… Are we really so sure "we're not in Kansas any more"? Quantitative methods and research epistemology in psychotherapy: a critical perspective

by Massimo Grasso[*] and Pietro Stampa[**]

> evidence — *n.* 1. the state of being evident **2.** something that makes another thing evident; sign
>
> evident — *adj*. [< L. *e*-, from + *videre*, see] easy to see or perceive; clear
>
> from *Webster's New World Dictionary*

Introduction

An ideological illusion turns up periodically on the scene of the cultural debate, appearing every thirty to fifty years. The last time it took the semantic form of the "neutrality of science"; today it is in the guise of its "a-theoretical" nature.

The scientific-professional community is awash with a sort of repulsion towards the components and products of non-immediately quantifiable thought, in favor of an objectivizing reductionism that vastly overrates technology, servomechanisms, shortcuts and schematicism. The prevailing tendency is to reduce every continuum and all complexity to simple, discrete elements that can be measured in a linear way. The role of subjectivity in models of representation of reality is drastically cut down — subjectivity becomes "noise", and in order to get rid of it more easily without being forced to recognise it, it is translated into numbers. One imagines observable "facts" beyond all interpretation. This is a remarkable turnaround: it is no longer god that is dead, but Nietzsche.[1]

The illusion changes its name, but it is still the same. A historian of ideas could reconstruct its route from the middle ages to our times: but such an operation of *Begriffgeschichte* is outside our domain and outside the spatial and conceptual limits of this article — we will therefore confine ourselves to talking about it in terms of current interest, with an occasional flashback to the 1950s-70s. And we will obviously confine ourselves to the domain of the research and professional practice of clinical psychology and psychotherapy, in which the pervasiveness of the illusion, as if by a "domino effect", brings a certain numer of conceptual consequences that constitute a corresponding number of epistemological pitfalls, which give rise to further systematic distortions of reality in the representations of interpersonal relations in instituted and non-instituted contexts, and of the mental life of the single subjects involved in the relationships themselves.

This illusion has been the focus of some of our recently published work (Grasso & Stampa, 2005, 2006, 2007; Grasso, 2006) in which we have tried to examine in depth some questions concerning the concepts of health and mental illness and their connection to the diagnostic and therapeutic pratice in the psychological and psychiatric domain, issues that will in part recur in this article.

Let us go over the central issue. Thinking about clinical work in psychology and in particular about the outcomes of the clinical psychology and psychotherapy intervention, and about the dynamics involved in these processes, has in recent years been gaining in importance. But within this thinking the aspect that has been dealt with the least is precisely the one that, more than all the others, could give it meaning: we are referring to its methodological framework.

As we will try to explain below, we have often found ourselves dealing with positions that have given sustenance to our observations and arguments, at times very critical because of the

[*] Full Professor of Clinical Psychology – "La Sapienza" University of Rome.

[**] Clinical psychologist in private practice, Rome; teacher of Psychology of organizations and educational institutions – University of Chieti.

[1] References for the jokes: respectively, Preface to *Thus Spoke Zarathustra. A book for all and none* (1883-1885 [1976]): «At one time a sacrilege against God was the greatest sacrilege, but *God is dead*, and so all these sacrileges are dead too"; and § 481 of the posthumous collection published under the title of *The will to power* (1901 [2006]).

simplifications and reductionisms they feed on. As in the well-known film *The Wizard of Oz*,[2] we have had the feeling that we have run into many Dorothy Gales, who, having left the mediocre reality of the farm where they live – sepia-coloured in the film – and with their big eyes wide open upon seeing a world suddenly in colour, where everything is big and beautiful, marvellous and amazing, turn to their little dog and say, *«Toto, I've a feeling we're not in Kansas anymore».*
But are we, researchers and professionals in clinical psychology and psychiatry, really sure that we are not in Kansas any more?
In other words, we felt that research in clinical psychology and psychotherapy has often been fed by facile enthusiasm and dubious excitement about seemingly simple, brilliant solutions to complex problems: but often they are not simple solutions but rather, in our view, simplistic expedients. As in the film promotion, we seem to have heard now and then resounding slogans like ‹‹*Mighty Miracle Show of 1000 Delights*» or «*Gaiety! Glory! Glamour!*», just as the belief has been passed on the us by someone that we are unquestionably on the *Yellow Brick Road*, that is, on what we could call the "right"[3] road, with no possible doubts or second thoughts.
We know very well the risk we are running by adopting a critical position (and we will state at the outset that we do not enjoy it), in order to dampen the raptures of the many elated Dorothies encountered, to embrace the viewpoint of the *Wicked Witch of the West*, if only to avoid ending up being melted by a bucket of water on the kitchen floor: but also because, more substantially, our intention is not to smother the yearning for research and for the new and find ourselves trapped in inertia and immobility[4]. Our position is therefore not an easy one and we realise that: however, we do want to point out, if our eyes reveal it, like Andersen's boy («*but he has nothing on at all*»[5]), the "emperor's possible nudity" and perhaps remember, and remind ourselves, that behind the magnificent wizard of Oz, there may at times be hiding a harmless old man from Omaha, Nebraska.

Before examining our criticisms (constructive, we feel) in detail, we will offer the readers three brief examples of current clinical attitudes.

Three clinical fragments

(1)

[2] Famous film directed by Victor Fleming with screenplay by Noel Langley, Florence Ryerson, Edgar Allan Wolfe, Usa 1939, starring the 17-year-old Judy Garland. The film was based on the book by L. Frank Baum who wrote numerous children's stories about Oz, the fantastic land that owes its name to the two letters identifying the bottom drawer of the writer's filing cabinet. The film refers specifically to the book *The Wonderful Wizard of Oz* (1900).
[3] The traps that can be hidden on this golden road were dealt with by, among others, Stuart M. Kaminsky (1978) in his most enjoyable *Murder on the Yellow Brick Road* (in Ialian *La strada di mattoni gialli*, Giallo Mondadori n. 1795 of 26 June 1983).
[4] On the other hand, it is Dorothy herself who paradoxically, but only seemingly, quickly gives up the fleeting and unfounded elation of the dream and the escape — «*There's no place like home*» —, like the right minded, conformist America of the 1930s-40s, and soon proves to be the most strenuous defender of traditional values, conformism and the return to "normality". And as we have already got onto this subject, it is worth recalling the comments on the world of Oz and its deep roots in the "foundation myths" of the culture of the United States, made by a thoughtful critic, Neil Earle, who in *The Wonderful Wizard of Oz in American Popular Culture: Uneasy in Eden* (1993), argues that the work contains a series of profoundly American themes: the Frontier, the importance of hard work, the possibility of getting what you want and making your dreams come true thanks to personal gifts like willpower, intelligence, courage, a good heart, and lastly individual liberty seen as an absolute value. To further extend our metaphor, all these qualities and these values put together perhaps make a good pioneer, but — so it seems to us — they are not enough (and some are superfluous) to make a good *scientist-practitioner* in the field of psychology-psychiatry-psychotherapy …
[5] Cfr. *Emperor's New Suit*, in *The Complete Hans Christian Andersen Fairy Tales* (ed by Lily Owens) (1981), New York: Avenel Books. "*Men han har jo ikke noget paa*", in the original Danish "*Keiserens nye Klæder*", published on 7 April 1837 in the collection "*Eventyr, Fortalte for Børn. Første Samling. Tredie Hefte*".

A patient — a well-educated fifty-year-old woman — during a psychotherapy session recounts the following experience.

Her gynaecologist, whom she goes to once a year for routine checkups, after asking her about her emotional state and some aspects of her sex life, expresses the opinion that she is suffering from a mood disorder linked to the menopause, and prescribes an anti-depressant.

In the next few days the patient goes to a physician who has been treating her for a lingering bronchitis. The patient feels well, and says happily that she is cured and has therefore stopped taking the antibiotics. The doctor kindly chides her: it is up to him to say whether or not she is cured! The patient gets undressed, the doctor proceeds with the auscultation using his phonendoscope. He promptly hears that the bronchitis is still active despite the woman's sensation of wellbeing, so she must continue the treatment for a while.

The patient then mentions the gynaecologist's prescription, asking the doctor his opinion. In response, first she gets questions: do you feel sad? — are you drinking too much alcohol? — when you wake up does the day seem to be pressing down on you, loaded with toil and problems? — do you cry often? — do you wake up at night and have trouble falling asleep again? — are you easily irritated? — do you feel a "reduced sex drive"? — do you think of suicide? — etc. The patient cooperates as well as she can, but she does not answer a lot of the questions with a clear Yes/No, but rather, with "perhaps", "sometimes", "it depends"…

At the end of this examination, the doctor informs the patient that there is depression, but it is not as serious as the gynaecologist thought: he therefore prescribes another "lighter" anti-depressant.

The patient comments on the story thus far as follows: «You know doctor, I would not like to take these anti-depressants, in part because psychotherapy has got me used to considering my emotions in a more complex and nuanced way than how they can be expressed in schematic questions and answers; and in part because — to tell you the truth — I didn't always answer the doctors truthfully … On certain more intimate subjects I have difficulty talking about myself: in psychotherapy too, at the beginning, it took months and tens of sessions before I found the courage to reveal some of my thoughts and moods…In short, what was the basis for the two doctors diagnosing depression, serious or not? I didn't let on, but actually I was a little reticient! Depressed? Of course I'm depressed. I already knew that myself!».

