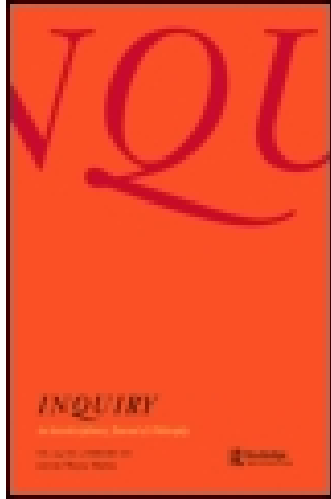


This article was downloaded by: [University of Sydney]

On: 03 May 2015, At: 12:34

Publisher: Routledge

Informa Ltd Registered in England and Wales Registered Number: 1072954 Registered office: Mortimer House, 37-41 Mortimer Street, London W1T 3JH, UK



Inquiry: An Interdisciplinary Journal of Philosophy

Publication details, including instructions for authors and subscription information:

<http://www.tandfonline.com/loi/sinq20>

Psychoanalysis - myth or science?

H. J. Eysenck ^a

^a Institute of Psychiatry , University of London ,

Published online: 29 Aug 2008.

To cite this article: H. J. Eysenck (1961) Psychoanalysis - myth or science?, Inquiry: An Interdisciplinary Journal of Philosophy, 4:1-4, 1-15, DOI: [10.1080/00201746108601323](https://doi.org/10.1080/00201746108601323)

To link to this article: <http://dx.doi.org/10.1080/00201746108601323>

PLEASE SCROLL DOWN FOR ARTICLE

Taylor & Francis makes every effort to ensure the accuracy of all the information (the "Content") contained in the publications on our platform. However, Taylor & Francis, our agents, and our licensors make no representations or warranties whatsoever as to the accuracy, completeness, or suitability for any purpose of the Content. Any opinions and views expressed in this publication are the opinions and views of the authors, and are not the views of or endorsed by Taylor & Francis. The accuracy of the Content should not be relied upon and should be independently verified with primary sources of information. Taylor and Francis shall not be liable for any losses, actions, claims, proceedings, demands, costs, expenses, damages, and other liabilities whatsoever or howsoever caused arising directly or indirectly in connection with, in relation to or arising out of the use of the Content.

This article may be used for research, teaching, and private study purposes. Any substantial or systematic reproduction, redistribution, reselling, loan, sub-licensing, systematic supply, or distribution in any form to anyone is expressly forbidden. Terms & Conditions of access and use can be found at <http://www.tandfonline.com/page/terms-and-conditions>

PSYCHOANALYSIS — MYTH OR SCIENCE?

by

H. J. Eysenck

Institute of Psychiatry, University of London

In this paper an attempt is made to look at Freud's contribution from the point of view of its scientific validity. A factual survey is made of the results of psychoanalytic psychotherapy, of the kinds of facts and arguments used to support the psychoanalytic doctrine and of the experiments carried out to test it. The conclusion arrived at is that psychoanalysis and the theories associated with it is not a science, but a myth; adherence to it is based on emotion and prejudice rather than on fact and reason.

I. Introduction

Psychoanalysis presents a rather curious dilemma to those who would evaluate it. In psychiatry it has become the leading school to such an extent that in some countries, particularly the United States, it is almost impossible to obtain a leading post, either in academic life or private practice, without having undergone a training in analysis and thus having been exposed to a most efficient form of "brain washing". Similarly, among novelists, film makers, journalists, teachers, philosophers, and even among the general public, psychoanalysis is almost the only type of psychology at all well known; indeed, to most people, psychoanalysis is psychology. Even cartoonists have joined in this chorus of agreement, to the extent that "the psychoanalysis joke" has become as much of a standby in *Punch* as in *The New Yorker*. In a very short time Freud's original heresies have thus become widely accepted, and even more widely acclaimed — a fact which seems to go counter to Freud's own deduction from his theories that there would be a particularly strong and virulent *resistance* to the acceptance of psychoanalysis, as compared with other new beliefs. In actual fact, psychoanalysis was accepted much more readily, much more widely, and much more uncritically than almost any other set of comparable, revolutionary and new ideas. If Freud's prediction

does indeed follow from his theory, then the facts seem to have disproved this prediction as well as so many others.

