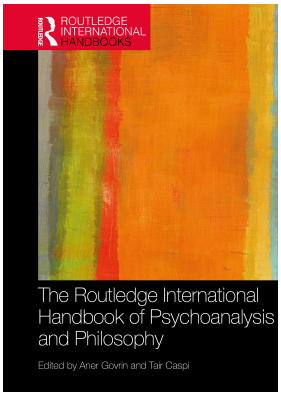

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Psychoanalytic Evidence

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PSYCHOANALYTIC EVIDENCE
The Old and the New

Ed Erwin

The old psychoanalytic evidence was amassed between 1900 and 2000. The new was published in this century.

We could ignore the older evidence, but many of the fundamental questions about how to interpret and weigh this evidence have not been answered to everyone's satisfaction. The issues that were raised in the last century will arise again in interpreting the new evidence.

The Old Evidence: Psychoanalytic Treatment

Establishing the effectiveness of a long-term therapy like psychoanalysis has long posed difficult problems. One is this: without random assignment to treatment and a placebo control, one cannot rule out spontaneous remission and placebo effects.

One might try to include a placebo control in outcome studies of psychoanalysis, but this is not easily done. It is one thing to compare a pill for depression to a sugar pill, quite a much harder thing to devise a placebo control for psychoanalysis. What would we provide to patients in the placebo group that would take the place of talking to the analyst, free association, and dream analysis?

Even if this conceptual problem were overcome, there would be a moral obstacle to giving a placebo treatment for two years or more to patients suffering from serious psychological problems such as clinical depression, agoraphobia, and severe anxiety.

Instead of relying on clinical trials, many psychoanalysts looked for alternatives. The American Psychoanalytic Association for a long time used base rates of improvement to show that psychoanalysis is effective in about two-thirds of all cases. Eysenck (1952) and Erwin (1980) showed that this way of proving effectiveness is too flawed to work.

Others resorted to philosophy arguing, as Thomas Kuhn (1970) had, that evidential standards need to be relativized to a paradigm. For behavior therapists, experimental evidence was a must on their standards, but were not needed in a psychoanalytic paradigm. On this issue, see Bachrach et al. (1991).

Seligman's Questionnaire Methodology

Many found more promising the effectiveness questionnaire methodology developed by the psychologist Martin Seligman. Seligman published his questionnaire in *Consumer Reports* (CR)

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and reported on its findings in *American Psychologist* (1995). His conclusions were about psychotherapy in general, not psychoanalysis in particular, but others have applied his methodology to psychoanalysis (Freedman et al., 1999).

The idea that we can determine not only whether people undergoing psychotherapy improved after treatment, but also what caused the improvement by asking them for their opinions does not seem especially plausible. Seligman is aware of this fact. He agrees that credible alternatives to saying that the treatment worked need to be ruled out, but claims to have done this by using "internal controls" to discount rival hypotheses that say the beneficial effects were due to spontaneous remission or placebo factors.

The argument, roughly, is this: based on the CR figures, we can infer the following conclusions, which in turn can serve as internal controls: (l) longer duration of psychotherapy correlates with more improvement; (2) psychotherapy alone does just as well as psychotherapy plus drugs for all disorders, and given the history of placebo controls being inferior to drugs, one can infer that psychotherapy would have outperformed such controls had they been run; (3) marriage counselors treat the same sorts of problems as professional psychotherapists, but do significantly worse than psychotherapy professionals; and (4) family doctors also do significantly worse than mental health professionals when treatment continues beyond six months. The most likely explanation of these four facts, the argument continues, is that the psychotherapy received by respondents was generally effective, and the more of it the better; consequently, the CR study provides empirical validation of the effectiveness of psychotherapy as well as supporting longer term treatments.

How epistemically useful are Seligman's "internal controls"? The first correlation, between longer duration of treatment and effectiveness, cannot serve as a control for spontaneous remission. If a therapy is followed by spontaneous remission of symptoms, the longer the treatment, the greater the opportunity for spontaneous remission; so, even if factors outside the therapy setting were causing improvement, there would be more improvement with longer-term therapies.

The second internal control, that psychotherapy alone does just as well as psychotherapy plus drugs and that placebos are inferior to drugs, is also unsatisfactory.

