gained from the debate and, finally, argue about the reasons why the debate continues, as in the tale, without a clear end.

THE HISTORY OF THE DODO VERDICT

The first reference of the Dodo verdict is found in 1936

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in an article by Rosenzweig (1936), which contains the first formulation of the preponderance of the common factors in psychotherapy. Some decades later, the idea was revived in a qualitative review of the literature which concluded that there are no differential effects between the treatments (Luborsky, Singer, & Luborsky, 1975). Smith and Glass (1977) were the first to carry out the first quantitative review (meta-analysis) that attempted to summarize the data from 375 studies. They concluded, on the one hand, that all psychotherapies were more effective than no treatment; secondly, that the differences between the schools were negligible in practice, such that, ultimately, it would not be justifiable to claim that some were superior to others. Although the effect size among therapies is large for some (0.91 for systematic desensitization) and small-to-medium in others (0.26 for Gestalt therapy), the conclusion is based on the grouping of therapies as behavioral and non-behavioral therapies (including, together with Rogerian and psychodynamic therapies, some cognitive ones). Perhaps because the meta-analysis had not yet reached the level of veneration it has today, Hans Eysenck, known for his controversial statement on the ineffectiveness of all psychotherapies (except behavioral therapy), referred to this study as "mega-silliness" (Eysenck, 1978). The controversy has continued since



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then with analysis, meta-analysis and analysis of meta-analyses. Each study was followed by a replication (not strictly in the experimental sense) that questioned the previous findings based on an analysis, in many cases, with new criteria.

In response to the criticism that the study of Smith and Glass (1977) received, mainly due to the inclusion of studies of low methodological quality, Shapiro and Shapiro (1982) designed a new meta-analysis using the studies that included comparisons between treatments plus a control group. Of the 143 studies examined, they found differences in the effect size ranging from 1.06 (equivalent to a large effect size) for behavioral and cognitive therapies to 0.40 (small to medium) for psychodynamic/humanist therapies, although the latter are poorly represented and may have been interventions using a "straw man" comparator, designed to offer no therapeutic results. The authors' conclusion is that there are modest differences between the treatments. Within a general critique of the meta-analytical procedure, Wilson and Rachman (1983) criticized the study by Shapiro and Shapiro (1982), saying that the analysis was unrepresentative of clinical research and practice. Shadish, Matt, Navarro and Phillips (2000) analyzed the studies that were representative of patients and treatments in "real life". They found moderate differences between treatments (the mean effect size between treatments was 0.41) in favor of behavioral treatments versus non-behavioral ones, and the effect was greater when primary outcomes were used.

The meta-analysis by Wampold et al. (1997) represents the most direct test of the Dodo verdict. Unlike the previous attempts, this included only studies that compared "bona fide" psychotherapies with each other, i.e., treatments in which there is a clear therapeutic intention, offered by trained therapists, with psychological basis and available for the therapist community. (These could be described as fully-fledged psychotherapies.) Moreover, they avoided classifying the treatments into general categories (e.g., behavioral, psychodynamic, etc.) to avoid questioning the validity of these categorizations. While the mean effect size of the different therapies was 0.19 (i.e., small), a second analysis, measuring homogeneity using the Q statistic, in accordance with the authors' hypothesis, reduced the mean effect sizes virtually to zero, confirming the equivalence of psychotherapies.

The study by Wampold et al. (1997) has been criticized for several reasons. First, it has been observed that about 69-80% of the studies included in this meta-analysis involved comparisons between different forms of cognitive-behavioral therapy (cognitive therapy, desensitization, exposure, relaxation, skills training, etc.), which could mask the differential effects with other therapies (Crits-Christoph, 1997; Hunsley & Di Guijo, 2002). Furthermore, according to Crits-Christoph (1997), only about 45% of the 114 articles had a DSM disorder as the object of treatment, and about one third of the studies had preclinical student samples. Thus, of the 114 studies, Crits-Christoph (1997) retrieved 29 independent studies that did not compare different forms of cognitive behavioral therapy and did not include students. In this subsample, at least 14 studies offered some significant differences between studies with a large effect size (Crits-Christoph, 1997). Wampold et al. (1997) point out, appropriately, that within a set of nearly 3,000 dependent variables, counterexamples can always be found if we carry out a post-hoc selection of a number of variables. Generally, the response to the criticism received rests on the idea that the differential effects of therapies are, at best, weak. The Earth is not perfectly spherical, they say, but this does not mean it is flat (Wampold et al., 1997). A simile that may be used both to defend and to counter the arguments in favor of the Dodo verdict.