(2)

A man of about forty comes to a psychological consultation. He has a specialized legal training but works for a public corporation where he carries out routine secretarial work and job placements, which makes him feel deeply angry and humiliated. He presents a major free floating anxiety disorder, which tends to dwell obsessively on properception (he feels a sort of itch in the head; what can it be? — he feels a "weight" on his stomach: is it serious?…)

The patient describes his meeting with a psychiatrist to whom he had initially been referred by the family doctor.

The patient explains in detail (and with some agitation) the variety and intensity of the symptoms: the psichiatrist, who up to now has been listening in silence, gives him a short questionnaire "Not at all / A little / Quite a lot / A lot", with items like: I feel upset, I feel cheerful, I am worried about possible accidents, etc; the patient compiles it carefully.[6]

At this point the psychiatrist asks him to lie down on the couch and puts electrodes on his fingers, then turns on a device that makes a sound of varying loudness while lights come on; the patient is invited to relax and follow the rhythm of these sensorial stimulations — in short, a biofeedback programme.

The treatment ends after a few minutes: the psychiatrist asks the patient to come back to the desk and asks «How do you feel now?» Reply: «Exactly the same as before». The patient is asked to complete the questionnaire again, and — why doubt it? — the result is identical to the previous time. The psychiatrist then explains that the efficacy of a therapy is measured over time, and offers the patient a "package" of 10 sessions, also telling him the cost (incidentally, quite high). The first session, the demonstration, is free: the patient will only pay for the consultation (an average fee).

The patient leaves the consultation room thinking the psychiatrist must be mad if he expects him to come back, and later — hoping to find a very different welcome — he goes to the psychologist recommended by common friends.

(3)

[6] Probably the X-2 form of the *State-Trait Anxiety Inventory* by Spielberger and Vagg (1970 [1980]).

This patient is about forty years old, with a low education level, and is affected by a major disorder in controlling her impulses: she is bulimic, smokes too much, spends too much on useless purchases. She has been in psychotherapy for a while, and one day she tells the psychologist about yet another attempt to give up smoking. After uselessly trying nicotine patches, ear clips, acupuncture, acupressure, pranotherapy, Bach flowers … she has come to a doctor who practises hypnosis and "guarantees" — as is explained in a brochure scattered around the crowded waiting room — a positive outcome in 85% of the cases (she was told about him by a friend who actually gave up smoking after attending several sessions).

After the introductions and having acknowledged the request to "make her get over the smoking habit" (not to "be helped to give up smoking", notice that the agreement therefore envisages that the result will be completely the responsibility of the doctor) the patient was asked to sit on a chair in front of the desk, and the doctor started talking calmly and evenly. He told her that when he was at primary school he used to go around with another boy who however turned out to be the classic "bad company", a kind of Lucignolo: so he (the doctor, who now seemed to be at least 70 years old) played truant, did not do his homework etc etc. He realised that this was not the way a good student behaved, though. And so what did he have to do one fine day? He had to stop seeing the bad boy, etc.

The patient, at this point, feels sleepy, her eyes close and she can hardly follow the story. The doctor then tells her to "let herself go", to lean back on the chair and not to think of anything. Who knows why (actually, the psychologist listening to the story has an idea, but no matter), at this point the patient suddenly feels alert and also a little suspicious … The doctor does not seem to notice and continues talking in a monotone: now he evokes the image of black mud that slowly spreads through the room, reaching the patient's feet, starting to come up, there, it's already at her ankles …

When the mud reaches her calf, the patient pulls herself up and says words to the effect :"Excuse me doctor, but now I'm completely awake and I'm also a little irritated!", expecting the doctor to reorganize the setting in some way, to calm her down, so the session can continue.

"Madam", replies the doctor sharply, "it is you who is too stiff, too defended. If you do this you will never stop smoking. In these conditions I cannot do anything", and he interrupts their meeting brusquely (Who's next?…).

The fee for the session, which lasted half an hour: 180 euro. The patient left feeling so angry that she smoked two packets of cigarettes, one after another, that afternoon.

Some observations

Notice that these short sketches (as they appear both in the sense that the term has in English not only in clinical language, but also in that of variety theatre) present complementary situations.
The first case: the doctor that received the woman after the prescription for antidepressants by the gynaecologist, had a totally different attitude in assessing the physical state and the emotional state.
As for the bronchitis — from which the woman "felt" cured, the doctor had no hesitation in relying for the assessment on an objective conceptual and clinical-instrumental system, considering the patient to be an unreliable source; for the assessment of the emotional state, the patient's subjective feelings and experiences suddenly become such a valid source that they even allow the doctor to estimate the "seriousness" of the disorder, which moreover in the patient's mind is not represented in terms of a "pathology" (unlike the breathing problems due to the bronchitis), but in an existential dimension which she is familiar with thanks to the introspective capacity developed and valorized by psychotherapy, endowing her with interpretative categories and models.
Just let us make an incidental observation on a question we will return to later. The "naive" representation that the patient has of her psychopathology (cfr. for example Di Nuovo, 2000) has long become part of the data on which qualitative research in psychotherapy can be based: in spite of this, it is somewhat strange that — both in the anamnesis, and in the diagnosis, and in the assessment of the treatment outcome — what is relied on is the patient's capacity to provide reliable data about herself when she reports her mental life, while it is quite clear to all the doctors that the patient is unreliable when she reports her impressions of her physical state.
Second case: The psychiatrist has found — though with a method that would seem to be verging on the comical, if it were not for the patient's psychic suffering — that the "therapeutic" intervention has produced no change in the mental state of the patient, who reports subjective symptoms that perfectly overlap those he had before having the treatment. But since that treatment is envisaged as being successful in curing those symptoms, its failure, as shown by the patient's words, is not

predictive of later therapeutic failure. In this case, therefore, the patient is not considered to be a source of reliable data about himself, because this data does not match what is foreseen by the protocol of treatment.
And on the third case: Here too it would be laughable if it were not bordering on extortion. The hypnotist doctor may succeed or fail to induce subjects into a psychophysical state of trance: if he fails, it is their bad luck. Who knows, if he had not ordered the patient to relax (the patient was already more than relaxed, but perhaps he did not notice this), he would have been successful in his intention: but the patient reacted (she detests anyone giving her orders), and the spell was broken. The obligation was to provide the means, not the results: the session must be paid for all the same. (We will return to the ethical aspects of the matter later).
Is there a difference between the approach of the psychiatrist (how can we define it?, hyper-behaviorist? quantophrenic?)[7] and that of the hypnotist doctor? The latter was openly lacking in and uninterested in tools of verification, the former used them for false ends: the result is the same — by one and by the other the relationship was not for one moment considered as a factor for knowledge or for treatment at any level. The patients thus found themselves reified, reduced to objects devoid of personality and conscience, while in case (1), as we have already seen, subjectivity was promoted as being irrelevant to single-source diagnostic and prognostic purposes (bronchitis) and perfectly adequate and congruent with information for the same purposes (depression).

Some (indispensable) comments on method

Comments on the concept of mental health

We devoted much of the work cited above to analysing the arguments used by a certain number of American authors in support of different definitions of "psychic normality".
We have therefore had the opportunity of grappling with the view expressed on the concept of mental health by G. E. Vaillant (2003), with the "positive psychology" approach and the opinions on mental health of some of its most representative exponents (Seligman, 1991, 2002; Peterson & Seligman, 2004) and of the consequences deriving from such premises at the level of research design: one example for all is that of the research note published in 2001, on the connection between longevity and the expression of positive emotions, entitled *Positive Emotions in Early Life and Longevity: Findings From the Nun Study* (Danner, Snowdon & Friesen, 2001). And also with the opinions of J. C. Wakefield (1992, 1997, 1999, 2004, 2005) on mental disorder as a "harmful dysfunction". Finally we have come up against the strange and absolutely a-contextual diagnosis made by the Confederate doctors at the time of the civil war, of runaway slaves: *drapetomania*[8] or "runaway slave disorder".
But from the confederate doctors and their questionable diagnostic criteria, let us try to approach our own times, in which we believe the criteria used are far clearer and more scientifically grounded. And let us keep talking about the relation between white and black Americans.

[7] The expression is well known as belonging to the Russian sociologist P.A. Sorokin, (1965), whose work it would be interesting to re-examine on another occasion.
[8] Literally, "mania of running away", in the sense of the unauthorized leaving of an obligatory place; "deserting": in Greek military language *drapètes* (from the verb *drapetèuo*, run away, escape) is a deserter (*author's note*). One interesting thing: the rough and short sighted Confederate doctors did not realise that the culture produced in this sort of epidemic was not confined to expressing suffering, faith and hope, but also contained precise and constantly updated sub-codes with precise instructions for the treatment of the "symptom". As G.C. Roncaglia in *Il jazz e il suo mondo* (2006, pp. 43 ss.) recalls, as regards spirituals, Martin Luther King pointed out that this way of singing was also a way for the blacks to communicate among themselves without their masters understanding, even if they were present: «We used to sing of the "heaven" waiting for us», wrote the leader of the black civil rights movement, «[and] the word "heaven" for us meant "Canada", which we hoped to be able to reach». The songs contained directions in code about «safe houses, refuges, resting places and shelters» in a long chain that from the deep South "permitted and supported the flight of slaves across the United States as far as the border with hospitable Canada".

