

# A short comment on Huber et al.'s (2012) comparison of cognitive-behaviour therapy with psychoanalytic and psychodynamic therapy for depressed patients – a three-year follow-up study

## Introduction

The study of Huber et al.<sup>1</sup> attempts to investigate the effectiveness of long-term psychotherapies. Cognitive-behaviour therapy (23 patients) was compared with psychoanalytic (30 patients) and psychodynamic therapy (25 patients) in the treatment of patients with a primary diagnosis of unipolar depression. In a prospective design these patients were compared at pre- and post-treatment and three-year follow-up. Outcome measures were different questionnaires (Beck Depression Inventory, Global Severity Index for measuring symptoms, Inventory of Interpersonal Problems, Social Support Questionnaire for measurement of social-interpersonal functioning, INTREX Introject Questionnaire for measuring personality structure). Comparative effectiveness of the experimental groups was analysed using mixed models. The authors found significant outcome differences between psychoanalytic therapy and cognitive-behaviour therapy in depressive and global psychiatric symptoms, partly social-interpersonal and personality structure at three-year follow-up. Psychodynamic therapy was superior to cognitive-behaviour therapy in the reduction of interpersonal problems. Psychoanalytic therapy shows significantly longer-lasting effects compared to cognitive-behaviour therapy three years after termination of treatment, which is discussed as a dose-effect. Based on their investigation, the authors recommend treating patients with unipolar depression preferably by psychoanalysis. As is the case with other nomological studies of the effectiveness of psychoanalytic treatments—they all came to the conclusion that psychoanalytic treatments are effective,<sup>2</sup> psychoanalysts might be pleased by the situation and the outcome of this study. On closer scrutiny however, it becomes recognisable that the study by Huber et al. is to be subjected to the same critique we<sup>3</sup> put forward in discussing the study by Leuzinger-Bohleber et al.<sup>4</sup> Huber et al.<sup>1</sup>'s study gives reason to recall some of our former arguments. In so doing, I will restrict myself to the authors' comparison of psychoanalysis and cognitive behavioural therapy, start with my comments and end with some general remarks regarding the nomological attempts at verifying the effectiveness of psychoanalytic treatments.

## Comments

- The studied depressive patients are by no means a representative sample of all of these patients. They have a common characteristic in that they came for treatment to an outpatient clinic.
- The studied psychoanalytic treatments could not lay claim to be a representative sample of all psychoanalytic treatments occurring in daily practice. It seems as if the 14 (!) psychoanalysts used in the study were just those that happened to be willing to participate. As in every other country, it is the case in Germany that psychoanalysts devote themselves to diverse sub-schools of contemporary psychoanalysis (e.g. orthodox Freudians, Lacanians, neo-Kleinians, post-Kleinians, contemporary Kleinians, ego-

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- psychologists, self-psychologists, object-relations theorists, intersubjectivists, social-constructivists, etc.). Such proper representation is particularly necessary because representatives of these groupings use different and contradictory therapeutic and technical concepts in their treatments which are regarded as equally valid.<sup>5-8</sup> For example,<sup>9-13</sup> found in her investigation into the working methods of 65 psychoanalysts (62 members of the IPA) immense discrepancies and eclecticism with regard to their understanding of both technical concepts and the way that technical rules should be applied.
- The extent to which the positive outcome was possibly due to a different empathic and supportive attitude of psychoanalysts and cognitive behavioural therapists is not considered. For example, in his replication of the *Consumer Report Study*, Hartmann<sup>14</sup> has shown convincing evidence that a supportive attitude of the therapist increases the chance of a successful treatment by the factor 3. It cannot be ruled out that the success was not so much based on the psychoanalytic treatment but on the supportive attitude of psychoanalysts.
  - The authors mention a possible effect of the treatment dose on their results. The patients treated psychoanalytically received a mean of 234 sessions (in 39 months), and the patients treated by cognitive behavioural therapy received a mean of 45 sessions (in 26 months). Huber et al.<sup>1</sup> hold the view that the "obvious difference in treatment dose ... may account for the difference across all dimensions of outcome." Nonetheless, they conclude that their "results support the assumption that PA [psychoanalysis] is superior to CBT [cognitive behavioural therapy]" (ibid., p. 312). In the view of their former statement, this conclusion is not justified. This conclusion is also not tenable, because Hartmann's<sup>14</sup> replication study also shows that psychoanalysis and cognitive behavioural therapy are effective to the same extent if they do not differ as regards duration.
  - It remains unclear as to how it was possible to compensate

for a phenomenon described as the “experimenter effect”<sup>1</sup>.<sup>15</sup> How could the possibility be ruled out that patients reacting in accordance with the hypothesis showing an improvement in their pathologies were not behaving in accordance with the analyst’s (or behavioural therapist’s) expectations? This question is all the more pertinent because the authors’ emphasis the central importance of transference in psychoanalytic treatments and it is the patient’s transference that induces the patient to adopt the analyst’s mutative interpretations.<sup>16</sup>

