

ROLF SANDELL, Ph.D.

ON THE VALUE OF DOUBLE VISION

Abstract. This article is, first, an analysis of arguments that have been leveled against systematic empirical research in psychoanalysis. The arguments are basically two: It cannot uncover unobservable processes, and it cannot account for the uniqueness of the psychoanalytic dyad. Therefore, systematic empirical research is said to be incompatible with basic tenets of psychoanalysis. Both arguments are refuted as being based on mistaken assumptions. The study of unobservable phenomena is not specific to psychoanalysis but is what psychological research in general is about, and over and above the striking variation among human beings, there are systematic commonalities to be revealed. Then, a method to study these regularities, while safeguarding systematic individual differences, is demonstrated and suggested to offer a kind of double vision in psychoanalytic research.

Keywords: psychoanalytic process, regularities, systematic research, case studies, heterogeneity, homogeneity

In my opinion, there is no more fertile way to think about what makes humans tick, with all their complications—and precisely because of these complications—than thinking psychoanalytically. I am therefore convinced that psychoanalysis, as a general theory, will survive and eventually take precedence over the more simplistic theories that are being suggested—although its terminology may be changed to avoid the “psychoanalytic contamination” that seems to arouse such hostility in many quarters.

Convinced about the viability of the general psychoanalytic theory, I am far less optimistic about the future of psychoanalysis as a practice,

both in the sense of clinical practice and research practice. I believe most readers are well aware of the attacks and criticisms that are leveled against psychoanalysis from the outside. This is normal and expected when different theoretical schools and disciplines confront one another. What is probably of greater concern is that *within* the psychoanalytic community there is a rift that I fear threatens to tear this community apart.

The formation of this rift is basically due to different views on what constitutes research in psychoanalysis. To simplify the argument somewhat, the two strands are those of so-called clinical research and so-called empirical research. I write "so-called" in order to emphasize that the terms "clinical research" and "empirical research" are porous concepts that seem to be given whichever meaning the contestants believe make their arguments stronger. In general, it seems that "clinical research" is used to refer to the oral or written presentation of a case in order to illustrate some theoretical idea, using the analyst's recollection of the in-session interaction, sometimes supported by note taking in real time or retrospectively, but without any formal paraphernalia like tests, questionnaires, or tape records. In contrast, "empirical research" seems to be used to refer to studies based on group averages on questionnaires or scales, in a more or less predetermined formal longitudinal or cross-sectional design, but *also* to single-case studies where more-or-less formal structured instruments are applied to continuous records on tape or paper.

The reason why I call this "a rift" is that there is mutual criticism leveled against one another, and the criticism can sometimes become violent and venomous. One recent instance is a paper by Irwin Hoffman that was presented as a plenary address at the winter meeting of the American Psychoanalytic Association in 2007, and was greeted with "a wildly enthusiastic standing ovation that outdid anything I had previously seen or heard at the American," according to a reliable witness, Arnold Cooper (Hoffman, 2009). I really don't mind that Dr. Hoffman holds the views he does; what bothers me is the reaction they met, as if there were a war. As I consider myself belonging to the enemy with which he fought, I should like to take the opportunity to respond. Before going on, I will agree with Dr. Hoffman in using the concept "systematic research" rather than "empirical research." I believe, or I hope, that all psychoanalysts are empirical in the sense that they base their conclusions on observations. But I find it interesting that the quality Dr. Hoffman evidently sees as the distinguishing feature in the activities he criticizes is that they are *systematic*. And I agree—and I value it.

In my understanding, there are basically two essential arguments (and a few more marginal ones) leveled at systematic research in psychoanalysis, and I will deal with them one at a time.

The first, and probably the principal argument, suggested by Andre Green, among others, is that *the subject matter of psychoanalysis, the unconscious, is by definition unobservable and cannot, therefore, be studied in any other way than in a psychoanalysis*, certainly not by "positivistic," "objectivistic" methods. This is a peculiarly ignorant idea, because the lack of observability is obviously not unique to psychoanalysis. Indeed, what defines the whole science of psychology is the fact that its phenomena are unobservable—except for radical behaviorists. Psychological phenomena are phenomena of the mind, and the mind is by definition unobservable. Psychology is basically conceptualization and "indirect observation" of unobservable phenomena. A classic unobservable phenomena, almost from its beginning, is intelligence. Nobody has ever seen intelligence; what we may see are its traces or indicators. That is indirect observation. Take another concept: memory. We may only observe its manifestations. Still another one: Attitudes do not exist except for what we conceive of as their derivatives. We are always dealing with derivatives or indicators, signs or traces, when we are studying psychological phenomena, including psychoanalytic ones. It is true that some phenomena seem to be "deeper" than others and therefore more "genuine," more fundamental, and psychoanalytically more interesting and important. It is generally assumed, for instance, that unconscious processes are "deeper" than conscious ones. But this belief in levels of depth is essentially based on reifying interpretations of metaphors, like the topographical iceberg, and some location "sub" or beneath something "conscious." However, processes that we believe are less deep or more conscious, like problem solving and other cognitive processes, are equally unobservable or latent, and I'm not sure that the concept of depth has ever been defined in any empirically defensible way. Just as mental structures or processes may only be inferred on the basis of indicators in the form of their derivatives, the attribution of depth to psychological phenomena can only be based on a set of indicators of *depth*.