First, in the treatment of depression, in two-thirds of all cases, placebos are not inferior to pharmaceutical treatments (Erwin, 1997). Second, some psychotherapies do better than others; see Dobson's (1989) reviews of meta-analyses showing the superiority of Beck's cognitive therapy for depression. Without knowing which kind of psychotherapy each respondent had received, we cannot know if they would have fared worse had they been given a placebo. Third, and most telling, without a placebo control, it cannot be inferred that any psychotherapy, including Beck's, is effective.

After criticisms were made of the Beck studies, the National Institute of Mental Health did a randomized controlled trial including a sugar pill as a placebo, Beck's psychotherapy and a second psychotherapy. Neither psychotherapy could beat the sugar pill, and neither did the standard pharmaceutical treatment also included in the study (Elkin et al., 1989). Contrary to what Seligman claims, we cannot reasonably infer that psychotherapy would have outperformed a placebo control in the CR study had one been included.

Seligman's (3) and (4) are also useless as internal controls. Conclusion (3) says that marriage counselors do significantly worse than psychotherapy professionals; and (4) says the same about family doctors who treat psychological problems when treatment continues beyond six months.

The evidence goes against both claims. In 41 of 42 studies comparing professional psychotherapists and paraprofessionals, the professional psychotherapists failed to outperform the paraprofessionals (Durlak, 1979). After reviewing improvements in the methodology of such studies and refinements in the evidence, Christensen and Jacobson (1994)) render the following

verdict (p. 9): "Yet, whatever refinements are made, whatever studies are included or excluded, the results show either no difference between professionals and paraprofessionals, or, surprisingly, differences that favor paraprofessionals."

Finally, the results in the CR survey were not impressive. Only 13 percent of those receiving Seligman's survey responded. There is no way of knowing if the non-responders experienced far less improvement than those who responded. It should also be noted in passing that the spectacular results – 90 percent improvement – were arrived at by lumping together those reporting a great deal of improvement with those who judged that they had improved "somewhat" or a "lot." If one asks how many said that they had improved a great deal, the answer is: 54 percent, a percentage found in psychological studies of patients receiving no treatment at all.

In short, the CR survey provided evidence that a certain group undergoing psychotherapy were of the opinion that they had improved to some degree because of their therapy, but placebo factors or spontaneous remission were not ruled out as the cause of their perceived improvement.

Conclusion: Seligman's study did not provide evidence that psychotherapy of any form is effective for any clinical problem.

In a second study, a group of psychoanalysts, Freedman et al. (1999), followed up on the Seligman study. They claim that their study provides a replication of the CR finding that longer therapy tends to produce better results. For reasons I gave earlier, this was not demonstrated in the *Consumer Reports* study.

In their study, 99 outpatients attending the IPTAR clinic in New York City responded to the Consumer Reports questionnaire. On the basis of the patient responses, the authors claim to have demonstrated that as a result of receiving psychoanalysis, the quality of life of the patients who responded had been enhanced and, further, that duration and frequency of therapy contributed toward this end. The authors close with a bold declaration: "Our empirical findings, together with those in the evolving literature, establish this as a clinical fact" (Freedman et al., 1999, p. 770).

What were the findings that establish this clinical fact? Freedman et al. found an incremental gain in effectiveness scores from 6 months to over 24 months of treatment. Before concluding that the results support the Freedman et al. hypothesis, certain questions need to be answered.

First, how was the effectiveness score calculated? Following the practice of the original *Consumer Reports* Survey, it was composed of three factors: specific improvement with respect to the problems that led the respondent to therapy; satisfaction with the therapist's treatment; and global improvement, that is, how respondents felt at the initiation of treatment and how they felt after treatment had ended. Points were then assigned in each category depending on whether the respondent rated his or her improvement with respect to each condition as "a lot better," "somewhat better," and so forth.

The third factor, global improvement, is not a measure of specific improvement with respect to the problems that led the respondents to therapy. This factor measures how respondents felt at the time of the CR survey compared with how they felt when they began treatment. This does not tell us anything about diminishment of the problems that led them to seek treatment. There might have been a lot of improvement with respect to their marriage, their finances, their job prospects, or their physical health leading them to conclude that their lives were better, but little or no improvement on the problems that led them into therapy.