- f. More significant however, is the question as to whether the patients were treated by behavioural therapy or by psychoanalysis at all. It is not sufficient to simply state that the patients went to see experienced behavioural therapists for one session per week, or experienced psychoanalysts for 2 to 3 sessions per week, lying on a couch. Gill<sup>17</sup> stated almost 50 years ago that it is not adequate to define psychoanalysis
- g. by such quantitative matters as how often the patient comes, or by such matters of physical arrangement, such as the recumbent position and inability to see the analyst. These features are important but they are important only as auxiliary devices to enable the application of certain technical principles.
- h. Similarly, it does not suffice to state that behavioural treatments consisted of a combination of “cognitive and behavioural techniques in different extent”,<sup>1</sup> and to define psychoanalysis as a “predominantly verbal, interpretative, insight-oriented approach which aims to modify or re-structure maladaptive relationship representations that lie at the root of psychological disturbance”, involving “careful attention to the therapist-patient interaction, with thoughtfully timed interpretation of transference and resistance” (ibid.). To attribute the outcome of the investigated treatments to behavioural therapy or psychoanalysis would imply that, as Bachrach et al.<sup>18</sup> point out, it is “demonstrated that the treatment being evaluated is taking place,” i.e., that it had been established that the patients were actually treated by a combination of “cognitive and behavioural techniques in different extent” or by “thoughtfully timed interpretation of transference and resistance” respectively. Such proof is missing,<sup>2</sup> so that the methodology of the authors’ investigation becomes analogous to a study in which the effect of medications is assessed under the condition that patients with the same disease are treated by doctors having different qualifications but the kind of medication prescribed is not known.

## Conclusion

In nomological psychoanalytic research on the effectiveness, a multitude of divergent methods were used (compare, for instance,<sup>19–25</sup> other attempts, all applying completely different methods, have been assembled by Brandl et al.,<sup>2</sup>). In the epistemological understanding findings are specific to the method used, and in order to acquire knowledge of the object of the study the method used must be

<sup>1</sup> This phenomenon was first noticed when it was found that subjects who behaved in accordance with the relevant hypothesis reacted not only to the conditions introduced, but also to the experimenter's expectations as to the outcome of the testing.

<sup>2</sup> For instance, Huber et al.<sup>1</sup> could have used the criteria developed by Waldron et al.<sup>35</sup> that enable psychoanalysts to decide whether the phenomena conceptualized in treatment theory also exist in their treatments.

appropriate to the particularity of that object. Given that such divergent methods and methodological approaches are employed, it is hardly conceivable that all of these approaches are going to be in accordance with that object. It seems to me that it is much more plausible to assume that the outcomes—all of them claimed to have verified the efficiency of psychoanalytic treatments—constitute methodologically determined artefacts. Furthermore, it is also questionable as to whether the results of the studies can be generalised at all in a nomological sense if any psychoanalytic treatment is unique.<sup>26–30</sup> In sum, as is the case in other studies in the study by Huber et al.<sup>1</sup> too, it remains indistinct how the measured differences were brought about. With regard to the general situation of the nomological attempts at verifying the effectiveness of psychoanalytic treatments one is reminded of Freud’s statement after having read Semon’s book on “Mneme”. On November 14<sup>th</sup> in 1906, he said: “The book is characteristic of those pseudo scientists who imitate exactitude merely by operating with numbers and concepts”.<sup>31</sup> In my view, this situation is akin to that kind of naïve falsificationism of empirical research practiced in social sciences that Lakatos<sup>32</sup> criticised, namely, that these investigations provide solely “phony corroborations and thereby a semblance of scientific progress where, in fact, there is nothing but an increase in pseudo-intellectual garbage.”<sup>33,34</sup>

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## Conflict of interest

The author declare there is no conflict of interest.

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