So, the argument is false at its outset. But even if it were true, it reveals a misunderstanding of what research is about. Research, any piece of it, is not directed at any general phenomenon, like the unconscious; it is directed at, or directed by, specific questions about specific features of such phenomena. Systematic research in psychoanalysis does

not necessarily have to address unconscious phenomena in order to be worthwhile. There are myriad interesting questions that can be formulated about psychoanalysis, its processes, its outcomes, and its protagonists, the analysand and the analyst, and all of them do not require the unconscious mind to be revealed or inferred in order to be properly answered.

In addition, there *are* ways of doing systematic research that may indeed reveal processes of which the actors seem to be unaware—and of which the researcher, too, is unaware until the data are there. I will offer one example. It is an old study by now, published in 1972 by Harold Sampson and other members of the San Francisco Psychotherapy Research Group, and I choose it because its title is significant in this context: “Defense analysis and the emergence of *warded off* mental contents” (Sampson, Weiss, Mlodnosky, & Hause, 1972; italics added). This study is a model of systematization of research in psychoanalysis. The first step involved intensive clinical study and induction in order to discover, describe, and conceptualize changes in the specific defense of undoing during a psychoanalysis with Mr. A, who was severely obsessive-compulsive and very much against recognizing or acknowledging any emotions or feelings whatsoever. The second step was to determine that these changes could be reliably observed by independent judges using coding or scaling procedures “which minimized clinical judgment” (p. 526). The third step was to determine whether these observed changes developed according to the theory, the integration-regulation model of defense analysis that the authors had suggested. The fourth step was to develop reliable measures of the patient’s affect experiences. In the fifth stage, the authors were finally able to test, statistically, their hypothesis that the integration of undoing enabled the patient to experience strong affects that had theretofore been warded off. The tests supported this hypothesis.

One may notice that this was done 40 years ago, and one should consider that the methodological sophistication has developed considerably since then. This is amply demonstrated in a recent book, *From Psychoanalytic Narrative to Empirical Single Case Research* (Kächele, Schachter, & Thomä, 2009). The authors use the tape recordings of a psychoanalytic specimen case, Amalia X, to illustrate the fruitfulness of systematic studies of change, involving, among other things, systematic ratings of emotional insight, content analysis focusing on self-esteem and other themes, dream series analyses, systematic analyses of transference

relationships using the core conflictual relationship theme method (Luborsky & Crits-Christoph, 1998), so-called plan formulations in the context of the control-mastery theory of Weiss, Sampson, and the Mount Zion Research Group (1986), the general processing of observations according to the Psychotherapy Process Q-Sort (Jones, 2000) and, finally, a number of quantitative methods of linguistic analysis (Mergenthaler, 1996) applied to the transcripts of the Amalia X tapes. A psychoanalytically interested reader has to be extremely impressed and inspired by these studies, whatever his or her prior attitudes toward "empirical research" may be.

So, it is indeed possible to do systematic research on the psychoanalytic process, even on its unconscious or warded off aspects. I would go even further in this argument and assert that it is all the more probable to identify processes that neither the analysand nor the analyst has been aware of if we apply some kind of quantitative, and even automatic, statistical pattern-finding methods to texts, questionnaires, or observational scales. I say this to rebut the idea that *questionnaires and observational scales only reach the conscious level but never the unconscious*.

This is a corollary of the argument for the notion that the unconscious cannot be inferred by any other approach than psychoanalysis on the couch. After reading Freud I have always believed that *any* act of behavior is a compromise and thus may reveal traces of conscious *and* unconscious, of ego, super-ego *and* id, of benevolent objects *and* hostile ones, etc. I cannot see why responses to questionnaires should be an exception.

As you may have noticed, the examples I gave were single case studies, Mr. A and Amalia X, and that is because I believe single-case research is more palatable than group studies are to people who are hostile to systematic research in psychoanalysis. In his critical paper, Hoffman (2009) makes the point, quoting Daniel Fishman, that "there has been a revival of the interest in the case study's potential to create viable scientific, psychological knowledge that is not inferior to experimental, group-based knowledge, but rather complementary to such knowledge—especially in the area of psychotherapy research" (p. 1044). That is probably correct. In fact, much research in organizational psychology, neuropsychology, cognitive psychology, and other branches of psychology—although not so much in psychotherapy research—is single case research. But there is a vast difference between a single case *study* and the genre of case *reports* or case *stories* that we are used to finding in psychoanalytic

journals. A scholar in Wikipedia¹ writes: "Case study research means single and multiple case studies, can include quantitative evidence, relies on multiple sources of evidence and benefits from the prior development of theoretical propositions. Case studies . . . can be based on any mix of quantitative and qualitative evidence." I honestly think, by the way, that *systematic qualitative* research is underused in psychotherapy research, but that is another matter.