We will start for example from a recent article (Ward, 2005) that sets out to explore the subjective experience of psychological counseling on the part of African-American clients.
We therefore discover that African-American clients, perhaps because they are (constitutionally?) "suspicious" of scientific research and, even more, of researchers – as Pinn, Harden & Blehar, (2002) show – engage in (what a surprise!) an activity of continuous evaluation of their interlocutor (whether it be therapist or counselor) to decide whether or not to trust him. And this activity refers to three main aspects: the importance they attribute to their black identity, any involvement in legal proceedings, and the similarity of ideological viewpoints. This influences their perception of security in a therapy and the assessment of the effectiveness of the counselor, determining (who would have thought it?) greater or lesser degrees of openness and self-disclosure. Needless to say, *self-disclosure* is organized along a continuum that goes from absence of openness, through superficial openness, selective openness and lastly total openness.
It is interesting to notice that this exposition of commonplaces comes form an author who tells us she herself is black. It is also interesting to look at an episode cited in the article, which Ward finds emblematic in connection with her interest in the issue dealt with in her scientific work.
The episode is the following: our author is going to work (a *Community Mental Health Centre*) and on the bus, she happens to sit next to a black woman who immediately strikes up a conversation and identifiying her as a student (it is well known that blacks are born evaluators!), asks her what she does. Ward replies that she deals with psychology of counseling and her interlocutor asks if there is one area she is particularly interested in (here too, it is well known that blacks are not open and are suspicious; by the way, who is the counselor and who is the client?) and she says she is specifically interested in cultural problems and counseling. At this, the black woman compliments her saying, "we need people to deal with this stuff", and she tells her an anecdote.
Girl, let me tell you what happened to me recently. Sometimes I have mood swings you know. When the weather gets cold, I sometimes get depressed and unhappy, you know. Part of my unhappiness is related to being a Black woman living in a White city. Anyway, I decided to tell my doctor about my mood swings. He recommended counseling. At first I did not want to go, because you know how us Black folks feel about counseling. Plus, I don't want anyone to think I am "crazy."
Well, I finally got the courage to go to counseling; I thought it might be helpful. During the first 30 minutes the counselor tells me I need to see a psychiatrist for medication. What's wrong with that damn counselor? He didn't even spend any time getting to know me; he immediately started talking about medication. I just needed someone to talk to, but instead he is talking about medication. Well, girl, I never went back to see him." (Ward, 2005, p. 471)
The author quickly points out that unfortunately, this woman's perception of counseling and counselors is very common among African-Americans who go to mental health services, especially among women. In fact the decision to break off the consultations after only one session is in line with what Wade & Bernstein (1991) discovered: the scientific community is grateful to these researchers, who have the merit of revealing that African-American women's attitude to counseling and counselors (who knows why it is the women's attitude: maybe due to the very well known and scientifically proven phenomenon known as "female intuition"?) develops right from the first session influencing (here too, who would have thought it!) their decision whether or not to continue the consultations.
After the pioneering (!) work by Wade & Bernstein, the author complains that information on the issue is only anecdotal and this makes it necessary to explore the problem in a theoretically based analysis: the results of this analysis have been briefly outlined above.
We are not in a position to know the reactions of the person who inspired the work, the black woman on the bus, but we can imagine them: not one word is said, in Ward's work, on what she was told almost as an introduction to the short anecdote told by her chance interlocutor: "*Part of my unhappiness is related to being a Black woman living in a White city*".
The depth of such a statement which, however, refers to a stratified contextual dynamic in hundreds of years of *black and white Americans* living together (in which some influence, especially as regards the confidence in professional mental health figures, must surely have been exerted by diagnostic operations and we suppose by therapeutic operations of the drapetomania type mentioned above!), is completely ignored in favor of general statements like the one in the conclusion, in which the author claims, among other things, that

the findings from this study emphasize the importance of hearing and honouring the voices of the clients we serve (Ward, 2005, p. 480).

As if this were not so obviously implicit that one does not need sophisticated empirical investigations to demonstrate it and perhaps to remind those, like the counselor in the anecdote, who are implicitly stigmatised *only* because they are not up to the task. In this way a relational problem which, as such, is deeply influenced by contextual dynamics, is reduced to a *simple* fact of technical skill/lack of skill.

Yet again, the importance of listening to and respecting the voices of the clients we serve, does not *in itself* constitute a basis of the clinical action in psychology, but it is worth underlining it insofar as

clients' perceptions of counselling determine to a large extent the effectiveness of therapy (Barak & LaCrosse, 1975; Constantine, 2002; Paulson et al., 1999 — cit. in Ward, 2005, p. 480).

And as we can see, this statement, which is so obvious that it almost sounds offensive, is supported by its own little repertoire of quotations to the point.
In conclusion, therefore, from what we read above it seems that the problem of the blacks vis-à-vis the institutions responsible for counseling and psychotherapy are both that they do not feel listened to and that they feel they are treated as being "mad". On the other hand, however, it seems that the psychologists (remember the anecdote) do actually tend to behave in this way towards them. This vision of the problem seems to us to be naïve and reductive, because it does not take into account extremely important contextual variables concerning the relationship, as it has developed over time, between blacks and whites in America (but there may also be something to say about Asians, Latinos, Italians, etc.).
How can we interpret all this? We seem to be looking at a sort of naïve empiricism, descended directly from an obsolete *paradigm of simplification.* In this perspective, the world is complete unto itself, governed by linear mechanisms in which one can identify clear-cut categories of cause and effect. It is a paradigm, in other words, in which reality is assumed to be characterised by the order, stability and regularity of phenomena. If this were not so, it would not in fact be logical to try to identify precise connections between disorderly, unstable phenomena. The processes of isolation, separation and quantification required by the traditional experimental method are logically admissible only in a paradigm in which reality is described as a regular organic set, in which objects have their own characteristics, independent both of the observer and of the context in which they are found: in this case scientific knowledge is based on direct observations of reality – or on an *empirical* basis "free from any prejudice" – from which one starts, in order to reach the formulation of general principles that can explain the facts observed (Grasso, Cordella & Pennella, 2003).
But is it not true that the contribution of epistemology, specifically within psychology and, in particular, clinical psychology (cfr. for example, Lombardo & Malagoli Togliatti, 1995) has produced an enormous amount of material for thought?
Do we need to recall that it has been established that the interaction between the phenomenon, the observer and the tools used in the process of knowledge-getting is not only inevitable but must also be considered the *object of knowledge?* That the concept of *cause* has been joined by that of *condition* and that, as well as linear causality, different processes such as *interdependence* and *probability* have been valorized? And that the latter is no longer considered an expression of the inability to define the *certainty* of the links existing between the phenomena observed, but rather, a characteristic that constitutes reality?
It is widely known how greatly the role of empirical induction has been cut down to size over time. When the idea of a permanent reality independent of the observer collapses and the observer comes to be placed in a complex network of interdependence with the phenomenon observed, it becomes inevitable to declare that observation heavily depends on the theories – naturally in the broad sense - of the observer. In contrast to what was stated by the empirical epistemology underlying the paradigm of simplification, the scientific theories therefore seem to be elaborated not so much starting from sensitive data, but more from the beliefs that can be held about the phenomena one intends to study. Something else that has collapsed is the idea of a *cumulative*

dimension of knowledge – typical first of the enlightenment and later of positivist culture – in which scientific development was pictured as a sort of summation, in favor of the idea of knowledge as a process that comes about by eliminating errors.
The concept of simplification has long been replaced by that of complexity and by the consequent *paradigm of complexity* (Benkirane, 2002), in which the object of scientific interest is recognised in the totality of its elements and in the interactions between these elements and the environment in which they are situated, and of which there has been much discussion in the last thirty years.
Where has the dimension of complexity gone now? How can it be found in the studies we have quoted? What we are seeing seems, if you will excuse the term, to be a sort of simplification of the concept of complexity.
We believe in fact that it is possible to regard a system of elements definable according to the functions they serve, as "complex". These elements, however, are not simply "part" of the system – if they were, the system would be fragmentable and therefore by definition not "complex" – but they have their own ontology that depends on the context in which they are located. Outside the system, these elements have no meaning and the system itself, without these parts, loses its identity.
In the light of these considerations, let us now look at a singular debate that took place in March 2006 in the pages of the journal *Psychotherapy Research*. The issue opens with the introduction by the editor Clara E. Hill who presents the readers with the topic, to which a special section will be devoted, in the following words:

> In the psychotherapy community, the issue of whether the effects of therapy are due to the treatment itself or to the therapist has been hotly debated (Hill, 2006, p. 143).

And further on:

> If it is the treatment that works, the implication is that we need to have carefully specified treatment manuals that detail exactly what any good therapist would do at different points in the treatment process. If, alternatively, it is the therapist who is the responsible for change, we need to focus on selecting good therapists and fostering their personal growth and development. Finally, if it is actually the clients who are the major contributors to therapeutic effectiveness, we must focus on developing better diagnostic tools and tailoring our treatments to different types of clients. Hence, understanding more about the sources of effects directly influences practice, training and research (Hill, 2006, p. 143).