The second of the three factors, satisfaction with the therapist, also does not measure improvement in the patients' presenting problems.

Even if other factors are ignored, the results of the study were not encouraging. On the only factor that matters in assessing the effects of the treatment, symptomatic improvement, it failed

to be significantly related to duration of treatment. Only liking the therapist's treatment was so correlated, and that could just as plausibly be interpreted as the reason for staying in treatment for a longer period rather than being an effect of longer treatment.

There are other problems with the study, some that are likely to reappear when the questionnaire method is used again. The authors label the therapy they studied "psychoanalytic psychotherapy," but this term is used to designate psychoanalytically oriented psychotherapy, which is not psychoanalysis.

The most significant problem, however, pertains to the authors' two central claims that (1) as a result of receiving psychoanalysis, the quality of life of the patients who responded had been enhanced, and (2) that duration and frequency of therapy contributed toward this end. Without controls for spontaneous remission or placebo factors, the authors provide no evidence that any improvement in quality of life was caused by psychoanalytic psychotherapy.

Seligman's questionnaire methodology has been used by others to study the effects of psychoanalysis (Sandell et al., 2000), but without success. This situation is not likely to improve for a simple philosophical reason.

If a placebo control is included in a study, we can rule out placebo effects if the treatment group did significantly better on the outcome measures than the placebo group. Internal controls cannot serve the same epistemic function; if they could, pharmaceutical companies paying for clinical trials could save millions of dollars by substituting internal controls for randomization, blinding, and placebo controls.

For researchers relying on internal controls only, the best they can do is to find significant correlations between certain factors, but without adequate external controls, they cannot establish the cause of the correlation. It could be accidental or due to a third variable. In a well-known case, a researcher concluded that he had found evidence that drinking a certain amount of coffee each day significantly increases the risk of pancreatic cancer, despite the fact that his study was not about coffee drinking. He had noticed that a subset of his subjects developed more pancreatic cancer than the others and they drank more coffee. His interpretation of his study results was later decisively discredited.

It is not logically impossible to establish causation relying on internal controls only, but to adopt this as a general strategy is to take another step backwards in trying to support psychoanalysis.

The Old Evidence: Psychoanalytic Theory

For almost a century, Freudians relied for supporting evidence on single case studies of patients treated by psychoanalysts. Many arguments were given to justify the case study method (for review, see Grünbaum, 1984). In the end, this type of evidence proved too weak to support any distinctively psychoanalytic theory (Grünbaum, 1984; Erwin, 1996).

Although Freud and most of his followers spurned the need for experimental studies, about 1500 were done (Kline, 1986, p. 205). They are reviewed, some favorably, by Fisher and Greenberg (1977, 1985) and Paul Kline (1981). Erwin (1996) argues that none of these studies provides empirical support for any distinctively Freudian theory. The failures to confirm are mainly due to methodological problems. These include (1) the failure to provide replications; (2) an overreliance on projective tests that have not been validated such as the Rorschach test and the Blacky cartoons; (3) the testing of hypotheses that were not distinctively Freudian; and (4) a failure to rule out credible rival explanations.

With a few exceptions, these failures to confirm are not evidence of falsity. Better studies or use of an entirely different sort of evidence such as evidence from neuropsychoanalysis (see the next section) might eventually confirm some major psychoanalytic theories.

The 21st-Century Evidence

In the present century, many psychoanalysts have rejected the traditional reliance on single case studies. Some have done experimental studies, the majority of which have been of short-term psychodynamic psychotherapy. Others have relied on evidence from neuropsychoanalysis.

The Fonagy Review

The Fonagy (2015) study provides a comprehensive review of the new outcome studies and meta-analyses of treatments for the major categories of mental health disorders. Fonagy concludes that the evidence generally but not invariably shows PDT (psychodynamic therapy) to be effective for depression, some anxiety disorders, eating disorders, and somatic disorders. The strongest evidence, Fonagy concludes, also supports long-term psychodynamic treatment of some personality disorders, particularly borderline personality disorder.

In his work, Fonagy encountered a philosophical problem due to the multiplication of therapies just as behavior therapists had when they stopped identifying a treatment as behavior therapy if it had a conditioning foundation (Erwin, 1978).

