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Original article

Fifteen years after the INSERM report. Psychoanalysis's efficacy reevaluated☆

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ABSTRACT

Objective. – Since the 2000s, "mental health" policies in France have to be based on evidence. A report on various forms of psychotherapy, published by the INSERM (the French National Institute of Health and Medical Research) in 2004, and whose conclusion was that there is no evidence of effectiveness for psychoanalysis, remains an important reference. The aim of this article is to re-examine its scientific value, fifteen years later.
Method. – After returning to the context in which this report was written and the controversies surrounding its publication, we propose a review of the efficacy studies that have since evaluated psychoanalysis. This recent literature, mostly international, remains relatively little known in France.
Results. – The INSERM's expert report – whose status could in part be promoted at the time of its publication – now appears to be relatively dated, both in terms of methodology and in its conclusions. The majority of current efficacy studies show that psychoanalytical and cognitive-behavioral therapies do not show any significant differences in efficacy for all known disorders.
Discussion. – These results give rise to debate. Some scientists believe that they are only due to a lack of rigor in the experimental protocols. Others, on the contrary, maintain that the search for *specific factors* of efficacy is a dead end and promote studies in natural conditions, in order to assess the *cumulative contextual factors* of therapeutic effectiveness.

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Conclusions. – Without prejudging future research orientations and results, it nevertheless appears that the INSERM report can no longer be the dominant reference in France today for recommending psychotherapeutic "good practices." Research has progressed considerably. In view of the international scientific literature available today, psychoanalysis is one offer of care to be advocated among others – a position to which many countries subscribe.
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1. Introduction

Since the 2000s, political decisions that concern "mental health" must be supported by scientific data on the efficacy of various therapies [1,2]. This is one example, in the health care field, of the general principles of a "new public management" that attempts to achieve excellence in the cost/benefit ratio of investments made by the State [3–5].

It was in the name of "good governance" based on science that the General Direction of Health (DGS, in French), the National Union of Friends and Family of the Mentally Ill (Unafam, in French), and the National Federation of Psychiatric Patients and Ex-Psychiatric Patients (Fnap-psy, in French) commissioned the National Institute of Health and Medical Research (Inserm, in French) to prepare a report in 2002 on the comparative efficacy of different psychotherapies. The explicit objective was to improve the offer of mental health care in France, by recommending treatments that science had judged to be the most beneficial.

The report, entitled "Psychotherapies: Three Approaches Evaluated," appeared in 2004. Composed of a review of the literature available at the time, it was followed by a conclusive synthesis. Only studies of efficacy carried out *in a laboratory setting* (randomized controlled trial) and the meta-analyses of these studies were included in the evaluation – a decision that was not without controversy, even within the committee of experts charged with preparing the report. This means that clinical research *in natural conditions* (controlled observational studies on single cases and/or on series of cases) was excluded, even though these studies have, historically, been more widely used in research in psychoanalysis, in light of their greater clinical significance (despite a weaker internal validity) [6].

Thus the report's inevitable conclusion: comparatively speaking, there is less experimental proof of psychoanalysis's efficacy than there is for cognitive-behavioral therapies (CBT) – with the latter having been studied in clinical trials since the mid-1980s. While the absence of "greater statistical proof (*in laboratory settings*)" does not signify that psychoanalysis is ineffective, in France, as in the Anglo-Saxon world, CBT has become *the* recommended therapy.

In consequence, the High Health Authority (HAS, in French) initiated a policy of implementing so-called *cognitive-behavioral* "good practices" in the public health and medico-social fields, while discouraging the psychoanalytic approach. This policy has continued at an ever-increasing speed. To give just a few examples:

- in a report from 2012, psychoanalysis was included among "non-consensual overall interventions [for autism] [. . .] [in light of] the lack of data on its efficacy" ([7], p. 27). As a consequence, the French government's third Autism Plan (2013–2017) promoted "behavioral and educative interventions" ([8], p. 10), a choice that was pursued in the National Strategy for Autism Among Neurodevelopmental Disorders (2018–2022), which makes note of "the difficulties in applying recommendations for professional best practices" ([9], p. 90) in various public mental health structures, where psychoanalysis has traditionally had a stronghold;
- in late 2019, a report by the office of the Inspector General for Social Affairs (IGAS, in French) declared that "there are therapeutic approaches whose effectiveness is based in science" ([10], p. 4), thereby calling into question the content of certain psychology programs in French universities (many of which are psychoanalytically oriented) and, by extension, the value of the diplomas delivered by

these universities and the possibility, conferred by these diplomas, for professionals to practice in public institutions;
- at the moment, a reform of Medico-Psycho-Pedagogical Centers (CMPP, in French) is being considered, starting with experimentation in the Nouvelle-Aquitaine region –, which would eventually force these institutions, specialized in mental health care for children and teenagers, to "reposition" themselves (or lose their state funding), by adopting "methods of intervention that are in conformity with professional best practice recommendations" ([11], p. 5).