In all this chorus of jubilation, however, there is still a hard core of unbelievers; a group of people to whom the whole story of psychoanalysis is little but a repetition of the famous fairy-tale about the Emperor's new clothes. And it is curious to note that these dissenters tend to be found mostly among those who have been trained in scientific method and who have adopted psychology as their profession. There are very few experimental psychologists or leading psychological theoreticians who accept the Freudian doctrine, and the majority tend to regard it as so much beyond the pale that they do not even consider it necessary to discuss and argue its pretensions. We thus have the curious position that psychoanalysis is widely accepted among lay people and others untrained in psychology, ignorant of experimental methods and incapable of evaluating empirical evidence. On the other hand, we have a widespread rejection of psychoanalytic claims by those knowledgeable in psychology, experienced in experimental methodology and well able to evaluate empirical findings. The most obvious hypothesis suggested by this state of affairs would seem to be that psychoanalysis is a myth; a set of semi-religious beliefs disseminated by a group of people who should be regarded as prophets rather than scientists. It will be the purpose of this article to investigate to what extent this hypothesis may contain seeds of truth, and to what extent it may be a mischievous caricature of the state of affairs as it exists at present.

First of all, however, we must deal with a type of argument which is sometimes presented by supporters of the psychoanalytic movement. What is said is something like this. Psychology by its very nature cannot be a *Naturwissenschaft*, i.e. a natural science like physics or physiology, but it must be a *Geisteswissenschaft*, i.e. a kind of intuitive, humanistic discipline; that psychology cannot *explain* behaviour in terms of general laws, but can only *understand* it in terms of each individual's own intuitions. This is a line taken, among others, by Husserl and other German philosophers whose *a priori, ex cathedra obiter dicta* have attracted far more attention than their lack of factual information of modern psychology and its ways of working would seem to warrant. Fortunately, however, it will not be necessary here to deal with the erroneous arguments of what Windelband has christened the *idiographic* school, as compared with the *nomothetic* school; we need only look at Freud's own claims, and those of his closest followers, to realize

that he himself would not have wished to hide behind the skirts of this particularly unattractive mother figure. He believed, and stated unambiguously on many occasions, that psychoanalysis was a scientific discipline like any other, that its laws had the same claims to universality as do those of physics; that its predictions were scientific predictions which could be tested empirically, and that the whole outlook and tenet of psychoanalysis was deterministic. Those who now wish to escape from the consequences of the empirical testing of psychoanalytic doctrines by claiming that psychoanalysis should be judged on other terms, are presenting us with an argument which would not have appealed to Freud at all, and I will not attempt here to deal with these evasions of the central issue. The only interest the Freudian doctrines have lies in their factual content, and in the conclusions that flow from this content in the way of psychotherapeutic treatment. It is always possible to defend a religious belief on non-empirical grounds: "I know that my Redeemer liveth" is not intended to be a scientific statement equivalent to "I know that the neutrino exists". If we now make such a statement as "the Oedipus complex is universal and has certain definite behavioural consequences" we can take this as equivalent either to the religious statement, to be chanted in unison at psychoanalytic conferences but having no relevance to factual matters, or we can regard it as a statement of the scientific kind to be evaluated in empirical terms. I would like to suggest that if psychoanalytic statements are of the former type, they are of no interest whatsoever, except to the student of religious beliefs. If they are of the latter type, then the interest is in direct proportion to the amount of evidence which can be brought to support them. In other words, psychoanalysis is a science, subject to the usual dictates of scientific argument and scientific evidence, or it is nothing.