The same problem arises for psychodynamic psychotherapies. There are now more than 30 types based on different and sometimes conflicting psychological theories. Without being able to define "psychodynamic therapy," how does a reviewer of apparently favorable evidence know whether or not it supports the effectiveness of PDT?

As Fonagy points out, giving a definition of PDT has become more difficult in recent decades because of changes in PDT and cognitive behavior therapy leading to a blurring of the differences between the two.

He tries to get around this problem by adopting a "pragmatic" approach which uses "self-declared allegiance" as the guiding principle as to what constitutes PDT. However, this creates a problem for his entire review. Some therapists might see what they are practicing as cognitive behavior therapy, while others using essentially the same treatment may see it as psychodynamic therapy.

Without having any way of knowing whether or not a treatment described by someone as PDT truly is psychodynamic therapy, there is no way to separate studies of genuine psychodynamic therapy from masqueraders. Consequently, there is no way of knowing if any improvement found in the studies was due to psychodynamic therapy.

The Smit, Huibers, Ioannidis, Van Dyck, Tilburg, and Arntz (2012) review reports on a meta-analysis of randomized controlled trials of the effectiveness of long-term psychoanalytic psychotherapy (LTPP), which, again, is not psychoanalysis. The reviewers found that the recovery rate of various mental disorders was equal after LTPP or various control conditions including treatment as usual. They conclude that evidence for the effectiveness of LTPP was found to be limited and at best conflicting.

The review did not include randomized controlled studies of psychoanalysis because after an extensive search, the authors did not find any. This is the same situation that prevailed at the time of Erwin's (1996) review of outcome studies of psychoanalysis. There were no randomized controlled studies. On this issue, nothing has changed.

Psychoanalytic Theories and Therapy, and Neuropsychoanalysis

After decades of criticism of the case-study evidence for psychoanalytic theories and therapy (Grunbaum, 1984; Erwin, 1996), hope for support for psychoanalysis waned as the number of new patients entering psychoanalysis dropped to zero. The situation, however, has become more favorable largely due to recent work in neuropsychoanalysis.

Mark Solms, one of the leading proponents of neuropsychoanalysis, points out (Solms, 2004) that there are now interdisciplinary groups devoted to psychoanalysis and neuroscience in almost every major city in the world; that a new society has been formed, the International Neuro-Psychoanalysis Society, and that a journal, *Neuro-Psychoanalysis*, is now publishing papers on topics of common interest to psychoanalysts and neuro-scientists. There are also annual conferences on neuropsychoanalysis.

These events are consonant with, and likely reflect, developments in general psychology where there has been a trend towards neuroscience (see Satel and Lilienfeld, 2013). Despite these positive developments, some, most notably Blass and Carmeli (2007), have raised doubts about the entire neuropsychoanalytic enterprise.

The Blass and Carmeli Arguments

In a 2007 paper, Blass and Carmeli challenge the idea that neuroscientific findings are relevant and important for the development and justification of psychoanalytic theory and practice.

In a second paper (2015, p. 1155), Blass and Carmeli go far beyond their original point about the weakness of the neuropsychoanalytic arguments. They boldly claim that neuroscience has no contribution to make to psychoanalysis and it can never have any.

In their original paper, Blass and Carmeli examine arguments of neuropsychoanalysts in four areas. They argue that neuroscience has little or no relevance to psychoanalysis in any of these areas. Their arguments are important given the amount of attention paid to work in these areas, but they leave open the possibility that neuroscience will prove useful in areas they do not discuss. Consequently, the authors have not shown in their 2007 paper that neuroscience findings are never important and relevant to psychoanalysis.

Blass and Carmeli, however, have more general arguments, some of philosophical interest. One is worth quoting:

> Neuroscience can describe the neural networks underlying psychological phenomena, patterns, and tendencies, but these phenomena, patterns, and tendencies are recognized and their laws specified without any information regarding the neurons that function concomitantly. Only once these are recognized on the psychological level can neuroscience proceed with its description, but it does so without adding anything to the psychological knowledge already obtained.
>
> *(Blass and Carmeli, 2007, p. 10)*

This last sentence is crucial to their argument and should be challenged. Some psychoanalysts once tried to specify the causes of schizophrenia in terms of malfunctioning family relationships, but evidence of brain malfunctioning in schizophrenics was relevant to deciding whether their attempts would succeed. Studies of the brain made it likely that such attempts would fail. The neuroscience evidence was both relevant and important. The phenomenon of malfunctioning family relationships were recognized on the psychological level, but neuroscience added something important to the psychological knowledge, significant information about the causal role of these family relationships.