From what has been reported, it can be seen that the confrontation around Dodo is polarized between those who defend the existence of differences between psychotherapies, generally in favor of cognitive-behavioral therapies, and those who argue that the data confirm that all therapies work equally and therefore we should look in the unspecific or common elements for the key to success. The debate continues to generate a succession of analysis and re-analysis of studies on the effectiveness of psychotherapies (see Baardseth et al, 2013; Marcus, O'Connell, Norris & Sawaqdeh, 2014; Tolin, 2010). Significantly, some of the meta-analyses, such as those by Shapiro and Shapiro (1982) or Smith and Glass (1977), are referenced both in support of (e.g., Wampold et al., 1997) and against (e.g., Tolin, 2010) the Dodo verdict.

SOME CAUTION IN GENERALIZING THE RESULTS

With the main issue pending resolution, one should be cautious about the conclusions that can be derived from



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the available data. Whether or not the differences found by the research are statistically or clinically significant, the verdict of equivalence must not be generalized beyond the treatments studied. Of the hundreds of psychotherapies available, it can only be said that a few of them have been evaluated with some experimental rigor. Even in the most studied fields, there are serious limitations. Although more than a hundred studies have compared the results of different psychotherapies for adults with depression, none of these studies has enough power to detect clinically significant differences (Cuijpers, van Straten, Bohlmeijer, Hollan & Andersson, 2010).

Most of the disorders, about 300 in the DSM-IV, do not have comparative studies of therapies with adequate control so it would not be reasonable, based on the available data, to sustain that two therapies are equally effective for any of the disorders we can find described in the manuals of psychopathology. It may well be that there are no differences or they are minimal among the therapies offered proficiently for one disorder (e.g., depression) but that other important differences are found for another (e.g., agoraphobia) (Chambless, 2002). In fact, it has been observed that meta-analyses that compare data from all kinds of treatments, clients and conditions may not be very appropriate for clinicians, since the significant differential effects among the treatments for a disorder may be "compensated" by the opposite effects in other conditions or "swept away" by a general tendency not to find significant differences (Chambless, 2002). We can find several comparative studies of therapies for depression or anxiety disorders but, for example, in the field of psychosis or addictions there are still very few direct comparisons between therapies. The same caution should be taken regarding the generalization of the results of studies in adults with a mental disorder to other populations such as children and adolescents, the elderly, or people with chronic pathologies or mental or physical comorbidities (Hofmann, Asnaani, Vonk, Sawyer, & Fang, 2012).

Another aspect to consider is that although there is much literature, of varying degrees of rigor, on the benefits of psychotherapies, the literature on the harmful effects of the therapies is almost non-existent (Lilienfeld, 2007; Mohr, 1995). The choice of the appropriate psychotherapy for a case should be based not only on the expected benefits, but also on the safety of the therapies. The most adventurous interpretation of the meta-analyses

reviewed would be the one that would lead one to assume that therapies, as crazy as, say, coercive restraint therapy, which have at most some anecdotal but no experimental support and a high potential to cause harm (Mercer, 2005), are sheltered by the supposed equivalence of the effects of all therapies for all conditions. A recent survey with data from nearly 15,000 people, who received psychological treatment for anxiety and depression in England and Wales, suggests that about 1 in 20 thinks that the psychological treatment received had a lasting adverse effect (Crawford et al., 2016). In fact, patients may be underestimating the damage, because it has been found that between 5% and 10% of adult patients participating in clinical trials of psychotherapies end up worse than at the beginning of treatment (Lambert & Ogles, 2004). Moreover, it is expected that in ordinary clinical practice the situation is more disadvantageous: the results suggest deterioration rates of up to 14% in some places (Hansen, Lambert & Forman, 2002). Moreover, in a context in which it is assumed that there are effective treatments for a variety of problems and disorders, it should be noted that the most common problem of ineffective therapy is not the harm caused, but that it deprives the patient of the benefits expected from another therapy and prolongs the discomfort unnecessarily. While there have been initiatives to consign effective therapies for different disorders (e.g., Australian Psychological Society, 2010; Chambless & Ollendick, 2001; Nathan, Gorman, & Salkind, 2005), less effort has been devoted to reporting therapies without empirical support or that have potentially adverse effects.