What I would like to address now is another argument put forth by many clinicians against systematic research: *The psychoanalytic process, and the persons of the analysand and the analyst, are unique and cannot, therefore, be investigated by group studies.*

I certainly agree that all cases are unique considered in their entirety, but I also have to agree with Dr. Irwin Hoffman (2009) when he himself raises the doubt that "If the principle of . . . uniqueness is so decisive, how can one learn anything from systematic empirical research or," and I want to emphasize the following, "for that matter, from case studies, and how can any 'progress' be made in accruing knowledge over time?" (p. 1051). The question is, what would be the point of reporting or presenting a case if one really believed that the case was *totally* unique, not resembling any other case in any way? I think we all believe that there are basic dimensions of similarity among persons that will make it reasonable to make some not too far-reaching generalizations. In fact, I often find myself allured listening to a case report, thinking of the ways the analysand resembles some analysand or patient of my own—sometimes even thinking that my patient may secretly be seeing this other analyst, too.

So, assuming and accepting that we are not wholly unique, how generalizable is a single case? That varies, of course, with the dimensions of generalizations. However, one thing that strikes me quite often is that, despite the uniqueness of humans, and over and above their individualities, we may find such obvious regularities, as in Figure 1.

This is a beautifully regular curve based on more than 400 cases, showing how patients change in psychoanalysis and psychotherapy, *at an average*. "At an average" is an important addendum. Statistically speaking, an average is an expectation, and it is indeed so called in statistics. If we did not know *anything* else about a case, our best guess about the fate of

¹ http://en.wikipedia.org/wiki/Case_study

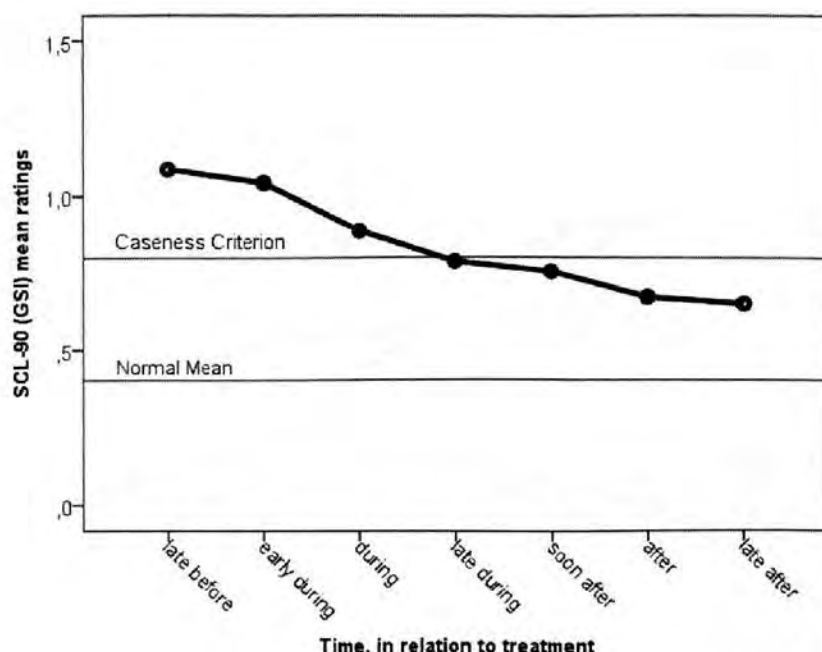


Figure 1. Average trajectory of 413 patients in psychodynamic therapy or psychoanalysis across treatment stages. "Caseness criterion" refers to the score value that statistically distinguishes persons who are likely to be or become "psychiatric patients" from persons who are not.

it would be that it would develop according to this curve. Thus, when we plot patients' distress the last years before treatment, it remains more or less stable. Then, when treatment begins, there is a steady decline until, the last year in treatment, distress becomes normal, so to speak, that is, the curve crosses the border to what may be considered the "normal" range. And, as you see, patients' states continue to improve for another three years after termination.

But the regularity does not end there. If we break down all these cases by type of treatment, we find another regularity, as in Figure 2.

Patients continued to improve at a significantly higher pace in one treatment than in the other treatment. Isn't it a wonder that a number of unique cases are similar enough to form such regular curves? And that is not a mere happenstance, because we may rule out chance by the use of statistical significance testing.