There is a point of logic here to be considered. A purely psychological theory that says nothing about neurons may by itself logically entail nothing about the brain; so, it might appear that neuroscience is irrelevant to a psychological theory's truth or justification. What this overlooks is that adding other premises to the propositions of the psychological theory may guarantee the relevance of neuroscience.

To take a controversial example, Freud's theory that dreams are instigated by repressed wishes says nothing about the brain, but we make neuroscience relevant if we combine his theory with the premises that (1) dreams are instigated by unconscious wishes only if higher-level motivational brain processes are responsible for the appearance of the dream; (2) that these higher-level processes cannot be responsible if all dreaming occurs only during REM sleep, assuming that REM processes are derived from automatic activity coming from the brain stem, and (3) in fact all dreaming occurs during REM sleep. This last premise was successfully challenged by work in neuroscience.

Blass and Carmeli have another argument, which they may consider their most important one. Psychoanalysis, they argue, is concerned with meanings as mental phenomena (2015, p. 2). Since it is a process and theory geared towards understanding the latent meanings and psychic truths determining the human psyche, neuroscientific findings are irrelevant to its aims and practices.

There is good reason to question this description of psychoanalysis.

In the 1970s, many psychoanalysts adopted a hermeneutical interpretation of psychoanalysis according to which clinical work does not consist of causal explanations but of ascriptions of meaning (Strenger, 2002). Jürgen Habermas, Paul Ricoeur, and many others said that the same about Freud's theory. The theory is concerned with ascriptions of and understanding meanings. It is not a theory about causation.

At first glance, there seems to be substance to the hermeneutical reading of Freud. Key Freudian hypotheses seem to be about meanings, the meaning of such items as neurotic symptoms, dreams, and slips of the tongue and pen.

In speaking of the meaning of behavior and mental states, the hermeneuticians were not talking about linguistic meaning, that is, meaning in the sense that words and sentences have meaning. If psychoanalytic theory were truly about linguistic meaning, neuroscience would likely not contribute anything to its understanding, but this reading of psychoanalysis would be utterly implausible. Freud's theories about dreams, the etiology and treatment of neuroses, and repression are not about the meaning of words. They are about dreams, treatments of neuroses, and repression.

Those favoring a hermeneutical reading of Freud generally were not talking about linguistic meaning (Ricoeur might be an exception). Their central claim was that psychoanalytic theory is not a causal theory. It is a theory that explains its phenomena in terms of meanings.

A good illustration is Freud's case of the woman obsessed with writing down the serial number of every one of her kronens, a unit of currency, before parting with it. Freud says that "in obsessive actions everything has its meaning and can be interpreted" (1907, SE 9: 122). This is clearly consistent with a hermeneutical approach.

What was the meaning of the woman's obsession? She became obsessed with writing down the serial numbers of her bank notes after her lover declared that he would never part with a 5-kronen note she had given him because it had passed through her hands. Because she had doubts about his intentions, she meant to challenge him later, but she realized that without knowing the serial number of the original note, she would have no way of knowing if the 5-kronen note he might show her was the original.

Freud comments that her doubt remained unresolved and it left her with the compulsion to write down the number of each banknote, by which it can be distinguished from all others. This, Freud claims, is the meaning of her obsessive actions. Yet, what did he mean in saying that her unresolved doubt left her with the compulsion except that it caused it? If the doubt had made no difference to the compulsion, there would have been no reason to single it out as the meaning of the compulsive behavior.

What this cases illustrates is that when Freudian theory talks about the latent meaning of a dream, the meaning of a neurosis or slip of the tongue, these hypotheses presuppose causal propositions either for their truth or proof. If we reinterpret Freudian theory so that all of the implicit and explicit causal hypotheses vanish, then what is left cannot explain the phenomena that Freud was trying to explain.