One obviously tends to think of the vexed question of the importance of so-called specific or a-specific factors in determining the effectiveness of a psychotherapy treatment, dressed up for the special occasion in its Sunday best. But apart from anything else, the approach to the question seems to suddenly cancel out any reference to the most salient aspects of theoretical thinking in psychology and in particularly in clinical psychology and psychotherapy: the dynamics of relating. Or perhaps it seems to unilaterally highlight the dimension of individual behavior, at the expense of relational behavior. But in this case what is being referred to is a sort of old-style behaviorism, almost totally impermeable to any contextual reference. Clara E. Hill's introduction leads into two research articles that offer to cast light on the topic, working, and this is undoubtedly a unique opportunity, on the same set of data: the results of the National Institute of Mental Health's Treatment of Depression Collaborative Research Program. The psychotherapy models considered are *cognitive-behavior therapy* (CBT) and *interpersonal therapy* (IPT). The patients considered are 119 assigned randomly (!) to 17 therapists, 60 to 9 IPT therapists and 59 to 8 to CBT therapists. The two studies (Elkin, Falconnier, Martinovich & Mahoney, 2006; Kim, Wampold & Bolt, 2006), reach (would you believe it?) conclusions that are diametrically opposed.
In fact, the contribution of Elkin, Falconnier, Martinovich & Mahoney (2006) underlines:

> There was no indication of significant therapist effects in the current analyses despite the use of a more efficient model (because all available data may be included) and a conception of treatment progress that focuses on rates of change, adjusted for baseline expectations, instead of focusing on status at a somewhat arbitrary point (p. 151).

Kim, Wampold & Bolt (2006), on the other hand, state:

> Several multilevel analyses if the NIMH TDCRP data revealed sizable therapist effects, ranging from 1% to 12% depending on the outcome variable and the model adopted. Overall, a simple mean of all the estimates was about 8%.
>
> [...]
>
> it seems clear that therapists were an important source of variability in these data.
>
> [...]
>
> the results suggest that, with regard to outcomes, therapists are more important than treatments (p. 167).

It would be sensible to stop here, and, as we used to say as children, have a good look at our conscience. In depth. But instead, in a sort of scientific-methodological-statistical death-wish, the analysis of these results, not so much diverging as decidedly contrasting, is entrusted to scholars of merit, Soldz (2006) and Crits-Christoph and Gallop (2006), for them to comment on and find justifications for the two different conclusions. And that is not all: there is still space for a rebuttal by the coordinators of the studies examined (Elkin, Falconnier, Martinovich & Mahoney, 2006b; Wampold & Bolt, 2006). As a result, over a third of the journal is taken up with this question.
Here we do not want to examine in depth the arguments produced. It is enough to underline the artificiality of the whole debate, as is also noticed by Dazzi and De Coro (2007), although they do not reach the same conclusions as us:

> The central issue is: how can the effect of the "therapist" variable be reduced in outcome research, to obtain sufficiently "controlled" studies that document the effects of psychotherapy on the patient? The various authors debate the possibility of constructing plurilevel models to enable the many factors to be kept together, and at the end of the debate, it is underlined that the attempts to construct models of data analysis that reduce the therapist effect have no sense, since all the research shows the importance of the therapist for the outcomes of psychotherapy and, as soon as the effects of the therapist are made to disappear, "all the rest will disappear"! (Wampold & Bolt, 2006, p. 186).

We wonder: wasn't this the expected result? Wouldn't we immediately say that the idea of seeing if the therapist variable can be eliminated in psychotherapy, or else proving that it counts, is an absolutely rearguard idea in the overall thinking on psychotherapy? And yet this "debate" takes up almost half the journal, only to reach conclusions, or rather, non-conclusions that seem to us could have been largely anticipated without being burdened with the highly sophisticated structure found in the two research projects. It is therefore impossible to avoid being malicious and quoting a well-known Italian saying: *it's a sin to think the worst, but it hits the nail on the head*.[9] In other words, it comes naturally to think that interest in research and the attempt to look for answers, through research, to many questions posed by psychotherapy practice, actually represent something strongly conditioned by a "market", firstly, that of research and its funding, but also that of the provision of psychotherapy and its fruition by the clients, which in the United States is so markedly subordinated to the expectations of the insurance companies. Don't try to tell us that the purpose is the best use of available resources: also implementing this kind of research involves a precise choice about how to use the taxpayers' money[10].

[9] *By the by,* life senator Giulio Andreotti, indestructible icon of the political scene since the post war period, who is credited by the *vox populi* with being the author of the saying, has several times ceded the honour to an unnamed cardinal of the Vatican Secretariat of State.

[10] We are advised, for instance, that the work cited by Crits-Christoph & Gallop (2006), «was funded in part by National Institute on Drug Abuse Grants R01-DA018935 and R21-DA016002» (p. 180). Our curiosity is whetted: what is the connection between drug abuse and therapist influence in psychotherapy, from the point of view of research funding? Don't they seem, at first sight, different issues and therefore with different sources of financial resources? Perhaps part of the explanation can be found in the simultaneous presence

Soldz (2006) warns:

> Models, being idealizations, are not true representations of reality.
>
> [...]
>
> Thus, there is no such thing as a correct model (p. 175).

If a scholar like Soldz finds it necessary to clarify such a *basic* point, then it means that he is not talking so much about the methodological aspects of the research in question[11], as about the whole cultural design and, if we can use the term, about the philosophy of science governing the research designs being examined. The question therefore shifts from the sense of the research projects proposed, to the quality of the referees that evaluate such works for publication, in short, to the quality of the journal hosting them and to its cultural policy: yet again, we underline that, *thinking the worst may be a sin, but it hits the nail on the head*.
In this perspective, a particular position in studies on the efficacy of psychotherapy treatments is occupied by the so-called EST (*Empirically Supported Treatments*), that spring from so-called *evidence based practice.* It may be interesting to quote on this the critical positions expressed, for instance, by Westen and coll. (Westen, Morrison & Thompson-Brenner, 2004), who though identifying some fundamental aporias in this kind of research, do not in our opinion take their anaylsis to the extreme consequences.
They state, in fact, that

> many of the assumptions underlying the methods used to test psychotherapies were themselves empirically untested, disconfirmed, or appropriate only for a range of treatments and disorders. (p. 632)

And also that

> unqualified statements and dichotomous judgments about validity or invalidity in complex arenas are unlikely to be scientifically or clinically useful, and as a field we should attend more closely to the conditions under which certain empirical methods are useful in testing certain interventions for certain disorders (p. 632)

Such statements basically seem to disprove not only the results, but also the overall methodological design of much of the research work in psychotherapy. Now, the malicious question that springs to our lips is the following: since such research work rejects any dimension of complexity (though still invoked by Westen and collaborators!), many of its basic assumptions, as we have tried to show above, immediately prove to contain very little sense, so how can it be that for twenty or thirty years they have found space and dissemination in numerous (respected!) journals, they have enjoyed conspicuous funding, and have involved scholars and researchers in conferences and symposia in many parts of the world? Perhaps once a “gold-bearing” vein is discovered, it is necessary to exhaust it (perhaps while “holding your nose”)[12], before going on to exploit the next one? If the blind adoption of a perspective made up of simplification and reductionism guarantees a consistent “yield”, and if it still guarantees it, must we expect that in the near future, an equally sizeable advantage may come from the belated and perhaps fictitious (is it

---

in the same journal, but in a different section, of an article on drugs and counseling written by the same authors (Cfr. Barber, Gallop, Crits-Christoph, Frank, Thase, Weiss & Connolly Gibbons, 2006)?

[11] Although, also at this level, Soldz feels the need to point out several things that seem to us not unimportant. In fact, they seem so important that they cast doubt on much (all?) of the research dealing with these issues: «it is important to keep in mind that the TDCRP data set, although one of the larger psychotherapy data sets, is quite small for sustaining the analyses reported here» (p. 175).

[12] *By the by 2,* this other widespread totally Italian commonplace was claimed by the well-known journalist Indro Montanelli concerning his vote for the Christian Democrats in the turbulent 1970s, when the Communist Party seemed to be about to gain the relative majority in Parliament.

exaggerated to say crafty?), conversion to a hasty, superficial vision of complexity? It is as if the important thing is managing to remain part of the "scene".[13]
We therefore step warily when Westen and colleagues argue that

however, is that the time has come for a thoroughgoing assessment of the empirical status of not only the data but also the methods used to assign the appellations empirically supported or unsupported (p. 632)

And so? How can one justify a debate like the one we have referred to above?
Recently Westen has again returned to the issue, reiterating that:

At a time when a drug or a therapy, to be marketable, have to prove their efficacy in carefully controlled studies, psychotherapy, too – assimilated to a medical therapy – must present itself on the market of rival therapies to its possible consumers with a pedigree of empirical evidence of its efficacy.

EST experimentation requirements The research requirements and the type of epistemology guiding the EST researchers place specific constraints on studies designed to elaborate and evaluate the treatments (Chambless & Ollendick, 2000; Kendall, Marrs-Garcia, Nath & Shedlrick, 1999; Nathan, Stuart & Dollan, 2000).

Here are some of them:

- psychotherapy validation studies must exclude subjects that undermine the homogeneity of the experimental and control samples;

- the treatments to assess must be brief, or have a pre-established duration, because the protocol must be the same in all the cases and the clinical picture presented by the patients at the beginning, and end of the therapy and in the follow-up must be assessable with all other conditions being equal;

- the therapies must aim at the treatment of a single clinical disorder that can be diagnosed with psychopathology manuals accepted by the international community (Goldfried, 2000) and must be specific for that disorder; consequently the samples for EST validation will exclude patients with co-morbidity, and any changes presented in disorders other than the target disorder will be ignored (Wilson, 1998);

- the treatments to be assessed must be manualized and the extent to which the real practice of the professionals involved in the studies adheres to the protocol must be monitored.

Summing up, the EST movement tries to elaborate brief manualized psychotherapy treatments designed to treat specific disorders, and it evaluates them using samples of homogeneous subjects treated (and/or evaluated in an identical way and only for the target pathology (Gazzillo & Lingiardi, 2007).