META-ANALYSIS: PROS AND CONS

Since the psychologist Gene Glass coined the term meta-analysis (Glass, 1976), this type of analysis has gained considerable prestige, especially in the last two decades, being considered the form of systematic review with the greatest scientific rigor. The accumulation of research and publications makes the task of being up-to-date reading everything insurmountable. Compared to the original studies, meta-analyses have their own advantages: they allow a greater generalization of their results compared with individual clinical trials, since the samples are drawn from different populations. The meta-analysis improves both the power of small or inconclusive studies to answer questions such as the ability to evaluate and explain the



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discrepancies between the results of different studies. Furthermore, the meta-analysis facilitates the identification of trends that may go unnoticed in individual studies and it enables us to detect areas that need further investigation.

However, as with any other analysis, different assumptions and methods can provide different answers. Depending on the studies included (and excluded), the types of analysis and the interpretation of the results, we can accept or reject the hypothesis, in this case, of the equivalence or lack thereof of psychotherapies. Among the biases that may affect the results of the meta-analysis, one that is particularly problematic is what is known as publication bias, which arises when the probability that a study is published is not independent of its results; in other words, it refers to the tendency to publish positive results and to keep negative ones in the drawer. Moreover, meta-analyses cannot improve the quality of the original studies and they are exposed to problems stemming from mismanagement of the bias. For example, it is clear that in psychotherapy it is more difficult to apply masking procedures than in studies with drugs, particularly double-blind ones. However in only about 45% of psychotherapy trials (in contrast to 98% in drug trials) is a blind evaluation of the results applied (Huhn et al., 2014). The few meta-analyses that examine the application of double or single blind have confirmed the existence of bias in the sense that, for example, trials that apply masking procedures have lower effect sizes than studies that have not attempted during the study to conceal somehow which treatment was effective. For example, the trials of psychotherapy for depression with the least methodological rigor (and masking is a clear indicator of this) generally had a bigger effect size than higher quality trials (Cuijpers et al., 2010). Among the factors that may artificially enlarge the effect sizes of meta-analyses on psychotherapies, including studies with small samples and low power is especially relevant (Kuhberger, Fritz, & Scherndl, 2014).

In sum, the validity of the meta-analysis is likely to be affected by the methodological quality of the studies included, due to the different types of publication bias and choosing the inclusion criteria of the studies (Finckh & Tramer, 2008).

ALL POSITIVE, NEVER NEGATIVE

Science tends to confirm hypotheses, and apparently it

almost always succeeds. According to Fanelli, (2010) more than 80% of published studies totally or partially reject the null hypothesis, that is, they find the differences or associations they were seeking. Psychology (along with psychiatry) has the biggest tendency to publish positive results, up to 5 times more in comparison with space science, which is at the other end of the list (Fanelli, 2010). This is not a new observation; in 1959 Theodore Sterling analyzed a number of psychology journals and found that nearly 97% rejected the null hypothesis (Sterling, 1959). Thirty years later the results had not changed much: 94% of studies showed positive results, suggesting that many studies with negative results remain unpublished. The medical journals with which they compared the results had "only" 85% positive results (Sterling, Rosenbaum, & Weinkam, 1995).

Although this overall bias is significant, it does not necessarily invalidate the results of a particular study or the conclusions resulting from it. The problem is that the biases also occur within individual studies. The estimate of the frequency of biased articles in the prestigious journal *Psychological Science*, presented as the benchmark for the best research in the field, gave the result that 82% of the articles analyzed were biased (Francis, 2014). This certainly questions the validity of many of the articles published.