While public decision-makers continue to justify their "mental health" strategies in light of the Inserm report's recommendations ([10], p. 40, n. 127), we can still interrogate, from a scientific point of view, the relevance of referring to such a "*national*" expert document (practically never cited in the scientific literature in the rest of the world), which is, furthermore, the only attempt in French history to evaluate different psychotherapies, which provoked controversy when it was published, and which has been strongly contradicted by more recent research.

Since the turn of the millennium, there are, in effect, convincing data that clearly call into question the conclusions and recommendations of this report. Today, the field of research on psychotherapies can point to numerous studies on clinical effectiveness, whose methodologies are more rigorous than those of the Inserm report, and whose results converge. Articles of reference periodically refer to these studies in the scientific literature.

Regarding the specific case of psychoanalysis, there is now a substantial number of randomized controlled trials (RCT), as well as meta-analyses that measure its efficacy using higher standards of proof. At the same time, the methodological protocols of observational case studies, traditionally used to evaluate psychoanalysis's effects, have been reinforced in order to meet today's norms of scientific rigor.

A notable fact emerges from this literature: psychoanalysis – as a psychodynamic therapy, using verbalization, centered on the unconscious transference relationship, both interpretative and supportive – *is effective*, both in the short-term and in the long-term, and this for almost every known disorder. In addition – another noteworthy fact –, psychoanalysis appears to be, overall, *as effective* as CBT, and here again for every known disorder. This is the conclusion at which a majority of researchers studying the effectiveness of different psychotherapies has arrived [12–22], even if some of them (and, in particular, partisans of various cognitive-behavioral approaches) still think that this simply reflects the absence of better methods for deciding between different types of therapies [23].[1]

If these scientific results remain, for the most part, unknown to French psychoanalysts – who are, in general, not very interested in these questions of evaluation [26] – they call into question the conclusions of the Inserm's expert report from 2004, and therefore its status as a reliable decision-making tool. In consequence, it appears necessary to relay the more recent scientific consensus in this article, both for the benefit of those who put public health policy into practice (and whose duty it is to be informed), and well as for the benefit of the public at large, in the name of the public's right to information.

It is not my ambition here to give an exhaustive treatment of the complex stakes of this international field of study (for a more detailed presentation, see [27]). Instead, I will give a brief presentation of the situation, 15 years after the appearance of a report whose recommendations are even more debatable now than they were at publication. I will undertake this in two stages:

- first, in recontextualizing the scientific controversies that surrounded its creation and its publication (2);
- secondly, in providing a synthetic restitution of the convincing *current* data on the effectiveness of psychoanalysis (3).

[1] The advocates of CBT often affirm that these therapies will ultimately be recognized as "the most effective of all" in light of the scientific-*experimental* status of their concepts. To which advocates of psychoanalysis respond that these arguments are more declarative than scientific [24,25].

2. A controversial evaluation

From the moment of its publication in 2004, the Inserm report "Psychotherapy: Three Approaches Evaluated" created division in the public. Different groups of psychiatrists and psychologists (as well as psychoanalysts) denounced its bias, and, in consequence, the illegitimacy of its publication on the official website of the Ministry of Health – the latter giving it a form of political recognition. This led to the Health Minister at the time (Philippe Douste-Blazy) to remove it, while the report remained accessible on the Inserm website, as a scientifically autonomous document.

The psychiatrist Jean Cottraux – a fervent defender of CBT and one of the co-authors of the report –, infuriated by this retraction, wrote a "Brief" in the journal *Science*, wherein he denounced an "act of [political] censorship" [28]. The foundations of the report's instrumentalization were laid with the publication, in the same year, of the *Black Book of Psychoanalysis* [29]. The *Anti-Black Book of Psychoanalysis* – a response written by a collective of practitioners from an important school of psychoanalysis – was published the following year [30].

But beyond the mutual accusations of the political instrumentalization and counter-instrumentalizaion of science – an outpouring of accusations that the national press seized upon with a certain delight [31,32] –, another controversy was taking place on a more scientific plane, between specialists of the disciplines and practices in question. This discreet, technical controversy was less frequented publicized.