II. Effects of Psychotherapy

Psychoanalysis was originally introduced as a method of treatment of neurotic disorders, and as a theory to explain the causation of disorders. The theory has undergone many subtle changes, and I shall assume it to be too well known to require restatement except in the very briefest outline. To the psychoanalyst neurotic symptoms are merely the observable signs of underlying complexes, repressed well into the unconscious but too strong to remain completely suppressed. These complexes date back to childhood years and are associated with

the Oedipus complex which is their *fons et origo*. Treatment consists in *uncovering* the original infantile experience which laid the basis for the later neurosis.

This type of treatment has now been going on for some sixty years, and many thousands of psychiatrists and psychoanalysts have been practising it in practically all the civilized countries of the world. One would imagine that after all this time some definite knowledge would have accumulated about the effectiveness of psychotherapy as so practised. This, it is interesting to report, is not so. Psychoanalysts have always been eager to hide their light under a bushel as far as evidence of the success or otherwise of their treatment is concerned. This contrasts rather sharply with the impression, given wittingly or unwittingly by psychoanalysts, that their method is the only one which gives positive and lasting results in this field. What psychoanalysts have usually done has been to publish individual cases, almost invariably cases in which the patient got better, and to argue from these illustrative examples to the general case. The argument may be formally stated in a way that exposes it as one of the classical examples of the *post hoc ergo propter hoc* fallacy. The fact that a patient, John Doe, who is suffering from a phobia, gets better four years after psychoanalytic treatment has been initiated, is not proof that John Doe has got better *because* of such psychoanalytic treatment, and to reason thus even by implication, is so obviously absurd that I will not waste space by arguing the case. There is no method of treatment, from prayer to giving neurotics cold baths, and from hypnosis to extracting their teeth in order to eliminate septic foci, which has not given rise to similar claims to those of psychoanalysis, and which has not published clamorous and lengthy accounts of "cures" so accomplished. Clearly the assessment of therapeutic claims in this field is complex and difficult and requires a certain degree of sophistication.

The most obvious difficulty that arises is the problem of what is sometimes called *spontaneous remission*. It is well known that neurotic disorders often clear up without any formal treatment of any kind; indeed this is true of the majority of cases. They also clear up after types of treatment which are completely non-specific and which, according to the psychoanalysts, should have no effect at all. A particularly good example is the famous study of Denker in which he studied five hundred severe neurotics who had complete disability pensions because of their neuroses. Not only did these five hundred fail to receive any kind of psychoanalytic treatment; they were also,

because of their pensions, highly motivated to retain their illness. Nevertheless, some two out of three completely recovered within two years, having had no other treatment than the usual pink pills and pep talks of their G.P.s. After five years the percentage of recoveries rose to some 90 per cent. There are many other studies giving rise to similar conclusions, to wit, that neurotic disorders are generally of a self-terminating kind and, however severe, are not likely to last for more than two or three years even when left untreated, or when treated by people with no training in psychiatry or psychoanalysis.¹

To prove its efficacy, psychoanalysis would clearly have to do better than this. If people treated by psychoanalysis did not recover more quickly or in greater numbers than when left untreated, then clearly the claims of psychoanalysis, as far as its curative powers are concerned, would have to be rejected. Actually one might anticipate a positive showing for psychoanalysis even though the method was not in fact efficacious. The reasons for this are as follows. Psychoanalysts, by and large, only treat the better-off and more intelligent types of patient, and furthermore they tend to select their patients very stringently in terms of their likelihood to benefit from treatment. On these grounds their patients should have a better recovery rate than the more unselected groups on which the spontaneous recovery base line was established. In actual fact the data suggest very strongly that, if anything, patients treated by psychoanalysis *take longer to recover and recover to a lesser extent* than do patients left untreated. This conclusion is arrived at by averaging the claims made by various psychoanalysts and psychoanalytic institutions with respect to their patients. These claims are taken at face value, although there is the ever-present danger that each analyst would be prejudiced in favour of his own successes, thus giving a more optimistic view than would be warranted had an independent examination been made of the patients.