The eagerness to reach the famous threshold of statistical significance has been described as psychology's "dirty little secret" (Lambdin, 2012). Contrary to what it would be reasonable to expect, it is three times more likely to find psychology studies just reaching than just failing to reach significance (Kuhberger et al., 2014). It is known that flexibility in the data collection and analysis allows us to present any result as significant (Simmons, Nelson, & Simonsohn, 2011). Studies on psychotherapy that present positive results may be basically studies that should not have rejected the null hypothesis, but which, after the analysis, ignored the primary outcome and emphasized the post-hoc analysis of secondary or subgroup variables. As an example, the effectiveness of acceptance and commitment therapy (ACT) for psychosis was initially supported by a clinical trial which reported that four sessions had halved rehospitalization in patients with psychosis after four months follow-up (Bach & Hayes, 2002). An eloquent graph with the survival analysis highlighted the goodness of the treatment compared to treatment as usual. The study has been cited many times



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in the literature. However, rehospitalizations are not even a recommended measure for studies focusing on relapse. Rather, it may be that it was a decision made in view of the trial data, a way therefore of inappropriately embellishing the article. The fact that the symptoms doubled in the follow-up in the group that received ACT compared to the control group was interpreted by the authors as an indirect measure of acceptance, ultimately, supporting the therapy. However, the emotional distress that the symptoms generated, contrary to what could be assumed by the theory of ACT, was no less for the ACT than for the control group. The believability of the symptoms did however change, which is assumed not to be the aim of this type of therapy, but something more akin to cognitive therapy.

This is not to go against the use of the statistical and scientific method, but rather its misuse and particularly against misinterpreting the alpha level of significance and its overestimation compared to the other parameters offered by the statistical analysis. An unacceptably high percentage of research psychologists admit to having engaged in questionable research practices, which can distort the results, in order to obtain positive data. Although only a fraction actually falsifies the data deliberately; other habits are much more common, such as only reporting some dependent variables or deciding to collect more data if significance is not reached (John, Loewenstein, & Prelec, 2012).

This tendency towards the positive is combined with the desire of the authors, editors and reviewers for novelty in comparison with replicating studies. An analysis of a sample of 500 psychology articles published since 1900 showed that about 1% were replications of previous studies and, unlike what happens in other disciplines, most of them confirmed the results (Makel, Plucker, & Hegarty, 2012). The low statistical power of studies, questionable research practices and the tendency to publish statistically significant results have been proposed as the main factors that contribute to the replicability crisis in psychology (Ioannidis, 2014; Nosek, Spies, & Motyl, 2012, Open Science Collaboration, 2015).

Thus we tend to present studies with positive results reinforced by the lack of independent replications to corroborate or refute their findings. This creates a gaudy science, with many sparkling results but which is ultimately unreliable. In the field of psychotherapy research, it can be expected that these effects (or defects)

favor, in short, the impression that everything works and everything goes.

These practices, which distort the results of the research, do not only affect studies of the efficacy of psychotherapies, or even research in psychology generally (Chan & Altman, 2005), although it does seem that our discipline is especially vulnerable to them (Fanelli, 2010). So we must not ignore a number of proposals to minimize these biases, such as the prior public registration of the clinical trial plan and its faithful reflection in the data presented in the article (De Angelis et al., 2004). However, a recent study of these practices reported that the public registration of the protocol and submitting articles with primary results occurs in less than 20% (32 out of 170) of trials on the treatment for depression. In addition, trials with psychotherapy were less likely to be duly registered and published than trials with antidepressants (Shinohara et al., 2015).

ALLEGIANCE TO THE PSYCHOTHERAPEUTIC MODEL

It is beyond the scope of this paper to make a detailed review of the potential study biases, but at least one more should be noted that directly affects the comparative studies of psychotherapies: the researcher allegiance to the psychotherapeutic model. This bias has to do with a greater probability of finding positive results for the model to which one is attached. This is a bias with a substantial and robust effect on the comparative studies of psychotherapies (Luborsky et al., 1999; Munder, Brusch, Leonhart, Gerger, & Barth, 2013), and it can be particularly expected in studies in which the same therapist applies more than one treatment. However, only about 3% of the studies measure this effect and most do not even mention it (Falkenstrom, Markowitz, Jonker, Philips, & Holmqvist, 2013). Although some of the meta-analyses discussed have made efforts to control it (for example, Tolin, 2010), a *post-hoc* control continues to be problematic and it can be a source of new biases. The study by Luborsky and colleagues obtained that a combination of three different methods of assessing researcher allegiance to the treatment may account for about 70% of the results, although the three methods only correlate weakly among themselves, suggesting that any method may be measuring something more (and something different) than that which each of them assumes is researcher allegiance (Luborsky et al., 1999). One implication of this is that attempting to correct the



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effect *a posteriori* can actually be an overcorrection that sweeps away the real differences between treatments. For this reason, the proposal to monitor the effect seems more appropriate if, both in the individual studies and the reviews, the participating researchers represent different therapeutic approaches.