This particular controversy was inaugurated, in 2004, by a calling into question of the report's methodology [33]. This was soon followed by a collective of psychoanalytically oriented psychiatrists and psychologists – Roger Perron, Bernard Brusset, Clarisse Baruch, Dominique Cupa, and Michèle Emmanuelli – offering up a critique of the expert committee's choices during an academic conference.[2] Finally, a different text presented a formal critique of the epistemic statute of the report [35].

The Inserm report consists, in effect, of a synthesis of data pulled from scientific articles, the latter selected from the international literature available at the time. A "level III" study, it is considered less conclusive than RCTs ("level II" studies) or meta-analyses of RCTs ("level I" studies). Thus, the Inserm did not carry out any evaluative process, clinically speaking. The expert committee simply examined a selection of the existing research, with relatively variable levels of rigor. Of course, any selection of the scientific literature supposes that certain criteria have been determined. The objections raised by the psychoanalysts point out that:

- the report required a preliminary constitution of a group of experts (who were they? what was their clinical background? how and by whom where they chosen?);
- it was necessary to work from a theory of "mental illness" (what does it mean for an individual to be mentally ill? with which diagnostic tools is this evaluated?);
- these choices implied, as a corollary, a theory of care (what does it mean to "get better?," to "recover," to "heal," for a given person? with what tools can one measure this?).

At every one of these steps, epistemic decisions were made that determined the process. It is not my goal here to relay the details of every critique made of the final report. For this, readers can consult the source texts [6,33,35–42]. By inventorying the limits and the biases *that were ultimately acknowledged by certain defenders of the report* several years after the controversy, my goal is simply to restitute, as faithfully as possible, the scientific value of the 2004 report.

One practitioner-researcher is in an interesting position. Philippe Cialdella, a psychiatrist and a specialist of meta-analyses [43–46], laid out a nuanced position in the aftermath of the controversy, in a written response [47] to a text produced by Perron, Brusset, Baruch, Cupa, and Emmanuelli [35]. While he reminded his readers that the text reflects only his personal point of view, his "defense" *took the objections into account*, and had the advantage of being a more measured point of view. Indeed,

[2] *Cf.* the European Inter-University Seminar on Teaching and Research in Psychopathology and Psychanalysis (SIUEERPP in French) held on May 8, 2004, entitled "Soigner, enseigner, évaluer," which led to the publication of a special issue of the journal *Cliniques méditerranéennes* [34].

Cialdella's text was reread by a critic of the Inserm report (J.-M. Thurin), one of its vigorous defenders (J. Cottraux), and by outside observers who were less directly involved: psychiatrists, pharmacologists, or philosophers (H. Chabrol, M. Cucherat, J.-D. Guelfi, P.-H. Castel). Cialdella's article thus provided a reasonable evaluation of the Inserm's expert report, and one that was more scientifically relevant than that of other psychiatrists [48].

While refuting the accusations of statistical bias and of biases of allegiance made by his *psychoanalyst* colleagues (psychiatrists and psychologists), Cialdella, with intellectual honesty:

- judged as justified the critique formulated by these psychoanalysts regarding the use of parametric methods, such as "the use of Student's '*t*' without verification of the distributions in level 1 studies. [. . .] This is a real limit [. . .] to the Inserm's expert report" ([47], pp. 787–788);
- recognized that the mixture (with no precautions other than a preliminary announcement) of level-1 studies founded on Glass's "Δ" and Cohen's "*d*" can generate "around ± 10%" of margins of error in the results (a margin that he found acceptable) ([47], p. 788);
- admitted that "there is a problem in the presentation of the results in the Inserm expert report, which are given according to the level of proof (of absolute efficacy)" ([47], p. 788), whereas the expert report was not a statistical study; invoking levels of evidence, whatever the metric, has no meaning beyond a metaphorical one;
- agreed that "the greater number of studies on the absolute efficacy of CBT, compared to those of PT [psychodynamic therapies] means that the former reach the threshold for a level of proof, whereas the latter do not" ([47], p. 788);
- sympathized with the analysts' assessment that "the DSM-III [. . .] broke up the old neuroses (especially hysteria and anxiety neurosis) into a variety of rubrics on the I and II axes and introduced new categories, such as social phobia. Because of this, the seeming lack of proof of the absolute efficacy of PT could perhaps be partially counterbalanced by the proofs obtained for the neuroses, which were not selected in the report's 'synthesis' chapter" ([47], p. 788);
- reminded readers, by correctly restituting the expert report's level of evidence, that "indirect comparisons of absolute efficacy are without any conclusive value [. . .] as they are distorted by experimental conditions that often differ widely," and that there are, in fact, "unfair indirect comparisons" in the Inserm report, which greatly undermine its conclusions ([47], p. 788–89);
- noted that "the Inserm expert report disappoints in its failure to take into account the multiplicity of evaluative criteria and comorbidities" ([47], résumé), which is a consequence of the report's being based on RCTs and meta-analyses thereof. One criterion of RCTs is the absence of comorbidities for a subject's inclusion into a research cohort – which is the opposite of what occurs in daily clinical practice.