In Figure 3 is another, more recent example: three groups of 40 patients each, diagnosed as work-related burn-out cases. Two of the treatments were manualized group therapies, a cognitive one, cognitive group therapy (CGT) and a psychodynamic focused group therapy (FGT) with

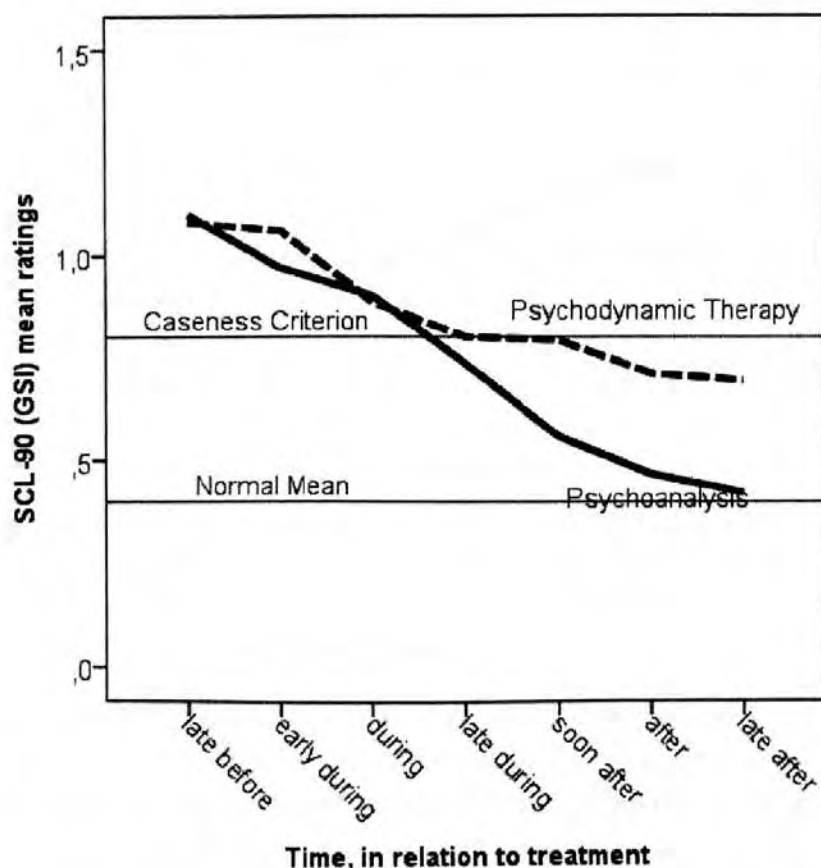


Figure 2. Average trajectory for patients in psychoanalysis (solid line) or psychodynamic therapy across treatment stages.

experienced therapists, and then there was a third comparison condition (CC), which was a so-called treatment-as-usual group.

The group therapies were of six months duration and after that, the patients were followed up until five years after inclusion. Obviously, there was no difference of any practical or clinical or statistical significance between the three groups' averages. Trajectories and curves like these are the routine way to present outcomes in research studies, and the general conclusion is that different psychological treatments are about equally effective. But now comes the very interesting thing, in my opinion. When we have access to data on individual cases, it looks like Figure 4.

It looks more or less like chaos, does it not? And this is really not an exceptional case. I really think the average trajectories seriously misrepresent or distort what happened in these treatments. So, here's our dilemma: On the one hand, keeping in mind that people are extremely different in psychological matters, a single case is often too specific and of