But what kind of psychotherapy and what types of patients are we talking about? About "real" therapies and patients? In the picture we are offered, it seems a little ambivalent to state weakly and over-defensively that

we are not advocating against evidence-based practice (Westen, Morrison, Thompson-Brenner, 2004, p. 632)

A good question would in fact be: but what do we mean by evidence? We have tried to give an answer.

Not all the evidence is overt fact, not all the overt facts are evidence

[13] On this point, an in-depth study should be made of the importance for psychologists and psychiatrists of publishing in journals with a high impact factor, to which it is not easy to gain access if the experimental design of the work presented does not respond to the standard research criteria in the natural sciences field. Although this should not have much importance (in fact it has "zero") from the point of view of clients/patients, it has a very great deal for the professionals and researchers whose access to prestigious positions in Universities and Health Services, public and private funding etc, depends on the IF. In Italy, too, some doctors have temporary contracts with Scientific Research Institutes (IRCCS) with payment directly proportional to the IF. We will examine this issue on another occasion.

In a recent article — which appeared in the same volume in which we ourselves published one of the pieces cited above (Grasso & Stampa, 2007) — Francesco Mancini and Barbara Barcaccia (2007) warn researchers and professionals to translate the expression "evidence based": properly from the English: medicine or psychotherapy based on the *scientific evidence* ("*le prove scientifiche")*, not on the obvious ("l'e*videnza")*. After quoting a work by G. Ravaglia (2003) in which the author attacks the concept of "evidence" which he considers philosophically "clumsy", Mancini & Barcaccia write:

> It is hard to prove him wrong. If it were not for the fact that '*evidence'* means "proof", not "obviousness", so *evidence-based* psychotherapy is […] simply a psychotherapy based on experimental proof of efficacy […] Nothing to do with the ideological arrogance inherent to the concept of "obvious fact". In the domain of science nothing is "self-evident".[14]

There are no objections on the linguistic level: just a slight slap of the wrists for those naïve dunces who may have thought of "evidence" in a judicial sense (the suspicion is evoked with elegant understatement by Del Corno & Lang, 2006, p. 59). There is however one small problem, which we would like to point out even to those who know English well. Can one talk about "scientific evidence" in the same way in medicine and psychotherapy? Yes and no.' Yes', if we are thinking of an abstract experimental condition, which manages to annul the context — not an easy feat. It is not simple matter, for instance to study tolerance to frustration, or the anxiety state (see clinical sketch n. 2 reported above) or the evocative memory, independently from the emotional/institutional dimension in which the setting develops, from the relationship between the experimenter/observer/agent and the experimental subject — which depends on several factors: personality, institution and organization of the setting, implicit ideology of the conceptual design of the research and many others — and independently from the connections existing deep in the subject between the processes of acquiring, storing and elaborating information, of attention and concentration. And then it depends on the subject's basic culture and abilities etc. And yet this "yes" seems to dissolve in a labyrinth of uncontrollable variables.
And on the other hand, 'no', one cannot talk about "scientific evidence" in psychology in the same way as in the natural sciences that underlie the thinking and the practice of medicine. The physiology experiments, the results of which were contrasted to the Freudian approach to origins — the exploration of the unconscious using hypnosis — still tell us little or nothing about how human beings, including therapists and patients, interact with each other.
We have already dealt with this question in this journal (Grasso & Stampa, 2006), arguing as follows:

> In medicine the distinction between "normality" in the statistical sense and in the clinical sense is at the center of a literature that is now consolidated and clear in its conceptual structure (starting with G.Canguilhem, 1966, just to mention one name). Most people have certain disturbances, produced precisely by the human organism's capacity to adapt to environmental conditions and individual and collective lifestyles in constant development: despite this, such disturbances have to be diagnosed, treated, "managed", subjected to adequate preventive measures where possible etc.
>
> The attitude on the part of medicine is made possible by the logical chain of knowledge that unwinds like a real hierarchy of knowledge: physics —> chemistry —> biology —> physiology —> general pathology —> special pathologies —> semeiotics… each of these disciplinary areas is methodologically "supported" by the one before it in the list.
>
> Now, thanks to this conceptual "pyramid", it can be established what "ideal" functioning is — and hence "normal" in a certain subject and in certain conditions — of an internal organ; and the range of variability that probabilistically defines its possibility of continuing to perform its role in the organism; and (if possible) how to

[14] The short-circuit "evidence = self-evidence" here seems to be highly significant of the pressure to eliminate subjectivity from science in general. It did not come into Mancini and Barcaccia's heads that a real fact could be "evident" *for somebody that enters a relationship with that fact,* rather than evident "in itself"? There are also some who, when faced with the same fact, do not find it obvious: it is the classic gag of Holmes & Watson, of Poirot & Hastings, or other detective couples when faced with the clues to a crime. We deal with the question a little later in this article.

intervene to ensure that the functionality is within this range. If and when it exceeds the upper and lower limits, a pathology is called a defect or an excess: also the second case is a deficit because the point is that "normal" functionality has been compromised

It is impossible to apply the same epistemological picture to psychology. There is nothing, in mental life, that can easily be ranked, ordered, studied and, even less, modified using a linear logical procedure like the one schematized above.
At this point we would like to say a few words in favor of a model of psychotherapy actually based on what is obvious, instead of on presumed "evidence". Del Corno and Lang (2006, p.59), quoted above, on this write:

But what can we define as "evident"? Without getting into a philosphical discussion that would lead us astray (and that perhaps should be done, sooner or later),[15] we have to acknowledge that the "evidence" sought by those who investigate the efficacy of a psychotherapy is not confined to the domain of what is measurable in the best RCT tradition.[16] There is a more *blurry* or *fuzzy* area that belongs to the patient's subjectivity (as well as to that of the psychotherapist) and to the relationship between them.

Moreover, what can possibly be meant in the *DSM-IV* when it says, on "Criteria for substance-induced disturbances",

B. There is *evidence*[17] from the history, physical examination, or laboratory findings of either (1) or (2):

(1) the symptoms developed during, or within a month of, Substance Intoxication or Withdrawal

(2) medication use is etiologically related to the disturbance

C. The disturbance is not better accounted for by a disorder that is not substance induced […]

It really seems that also in the *DSM-IV*, as the support to descriptive psychopathology, there is a reassertion of the circumstantial paradigm, which is medicine's oldest model of clinical reasoning, based on the evidence. This is not "self-evidence" — a privilege accorded by science only to Euclid's first four postulates (the unnaturalness of the fifth postulate being the foundation of non-Euclidian geometries) — but the evidence deriving from an appropriate use of the procedure called "abduction".
For many years the working group connected to some of the Psychology chairs at the University of Rome 1 and to the *Rivista di Psicologia Clinica*, has supported the model of the circumstantial paradigm as the best suited to explanation and interpretation in clinical psychology. In the Editorial of the last issue R. Carli (2007) again reminded us that he has repeatedly devoted articles to the question over the last twenty years:

Recently, with some colleagues interested in assessment in, I posed a question which at that time remained unanswered: *What are the factors that have made psychotherapy an activity sought after and followed by a great many people in most of the world, despite the lack of valid research that can definitively prove the efficacy of psychotherapy, in its results and its process?*
I think clinical psychology can assume two very different attitudes to this question. On the one hand there are those who are trying to operationalize clinical psychology procedure so as to make it a diagnostic and therapeutic action with its standards and its protocols, with the aim of returning to the mainstream of western medicine, organized within deductive and inductive epistemology. This means that abductive logic is completely ignored: the paradigm of clues founded and supported, as Carlo Ginsburg reminds us, by *physicians* like Arthur Conan Doyle (the creator of Sherlock Holmes), Giovanni Morelli (founder of attributionism in painting) and Sigmund Freud. On the other hand, there are those who wonder what "force" lies in the clinical psychology intervention and in psychotherapy, what evaluation of it has enabled it to

[15] We agree: and let us eliminate the "perhaps", and replace "sooner or later" with "soon". We in our humble way are already trying.
[16] The reference is obviously to the Randomized Clinical Trials model, in which the patients are assigned randomly to different groups with different kinds of treatment, in (theoretically) controlled conditions.
[17] Our italics. The Italian edition (1996) translates: «È evidente dalla storia, dall'esame fisico etc.» (It is evident from the history, the physical exam etc)

develop even without the contribution of the empirical approach. Perhaps those who practise psychotherapy organize its testing[18] as part of the procedure itself, in the relationship and the symbolic process characterizing it. If that is the area where the testing should be sought, then the objectivization of the disorder, the insistence on substituting the "objective" disorder for the problem as an eminently relational phenomenon, are aspects that may lead away from the more worthwhile road.

Let us return to *evidence* in the sense of a modality of knowledge-getting incorporated in the circumstantial paradigm.
It is well-known that during the 20th century the prevailing logic in scientific research — thanks to the theoretical work of the neo-positivists, the Vienna Circle, and Popper and Khun — veered progressively towards hypothetical-deductive models, in which the departure point of an experiment concerning a phenomenon is the previous research on the same class of phenomena and the corresponding theories of reference: after the formulation of the hypothesis, observation is used to verify it (in Popper, to falsify it).
As has been pointed out by many scholars, the limit of this method is psychological: see for instance, Gangemi (2002, p. 87):

> […] every researcher reads many books on research or theory, formulates a more or less credible, more or less important theory of his own, and then tends to seek data that confirms his theory. Normally he does this by looking for the data in whcih it is easier to find confirmation instead of following Popper's lesson that theories must be put to the test by trying to find evidence against them, not in their favor.