Such an actuarial comparison is, of course, defective from many points of view. It is difficult to be certain that the persons in the various groups are in fact suffering from equally serious disorders; and it is difficult to be sure that the criteria of "cure" and "recovery" used by different people are in fact identical. Much could be said in relation to both these points, but however much we might be willing to favour the psychoanalytic side, and however much our assumptions might strain probabilities, yet on no account can the figures be interpreted to give any support whatsoever for psychoanalytic claims. This verdict is borne out by several studies, much better controlled experimentally,

where patients have been divided into various groups, submitted respectively to treatments of various kinds or no treatment at all. The results of these studies bear out the findings that psychoanalysis has no apparent effect as compared with other treatments or no treatment at all; again, therefore, psychoanalytic treatment receives no support from the outcome of the experiment.

One might have thought that, with respect to children, psychoanalysis might be more positively placed, as these might be considered to be more impressionable and more easily cured. Here also, however, an extensive review of the literature shows a picture almost identical in every detail with that found in adults. There is no evidence that psychoanalysis of children produces any kind of effect on the neurotic symptoms of these children.

In 1952 I published a short paper listing the evidence and describing what I thought was the only possible conclusion to which it could lead, to wit, that the null hypothesis had not been disproved, i.e. that psychoanalysts had failed to show that their methods produced any ameliorating effects on people suffering from neurotic disorders. This brief, factual, and innocuous paper produced a whole shower of replies, critiques, refutations, arguments, and discussions; it did not, however, produce a single mention of a single experiment or clinical trial which had demonstrated a positive effect for psychoanalytic treatment. Indeed, in recent years the more official and better-informed psychoanalysts have become rather more chary of making any claims of therapeutic effectiveness for psychoanalysis. Glover, to take but one example, has explicitly rejected such claims in his latest book; the Chairman of the Fact Finding Committee of the American Psychoanalytic Association has explicitly stated that his Association had no positive evidence on the point, and did not make any kind of claim of therapeutic usefulness; Schmiedeberg and many other practising analysts have come to similar conclusions in print. It has been left to the large herd of faithful believers, who have no direct knowledge of psychoanalytic practices and are ignorant of the very existence of a large experimental literature, to continue to make claims which are not, in any way, supported by the evidence.

Why is it, the reader may ask, that in spite of its apparent uselessness, psychotherapy is so widely praised by people who have undergone it, and who claim they have been cured by it? The answer I think lies in a famous experiment, reported by the American psychologist, B. F. Skinner. He left a group of pigeons alone in their cage for

twelve hours but arranged for an automatic hopper to throw out a few grains of corn at intervals to the hungry animals. When Skinner returned in the morning, he found that the animals were behaving in a very odd manner. Some were jumping up and down on one leg, some were pirouetting about with one wing in the air; others again were stretching the neck as high as it would go. What had happened? The animals, in the course of their explorations, had happened to make that particular movement when the hopper had released some corn. The pigeon, not being a slouch at the *post hoc ergo propter hoc* argument, imagined that the movement preceding the corn had, in fact, produced the corn, and immediately began to repeat the same movement again and again. When finally another reward came tumbling out of the hopper, the pigeon became more firmly convinced of the causal consequences, so throughout the twelve hours the pigeon performed the movement and the hopper, at irregular intervals, dispensed the corn. To leave out the anthropomorphic terminology, and to put it in slightly more respectable language, we may say that the pigeon became conditioned to make a particular response in order to receive a particular reward. There is nothing mysterious about the experiment, which Skinner entitled "A Study in the Growth of Superstition", and we can directly relate it to the growth of the belief in the efficacy of psychoanalytic treatment, both among patients and among psychoanalysts themselves.