For all of these reasons, Cialdella concluded that the Inserm expert report "could be critiqued" ([47], p. 788). In order to achieve its stated objectives, the report should have accounted for its numerous biases and limits, and should have, therefore, been more moderate in its conclusions. Remember that, on such a frail methodological and empirical basis, the report peremptorily concluded that: "it is now clear that there are significant differences between the different approaches. The most positive effects are attributed to cognitive and behavioral approaches in overall comparisons [. . .]" ([49], p. 418).

By way of comparison, one of the international reference manuals on psychotherapies concluded, at the same moment (2004), that: "When examining these well-designed studies that compare different therapeutic orientations, the findings are consistently small or negligible" ([50], p. 166).

Cialdella arrived at his more modest conclusion in 2007, based on the data contained in the report. If we consider only the reliable part of the expert report, that is, the ensemble of the results of *direct comparisons* between different types of therapies, what do we find, according to Cialdella? That the statistical gaps are, in these cases, very small: "the examination of the studies or [the meta-analyses] of direct comparisons between PT and CBT doesn't demonstrate the crushing superiority of CBT [. . .]. [T]he differences between CBT and PT are weak, and rarely exceed an effect size of 0.20" ([47], p. 788–89).

It is useful to remember that the standard in the international literature is to consider an "effect size" (ES) as "large" when it falls between 0.8 and 1. An ES between 0.5 and 0.8 is considered "medium;"

"small" between 0.5 and 0.2; and "very small" when it is less than 0.2 ([51], p. 228). The report's data, when it comes to direct comparisons – the most reliable – thus indicate a difference that "rarely passes" the threshold of very small.

For Cialdella, the *non-significativity* of the results invalidates the critique of the results' partiality: how can an expert report that, fundamentally, did not find a notable difference between the effectiveness of psychoanalysis and that of CBT be suspected of allegiance bias? In so doing, he minimizes the report's conclusion – that CBT would have "clearly" won the battle of efficacy –, a final result that, as he himself admits, goes beyond the data that were patiently collected. In Jean-Michel Thurin's telling, this surprise break with the scientific method – due to the handling of the conclusions by the pro-CBT collective of experts – pushed him to distance himself from the published report ([6], p. 24).

Cialdella recuses the accusation of statistical bias, arguing that the Inserm report is not a statistical study (of level I) and cannot, therefore, contain bias of this order. So be it. But he fails to remind his readers that, in light of choices made, 113 of the 124 studies on the efficacy of psychoanalysis were excluded from the report's synthesis, with the resulting ratio of eleven versus several hundred "studies of the efficacy of psychoanalysis *vs.* CBT" ([6], p. 24).

Beyond the acknowledgment of bias and of the weak significativity of the results, Cialdella's position is to applaud the *existence* of the report as a first step towards what seems, to him, to be a necessity in the future: the comparative evaluation of different types of psychotherapy. With France lagging behind in international research, he defends the Inserm's attempt, even if he recognizes the problematic character of its conclusions. His article ends with him calling for the development of RCTs in order to test psychoanalysis and for the elaboration of more reliable meta-analyses, without, however, adhering to the report's methodological exclusivism – noting that "we should also carry out naturalistic studies, which remain indispensable for better appreciating the generalizability of the results obtained in [randomized] studies" ([47], p. 789).

The double program – experimental and observational – that Cialdella urges researchers to adopt to is exactly that which was put into place at the turn of the millennium in the field of the study of psychotherapies – including psychoanalysis. Starting in the Anglo-Saxon world, psychoanalysts began to multiply randomized controlled trials in order to test, in laboratory conditions, the *efficacy* of their practice, while continuing to improve their observational studies with the objective of more precisely understanding the *effectiveness* of their interventions – that is, the psychic processes of change, concretely induced. In France, after other calls to evaluate the effects of psychoanalysis [40], a vast project of *naturalistic* studies of effectiveness – *cf.* the "Network of Research Based on Psychotherapeutic Practices" (RRFPP, in French)[3] – was established in 2008, under the Inserm's aegis [52].