This problem arises all the more when the theories and hypotheses concern research fields in which the variables are highly complex and their observation involves a massive "intrusion": if Heisenberg's principle of indetermination works in particle physics, it is amplified as the scale of the phenomena grows until psychological, social and anthropological research in which the researcher may aspire to be "neutral" or "non-interfering", is made literally impossible.
Alternatively, inductive models are still present and being used in research: an example could be the *grounded theory* of Glaser & Strauss (1967), in which the formulation of hypotheses comes after the collection of data in the field (p. 23):

> In discovering theory, one generates conceptual categories or their properties from the evidence, then the evidence from which the category emerged is used to illustrate the concept.

Therefore, here too, *evidence* is again seen as something in the data that strikes the observer — we would add, reflecting the part played by the observer in the context that the data itself belongs to.
In the abductive model, first formulated by Ch. S. Peirce in 1878, the process of collecting the data, formulating and testing the hypothesis follows a path that enables adjustments to be made during the operation: the hypotheses are formulated, accepted or rejected, and reformulated depending on whether or not they satisfy a criterion of maximum plausibility as regards the phenomenon observed.
The circumstantial paradigm supporting abductive reasoning, as C. Ginzburg (1979, pp. 111-115) pointed out, suggested a clear break with the traditional scientific paradigm, but at the same time, accounted for an empirical way of proceeding that was at once rigorous and a source of reasonable certainties and that had always been typical of humanity, from hunting times among primitive men, through the writing of history, and the philological study of ancient languages, to the sedimentation of clinical experience in medicine:

[18] In meetings with psychotherapist colleagues, I suggested a testing "criterion" for psychotherapy work: the fact that the patient keeps returning to the psychotherapist, session after session. This statement may seem naive and misleading to some. I think one should consider it. Perhaps we have not yet identified the symbolic reasons for this "patient return", session after session; on the other hand, if this fact is considered apart from obligations and duties, it has an amazing force which deserves to be understood.

Between the Galilean physicist professionally deaf to sounds and insensitive to tastes and smells, and his contemporary the physician who risked making diagnoses by placing his ear to rattling chests, sniffing faeces and tasting urine, the contrast could not be greater.

The data on which abductive reasoning is based is *empirical facts*: but it is the observer himself that confers this status on certain data. The view of Peirce (Collected Papers, 1935-1966) of the three different logical procedures is the following:

	Rule: All the beans from this sack are white.
Deduction	Case: These beans are from this sack.
	Result: These beans are white.
	Case: These beans are from this sack.
Induction	Result: These beans are white.
	Rule: All the beans from this sack are white (until proven otherwise)
	Result: These beans are white.
Abduction	Rule: All the beans from this sack are white.
	Case: These beans are from this sack (probably)

Since Peirce suggests it, let us go back to the time of Galileo Galilei.
A good example of an erroneous deductive reasoning is in the chain of inferences that lead a minor character of *Promessi sposi* (chapt. XXXVII), don Ferrante, to conclude that the plague is an illusion:

"In rerum natura", he said, «there are only two species of things: substances and accidents; and if I prove that the contagion can be neither one nor the other, I will have proved that it does not exist, that it is a chimera. And here I am. Substances are either spiritual or material. That the contagion is a spiritual substance, is a wild claim that no-one would support; so it is no use talking about it. Material substances are either simple or composite. Now contagion is not simple substance; and this can be demonstrated in four words. It is not an airy substance; if it were, instead of passing from one body to another, it would fly straight to its sphere. It is not acqueous; because it would become wet, and be dried by the winds. It is not igneous; because it would burn. It is not earthen; because it would be visible. Nor is it a composite substance; because in any case it should be sensible to the eye and to the touch; and this contagion, who has seen it? Who has touched it? It remains to be seen whether it can be an accident. Worse than worse […]".

His fretus, that is, trusting in these things, he took no precautions against the plague, he caught it, and went to bed to die, like a hero from Metastasio, blaming the stars.

The theory of the four elements linked to the syllogistics of Aristotle (who in fact was a great, open-minded observer of nature) did not enable the erudite fool don Ferrante to make hypotheses on the physical nature of the illness, and the deductive procedure provided the logical demonstration of this.
But equally wrong was the conclusion reached inductively by the people and by the Health Tribunal on the same question: since at the same time as the plague was spreading, along the roads sinister figures had been seen daubing the walls with an oily potion, it was inferred that there was a cause and effect relation between the two events, and by arbitrary generalization, the myth of the "plague spreaders" was created. The result was a totally futile method of fighting the epidemic, hunting them, as also the good Renzo Tramaglino was to discover to his cost. Manzoni writes at the end of Chapt. XXXII:

The magistrates, weakened every day, and more and more bewildered and confused, used all, so to speak, the little determination in their powers, to search for these plague spreaders […]
The trials that resulted were certainly not the first of that kind: and neither can they be considered a rarity in the history of the law. So, not to mention ancient times, but just something of the times closer to the age we are dealing with, in Palermo, in 1526; in Geneva, in 1530, then in 1545, then again in 1574; in Casal Monferrato, in 1536; in Padova, in 1555; in Torino, in 1599, and again in that same year 1630, sometimes one person, sometimes many wretches, were tried and condemned to tortures, mostly atrocious, as being guilty of spreading the plague with powders or with unguents, or with *malle*, or with all these together.

On the other hand it is historically well-known that at least in the Republic of Venice, the authorities responsible for protecting the population from the plague had understood very well that the epidemic spread in a circular fashion starting from certain critical centers, and the best way to contain it was to set up around these centers a "health barrier"; this often coincided with the course of a river, along which it was easier to control movements and to forbid travellers to pass (Cipolla, 2007).
This seems to have been the result of an abduction.

- *Result*: in the towns and along the thoroughfares leading out of *x*, *y* and *z* cases of plague were found in the days following those when the plague appeared in *x*, *y* and *z*.

- *Rule*: in the days after the plague appeared in places *a*, *b* and *c*, there were cases in places *x*, *y* and *z* which are situated along the thoroughfares.

- *Case*: the plague (though we do not know how) spreads to towns along the thoroughfares (probably).

It followed that it was wise to close the thoroughfares in order to stop the epidemic from spreading.
Abduction, Peirce underlines continually, not unlike deduction and induction, is a way the mind work spontaneously, but it is distinguished by its eminently pragmatic character: it serves to take decisions in the presence of "dirty" and/or partial data, since we may never have at our disposal data that is complete and/or "pure" enough.
It is all too true that the problem of criteria that somehow guarantee the reliability of the interventions, psychological interventions in our case, goes further than the understandable concerns of insurance companies and primarily involves the interests and rights of the clients/patients. Klerman, starting from the discussion of the "Osheroff case" defines this problem with the expression the "challenge of efficacy", and that is linked to themes of great importance, not only clinical but also ethical and juridical (Stampa, 1990)[19]. To the "challenge" proposed by Klerman, Stone juxtaposed the importance for a *respectable minority* of the scientific–professional community to have the possibility to put into practice models of clinical evaluation and therapeutic intervention not subject to the reassuring conformism of the guidelines, standards and protocols of diagnosis-care.
It is an open question. We therefore claim the independent space of a *respectable minority*, who do not accept the principle of a clinical psychology *ancilla medicinae*, and as such forced to accept the ideological illusion of a biological paradigm incorrectly applied to mental life.
What seems to us all the more misleading and confusing is the attitude of these researchers and professionals who seek a mediation between incompatible requirements, in the name of a neutral "scientificness" which gets its paradigms from natural sciences and restores medicine — in whose

[19] The case concerns legal proceedings taken by a Dr. Osheroff, against the clinic of Chesnut Lodge, in which a psychotherapy approach had been tried with the depressive phase of his bipolar disorder. In the first weeks the clinical picture worsened considerably compared to the situation on admission and the patient was admitted to another clinic, obtaining a rapid remission of the symptoms (we do not know the length of time) thanks to the application of a standard protocol of therapy with antidepressants and electroshock. Chesnut Lodge decided to settle out of court and paid Dr.Osheroff a substantial sum as reimbursement. The affair (from 1983) gave rise to a heated debate between supporters of experimenting with innovative approaches to mental disorders and supporters of the need to provide patients with prior, reasonable guarantees of positive results in terms of remission of symptomatology.

domain the circumstantial paradigm[20] was created — to a system of impoverished "evidence" of relational aspects, in which the observer is here and the object is there, and if the observer himself is the subject of the clinical act (in psychotherapy anything else really is rather improbable..) the subjectivity is "objectified" through computation.
And such behavior seems all the more paradoxical (yet again!) when it is found among psychoanalysts, who on subjectivity as a working tool have age-old references at their disposal, repeatedly discussed, reviewed, corrected, perfected...
The problem of the "scientific" nature of psychoanalysis was posed right from the beginning, when the young Freud was accused by the Viennese doctors of playing around with a charlatan's methods, and several thousands of pages have been devoted to the issue during the 20th century and in these early years of the 3rd Millennium: we will not even try to sum up its general outlines here, but we want to report a comment made by E. Roudinesco (1999 [2000: pp. 91 ss.]), relevant to the more limited topic we are dealing with:

> While psychoanalysis was able to be save itself from Nazism thanks to the massive emigration of European Freudians towards the American continent between 1930 and 1940, this happened at the cost of a radical transformation of its ideals, its practice and its theory <…> The extremely pragmatic American therapists eagerly appropriated Freudian ideas. But they immediately tried to *measure* sexual energy, to *demonstrate* the efficacy of the treatments by multiplying the *statistics* and to investigate whether the concepts were applicable empirically to the concrete problems of individuals.