Neurotics get better regardless of treatment; this improvement constitutes the reinforcement, and is equivalent to the corn received by the pigeon. The actions of the psychotherapist are as irrelevant as is the behaviour of the pigeon in the experimental situation. Neither is instrumental in producing the reinforcement, but both become connected with it through processes of conditioning; thus a superstition is created, both in the pigeon and in the patient, linking the one with the other. Much the same is true of the therapist himself; for him too, the reinforcement is the improvement reported by the patient. This is independent of his actions, but because it follows them in time, the conditioned response is established. There is nothing in the published evidence to contradict this hypothesis, and much to support it.

III. The Data of Psychoanalysis

It has often been said that psychoanalysis is more than a curative technique, and that a failure to prove the efficacy of psychotherapy

would not necessarily invalidate the truth of the psychoanalytic doctrine in other respects. (Conversely, it might be said that even if psychoanalysis were found to be a successful method of therapy, this would not necessarily prove the truth of the psychoanalytic doctrine.) Up to a point this may be true, but I think it should be accepted only with grave reservations. In the first place, the whole doctrine of psychoanalysis was based on information obtained during the treatment of neurotic patients and in the course of trying to effect an amelioration of their symptoms. To admit that the primary purpose of psychoanalysis had resulted in complete failure, but that nevertheless the doctrine was correct and scientifically valuable, seems, on the face of it, an unlikely contingency ("By their fruits shall ye know them!"). But this, of course, is not all. If the theory is correct, then the method of treatment would seem to develop from the theory, and what is more, it should work in practice. Conversely, if the theory of psychoanalysis is correct, then *spontaneous remission* and the various non-analytic methods of treatment should not be effective and should leave the individual, if anything, worse off rather than better. Thus we have a quite specific deduction from the hypothesis which the facts disprove very thoroughly indeed; I shall come back to this point a little later on. While it thus remains a theoretical possibility that parts, at least, of psychoanalysis might conceivably be correct, although its therapeutic methods were shown to be useless, nevertheless we would require very strong evidence indeed, before accepting such a conclusion. A great deal of experimental work has of course been done in attempts to verify or disprove parts of the psychoanalytic structure. This is not the place to review this very large body of work; it must suffice to say that, on the whole, it has been very detrimental to the psychoanalytic claims. In saying this I must make one important distinction. Most laymen completely misunderstand the Freudian doctrine, and, therefore, mistake as confirmatory evidence, facts which in reality are quite neutral. Freud used certain well-known facts in a rather peculiar manner; the facts themselves may be true, but their verification does not imply that his use of these facts was correct. As an example of this, let me take the concept of *symbolism*.

The facts of the matter are clearly consistent with the notion that we frequently use symbols in our discourse, in our writings, and possibly also in our dreams. These facts have been known for thousands of years; the reader may like to recall the biblical dream of The Seven Lean Kine and the Seven Fat Kine! Modern apologists of the psycho-

analytic movement sometimes write as if Freud had discovered symbolism — as well as sex and a great number of other important factors! His actual contribution, however, has been quite different. He has suggested a possible mechanism and reason for the use of symbols, and he has suggested ways of deciphering the symbolic language of the dream. I do not know of any evidence to indicate that these contributions have a factual basis, and I know many reasons why they should be considered highly unlikely.