While they remain mostly unknown in the French clinical milieu – even though certain pioneering French research groups are referred to therein [53–56] –, the very numerous studies of effectiveness produced since the 2000s, especially on the international stage, offer renewed points of view on analytic practice, both on the identifiable mechanisms of its effectiveness and on its measurable clinical benefits. It is important, here, to give an overview.

3. Overview of current conclusive data on psychoanalysis's effectiveness

I will not go into the details of the methodological operations required to evaluate psychoanalysis. Each type of effectiveness study has its advantages and disadvantages, in view of the specificities of analytic practice [53,54,57–59]. In medicine and, moreover, in psychotherapy, the current consensus is that "there is no *universal* gold standard" ([60], p. 22) for methods of evaluation: "There is no 'best evidence,' except in reference to particular types of problem, in particular contexts" ([61], p. 106).

Regarding such precise evaluative stakes, researchers – whether they are psychiatrists, psychologists, psychotherapists, or psychoanalysts – will choose the methods that they consider the most appropriate [62–69]. With regards to analytic therapies, three large types of studies are currently being carried out:

[3] For a presentation of the project, readers can consult: http://www.techniques-psychotherapiques.org/Reseau/ProjetRRFPP.html.

- randomized controlled trials, in laboratory conditions;
- meta-analyses based on the former;
- controlled observational studies, on cohorts in natural conditions.

3.1. Psychoanalysis and clinical trials: the state of the literature

Starting with the first randomized controlled trials that included psychoanalysis, in the 1980s, the results indicated an efficacy that was generally equal among the different therapeutic approaches, whether this was for opioid dependence [70] or – in the case of a large-scale project carried out by the US National Institutes of Health – for depression [71].

Since these pioneering experiments, one can, with the necessary prudence, affirm that analytic therapies are effective and that they are, generally, *as effective* as other forms of psychotherapy that are tested (cognitive-behavioral, interpersonal, systemic, humanist, familial), for:

- *mild personality disorders* – which include, in part, the "neuroses," in the psychoanalytic sense of the term [72–75];
- anxiety disorders [76];
- anxiety comorbidities in panic disorders [77];
- social anxiety disorders [78,79];
- phobias [80];
- social phobias [81];
- post-partum stress disorders [82];
- panic attacks [83];
- post-traumatic stress disorders [84];
- pathological grief [85,86];
- severe depression [87–89], post-partum depression [90], depression in elderly people [91], depression in family caregivers [92];
- borderline personality disorders [93–99];
- problems in couples [100];
- intimate partner violence [101];
- alcohol dependency disorder [102];
- opioid dependency disorder [70];
- eating disorders (anorexia, bulimia) [103–106].

We could do a point-by-point discussion of the precise nature of the analytic therapies tested in these studies (several sub-currents exist), as well as of the types of CBT (they are numerous), or more precisely differentiate between the types of disorders and the types of results – or, better, examine the methodologies use for each of these studies.

However, there's no risk in concluding, based on these studies, that psychoanalysis, at least when it's based on several fundamental principles (*i.e.*, a psychodynamic therapy, using verbalization, centered on the unconscious transference relationship, both interpretative and supportive) is effective – and, in general, as effective as other approaches –, which is the conclusion arrived at by the vast majority of international syntheses of reference today [12–22].

3.2. Meta-analyses of psychoanalysis's efficacy: the current state of the literature

In order to accumulate the results of the aforementioned clinical trials into a statistically more reliable level of proof (what is referred to, today, as "level I"), different calculation methods have been invented – initially, in fact, in the field of the evaluation of psychotherapies [107]. Without going into the extremely complex details of their elaboration – still under debate –, these methods make it possible to create an index called the "effect size," which allows for the quantification of the probability that a working hypothesis H_1 (for example, the supposed efficacy of a therapy) is at the origin of the obtained results, in contrast with the null hypothesis H_0, constituted by the control group (whether this group receives no therapy or an alternative therapy). In other words, the larger an effect size is,

statistically speaking, for a given therapy, the greater the justification for rejecting the null hypothesis, which inversely augments the probability that the observable clinical effects can be attributed to the therapy under consideration and not to chance [108,109].