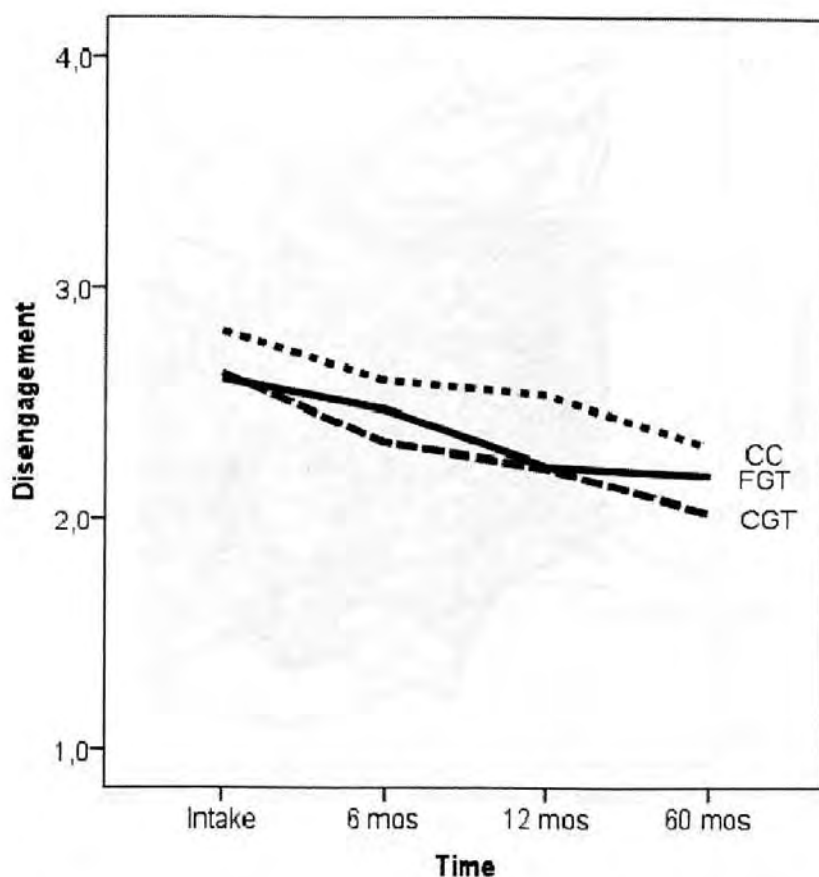


Figure 3. Average trajectories for 120 burn-out clients randomized on psychodynamic focused group therapy (FGT), cognitive group therapy (CGT), or a comparison condition (CC; treatment-as-usual), across time in treatment (6 months) and follow-up (5 years).

uncertain value for the sake of generalization. On the other hand, a group of cases is often too heterogeneous to be able to predict anything certain about the individual cases. So we have to find some way in-between a single case and a whole group, a way to analyze heterogeneity in ways that may meaningfully distinguish between types or classes of cases, taking due account of their similarities *and* their dissimilarities. And there are such ways, as in Figure 5.

I shall skip the statistics and technicalities, because they are of minor interest here. Each curve in this graph represents a number or a group of burned-out patients' decreasing disengagement in their work. For instance, look at the three lower curves. Here we have three types of patients who began their treatments at roughly the same level of distress or

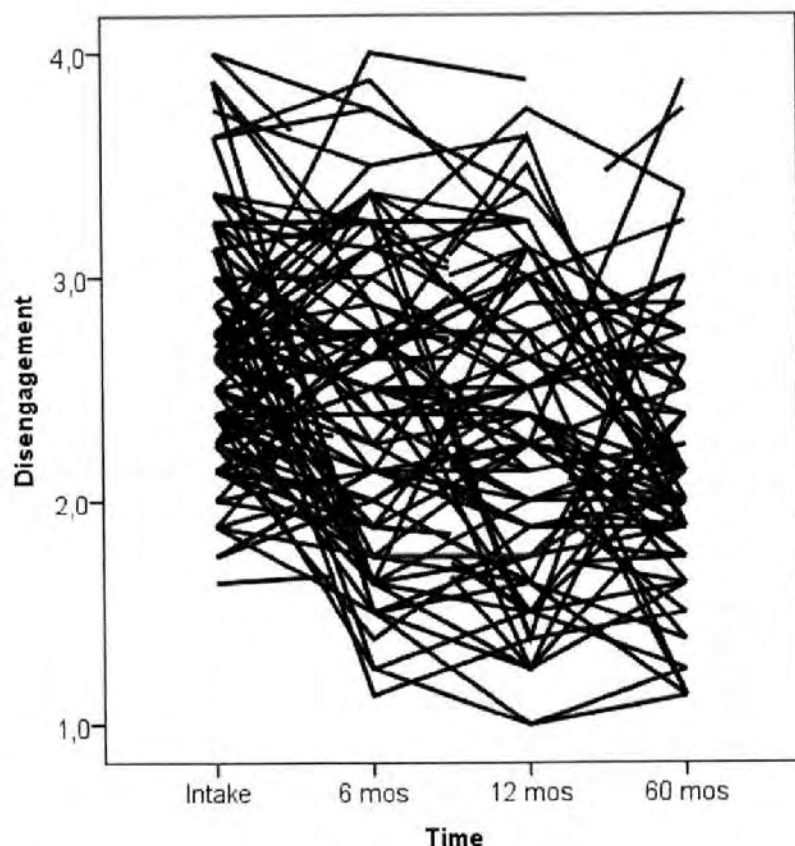


Figure 4. Individual change of 120 burn-out clients across time.

disengagement but improved at significantly different rates during treatment and developed in different ways during the follow-up period. And what makes this even more interesting is that we may identify variables that are associated with these different clusters of patients, demographic features and features of personality, for instance. The biggest group, for example, had an overrepresentation of women, clients of lower middle age, and clients high on one aspect of a basic personality trait called hedonic capacity. This was in contrast to the two smallest groups, where men were overrepresented and the level of hedonic capacity was relatively lower.