Let us take only one or two considerations into account. In the first place, one and the same dream is often interpreted along entirely different lines by different analysts; frequently these accounts are contradictory. It would seem, therefore, that if any one account is "correct", all the others must be false. We are not, however, given any means of deciding which is the "correct" account, nor is the possibility ruled out that all of these accounts are in fact erroneous and have no reference to reality. Analysts often suggest that the proof of the correctness of the interpretation can be found either in the fact that the patient accepts the interpretation, or else in the fact that the patient gets better after the interpretation has been made. Arguments of this kind are too illogical to deserve an extended reply; a patient's "acceptance" of an analyst's interpretation can hardly be regarded as scientific evidence. And as we have shown previously, the patients are likely to get better anyway, dream interpretation or no dream interpretation, and consequently the improvement is irrelevant to the truth or falsity of the theory. It must be admitted that in isolated and highly selected cases, a good case can sometimes be made out in favour of the Freudian notions. Thus consider the following example. A young girl dreams that a young man is trying to mount a rather frisky horse. He almost succeeds on two occasions and finally achieves success on the third. The analyst succeeds in elucidating the facts: (1) that the young man in the dream is the patient's fiancé, and (2) that the patient's nickname is "Cheval". His interpretation to her is that she wishes to have intercourse with her fiancé, and she volunteers the information that on two occasions she and her fiancé went so far in their love-making that she only just succeeded in extricating herself. So far so good; here we seem to have an excellent example of Freudian symbolism at work, together with his notion of "wish fulfillment". But remember that according to Freud's theory, the reason for the use of symbols was simply that the matter dreamed about was too painful or too intolerable for the mind of the dreamer to be accepted

without disguise. Is it really acceptable to be asked to believe that a young girl who went as far as this in her love-making found the notion of intercourse so painful to contemplate that it had to be disguised in symbolic form? Far from supporting the Freudian position, therefore, this particular example would seem to demonstrate that while it is true that symbols which have been known for thousands of years do indeed occur in dreams, yet the particular Freudian contribution which explains the occurrence of these symbols, does not fit the facts at all. Thus data which superficially may seem to support the Freudian view, can often be found, on closer inspection, to contradict it significantly. It is the admixture of true and long-known facts which makes creditable to the unwary reader, the peculiar and unwarranted use made by Freud of these facts; it is this feature of his theory which has led one famous psychologist to say of it: "What is new in it is not true, and what is true in it is not new."

It may be said altogether that for Freud there was a distinct failure to comprehend a distinction between a fact and the interpretation of that fact. This failure is rendered less obvious than it would otherwise be by Freud's excellent command of language and by his skill in presenting his case to its best advantage. But woe betide the reader who tries to separate the facts from the interpretations, in order to discover whether or not the former can in truth be said to give rise in any unequivocal manner to the latter! He will find his task made almost impossible by the skilful way in which Freud has hidden and glossed over important facts, and the brilliant way in which he has highlighted his interpretive account of what may, should, or ought to have happened, but which, as far as one can discover, probably never did happen. As a supreme example of this, the reader is urged to go back to Freud's original writings and reread his "Analysis of a Phobia in a Five-year-old Boy" — the famous case of little Hans. This has achieved considerable historical importance and has been universally praised by psychoanalysts as the inauguration of all child analyses. Let us have a look at little Hans, who developed a fear of horses after having seen a horse, which was pulling a bus along the street, fall down in front of his eyes. It is noteworthy that Freud only had one short interview with little Hans; all the rest of the material was provided by the father of little Hans, who, we are told, was an ardent follower of Freud. The father, as will be seen by anyone reading through the account, is constantly telling little Hans what he wants him to say, and usually continues until little Hans (who after