WHAT IS EFFECTIVE IN EFFECTIVE PSYCHOTHERAPY?

Rosen and Davison (2003), in their criticism of the position of Division 12 of the APA on the list of treatments with empirical support, use an example to illustrate the importance of focusing more on the mechanisms of action with empirical support than on the therapies in general. Suppose, say, that a clinician asks their driving phobia patients to wear a purple hat with magnets on while relaxation and other cognitive techniques are applied during *in vivo* practice. The clinician calls this method "purple hat therapy" (PHT), and argues that magnets reorient the fields of energy, accelerate the information processing, improve interhemispheric coherence and, in this case, eliminate the phobic avoidance. The therapy is more effective than the control treatment. The inventor of the therapy attributes its effectiveness to the fact that the patient wears the purple hat during exposure sessions. From there, articles on PHT can be published, training workshops organized for therapists on the use and placement of the magnets on the hat and, of course, therapy sessions applied with all the instruments of PHT. If all of this sounds too ridiculous, the authors go on to say, think of the development of some therapies, such as eye movement desensitization and reprocessing therapy (EMDR). Of this therapy, it has been said that what is effective (i.e., the desensitization and reprocessing) is not new, and what is new (i.e., the eye movements) is not effective (McNally, 1999).

Focusing on the therapies' mechanisms of change does not resolve the problem, but it can enable us to glimpse the complexity of the comparison among therapies. For example, Paunovic and Ost (2001) designed a trial to investigate the comparative effectiveness of exposure therapy and cognitive behavioral therapy (CBT) in the treatment of PTSD and found no difference between the treatments in any measure. As Neudeck and Wittchen (2012) note, no patient would agree to be exposed to a feared stimulus without prior instruction or the justification of the purpose of such proceedings (in which important elements of cognitive therapy may be incorporated), on

the other hand, in its final phase, CBT involved testing the validity of the patient hypotheses with "behavioral experiments." So determining the active ingredients of both therapies can be difficult and, if they are shared, studying their comparative efficacy is impossible.

Assuming that not all therapies with effective active ingredients work equally with all people (patients and, why not, therapists), there is another issue to be resolved. At a time of growing interest among researchers in the health field in general for personalized treatments, we know that it has long been recognized that psychotherapy research should not only focus on the effects of treatments, but also on what treatment and by who is the most effective for this individual with this particular problem and under what circumstances (Paul, 1967). These characteristics may include socio-demographic and clinical variables, patient preferences or biomarkers. For this purpose of identifying predictors and moderating factors of differential response to treatment, the direct comparison between different treatments is essential (Simon & Perlis, 2010).

Cuijpers et al (Cuijpers, Ebert, Acarturk, Andersson, & Cristea, in press) conducted a meta-analysis of studies that compared two psychotherapies directly with depressed patients with a specific characteristic (e.g., patients with HIV or cancer, or elderly people). They focused on the comparison of the six most studied types of psychotherapy for depression in adults (i.e., CBT, interpersonal psychotherapy, problem solving therapy, behavioral activation, psychodynamic therapy and non-directive counseling). A total of 27 specific characteristics were analyzed in the studies that met the inclusion criteria. The result was that CBT was more effective than other types of psychotherapy in elderly patients, in patients with comorbid addiction and in college students. For the other types of therapies, there was not enough statistical power to find differences between them. But if a more conservative approach of clinical relevance (an effect size of $g = 0.24$, implying a minimum of 16 studies per characteristic) were used, there was not enough power for any of the characteristics. This leads the authors to conclude that examining the comparative effects of different psychotherapies in specific groups is probably not the most efficient way to develop personalized treatments. The authors estimated that if the production of comparative studies continued at the same pace as it has to date, it would take over 300 years to examine the 27



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characteristics with the most popular psychotherapies when a laxer threshold of clinical relevance is adopted, and it would take over 1,300 years when using a stricter threshold (Cuijpers et al., in press). In any case, we should not assume that psychotherapy research will continue to develop at the same pace as it has to date; new technologies such as data mining and machine learning have great potential to transform mental health research using secondary analysis, as is already being done in other areas (Dipnall et al., 2016).