We have already expressed ourselves (Grasso & Stampa 2007) on this aspect of homologation of mental health to social conformism – and the consequent homologation of the process of "healing" to the client/patient's gradual assent to standards of self representation consistent with those proposed by the establishment (it is inevitable that this corresponds to the preparatory homologation of the therapist to the same standards …)

Roudinesco continues:

> In these conditions, across the Atlantic, independently of the orientations, psychoanalysis became the tool for an adaptation of man to a utopia of happiness. It took hold far less due to its system of thought or to the philosophical questions it raised, than to its capacity to bring an immediate solution to the sexual morality of the liberal and puritan society […].
>
> After catalysing the elaboration of the psychiatric gnoseology for thirty-odd years, psychoanalysis was finally rejected. Did not the psychotropes or other explanatory models of psychism, based on the *DSM-IV* and on new cerebral mythologies, perhaps provide faster therapeutic solutions than those famous "disorders"[21] that fixed the subject in a behavioral symptomatology? So, as the historian Nathan Hale so well underlines, the American anti-Freud partisans between 1970 and 1990 […] put forward brilliant assessments, tests and investigations: a whole experimental arsenal unsuited to explaining the reality of psychoanalytical practice and theory.[22]

---

[20] On this, see a book not yet published in Italy, which has aroused interest and curiosity in some sectors of the country's medical world: *Evidence-Based Medicine in Sherlock Holmes' Footsteps* by Jorgen Nordenstrom (2006), in which the author — who works at the Karolinska teaching hospital in Stockholm — proposes an application of the abductive procedure for the collection of clinical information and the monitoring of treatments.

[21] Significant, though not developed by the author, is this reference to the *disorders* that psychopathology tends to deal with descriptively and "objectively": the term itself evokes the break with a previous order, and the resulting confusion and malaise (due to the prefix *dys*- which from Ancient Greek we translate with a pejorative of the term it is linked to; in the Rocci dictionary, *ad vocem*, «indicates opposition, contrariness, doubt, difficulty, uncertainty, pain»). The breakdown of this order in mental life is therefore an existential event or condition, which appears *before* the psychopathology intervenes to describe, classify, and impose gnoseographic "discipline".

[22] Any radicalization of this approach had already been opposed in the 1980s by M. Edelson (see above all 1984 [1986]) with a research program that rejected the objections made by Grünbaum (1977a, 1977b; 1978; 1979) and tried to incorporate the "symptom-context" method of Luborsky (1967; 1973) and the bootstrap

Very brief conclusion (for the present)

One last comment. Most scholars engaged in the wide-ranging debate over research methods in psychotherapy seem to think of the question of its very possibility in diametrically opposed terms, at opposite extremes: as if a finding, an encoding and a rigorous mathematical treatment of the data were the only alternative to the rhapsodic intuition of imaginative therapists dangerously given to caprice or to intellectual *divertissement* at the expense of the reproducibility of "guaranteed" results. After the many radically critical comments that we have made on "evidence based" psychotherapy, we acknowledge in Mancini and Barcaccia (we could mention many others, but it is their article that is in front of us) a caution that we hope to see maintained in practical research.

Unfortunately the contrast between psychotherapy as an ineffable and elusive act on the one hand and psychotherapy as the dry, rigid juxtaposition of techniques on the other is not only useless but also harmful. Even those, like us, who see some positive stimuli in psychotherapy based on evidence of efficacy, do not have the idea that it must become the be-all and end-all, or an absolute paradigm of reference, but the belief that it may prove to be a useful tool to guide clinical activity, for those who approach it without prejudice.
Cognitivist colleagues, we still do not agree but we will take you at your word.

References

Barak, A., & La Crosse, M.B. (1975). Multidimensional Perception of Counsellor Behaviour. *Journal of Counseling Psychology*, *22*, 471–476.

Barber, J.P., Gallop, R., Crits-Christoph, P., Frank, A., Thase, M.E., Weiss, R.D., & Connolly Gibbons, M. B. (2006). The Role of Therapist Adherence, Therapist Competence, and Alliance in Predicting Outcome of Individual Drug Counseling: Results from the National Institute Drug Abuse Collaborative Cocaine Treatment Study. *Psychotherapy Research*, *16*, 229-240.

Baum, L.F. (1900). *The Wonderful Wizard of Oz.* Chicago: Hill.

R. Benkirane (2002). *La Complexité [The complexity]* Éditions Le Pommier [*La teoria della complessità*. (2007). Torino: Bollati Boringhieri].

Canguilhem G. (1966). *Le normal et le pathologique* [The normal and the pathological]. Paris, Presses Universitaires de France.

Carli, R. (2007). Editorial. *Rivista di Psicologia Clinica. Teoria e metodi dell'intervento*, *3*, www.rivistadipsicologiaclinica.it.

Chambless, D., & Ollendick, T. (2000). Empirically supported psychological interventions: Controversies and evidence. *Annual Review of Psychology*, *52*, 685-716.

strategy of Glymour (1974; 1980): however, his efforts seem to have had little effect on the more recent debate.

Cipolla, C.M. (2007). *Contro un nemico invisibile: Epidemie e strutture sanitarie nell'Italia del Rinascimento.* Bologna: Il Mulino.

Constantine, M.G. (2002). Predictors of Satisfaction with Counseling: Racial and Ethnic Minority Clients' Attitudes toward Counselling and Ratings of Their Counselors' General and Multicultural Counselling Competence. *Journal of Counseling Psychology*, *49,* 255–263.

Crits-Christoph, P., & Gallop, R. (2006). Therapist Effects in the National Institute of Mental Health Treatment of Depression Collaborative Research Program and Other Psychotherapy Studies. *Psychotherapy Research*, *16*, 178-181.

Danner, D.D., Snowdon, D.A., & Friesen, W.V. (2001). Positive Emotions in Early Life and Longevity: Findings from the Nun Study. *Journal of Personality and Social Psychology*, *80,* 804-813.

Dazzi, N., & De Coro, A. (2007). Psychotherapy and empirical research: two irreconcilable terms or the blueprint for a new paradigm in clinical psychology? *Rivista di Psicologia Clinica. Teoria e metodi dell'intervento*, *1*, www.rivistadipsicologiaclinica.it.

Del Corno, F., & Lang, M. (2006). Empirically Supported Treatments vs Empirically Supported Relationships. In N. Dazzi, V. Lingiardi, & A. Colli (Eds), *La ricerca in psicoterapia: Modelli e strumenti* (pp. 49-64). Milano: Cortina.

Di Nuovo, S. (2000). Strumenti qualitativi per la ricerca sulla psicoterapia [Qualitative Instruments fot the research on psychotherapy] Interview on the therapeutical change by R. Elliott. L*aboratorio di ricerca*, 2-3*,* http://www.isuri.net/Dinuovo2000.html

DSM IV (1994). *Diagnostic and Statistical Manual of Mental Disorders*, IV ed., Washington, DC: American Psychiatric Association.

Earle, N. (1993). *The Wonderful Wizard of Oz in American Popular Culture: Uneasy in Eden.* New York: The Edwin Mellen Press.

Edelson, M. (1984). Hypothesis and Evidence in Psychoanalysis. University of Chicago Press.

Elkin, I., Falconnier, L., Martinovich, Z., & Mahoney, C. (2006a). Therapist Effects in the NIMH Treatment of Depression Collaborative Research Program. *Psychotherapy Research*, *16*, 144-160.

Elkin, I., Falconnier, L., Martinovich, Z., & Mahoney, C. (2006b). Rejoinder to Commentaries by Stephen Soldz and Paul Crits-Christoph on therapist effects. *Psychotherapy Research*, *16*, 182-183.

Gangemi, G. (2002). *La costruzione sociale della logica: Tra concetti di relazione e concetti di attributi [The social building of the logic. Three concepts of relation and of attributes].* Milano: Giuffré.

Gazzillo, F., & Lingiardi, V. (Eds.). (2007). Making Clinical Research Empirically Relevant: Drew Westen's lecture at the Sapienza (14 June 2006). *Rivista di Psicologia Clinica. Teoria e metodi dell'intervento*, *1*, www.rivistadipsicologiaclinica.it.

Ginzburg, C. (1979). *Spie: Radici di un paradigma indiziario [Sign. Roots of a circumstantial paradigm]*. In A. Gargani (Ed.), *Crisi della ragione* (pp. 59-106). Torino: Einaudi.

Glaser, B.G., & Strauss, A. L. (1967). *The Discovery of Grounded Theory: Strategies for Qualitative Research.* Chicago: Aldine.

Glymour, C. (1974). Freud, Kepler and The Clinical Evidence. In Wolheim, R. (Ed.), *Freud*. New York: Anchor Press.

Glymour, C. (1980). *Theory and Evidence.* Princeton: Princeton University Press.

Goldfried, M. (2000). Consensus in psychotherapy research and practice: Where have all the findings gone? *Psychotherapy Research*, *10*, 1-16.

Grasso, M. (2006). Modelli di salute e patologia mentale: Implicazioni per la ricerca in psicoterapia [Health models and mental pathology. Implications for the research on psychoterapy]. *Trasformazioni*, *2*, 52-73.