Some favoring a hermeneutical interpretation denied this. They claimed that what explains the phenomena is its meaning. Causality plays no role. Here they ran into a problem they were never able to resolve and which damned the entire phenomenological enterprise. When meanings are brought in to explain, as in the case of the obsessed woman, they either made a difference to the occurrence of the events or they did not. If they did not, they do not explain; if they did, they were causes. A cause is something that makes a difference to the occurrence of an event.

If psychoanalytic theory is a causal theory, then in this respect it is like any psychological theory that attempts to explain mental events or behavior. Some theories may dispense with talk of meanings and instead speak of cognitive dissonance, self-efficacy expectations, beliefs, desires, or latent feelings, but these superficial differences in terminology do not explain why neuroscience may be relevant to them but not to theories that speak of meanings. Until this is explained, Blass and Carmeli's claim that psychoanalysis is concerned with meanings as mental phenomena may be true but adds nothing to their earlier arguments.

The upshot of this discussion is that the relevance of neuroscience to psychoanalysis needs to be discussed on a case-by-case basis. Its relevance and importance cannot be ruled out *a priori*.

The Positive Case for Neuropsychoanalysis

Most of the arguments for neuropsychoanalysis' relevance to psychoanalysis are empirical, but philosophical issues also play a role.

One pertinent philosophical question is whether psychoanalysis and neuroscience study the same entity. Some neuropsychoanalysts who speak of the mind-brain appear to be saying "yes." They are the same entity. The mind is the brain.

This issue was debated for years in philosophy when the most dominant theory of mind was the identity theory, which says that the mind and brain are one and the same, or, as formulated by philosophers who denied mental entities, all mental states are brain states.

The identity theory was eventually rejected by most philosophers because of its inability to deal with several problems. One quite relevant to neuropsychoanalysis is that the theory has been impossible to prove.

Neuroscientists can correlate mental and brain states by asking subjects what is going through their mind at a given point of time, and then using functional magnetic resonance imaging (fMRI) to take pictures of the brain, but even if we had perfect correlations, these findings would be neutral between the theory, claiming there is only one thing and dualistic theories that say there are two. The correlational results would not support the identity theory or dualism.

One positive philosophical argument for neuropsychoanalysis concerns the mind's causal dependence on the brain. The argument goes like this. If all mental events are causally dependent upon neural events, then wherever there is a psychological cause of a behavior or mental event, there will be a preceding neural event causally sufficient for the event to be explained, thus rendering the appeal to the psychological cause superfluous.

Suppose a woman slaps a man because she believes he has grossly insulted her. If her belief plus certain other psychological conditions, such as the woman's desire to respond, are sufficient to explain the slapping, there will be a preceding brain event such that once it occurs, it

necessitates the slapping. The reference to the belief and other psychological conditions can be skipped.

In general, wherever there is a causal sequence, N (neural event) → P (psychological event) → B (behavior), the middle link can be ignored. Since the occurrence of N is causally sufficient for B, the intervening psychological link is said to be not needed to explain or predict the behavior. All psychological explanations, then, can in principle be replaced by neuroscience explanations.

This "skipping a link" argument was used by B. F. Skinner (Erwin, 1996) and other psychologists. It is not a good argument. The dubious premise says that because the initial cause in the sequence necessitates the final result, the intervening link is not needed to explain the behavior. This is true of prediction but not explanation.

Making an insulting comment, on Skinner's view, is an environmental event, but how the target of the comment responds will depend on whether she interprets it as an insult. To leave out her belief about what was said about her, as Skinner recommends, is to fail to understand why the slap occurred. The same problem arises in using the skipping a link argument in neuroscience.

If the woman slapped the man because of what she believed, there will be a preceding neural event which in combination with other events is causally sufficient for the slapping, but the causal chain does not terminate with a brain event. The brain event in turn will be preceded by a biological-chemical event that is also causally sufficient for the neural event. If the skipping a link argument were sound, psychological explanations would be eliminable but so would all neuroscience explanations.

The Empirical Issues

Mark Solms, one of the leaders of the neuropsychoanalytic movement, claims that neuroscientists are uncovering proof for some of Freud's key theories, including his theories about unconscious motivation, repression, the pleasure principle, the idea that dreams have meaning, and the Id-Ego-Superego hypothesis. Before looking at the specific details of his arguments, some general problems with neuropsychoanalysis should be mentioned.