all was only five years old) gave some kind of consent. When even this produced no results, the father had no hesitation in saying that Hans really meant exactly the opposite of what he actually said, then treating this, in itself, as an established fact. Freud seems to have realized this to some extent and says: "It is true that during the analysis Hans had to be told many things which he could not say himself, that he had to be presented with thoughts which he had so far shown no signs of possessing and that his attention had to be turned in the direction from which his father was expecting something to come. This detracts from the evidential value of the analysis but the procedure is the same in every case. For a psychoanalysis is not an impartial scientific investigation but a therapeutic measure." Freud, himself, followed exactly the same procedure as the father because in his interview with the boy he told him "that he was afraid of his father because he himself nourished jealous and hostile wishes against him". The boy, his introspections, his sayings and his thoughts, are never really in the picture; what we always get is what either his father or Freud told him he should think or feel on the basis of their particular hypothesis. And whether the child could finally be made to agree or not, the result was always interpreted as being a vindication of the theory. No one who has a scientist's almost instinctive veneration for facts can regard this psychoanalytic classic as anything but a straightforward attempt to fit the child's testimony into the Procrustean bed of a cut and dried theory, previously determined upon; it is difficult to imagine anything little Hans could have said or done that could not in this manner have been transfused into support of the theory. Even so, however, there are glaring cases of inconsistency in the account; thus little Hans was afraid of the "black things on the horses' mouths and the things in front of their eyes"; Freud claimed that this fear was based on moustaches and eyeglasses and had been "directly transposed from his father onto the horses". In actual fact the child was thinking of the muzzle and the blinkers which had been worn by the horse that fell. Again Freud interpreted the agoraphobic element of Hans's neurosis "as a means of allowing him to stay at home with his beloved mother". Nevertheless, both the horse phobia and the general agoraphobia were present even when little Hans went out with his mother!

A very detailed examination of this case has been made recently by S. Rachman and J. Wolpe in a paper published in the *Journal of Nervous and Mental Diseases*, (1960, 130, 135-48) and the reader who

wishes to form an independent and unbiased idea of the reliability and validity of psychoanalytic investigations is urged to read both the original case and this review of it. He will find that the case of little Hans is very similar to all other cases published by Freudian writers, in supporting a gigantic pyramid of speculation on a small pebble of fact.

IV. The Behaviourist Account of Neuroses²

Conant, of Harvard, has pointed out that no scientific theory has ever been killed by the criticism directed at its inadequacies; what is required is an alternative and clearly superior theory. Such a theory, in my view, is at the moment in the process of being formulated by a number of American and British writers; its theoretical background lies in Pavlovian conditioning and modern learning theory, while its practical application has been labelled "behaviour therapy", to indicate its relationship to the tenets of behaviourism. What is maintained by this theory may be put very briefly thus. Neurotic symptoms are maladaptive actions and/or emotions which have become conditioned to certain types of stimuli. They can be removed by an appropriate process of extinction or counter-conditioning. There is no disease underlying these symptoms, and there are no complexes which produce new symptoms should the old ones be extinguished. All that we are dealing with in a neurosis is, in fact, the symptom or set of symptoms; once these are eliminated, the neurosis, as such, has vanished.

A simple illustration may make clear the meaning of some of these terms. Consider another infant, this time little Albert, an eleven-month-old boy who was being studied by Watson, the originator of behaviourism. Watson had been impressed by Pavlov's demonstration in which a dog becomes conditioned to salivate to the sound of a bell by being given food a number of times just after the bell has been rung. After some twenty pairings of bell and food, the bell alone (the conditioned stimulus) now produces salivation where previous to the pairing with the food, it had failed to do so. Watson made use of this paradigm in conditioning a phobia for rats in little Albert, who used to be very fond of these animals. Watson simply stood behind the infant with a hammer and an iron bar, and whenever little Albert reached for the rat (the conditioned stimulus) Watson would bang the iron bar with the hammer, thus creating a loud noise which frightened

little Albert. After a few repetitions, little Albert, as had been predicted, became afraid of white rats, and developed a phobia for them; indeed, as had also been predicted, this phobia generalized to other furry animals, such as rabbits. We thus have the experimental production of a phobia through the use of a mechanism, well understood and widely studied in the experimental laboratory, both in animals and men. There is no talk here of hypothetical Oedipus complexes, unconscious ideas, super egos, and ids, and all the rest of the psychoanalytic hagiology.

