

## Let's distinguish relative and absolute efficacy to move psychotherapy research forward

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### Abstract

**Objective:** Background regarding a recent debate between Cuijpers et al. (2019a, 2019b) and the authors (Munder et al. 2018) about the efficacy of psychotherapy for depression is given.

**Method:** A main reason for the discrepancy in Cuijpers et al.'s and our conclusions is discussed. **Results:** In our view the discrepancy is due, among other things, to a blurred distinction between questions of relative and absolute efficacy of psychotherapy. Although the efficacy of psychotherapy vis-à-vis alternative treatments may be ambiguous, there can be little doubt about the benefits of psychotherapy relative to no treatment. **Conclusion:** We do not think that raising fundamental concerns about the value of psychotherapy is a service to the field. We argue that moving the field forward requires a focus on how psychotherapy works and how the access to psychotherapy can be increased.

**Keywords:** efficacy, psychotherapy, depression, meta-analysis

## **Psychotherapieforschung braucht die Unterscheidung von relativer und absoluter Wirksamkeit**

### **Zusammenfassung**

**Fragestellung:** Eine aktuelle Debatte zwischen Cuijpers et al. (2019a, 2019b) und den Autor/innen (Munder et al. 2018) zur Wirksamkeit von Psychotherapie bei Depressionen wird dargestellt. **Methode:** Ein Hauptgrund für die Diskrepanz der Schlussfolgerungen wird diskutiert. **Ergebnisse:** Aus unserer Sicht lässt sich diese vor allem auf die Vermischung von Fragen der relativen und der absoluten Wirksamkeit von Psychotherapie zurückführen. Während die Wirksamkeit von Psychotherapie gegenüber alternativen Behandlungen uneindeutig sein kann, können kaum Zweifel an ihrer Überlegenheit gegenüber keiner Behandlung bestehen. **Diskussion:** Wir glauben nicht, dass die Erzeugung grundlegender Bedenken am Wert von Psychotherapie der Psychotherapieforschung einen Dienst erweist. Aus unserer Sicht sollte die weitere Forschung darauf fokussieren, wie Psychotherapie wirkt und wie der Zugang zu Psychotherapie verbessert werden kann.

**Schlüsselwörter:** Wirksamkeit, Psychotherapie, Depression, Metaanalyse

## Background

In this article we describe a recent debate about the efficacy of psychotherapy for depression between Cuijpers et al. (2019a, 2019b), Cristea (2018), Purgato et al. (2019) and the current authors (Munder et al. 2018) and offer an explanation for the discrepant conclusions drawn. This debate is important as it has implications for the future of psychotherapy research.

In their meta-analysis Cuijpers et al. (2019a) were interested in the effects of psychotherapy for depression. Using the title “Was Eysenck right after all?”, Cuijpers et al. put themselves in the tradition of Hans Eysenck who, in a series of publications in the 1950s and 1960s (cf. Munder et al. 2018; Wampold & Imel 2015), challenged the efficacy of psychotherapy. Cuijpers et al. (2019a) took a fresh look at this very fundamental issue by employing some of today’s methodological advancements. One of their decisions was to exclude studies that used wait list (WL) control groups based on evidence from a network meta-analysis (Furukawa et al. 2014) suggesting that WL “may be a nocebo and artificially inflate the effect sizes of therapies” (Cuijpers et al. 2019a, p. 22). Consequently, Cuijpers et al. based their final analysis on studies comparing psychotherapy with treatment as usual (TAU) or “other control” groups and concluded, based on the average effect size found in these studies, “that the effects for psychotherapy for depression are small” (p. 21) and that “it remains questionable whether Eysenck was truly right or wrong” (p. 29). Cuijpers et al.’s article clearly can be read as suggesting, much like Eysenck’s works, that there is something fundamentally wrong with the efficacy of psychotherapy. Or, as Cuijpers et al. (2019b) put it, that “the field is currently facing” “major problems” (p. 2).

In our re-analysis (Munder et al. 2018) we criticized some of Cuijpers et al.’s (2019a) methodological decisions. Our central critique was Cuijpers et al.’s exclusion of studies using

WL control and, consequently, we chose to retain studies with WL controls in our re-analysis. In our meta-analysis we found an effect size difference of  $g = 0.75$  in favor of psychotherapy vis-à-vis no treatment. Thus, our analysis showed clearly that psychotherapy is beneficial for patients with depression.

In their respective comments, Cuijpers et al. (2019b) and Cristea (2018) took issue with our analysis and conclusions and Cuijpers et al. (2019b) suggested that we ignored the “main message” of their paper (p. 1) and “the main problems of psychotherapy research” (p. 2).