Grasso, M., Stampa, P. (2005). *Correzione di deficit vs promozione di sviluppo in psicoterapia: implicazioni per la valutazione e per la ricerca clinica*. Relazione presentata al V Congresso Nazionale della Society for Psychotheray Research – Sezione Italiana, San Benedetto del Tronto 16-18 settembre.

Grasso M., Stampa P. (2006). Chi ha slegato Roger Rabbit? Diagnosi psichiatrica e modelli di salute mentale: Osservazioni su alcune criticità metodologiche per la ricerca in psicoterapia [Who *un-framed* Roger Rabbit? Psychiatric diagnosis and mental health models: observations on same methodological criticality for psychotherapy research]. *Rivista di Psicologia Clinica. Teoria e metodi dell'intervento*, *1*, www.rivistadipsicologiaclinica.it.

Grasso, M., & Stampa, P. (2007). *Diagnosi psichiatrica e modelli di salute mentale: Osservazioni su alcune criticità metodologiche per la ricerca in psicoterapia [*Psychiatric diagnosis and mental health models: observations on same methodological criticality for psychotherapy research]. In G. Nicolò, & S. Salvatore (Eds.), *La ricerca sui risultati e sul processo in psicoterapia* (pp. 109-128). Roma: Amore.

Grasso, M., Cordella, B., & Pennella, A.R. (2003). *L'intervento in psicologia clinica [The intervention in clinical psychology] : Fondamenti teorici*. Roma: Carocci.

Grünbaum, A. (1977a). How Scientific Is Psychoanalisys? In R Stern, et al. (Eds.), *Science and psychotherapy.* New York: Haven.

Grünbaum, A. (1977b). Is Psychoanalysis A Pseudo-Science? (I). *Zeitschrift für philosophische Forschung*, *31*, 333-353.

Grünbaum, A. (1978). Is Psychoanalysis A Pseudo-Science? (II). *Zeitschrift für philosophische Forschung*, *32,* 49-69.

Grünbaum, A. (1979). Is Freudian Psychoanalytic Theory Pseudo-Scientific By Karl Popper's Criterion Of Demarcation? A*merican Philosophical Quarterly*, *16*, 131-141.

Hill, C. E. (2006). Introduction to Special Section on Therapist Effects. *Psychotherapy Research*, *16*, 143.

Kaminsky, S.M. (1978). *Murder on the Yellow Brick Road*. New York: St Martin's Press.

Kendall, P.C., Marrs-Garcia, A., Nath, S.R., & Sheldrick, R.C. (1999). Normative comparisons for the evaluation of clinical significance. *Journal of Consulting and Clinical Psychology*, *67*, 285-299.

Kim, D., Wampold B.E., & Bolt D.M. (2006). Therapist Effects in Psychotherapy: A Random Effects Modeling of the NIMH TDCRP Data. *Psychotherapy Research*, *16*, 161-172.

Lombardo, G.P., & Malagoli Togliatti, M. (1995). *Epistemologia in psicologia clinica [Epistemology in clinical psychology].* Torino: Bollati Boringhieri.

Luborsky, L. (1967). Momentary Forgetting During Psychotherapy and Psychoanalysis. In R. Holt (Ed.), *Motives and Thought*. New York: International Universities Press.

Luborsky, L. (1973). Forgetting and remembering (Momentary Forgetting) during psychotherapy. In M. Mayman (Ed.), *Psychoanalytic Research*. New York: International Universities Press.

Mancini, F., & Barcaccia, B. (2007). *Psicoterapia "cum grano salis" [Psychtherapy cum grano salis"]*. In G. Nicolò, & S. Salvatore (eds.), *La ricerca sui risultati e sul processo in psicoterapia* (pp. 129-133). Roma: Amore.

Nathan, P.E., Stuart, S.P., & Dolan, S.L. (2000), Research on psychotherapy efficacy and effectiveness: Between Scylla and Charybdis? *Psychological Bulletin*, *126,* 964-981.

Nietzsche, F. (1883-1885 [1976]). *Così parlò Zarathustra: Un libro per tutti e per nessuno.* Milano: Adelphi.

Nietzsche, F. (1901-1906 [2006]), *La volontà di potenza*, Milano, Mimesis. *Note* — Nietzsche was not ready, when he died, for an edition of the work first published in 1901 as Vol.XV of *Grossoktav-Ausgabe* (Naumann, Leipzig, 1901). A second edition in 1906 was overseen by Peter Gast and Elizabeth Förster-Nietzsche (the philospoher's sister) in Vols. IX-X of *Taschen-Ausgabe,* an enriched and expanded version of the first edition, and the one on which almost all the later editions were based.

Nordenstrom, J. (2006). *Evidence-Based Medicine in Sherlock Holmes' Footsteps*, New York: Blackwell.

Owens, L. (Ed.). (1981). *The Complete Hans Christian Andersen Fairy Tales.* New York: Avenel Books.

Paulson, B.L., Truscott, D., & Stuart J. (1999). Clients' Perception of Helpful Experiences in Counseling. *Journal of Counseling Psychology*, *46,* 317–324.

Peirce, C.S. (1935-1966). *Collected Papers*. Cambridge, Mass.: Harvard University Press.

Peterson, C., & Seligman M.E.P. (2004). *Character Strengths and Virtues: A Handbook and Classification*- Oxford: Oxford University Press.

Pinn, V.W., Harden, J.T., & Blehar M.C. (2002). *Outreach Notebook: For the Inclusion, Recruitment and Retention of Women and Minority Subjects in Clinical Research*, (U.S. Department of Health & Human Services, Public Health Service, National Institutes of Health Publication No.03–7036). Consulted in 4 April 2004 at http://www.od.nih.gov/orwh/outreach.pdf.

Ravaglia, G. (2003). Psicoterapia "evidence-based", http://www.risorse-psicoterapia.org/movimento_est.htm (*Note — On 28 June the work was not present on the site*).

Roncaglia, G.C. (2006). *Il jazz e il suo mondo*. Torino: Einaudi.

Roudinesco, E. (1999). *Pourquoi la psychanalyse?* [Why the psychoanalysis] Paris: Fayard.

Sanders Thompson, V.L., Bazile, A., & Akbar M. (2004). African Americans' Perceptions of Psychotherapy and Psychotherapists. *Professional Psychology: Research and Practice*, *1*, 19–26.

Seligman, M.E.P. (1991). *Learned Optimism.* New York: Simon & Schuster.

Seligman, M.E.P. (2002). *Authentic Happiness.* New York: Free Press.

Soldz, S. (2006). Models and Meanings: Therapist Effects and the Stories we Tell. *Psychotherapy Research*, *16*, 173-177.

Sorokin P.A. (1956). *Fads and Foibles in Modern Sociology and Related Sciences*. Henry Regnery: Chicago.

Speilberger, C.D., & Vagg, P.R. (1970). *Questionario di autovalutazione STAI*. Firenze: Organizzazioni Speciali, 1980.

Stampa, P. (1990). "Malpractice". *Una nota su alcune condizioni epistemologiche e conseguenze pratiche dell'errore in psicologia clinica.* In P. Colamonico, G. Montesarchio, & C. Saraceni (Eds.), *Psicodiagnostica e psicoterapia: "Parliamo di errori"* (pp. 147-159). Roma: SIRP (Società Italiana per la Ricerca Psicodiagnostica).

Vaillant, G.E. (2003). Mental Health. In *American Journal of Psychiatry*, *160*, 1272-1284.

Wade, P., & Berstein, B.L. (1991). Cultural Sensitivity Training and Counselor's Race: Effects on Black Female Clients' Perception and Attrition. *Journal of Counseling Psychology*, *38*, 9–15.

Wakefield, J.C. (1992). Disorder as Harmful Dysfunction: A Conceptual Critique of DSM-III-R's Definition of Mental Disorder. *Psychological Review*, *99*, 232-247.

Wakefield, J.C. (1997). When is Development Disordered? Developmental Psychopathology and the Harmful Dysfunction Analysis of Mental Disorder. *Development & Psychopathology*, 9, 269-290.

Wakefield, J.C. (1999). Evolutionary versus Prototype Analyses of the Concept of Disorder. *Journal of Abnormal Psychology*, *108*, 373-399.

Wakefield, J.C. (2004). Realtà e valori nel concetto di salute mentale: Il disturbo come disfunzione dannosa. *Psicoterapia e Scienze Umane*, *4*, 439-464.

Wakefield, J.C. (2005). Il concetto di salute mentale: Una critica a Vaillant [The concept of mental disorder. A critique to Vaillant]. *Psicoterapia e Scienze Umane*, *1*, 91-96.

Wampold, B. E., & Bolt, D. M. (2006). Therapist Effects: Clever Ways to Make Them (and Everything Else) Disappear. *Psychotherapy Research*, *16*, 184-187.

Ward, E.C. (2005). Keeping It Real: A Grounded Theory Study of African American Clients Engaging in Counseling at a Community Mental Health Agency. *Journal of Counseling Psychology*, *4*, 471-481.

Webster's New World Dictionary (1977). New York: Harper Collins.

Westen, D., Morrison, K., & Thompson-Brenner, H. (2004). The Empirical Status of Empirically Supported Psychotherapies: Assumptions, Findings, and Reporting in Controlled Clinical Trials. *Psychological Bulletin*, 130, 631-663.

Wilson, G. (1998). Manual-based treatment and clinical practice. *Clinical Psychology: Science & Practice*, *5*, 363-375.