The important advantage with such a way of analyzing group studies is that we may *combine* heterogeneity and homogeneity. In this example, we take each individual person and explore to what extent he or she resembles any other individual person or persons in terms of change in this particular example. To the extent that people resemble one another according to a certain criterion, we group them together, thus allowing

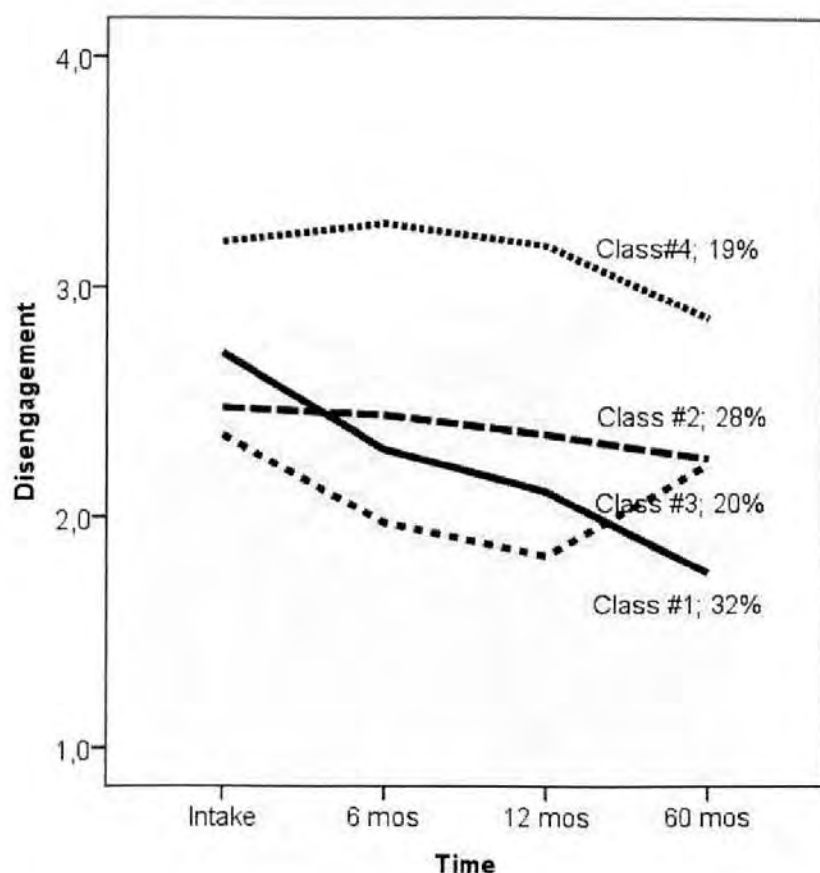


Figure 5. Trajectories across time for four latent classes summarizing the 120 individual burn-out cases.

us to generalize from one person in that subgroup to others in that same subgroup, but not to the whole group. This, in my view, combines the best of single-case design and group design in outcome research—and one may use the same approach in other kinds of research as well.

And now to another intriguing issue: Which party, the patient or the therapist, is most responsible for this outcome heterogeneity? We often seem to try to explain away therapeutic failures by pointing to deficits with the particular patient rather than with the therapist, whereas we are more easily prepared to take the responsibility for therapeutic successes. In this study, there were 10 group therapists involved and an unknown number of treatment-as-usual providers. In Figure 6, the average of their patients' trajectories are plotted against time, and the collective of treatment-as-usual providers are the dotted one.

You will see that the group of therapists is almost as heterogeneous as the group of patients. I guess that if one were to choose a therapist

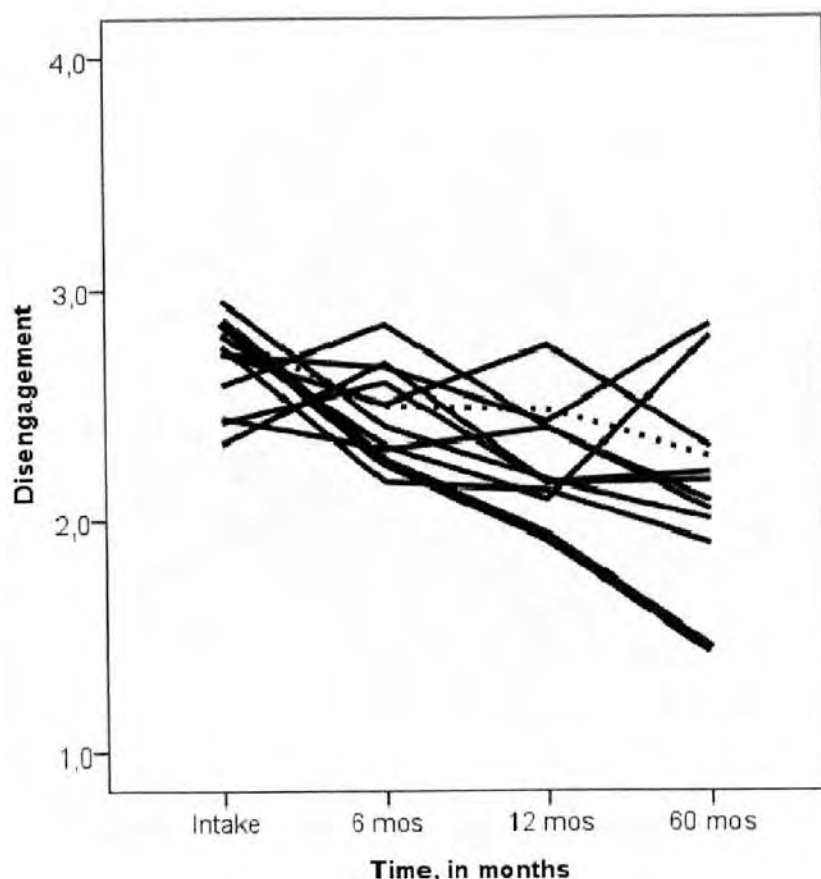


Figure 6. Across-patients average trajectories for the therapists of the 120 burn-out clients.