Apart from the specific factors of therapies and everything that happens in them (including what could be understood –not in a pejorative way– as the placebo effect and other effects related to expectations), there are several phenomena outside of the treatment that may also influence the response to it. We briefly discuss two of them: spontaneous remission and regression to the mean. The natural evolution of psychological disorders does not always tend towards chronicity but sometimes the symptoms resolve spontaneously without any need for intervention. A recent meta-analysis of untreated cases of major depression suggests that half remit spontaneously after a year (Whiteford et al., 2013), a fact that suggests, firstly, that the prevalence of disorders in the community is not, in itself, an adequate indicator of the need for treatment and, secondly, we may be overestimating the effect of treatments on the recovery from depression or other disorders. Moreover, regression to the mean, which has to do with the tendency for extreme values to approach the mean if measured repeatedly over time, may also be reinforcing the clinician's impression that everything is working. Because the patients often initiate the treatment, or they are selected for the study, when they are worse than usual (i.e., they score high on scales measuring emotional distress), it can be expected that after beginning the treatment an "improvement" would be noticed which may be mistakenly attributed to the intervention. These phenomena do not only affect the clinician but they also affect the studies with pre-post analysis without a control group.

THE MAGIC SPELL OF THE THERAPEUTIC RELATIONSHIP

Assuming that the differences found in comparative studies of psychotherapies are small or only in some subgroups of patients, some have postulated that the key to improvement could be in the common factors of the treatments (Wampold, 2015). This would be a more

parsimonious solution than assuming many different mechanisms that produce similar results. The common factors approach seeks to determine the main ingredients that the different therapies share with each other. The underlying belief is that these common elements are more important in accounting for the therapeutic success than the specific aspects that differentiate between the therapies. From this perspective, the specific models and techniques are not important as a mechanism for change but because they provide a plausible justification for the therapy to the patient and the therapist.

Rosenzweig (1936) was one of the first to write about possible common factors that may operate in different therapies. However, one of the most influential contributions comes from Jerome Frank (1961). Frank identified four common factors shared by different forms of psychotherapy and by most healing practices (such as the healing rituals of non-Western cultures) that have been designed to address the characteristic shared by all people who come to therapy: demoralization (Frank & Frank, 1991). Specifically, Frank notes that these factors shared by the healing practices are: (a) a relationship with emotional load based on the patient's trust in the competence of the therapist and the latter's desire to help; (b) an institutional context that is socially accepted and legitimated, which in itself raises the expectations that it will help the patient; (c) a justification (or myth) that provides an explanation of the problems and procedures for changing the patient; and (d) the tasks and procedures (or rituals) that demonstrate the competence of the therapist and give the patient a pretext for change (Frank, 1961). Since then, several researchers have helped to identify different categories of common factors that have guided conceptual developments and driven empirical studies. The best known advocate of this vision of psychotherapy research today is Bruce Wampold (Laska, Gurman, & Wampold, 2014; Wampold, 2015).

Over time, the number of common factors described has increased to close to hundred (Grencavage & Norcross, 1990). These authors organized the factors into five categories of higher order: patient characteristics, therapist qualities, change processes, treatment structure, and elements of the therapeutic relationship (Grencavage & Norcross, 1990). It has been repeatedly stated that common factors explain about 45% of the variance of the outcome of a therapy, compared to the 15% that is attributable to the specific therapeutic techniques (Lambert,



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1992). The most studied common factor is the therapeutic alliance, which has been identified as the main predictor of change in the different therapeutic modalities (Horvath, Del Re, Fluckiger, & Symonds, 2011).

The assumptions underlying the hypothesis of common factors are that (a) the different therapeutic approaches are relatively equivalent in efficacy; (b) these guidelines or approaches to treatment propose different theories of psychopathology, treatment and change; (c) common factors may be the most parsimonious explanation for the observed equivalence in efficacy; and (d) a therapist with a "competent personality" using any theory of change to implement a treatment with some consistency can help achieve positive results (Lambert & Ogles, 2014).