For the past 30 years, meta-analyses have shown a medium-to-large effect size for the efficacy of all psychotherapies, compared to control groups, and negligible differential effect sizes (between different psychotherapies), even when very small numerical gaps appear from time to time, in one direction or the other.

3.2.1. *Psychoanalysis's efficacy compared to control groups*

Statistical studies have thus shown that psychoanalysis has a clinical efficacy superior to that of control groups, *for all pathologies*, and in *general* [110][4], in the *short-term* [111,112][5], and in the *long-term* [113].[6]

Compared to control groups, psychoanalytic therapies appear to be particularly effective, in the *short-term*, for:

- common troubles (non-psychiatric) [114];[7]
- personality disorders with depressive disorders [115];[8]
- psychiatric disorders [116];[9]
- somatic disorders [117];[10]
- problems of social anxiety in adolescents [118].[11]

They also appear to be particularly effective, *in the long-term*, compared to control groups, for *complex psychic disorders* [119].[12]

Psychoanalysis's greater efficacy, compared to the placebo effect, is therefore not only proven by isolated clinical trials. Meta-analytic operations allow us to obtain a series of independent results *of a higher level*.

3.2.2. *Psychoanalysis's efficacy compared to other approaches*

Meta-analyses have also shown that psychoanalytic therapies have an equal clinical efficacy *compared to all concurrent approaches* and *for all pathologies*, in the short as well as in the long-term [12,13,22,111,114,120–122].[13]

Compared to other forms of treatment, psychoanalytic therapies prove to be just as effective as other therapies for:

- anxiety disorders [123];
- depression [124];
- alcohol dependency disorders [125].

In a direct comparison to CBT, psychoanalytic therapies are just as effective for:

[4] ES of 1.01 and 0.18 one year later.
[5] ES of 0.91 for the first, and ES of 0.71 for the second.
[6] ES of 1.8.
[7] ES of 0.51 to 1.08.
[8] ES of 1 to 1.27.
[9] ES of 1.39 for precise problems; 0.9 for psychiatric symptoms; 0.8 for social functioning, all of which increase over the long-term (1.57, 0.95, and 1.19, respectively).
[10] ES of 0.58 to 0.78.
[11] ES of 0.74 (note that CBT obtain an ES that is higher by 0.26, *cf.* ES = 1).
[12] ES of 0.44 to 0.68.
[13] I am not distinguishing between meta-analyses and critical reviews of meta-analyses. The latter ultimately refer back to the former.

- personality disorders [126];
- *depressive disorders* in general [127,128], both in the short-term [129–131] and in the long-term [132];
- schizophrenic disorders [133].

Psychoanalytic therapies, when combined with pharmacological treatment, also augment the degree and the quality of recovery in cases of mild to severe depression [134].

In this sense, the international literature indicates the absence of significant proof of the superiority of one type of therapy over others, even if this remains a periodically controversial subject [113,128,135–141], and even if, for certain disorders and under certain conditions, slight differences can sometimes appear: for example, in favor of CBT for obsessive-compulsive disorders [142], or in favor of PT for personality disorders [126] – notably (in the long-term) when these disorders are complex [138].

3.3. Controlled observational studies, in natural conditions

While randomized controlled trials were initially considered the gold standard of research protocols (borrowing from medicine and from pharmacology), increasingly, their practical limitations have undermined their *monopoly* on excellence, *in any case, as concerns the field of research on the effectiveness of different psychotherapies* [143–146].

Carrying out clinical trials requires a rigorous respect for very strict methodological clauses. In order to comparatively test a specific factor, it is necessary to neutralize any other factors that could possibly influence the outcome (relative to the phenomenon being studied), that is, de-complexify, in a laboratory setting, the *real-life situations* that are tested – situations that include a multiplicity of factors, even if one can never know with certainty what they are, nor to what degree they have an impact [61].

With regard to psychotherapies, it is necessary, for example:

- to neutralize the individual characteristics of the therapists (by standardizing the therapies and by establishing strict protocols for the "administration" of "treatments" to patients);
- to neutralize diagnostic differences by relying on tools with a strong inter-rater reliability (such as the DSM, which was developed to this end, after the Second World War);
- to neutralize the differences in evaluative procedures (by employing standardized scales of measurement);
- to neutralize the placebo effect (by creating control groups, as well as comparative groups);
- to neutralize selection bias (in randomly assigning patients to each of the test groups).