for oneself on the basis of this graph one would definitely choose the one shown as the thicker line. Of course, these therapists are too few to allow clustering in subgroups, but this is not an exceptional example. Therapists are probably almost as different as patients. Let me return to the beautiful outcome results from the Stockholm study (Sandell et al., 2000) in Figure 1 and break them down by the 219 therapists and analysts involved. Each line or curve represents one therapist or analyst and the average of his or her patients across the entire time span of about six or seven years, the last three of which were during follow-up after the termination of treatment. The result is shown in Figure 7.

Again, how may we bring order to this chaotic array? When we applied the same clustering technique that we used before, and correlated the clusters of therapists with the therapists' therapeutic beliefs and attitudes, according to a questionnaire that we call Therapeutic Identity, we ended up with five subgroups, like in Figure 8.

219 analysts and therapists

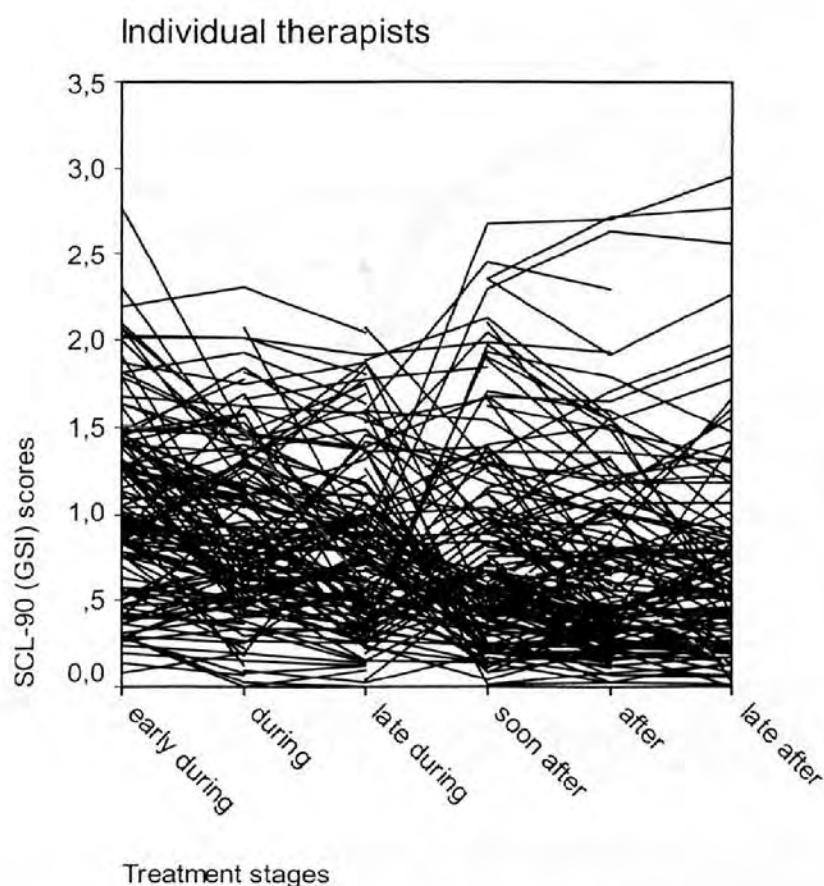


Figure 7. Across-patients average trajectories for the therapists of the 413 patients in psychodynamic therapy or psychoanalysis.

It is interesting that there were obvious correspondences between belonging to one or the other group and holding certain beliefs and attitudes. The two most successful groups valued kindness and warmth—but therapeutic neutrality at the same time—as important attitudes in doing psychotherapy or analysis, and they also considered psychotherapy (including psychoanalysis) to be largely based on intuition and inspiration, like a form of art. The therapists and analysts in the two least successful groups were more pessimistic about the possibility of patients developing and changing, and also felt that adjustment, adaptation, and covering up (in contrast to uncovering) were more important for therapeutic success.