Small sample sizes: Neuropsychoanalytic studies generally suffer from small samples sizes as do most other neuroscience papers. In a 2014 paper titled "Power failure: Why small sample size undermines the reliability of neuroscience," Button et al. (2013) found that the average statistical power of studies in the neurosciences is very low primarily due to small sample sizes. The consequences of this include overestimates of effect size and low reproducibility of results.

In a follow-up study, Szucs and Ioannidis (2017) analyzed statistical information extracted from thousands of recent cognitive neuroscience and psychology research papers. They conclude that the statistical power to discover existing relationships in these fields has not improved during the past half century.

Solms' Arguments

One subject of interest to Solms has been the bearing of neuroscience on Freudian dream theory. In a section headed "Dreams Have Meaning," Solms points out that the dream theory was seemingly discredited with the discovery of a strong correlation between dreaming and REM sleep, but more recent work in neuroscience shows that dreaming and REM sleep are dissociable states, controlled by distinct mechanisms. In short, dreams also occur outside of REM sleep.

Although these findings answer a serious objection to Freud's dream theory, they do not show that dreams have meaning in the sense Freud intended. That dreams sometimes have meaning

has been known for thousands of years; it is not peculiar to Freud, and it is widely accepted by many of Freud's critics (Hobson, 2004; Domhoff, 2004). What the critics question is whether dreams have the sort of hidden meaning postulated by Freud. It is important to note, then, that the assigning of meaning to dreams does not support anything specifically Freudian. In addition, the thesis that dreams have hidden meanings is not confirmed by evidence that dreaming occurs outside of REM sleep – removing one counterargument is not confirmation – and is not confirmed by any other evidence from neuroscience.

What parts of Freudian dream theory, then, are confirmed by recent neuroscientific work? Solms (2004) claims that contemporary knowledge of the dreaming brain is "broadly consistent" with Freudian theory, but he does not claim, and he would be wrong to claim, that any of the neuroscientific evidence confirms that dreams have hidden meanings, or that they require interpretation in therapy, or that dreams are wish fulfilments, or that dream censorship occurs, or that free association is useful in uncovering the hidden meaning of dreams.

It is possible that future neuroscientific research will support Freudian dream theory, but in considering this possibility, one needs to look at research already done in dream laboratories. The extensive evidence discussed by Domhoff (2004) makes it likely that central parts of the theory, including most of the theses mentioned above, are not just unfounded but false.

A second alleged success for neuropsychoanalysis is its proof for Freud's structural theory of the mind which postulates the Id, Ego, and Superego. In a paper providing this proof, Solms points out that the core brain stem and limbic system correspond roughly to the id; the ventral frontal system, the dorsal frontal region, and the posterior cortex amount to the ego and superego. But how does he know that any of these identities he postulates holds? There is an instinctual part of the brain, but without evidence that there is an unconscious mental agency or structure that has the basic properties that Freud attributes to the Id, such as being the source of much mental conflict and seeking its own gratification, why believe that the id is roughly identical to the parts of the brain Solms specifies?

Has Freud's theory of repression been vindicated? For doubts about Freud's repression theory, including doubts about its existence, see Erwin (1996, pp. 220–223). If Solms is right, neuroscience provides an effective answer to these doubts.

Solms (2004) discusses a 1994 study by Ramachandran of anosognosic patients who have damage to the right parietal region of the brain, making them unaware of certain gross physical defects such as paralysis of a limb. One such patient with a paralyzed left arm consistently denied that she had a problem. After Ramachandran artificially activated her right hemisphere, the woman suddenly became aware that her arm was paralyzed and that it had been paralyzed continuously since she suffered a stroke eight days earlier. Solms takes these facts as showing that the woman was capable of recognizing her physical deficits and that she had unconsciously registered her paralysis during the previous eight days. His interpretation, however, is open to challenge. Based on Ramachandran's description of what occurred, it is not clear that the woman unconsciously registered the fact of her disability at any time prior to having her right hemisphere artificially stimulated.