### **Relative efficacy versus absolute efficacy of psychotherapy**

To explain the discrepancy between Cuijpers et al.’s (2019a, 2019b) conclusions (echoed by Cristea, 2018) and ours, we find it helpful to be cognizant of the distinction between two basic research questions one can raise regarding the clinical value of health care interventions, such as psychotherapy (Wampold & Imel 2015). The first is whether an intervention has any clinically relevant benefit for patients, which is the question of absolute efficacy. The second question is how this intervention compares to other interventions for the same problem, which is the question of relative efficacy.

Following up on Eysenck’s studies, the stated objective of Cuijpers et al.’s (2019a) meta-analysis was to determine whether psychotherapy for depression was more effective than receiving no treatment, which is an examination of absolute efficacy. The first citation in Cuijpers et al. (2019a) was Eysenck (1952), which compared the available data on recovery of patients in psychotherapy or psychoanalysis to spontaneous remission, that is, to the recovery of patients who did not receive any treatment. Famously, this investigation led Eysenck to conclude that the “figures fail to support the hypothesis that psychotherapy facilitates recovery”

(Eysenck 1952, p. 323). In other words, Eysenck's study questioned the absolute efficacy of psychotherapy. Consequently, we chose the question of absolute efficacy as the focus of our re-analysis (Munder et al. 2018).

In their respective comments to our article, Cuijpers et al. (2019b) and Cristea (2018) both criticized our decision to retain studies with WL controls, pointing out that "[t]here is a clear difference between waitlist and other control groups ... and the effects of a therapy are significantly larger when compared to waitlist than when compared to care-as-usual" (Cuijpers et al. 2019a, p.1), a contention about which we, as well as the field, are well aware.

Why did we choose to retain studies with WL? Well, for two reasons: First, conceptually WL is the best estimate of how patients fare without any treatment, thus allowing for the optimal test of Eysenck's (and Cuijpers et al.'s, 2019a) claim regarding the absolute efficacy of psychotherapy. Second, the evidence cited by Cuijpers et al. (2019a) to back their claim that WL impedes spontaneous recovery is seriously flawed as we demonstrated in our re-analysis (Munder et al., 2018). Interestingly, neither Cristea (2018) nor Cuijpers et al. (2019b) refuted our critical analysis of Furukawa et al. (2014). In short, we used WL controls because they allow testing the research question in focus and there is no convincing evidence suggesting to do otherwise.

Why did we not follow Cuijpers et al. (2019a) in choosing TAU as the main comparator?

Because comparing psychotherapy to TAU addresses a different question entirely: Is a particular psychotherapeutic treatment more beneficial than mental health services offered in usual care. This is a question of relative efficacy. Unfortunately, TAU varies considerably, ranging from no treatment (where access to mental health services is limited) to psychotherapy services comparable to the psychotherapeutic treatment being studied (cf. Purgato et al. 2019), thereby making comparisons with TAU problematic (Watts et al. 2015).

We are not saying that questions of relative efficacy are not important. However, keeping in mind that it was Cuijpers et al.'s (2019a) decision to revisit the more fundamental question of whether psychotherapy works at all, our reanalysis focused on absolute efficacy. Cuijpers et al. (2019a) understood the dramatic “drop” in effect size they found as a result of “adjusting for several sources of bias” (p. 21). In our view it is rather due to their *shift* from a test of the absolute efficacy of psychotherapy for depression to a test of its relative efficacy, which is not a correction for bias but a fundamentally different focus altogether.

## **Conclusion**

Let's be clear, we are not “pretending everything is fine”, as Cuijpers et al. (2019b, p. 1) suggest. In several articles we have emphasized that biases in research detrimentally affect results and conclusions (e.g., Leichsenring et al. 2017; Wampold et al. 2017). Furthermore, we fail to see how making a rigorous estimate of the effects of psychotherapy for depression is “pretending” anything—estimates are themselves epistemologically neutral, as they are what they are. That psychotherapy is effective, as we demonstrate, does not preclude the need for improvement. The number needed to treat for psychotherapy versus no treatment is 3 (Wampold & Imel, 2015), so there is a clear need to improve. In many venues, the problem we see is limited access to psychotherapy and other interventions in many health care systems (Kessler et al. 2005; Vasiliadis et al. 2018).

We do not see how raising fundamental concerns about the worth of psychotherapy for depression, as Cuijpers et al. (2019a) do, can be seen as a service to the field or to people suffering from mental illness. As we stated in our original article (Munder et al. 2018): Let's focus on “ways that psychotherapy could be improved” (p. 6).

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