Coming back to the rift between the two strands of so-called clinical research and so-called empirical research, how shall we close it, how

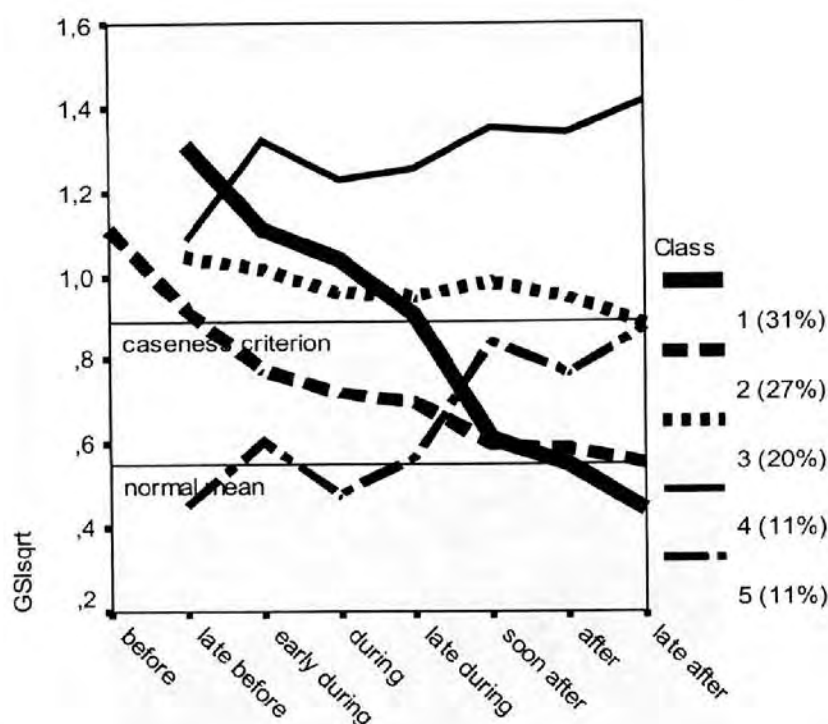


Figure 8. Trajectories across time for five latent classes summarizing the 413 individual psychodynamic therapy or psychoanalysis cases.

shall we manage the conflict? I do not think that attacks like those of Dr. Irwin Hoffman will help. Instead, I would argue we need to do something quite opposite and rather more difficult: try to have two different perspectives in mind at one and the same time. It is usually a good way to counter extremism, fundamentalism, and dogmatism. *Double vision* in research means having in mind two points of view simultaneously. One point of view involves focusing on what is generally expectable, not knowing the specifics of any particular case. This, of course, is what nowadays is naïvely called "evidence" in outcome research, but apart from the fundamentalistic misinterpretations and exaggerated claims, it is not totally uninteresting to know what the base rates of differential responsiveness are in the patient population, or to know that we succeed more often than we fail. There *are* regularities, indeed. The second point of view involves focusing on the individual differences and using a means of synthesizing to bring some order to the individualities. That way, we may find ways to select our cases, to predict what may be expected of them, and to sharpen our techniques to succeed even more often.

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Rolf Sandell, Ph.D., is a professor of psychology at Lund University, Sweden, and has published extensively on psychotherapy research, including long-term psychotherapy and randomized controlled trials, as well as on psychotherapists themselves, in such journals as the *International Journal of Psychoanalysis*, *Journal of the American Psychoanalytic Association*, and the *Scandinavian Psychoanalytic Review*.

SIGAL ZILCHA-MANO, Ph.D. AND JACQUES P. BARBER,
Ph.D., A.B.P.P.

LEARNING FROM WELL-TRAINED AND EXPERIENCED DYNAMIC PSYCHOTHERAPISTS:

RESEARCH ON THE EFFICACY OF DYNAMIC PSYCHOTHERAPY AND
ITS MECHANISMS OF CHANGE

Abstract. Is psychodynamic therapy (PT) an evidence-based practice? What makes PT work? In the present article we shall discuss empirical evidence for these as well as other vital questions. First, we shall examine the existing findings concerning two of the most widespread myths about PT: (1) PT is not an evidence-based therapy; (2) PT is not directed at and, therefore, not effective at alleviating symptoms. Second, we shall examine some of the existing findings regarding what it is that actually enables change in PT. The aim of the article is to provide some access to the knowledge accumulated from numerous studies on PT treatments, conducted by dozens of therapists, with the hope that it will benefit clinicians.

Keywords: psychotherapy research, evidence-based practice, effectiveness, mechanisms of change, alliance, adherence

Freud strongly believed in the importance of a scientific grounding in classical psychoanalytic theory. A scientific observer at heart and by training, he regarded psychoanalysis as a branch of science and the

Because the editor asked us to write a summary of our own research, we have overemphasized our work and have not been as comprehensive as we could have been in reviewing other researchers' work.

Address correspondence to Sigal Zilcha-Mano, Ph.D., The Derner Institute of Advanced Psychological Studies, Adelphi University, Hy Weinberg Center, Room 111, 1 South Avenue, P.O. Box 701, Garden City, NY 11530-0701. E-mail: sigalzil@gmail.com