Researchers who evaluate the efficacy of psychotherapies, even if they themselves laid out these methods (to respect the clauses of "evidence-based medicine"), have recognized the difficulty of applying them to the field of psychotherapies. The de-complexification of situations involving psychic care is, in effect, extremely difficult to put into *practice*. One does not administer a psychotherapy as one might administer a medicinal treatment. We can observe, in large, multi-site RCTs, significant differences of effectiveness in the results – not for the *types* of psychotherapies being tested, but in relationship to the *sites* where the trials took place. Researchers have been forced to suppose the existence of *local*, hidden factors that are not specific to the therapies being evaluated.

Therapists' personalities, their skills, their experience, and the quality of their interactions with patients have all been demonstrated to significantly influence the effectiveness of psychotherapy [147–152]. As for patients, the concrete state of their disorders, as well as their evolution – which is not taken into account by syndromic diagnoses –, their ability to adhere to each type of protocol (to feel as if the treatment is working, to believe in their therapist's approach), and the establishment – or not – of an interpersonal patient/therapist relationship have been shown to be determinative of the effectiveness of different types of psychotherapeutic treatment [14,153–156].

Moreover, clinical trials, because of their methodological strictness, can only test a very small number of people suffering from psychiatric disorders. In order to constitute sufficiently homogenous

groups of patients (with the same targeted disorder, "all things being equal"), researchers are obliged to exclude any patient presenting a comorbidity. But, in mental health, multiple pathologies are the rule, not the exception to the rule, as a subject's psychic functioning is a unified and indivisible thing [157–160]. Therefore, the most logically reliable results of RCTs are also, clinically, less significant [161,162], since they are only weakly representative of typical psychotherapeutic practice [163,164].

The situation in which a therapist, having taken refuge in a practical manual, administers a protocolized, targeted therapy (as if it were a mechanistically active pill) to an otherwise healthy patient (having only a localized, and well-defined disorder) *does not exist.* As a consequence, RCT results – which are also, let it be said in passing, rather unsatisfactory from a research point of view, since they are nondiscriminatory – remain very artificial. As tests of "laboratory therapies" ([165], p. 141), they offer few guarantees of effectiveness to patients and to their therapists, as soon as said patients and therapists find themselves *in the real world*. This critique of the illusion of "efficacy" has pushed the research field to attempt to determine the processual mechanisms of "effectiveness" [166]. The door to observational studies has been reopened. These studies, indeed, have the advantage of allowing researchers to measure effectiveness in a way that better reflects therapeutic practices in ordinary conditions.

When one can no longer experimentally "neutralize" the complexity of clinical phenomena, other tools, such as observation and description, allow the researcher to broaden her/his perspective. Finding "the" supposedly best therapy, *in and of itself* (that is, independently of context, as if we were dealing with a medication for the *psyche*), becomes less important than determining the contextual ingredients – those that are common to all therapeutic situations – of effectiveness [149,167].

Today, alongside RCTs and meta-analyses, there is more and more clinical research based on series of case studies and on single case studies, whose objective is to respond to the seminal question asked by G. Paul in the 1960s, one that went against the grain of the then-dominant experimental research model: "*What* treatment, by *whom*, is most effective for *this* individual with *that* specific problem, and under *which* set of circumstances?" ([168], p. 111).

These qualitative observational studies of effectiveness allow for a future quantification of their results and thus for certain statistical operations – including meta-analyses. Today, scientific research on the effectiveness of different psychotherapies requires both types of research, even if, in the political realm, deciders still favor the results of meta-analyses of experimental clinical trials – as we can see in France. And this thanks to the medical model, which, because of its irrefutable successes, promotes the ideal of a scientific knowledge that can effectively intervene in the real – *including for psychotherapies.* For clinician-researchers and for their patients, however, the understanding of the stakes of the concrete situation of a psychotherapy is just as important – if not moreso – than statistical probabilities in the lab.

Such controlled observational studies have, in any case, rigorously demonstrated psychoanalysis's effectiveness compared to a simple placebo effect, as well as its differential effectiveness: equal, for a large number of known disorders [20,169–171], to that of other approaches. But above all, these studies allow for the detailed and careful analysis of the concrete processes of change. It would impossible to cite all of the relevant studies that have been published since the 2000s. Readers can consult, if they so desire, the largest literature review on the subject, regularly updated by the International Psychoanalytical Association's independent research committee [59].