When the stimulation wore off, the woman once again believed her arm was normal and forgot that part of the earlier interview in which she acknowledged that the arm was paralyzed. Based on his observations, Ramachandran concludes that memories can be selectively repressed and that observing this patient convinced him of the reality of repression phenomena. It is not clear why this interpretation is mandated by his findings. The observed events could be interpreted as evidence of repression, but, they could equally well be explained by talking about brain deficits.

The brain impairment causes an incapacity to recognize the paralysis of the arm except when the parietal region is being artificially stimulated and it also causes an inability to remember being aware of the paralysis during the stimulation period; there is no storing of memories in the unconscious, and hence no Freudian repression.

A more recent study by Anna Berti and her colleagues (Berti et al., 2005) appears to support this alternative explanation. They compared the distribution of brain lesions in patients showing left spatial neglect, left hemiplegia, and anosognosia with patients showing neglect, left hemiplegia but not anosognosia. The authors found differences in brain lesions between two patients and concluded that anosognosia for hemiplegia is best explained by the involvement of motor and premotor areas. If this is correct, there is no need to bring in repression in such cases.

Even if the denial involved in anosognosia patients were motivated as opposed to being directly caused by specific brain lesions, there are findings that would raise questions about the postulation of repression. On Freud's theory of repression, repressed material is stored in the unconscious and can be returned to consciousness, but only if the anxiety associated with the repressed memory is removed. On this basis, one would predict that repressed memories would stay repressed without a removal of the anxiety and that the repression would not be lifted spontaneously. Yet studies of anosognosia find that it often does remit spontaneously. For example, in the study by Maeshima et al. (1997), anosognosia disappeared within three months in all cases (p. 696).

On another issue, Solms points out that major brain structures essential for forming conscious (explicit) memories are not functional during the first two years of life. Yet our infantile memories, despite not being consciously encoded, can affect adult feelings and behavior. This claim, however, is so general and vague that it says nothing which critics of Freud typically deny, nor does it warrant acceptance of any major thesis that is specifically Freudian.

On a final issue, Solms argues that if Freud was right about the operation of the pleasure principle, then damage to the inhibitory structures of the brain will release wishful, irrational modes of mental functioning; this prediction, he claims, has been confirmed. Patients suffering from Korsakoff's psychosis are unaware that they are amnesiac and fill in memory gaps with confabulations. Such patients, Solms continues, maintain false beliefs that are generated by the pleasure principle, but he cites no evidence that the ego or superego are involved. All that is claimed is that the patients, once their cognitive mechanisms are damaged as the result of brain lesions, construct confabulations to recast reality as they want it to be.

Some investigators interpret such confabulating activity in Freudian terms, but it is not clear what evidence justifies this interpretation. People often see the world the way they wish it to be; there is nothing new in saying this. The finding that victims of a certain type of brain disorder are not just randomly making up false beliefs about the world, but are constructing visions of the world as they wish it to be is interesting, but not specifically Freudian. What we are left with is: if Freud is right about the pleasure principle, then damage to a certain area of the brain will cause wishful irrational modes of thinking. The prediction is correct, but no evidence is provided that only Freudian theory makes this prediction.

Conclusion: I have pointed to problems with some of Solms' papers and to general problems with the neuropsychoanalytic literature including studies with small sample sizes; failures to rule out credible rivals to the hypothesis being tested; and the unargued identification of neural events or processes with mental events or processes postulated by Freud or other psychoanalysts. Some of these issues need to be resolved before neuropsychoanalysts can justifiably claim to have supported Freudian theory or any psychoanalytic theory. Yet without reviewing other papers by Solms and others working in this area, I am in no position to say that nothing done so far accomplishes this.

There was a low point for psychoanalysis in the last part of the 20th century when the traditional case method for obtaining evidence was discredited (Grunbaum, 1984; Erwin, 1996); when pharmaceutical and psychological alternatives to psychoanalytic treatment, such as cognitive behavior therapy, were being widely used; and when the rate of new patients entering psychoanalysis fell to zero. There was a lot of pessimism. Some predicted that psychoanalysis would never recover.

Things have changed. There are exciting things going on now in psychoanalytic research and not just in neuropsychoanalysis. Is Freud back? Solms says so (2004). Perhaps he is right, but given the serious defects in so many neuropsychoanalytic studies, more work will be needed before a confident answer can be given.

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