Having established a phobia, can we also cure it by applying the techniques of the conditioning laboratory? The answer is in the affirmative. We have conditioned the infant to respond with fear to the rat; we must now condition the infant to respond with a positive emotion instead. This presents one difficulty; the infant is so frightened by the sight of the rat that he will not be in a fit state to form the conditioned response to the rat opposite in sign to that already established. This problem, fortunately, is not insuperable. The fear of the rat is in part a function of its distance from the infant; remove the rat to the farthest corner of the room and give the hungry infant a piece of chocolate (the unconditioned stimulus) and the infant will munch the chocolate whilst cautiously eying the rat in the far corner. Repeat these processes a few times, bringing the rat closer on each occasion and finally the infant will be munching his chocolate whilst playing with the rat. The phobia has been cured never to return.

It is noteworthy that this simple, straightforward hypothesis explains equally well all the facts in the story of little Hans. The fear of horses is accounted for in terms of the traumatic instance of the collapsing animal in front of the bus. (Indeed, the child had been sensitized by two prior experiences with horses.) This conditioned fear of horses, and the open space in which the accident took place, requires none of the mumbo jumbo with which Freud surrounds a perfectly simple and straightforward happening which can be duplicated any day in the laboratory. Indeed Hans, himself, emphatically supports this view. This is what he says: "No. I only got it [the phobia] then. When the horse and the bus fell down, it gave me such a fright, really! That was when I got the nonsense." And the father says: "All of this was confirmed by my wife, as well as the fact that the anxiety broke out immediately afterwards." This view of Hans's phobia is strongly supported by Rachman and Wolpe in the paper already alluded to, and they also advance a plausible view about the decon-

ditioning of little Hans's phobia. We can say, therefore, that simple and straightforward as the behaviouristic account may be, it nevertheless accounts for all the relevant facts in little Hans's sad history, and it does so without requiring a vast amount of speculative elaboration.

If such a theory is indeed, in principle, correct, then we should expect it to furnish us with methods of treatment considerably superior to those advocated by the psychoanalysts. This appears to be the case. J. Wolpe has developed a number of methods for treating neurotic disorders, all of which are based on modern learning theory and the hypothesis that neurotic symptoms are nothing but conditioned maladaptive responses of one kind or another. In his recently published book, "Psychotherapy by Reciprocal Inhibition", he has published statistical data comparing the degree of success of this type of treatment with the published figures of psychoanalytic treatment, showing that behaviour therapy is not only very much shorter than psychoanalysis, but is also very much more successful; with an average of less than thirty visits, he reports successes in some ninety per cent of all cases. Not too much should be made, of course, of statistics of this type, because of the well-known difficulties attending all such comparisons. Nevertheless, the experience of others who have used similar techniques bears out Wolpe's contention that here we have at long last a theory and a method which do enable us, which psychoanalysis never did, to come to grips with the widespread neurotic fears and anxieties which are so characteristic of our time, to understand them and to cure them. Psychoanalysis has survived for so long, in spite of its continued failure to provide a successful method of cure, because *natura abhorret vacuum*. As long as no alternative theory was available which could account for the facts of neurotic disorders, and which could suggest new and successful methods of treating these disorders, so long was psychoanalysis in a safe and impregnable position. Its scientific and philosophical pretensions have long since been stripped away, and it has survived largely through inertia and through the large body of vested interests which have grown up in its wake. It is unlikely that the Emperor's new clothes will be admired for very much longer.

NOTES

- ¹ It is, of course, impossible in a short paper like this to document one's statements sufficiently to carry conviction. The reader who is interested in a thorough review of the facts and in a detailed list of references, will find these in the *Handbook of Abnormal Psychology* (Pitman, 1960) which I have edited. Of particular relevance is the chapter on "The Effects of Psychotherapy".
- ² Space does not permit to deal adequately with the behaviourist's interpretation of neurotic disorders, and the description of behaviour therapy. The reader who is interested in more extensive documentation, may be directed to the present writer's *Behaviour Therapy and the Neuroses* (Pergamon Press, 1960) which contains a very full account, both of the theory of behaviour therapy, and also of large numbers of empirical studies using the concepts and methods of modern learning theory for the purpose of treatment of various neurotic symptoms.