I can mention, however, a selection of recent work. For example, psychoanalysts have been able to *clinically* study, through the crossing of multiple scales of measurement:

- the precise differential effects of interpretations of the transference in the psychoanalytic relationship [172];
- the different sequences of psychodynamic recovery, for long-term analytic therapies [173];
- the precise link between new subjective positions and the recognition of a certain number of analytic processes [174];
- the stages of transitional psychic processes over the course of a psychoanalytic psychodrama [175];
- the reduction of behavioral disorders and the development of emotional experience and of relationships with others, in the case of childhood autism [176];

- the differential declension of the effects of the encounter, based on the pre-treatment state of two patients with identical diagnoses and treated by the same psychoanalyst [177];
- the impact of "dosing" analytic therapies for patients presenting depression and a personality disorder [178];
- the comparative measurement of symptomatic improvement, as well as various "structural" changes, for a range of therapeutic approaches (including CBT) [179].

4. Conclusion

In a now-famous article from 1982, Morris Parloff compared the ensemble of psychotherapeutic practices to a young Bambi, confronted with the Godzilla of scientistic and statist demands for evaluation. What would happen to these fragile, contextual, and complex (human-scale) experiences of care once they found themselves on the Procrustean bed of the experimental methodologies pushed by public health policies [180]?

Thirty years later, Richard Glass responded to Parloff. He observed that CBT enjoyed a period of widespread success in the 1990s (during which period the various psychoanalytical approaches fell behind), but that psychoanalysts now actively participate in the legitimate effort of evaluating their practices, including experimentally. The psychotherapeutic "Bambis" survived Godzilla's attack [181]. And psychoanalysis came out of this battle with a more subtle and critical reflection on its own objectives, its constituent principles, its limits, and its operational toolkit.

Five years ago, Leichsenring and his team updated a large-scale meta-analysis (previously directed by Leichsenring) on the effectiveness of psychoanalytic psychotherapies. The title speaks to the discipline's newfound combativeness: "The Empirical Status of Psychodynamic Psychotherapy. An Update: Bambi's Alive and Kicking" [182].

Today, psychoanalysis has reappropriated a part of its scientific history that had been long depreciated – both internally and externally. While this history remains less well-known in France than it is in the rest of the world, psychoanalysts have been quali/quantitatively evaluating their practices since 1917 [27]. Up until the 1980s, the majority of their production took the form of retrospective accounts, independent studies of individual therapies, and "pre-post" prospective studies of structured cases. From the 1980s up until 2000, analysts initiated large-scale projects of controlled observational studies. And over the past fifteen years, psychoanalysts have adopted randomized controlled trials – thereby giving themselves the possibility to produce meta-analyses of the effectiveness of their practices.

The experimental results (RCTs) have demonstrated their scientific value, albeit a limited one. Psychoanalysis has proven itself to be as effective as CBT or a number of other therapies – among the rare types of therapy that are tested and evaluated. Rosenzweig and Luborsky [183,184] referred to this impasse as the "Dodo's verdict," an allusion to the famous caucus race in *Alice's Adventures in Wonderland*, where the Dodo declares, after a race devoid of any common starting point, path, or finish line: "everybody has won and all must have prizes" ([185], pp. 64–65).

This conclusion, even in its restricted form – that is, a *non-relativist* conclusion that refers only to the handful of approaches that have been tested – remains controversial [13,144]. Whether or not it will eventually be possible to classify the different types of psychotherapy through clinical trials in experimental conditions, current research is increasingly oriented towards a vast program of studies in natural conditions, less centered on the *specific factors* of effectiveness than on its *contextual factors*, those that are "common" to different psychotherapeutic situations – all of which are seen as requiring evaluation, or even more.

Psychoanalysis is participating in this current research approach, to which a majority of practitioners of the various psychotherapeutic techniques adhere. The discipline is turning towards sequential intra-case studies, through which it attempts to understand – through the creation of inter-case series and the generation of statistical generalities – the different factors at work. In France, this current is represented by the Network of Research on Psychotherapeutic Practices (RRPP, in French), coordinated by Jean-Michel Thurin, at the Inserm (unit 669).[14]

[14] See http://www.techniques-psychotherapiques.org.

There is still much to be done, but it is undeniable that psychoanalysis has renewed its commitment to the field of research on psychotherapeutic effectiveness – thereby scientifically explaining the therapeutic benefits that it brings to patients.

In light of the conclusive data on the efficacy of other types of therapy, public health policy decision-makers in France should logically recommend (as is the case in many other countries) a plurality of approaches that have proven their effectiveness at one level – psychodynamic, cognitive-behavioral, psychosocial – of the caregiving relationship. The best interests of patients is at stake.

Disclosure of interest

The author declares that he has no competing interest.

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