This perspective is not free of questioning or opposing positions. Its scientificity is questioned based on a "reverse engineering" that tries to extract the basic therapeutic strategies, inducing them from a heterogeneous set of results of meta-analyses and reviews (Baker & McFall, 2014) and, furthermore, some factors have not been operationalized in order to be studied empirically (Weinberger, 2014). On the other hand, a criticism has been that many of the factors identified as common factors (for example, hope and expectation) are in fact the result of a relational process rather than actual therapeutic mechanisms of change, but we cannot determine how to activate them or how they participate in the complex process of change (Sexton, Ridley, & Kleiner, 2004). In the same vein, the ability of this approach to guide clinical practice and practical training is also questioned (Chambless, 2002) and, in short, the idea that what is essential is not the specific elements of therapy carries a message that may increase the gap between research and clinical practice (Sexton et al., 2004).

Contrary to the primacy of the common factors, one must remember that some therapies do indeed seem to work better than others for some disorders (Chambless, 2002). This has been assumed, in a way, even by the principal advocates of the common factors approach in some of their articles, when they recognize the short-term superiority of behavioral therapies for disorders such as phobias (Frank, 1979; Luborsky et al., 1975).

A meta-analysis of the general literature on the therapeutic alliance found a small-to-medium effect size of the correlation between the alliance and the therapeutic outcome (Martin, Garske, & Davis, 2000). Even if the therapeutic alliance were the most important factor, it

would still be necessary to train the therapists in the procedures that facilitate a good therapeutic alliance for each case (Fonagy & Clark, 2015). It is also important to note that the use of weak research designs can lead to the exaggeration of the importance of this factor. The alliance is often measured late in therapy when some patients have already improved. The correlation between the alliance and the outcome may therefore be a consequence, rather than a cause, of clinical improvement. Late alliance is related to the therapeutic outcome, but early alliance is not (Feeley, DeRubeis, & Gelfand, 1999). Studies of the patient-therapist alliance rarely measure the competence with which the treatment is offered, so one cannot rule out the possibility that the positive assessment of the partnership reflects the competence and sensitivity with which the treatment has been offered (Fonagy & Clark, 2015). It has been observed that therapists offering treatment with a high degree of fidelity to the treatment model have significantly better results than those without this fidelity (Durlak & DuPre, 2008).

Recent studies suggest that the quality of the alliance between the therapist and the patient is more dependent on the actions or characteristics of the therapist than of the patient, therefore, the therapist, and what he does, would be most important in achieving beneficial results (Del Re, Fluckiger, Horvath, Symonds, & Wampold, 2012). However, reference to the alliance as a common factor may be misleading in the sense that, although the importance of the alliance may be common to the different therapies, the process leading to the alliance and how the alliance creates the change may differ depending on the type of therapy being administered (Ulvenes et al, 2012; Webb et al., 2011), in other words, it is not so common. Common factors, such as the alliance, and specific factors are usually presented dichotomously, and advocates of each offer evidence supporting the primacy of one or the other, however, how these factors interact with each other to produce benefits is complex and it seems necessary to study them together (Hoffart, Borge, Sexton, Clark, & Wampold, 2012).

Regardless of whether psychotherapies obtain equivalent results, analyzing which part of the change in therapy is due to elements shared by different approaches is conceptual and clinically relevant. The key is not to determine which of the two is more important, the techniques or the common factors, but rather how they



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relate to each other in order to adapt them successfully to a particular patient (Norcross & Wampold, 2011). This means, in short, to enter into the nebula of the change processes to detail a myriad of new active ingredients, and not to give up separating the wheat from the chaff in order to stake everything on a magic spell with pre-scientific undertones.

CONCLUSIONS

Perhaps the main conclusion of all of the above is that there is still nothing conclusive in the field of psychotherapy research. This observation does not invalidate the results of the research in this field. There is reasonable support for the assertion that psychological treatments work for a wide variety of mental problems and disorders. The prevailing consensus, based on the best available data, is that psychological treatments are more effective than no intervention, the placebo and the "usual" treatment (DeRubeis & Crits-Christoph, 1998; Lambert & Ogles, 2004; Wampold et al, 1997). But we must admit that only a few therapeutic models, of the hundreds that exist, have been put to the test. In addition the studies that support the efficacy of the psychotherapies are subject to significant limitations, they have biases and methodological weaknesses and are exposed to questionable research practices that inflate the chances of finding positive results and, with this, the impression that everything is effective.

In the case of the most studied form of psychotherapy, cognitive behavioral therapy, a mega-analysis that includes 269 meta-analyses conducted on a wide variety of disorders (e.g., mood, anxiety, psychotic, food, substance use, somatoform), types of problems (anger, insomnia, stress, pain, cancer, etc.) and populations (children, the elderly) reveals that while some meta-analyses found no differences between treatments, most studies did find advantages of one over the others (Hofmann et al., 2012). Still, the conclusions of the authors are that high-quality studies are required to examine the efficacy of cognitive-behavioral therapy, the efficacy of CBT is questionable for some problems and, with the exception of children and adults, there are no meta-analytic studies with particular subgroups (Hofmann et al., 2012). Most of the studies lack statistical power, in particular, only provisional conclusions can be drawn on the comparison of psychotherapies for problems other than anxiety and depressive disorders (Tolin, 2010).

In this state of affairs, the verdict that everything works equally involves abandoning the effort to unravel the efficacious elements that serve to validate, refute or modify the theoretical models that underlie the various forms of therapy. In short, it may be a hasty assumption that further opens the gap between the clinical arena and a science guided by theories and validated by data.

While it can be observed that, in the list of empirically validated treatments, if not hundreds at least a number of therapeutic models are represented to a greater or lesser extent (Chambless & Ollendick, 2001), there is no theoretical model that has shown absolute superiority over the rest. The scarce development of the research on the mechanisms by which the change in psychotherapy operates does not enable us to determine why treatments with different theoretical assumptions achieve similar results at least for some disorders. Even assuming that these effective therapies have elements in common, we should determine which the essential ones are. A broad view of the common factors includes, as well as the expectation of improvement, trust in the therapist or the therapeutic relationship, and also mechanisms often considered specific to the treatment, such as encouraging the patient to be exposed to feared stimuli or encouraging the patient to practice certain behaviors (Lambert, 2005). In fact, the contextual model, which defends the essential role of the common factors, postulates that the specific ingredients of therapy not only create expectations, but also generate healthy actions, such as relying less on dysfunctional beliefs (cognitive-behavioral treatments), improving interpersonal relationships (interpersonal psychotherapy), accepting oneself more (ACT), expressing difficult emotions (therapies focused on emotion and dynamics), becoming aware of the perspective of others (mentalization therapies), and so on (Wampold, 2015). These healthy actions may in turn relate to each other, such that, by enhancing one, another is benefited.

While we can agree, based on the available data, that psychotherapies work to relieve mental health problems, little can be said about how they do this. The process of change in psychotherapy is extraordinarily complex. So it is important that, in an effort to identify the mechanisms of change, we take into account the variety of factors (and their interactions) involved in the therapy, without falling into the trap of simplifying and unduly limiting our ability to explain what is useful in therapy. The common factors



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approach expands the vision of psychotherapy by emphasizing the explanation of the change in ways that go beyond the treatment protocol and the theoretical model that guides it. Thus an oversimplified analogy of the active ingredients of the medical model is surpassed.

If, as seems to be the case, some problems and disorders require specific treatment techniques (e.g. exposure based therapies for anxiety disorders), while others may respond equally well to a variety of interventions, psychotherapists should be able to evaluate when the specialized techniques can be beneficial and when the common elements of psychotherapy, such as the therapeutic alliance may prevail (Marcus et al., 2014). It is an alliance that, in any case, must be constructed with reference to a theory that gives meaning to emotional distress and the possible procedures in order to deal with it. This emphasizes the complexity of the work of the clinical psychologist, which requires the clinical assessment skills, the proximity to the patient and management of care resources that go significantly beyond the application of a strict treatment protocol.

From the scientific perspective, and out of concern for public health, we clinicians must not stop giving preference to the interventions that have the most empirical support. While the underfunded research in psychotherapy advances with a pluralistic approach, given the significant difference in the degree of knowledge and confidence we have today in the various forms of psychotherapies for different disorders, the academic and professional institutions would do well to facilitate the dissemination of this information among the professionals and users. Together with this, it is essential that the training curriculum of the psychologist, and –in the case of the clinical psychologist– the aspects related to the critical reading of the scientific literature, be strengthened in order to be able to assess the validity and relevance of the results of research into psychological treatments in order to transfer them responsibly to patient care.

AUTHOR DISCLOSURE STATEMENTS

No competing interests exist.

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