Salzinger, K., and S. Pisoni, 1958. "Reinforcement of Affect Responses of Schizophrenics During the Clinical Interview." *Journal of Abnormal and Social Psychology 57:* 84–90.
———. 1961. "Some Parameters of the Conditioning of Verbal Affect Responses in Schizophrenic Subjects." *Journal of Abnormal and Social Psychology 63:* 511–16.
von Wright, G. H. 1971. *Explanation and Understanding.* Ithaca: Cornell University Press.
Wilkes, K. 1990. "Analyzing Freud." *Philosophical Quarterly 40:* 241–54.
Wollheim, R. 1991. "Desire, Belief and Professor Grünbaum's Freud." Paper read at the University of Miami, Coral Gables, Florida.

17

How Freud Left Science

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Sigmund Freud is remembered as the founder of the psychoanalytic movement. He is not usually remembered as an early computational psychologist or as a cognitivist philosopher of mind, but that is how he began. The historical claim is meant seriously. Freud was a cognitive scientist, with a clearly computational theory of mind and its functioning, and that understanding of his early work is amply justified by his texts. But the picture leaves a kind of historical puzzle: How did Freud the computational mental physiologist become something so apparently different, the founder and leader of the psychoanalytic movement? How did Freud the positivist become the leader of a movement widely regarded today as a marginal science, and regarded by many as a full-fledged pseudoscience?

Answering these questions must lead inevitably to a kind of debunking. Freud has been debunked often enough, and there is little need for another performance. There is some amusement to be had in considering recent silly accounts of Freud's intellectual transition, but that is not itself enough reason for the effort. There is a better reason for considering these questions. If we view Freud as a contemporary, as a cognitive scientist just like many among us, the character of his intellectual career may serve as a warning as to how easy it is to give up the search for truth, and as to how fragile is the pursuit of knowledge. Freud is a moral lesson.

The brief story is this: Freud began with a conception of science he had learned from the physiologists, and he attempted to apply it in a clinical setting. Together with a strong prior disposition for sexual etiologies, and a massive insensitivity to experimenter effects in clinical work, those scientific methods led Freud to publish two complementary accounts of the etiology of the neuroses. When, not long after their publication, Freud came to doubt one of those theories, he could not help but also doubt the reliability of his clinical procedures, and he did so. His professional reputation and his income depended upon the reliability of the clinical methods of which he had repeatedly boasted in lectures and publications. The result was a crisis of confidence, half-hearted attempts to detect experimenter effects in the analytic hour, and a prolonged period of equivocating and dissembling about his own claims regarding the neuroses. The resolution of the intellectual part of this crisis was the real genesis of what is popularly understood as psychoanalysis, and it was marked by the publication of *The Interpretation of Dreams* (1953–1974, vol. 4, 1–338; vol. 5, 339–627). Freud submerged all doubts about experimenter effects, claimed extended powers of interpretation for himself and those who practiced his procedures, and replaced the scientific methods he had learned from the neurophysiologists by a kind of formal caricature of them. This is not simply to say that Freud was a scientist before 1900 and a mountebank thereafter, but it is a history that shows a decline in rigor and candor. Freud never fully abandoned the conception of science he had learned in the 1880s, but it became increasingly hollow.

The Discovery of Sex

In 1887, after visiting Paris and working with Charcot, and a stint of hospital work in Vienna, Freud set up shop as a neurologist in private practice. From Charcot and from Breuer he had become interested in neurotic phenomena, especially hysteria. He had from the very beginning a strong inclination to see all neurotic phenomena as disorders of *sexual functioning*. There was nothing unusual in that attitude, save possibly Freud's tendency to look for sexual etiologies exclusively. This was, after all, the time of Krafft-Ebing and Havelock Ellis, and sex was very much in medical fashion. It is worth observing in passing some false historical accounts, including Freud's own.

Freud himself claimed that he came to the hypothesis of sexual etiologies very reluctantly under the force of overwhelming clinical evidence. That claim is reiterated by various biographers and disciples, including Ernest Jones (1953). The interest of the claim is apparent: Freud's evidence is not available to us, and if we suppose him rational and suppose him originally disinclined to sexual etiologies, then it is a reasonable surmise that if he was driven to sexual explanations the evidence for them must have been very convincing. Richard Wollheim offers the most remarkable argument for this account of things. Wollheim writes that in Freud's paper on the neurophychoses of defense, published in 1894:

> the importance of sexuality is newfound. Originally, under the influence of Charcot, Freud too had depreciated sexuality as an influential factor. In an encyclopedia article on hysteria of 1888, he had written "As regards what is often asserted to be the preponderant influence of abnormalities in the sexual sphere upon the development of hysteria, it must be said that its importance is as a rule over-estimated." Freud's views changed slowly, very much against the grain, under two powerful influences. First, there was the study of the "actual neuroses"—that is, psychic disorders entirely attributable to current or ongoing factors. . . . The other powerful influence in bringing Freud around to a new estimate of sexuality was his clinical practice in the various psychoneuroses, for he increasingly found that the trail of free association, in which he now put his trust, ended in sexual ideas. (1971, 19–20)

Wollheim's text-cropping is world-class. What Freud[1] actually published early in 1888, only a few months after beginning private practice is this:

> As regards what is often asserted to be the preponderant influence of abnormalities in the sexual sphere upon the development of hysteria, it must be said that its importance is as a rule over-estimated. In the first place, hysteria is found in sexually immature girls and boys, just as, too, the neurosis with all its characteristics also occurs in the male sex, although a great deal more rarely (1:20). Furthermore, hysteria has been observed in women with a complete lack of genitalia, and every physician will have seen a number of cases of hysteria in women whose genitals exhibited no anatomical changes at all, just as, on the contrary the majority of women with diseases of the sexual organs do not suffer from hysteria. *It must, however be admitted that conditions related functionally to sexual life play a great part in the aetiology of hysteria (as of all neuroses),* and they do so on account of the high psychical significance of this function especially in the female sex. (1953–1974, vol. 1, 50–51; emphasis added)

I think no one honest can doubt that Freud's disposition towards sexual etiologies preceded the development of psychoanalytic theory and the free association technique.

Freud was ambitious. His correspondence with Wilhelm Fliess in the 1890s fairly smells of scientific ambition. His book on aphasia had not been any great success, and *Studies on Hysteria* (1953–1974, vol. 3), while well received, was regarded as essentially a restatement of Pierre Janet's theories of neurosis. Both Janet and Freud described it that way. To make his mark, Freud needed something that was distinctly his own. He had developed original clinical techniques by the middle of the decade, but they were not the sort of psychological or medical hypothesis that would be noticed unless special results could be claimed from them. In 1895 and 1896 Freud announced such results in a series of papers on the neuroses. These papers, on the "actual neuroses" and on the "seduction theory" of the psychoneuroses, mark his first original psychological hypotheses.

Freud's paper on the "actual neuroses" distinguished two syndromes. One, neurasthenia, was a familiar diagnostic category. Neurasthenics were characterized by fatigue and irritability. The other disorder, anxiety neurosis, was a Freudian novelty. Anxiety neurotics suffer diffuse anxiety with no specific fear. Freud claimed that neurasthenia is caused by masturbation; when masturbation ceases, so will neurasthenia. Anxiety neurosis is caused by unsatisfactory sexual fulfillment, which for Freud included celibacy, withdrawal, the use of condoms, and other "sexual noxae." When the afflicted took up a "normal" sexual life, anxiety neurosis would disappear. Freud's hypotheses about the actual neuroses were contested by a number of physicians, and in print by Lowenfeld, who claimed to have a number of patients who suffered the symptoms of one or another of these disorders, but who very probably had normal sexual lives. Freud's defense was that apparent counterexamples to his theory were in all probability misdiagnosed cases of *psychoneuroses*, hysterics in particular. Psychoneuroses had an entirely different etiology and mechanism. Psychoneurotics, Freud claimed, could be reliably diagnosed by the method of psychoanalysis.

Freud published his account of the causes of psychoneuroses in 1896. He dealt principally with two disorders, hysteria and obsessional neurosis. Each, he claimed, was caused by a sexual assault in early childhood. Hysteria and obsessions were the consequences of

sexual child abuse. The difference between the disorders is that hysterics, after their first sexual contact, became sexually passive; they did not seek out further sexual contacts. Obsessives, in contrast, became sexually aggressive and sought further sexual encounters even in early childhood.

Surrendering the Seduction Theory

So by 1896 Freud had announced two very simple but startling hypotheses: neurasthenics and anxiety neurotics always suffer from concurrent sexual dysfunctions; hysterics and obsessives have always been seduced as children. The first hypothesis depended on the second, which had been established by psychoanalytic procedures, because psychoanalytic procedures, Freud claimed, showed that apparent counterexamples to his theory of the actual neuroses were misclassified. In the fall of 1897, in his correspondence with his friend Wilhelm Fliess, Freud announced that he no longer believed his theory of the psychoneuroses, and that he had concluded that the tales of seduction that he received from his patients were not generally true. This period of Freud's life is perhaps the most controversial. There are two principal accounts of what happened, neither of which is true.

One account holds that Freud correctly realized that the reports of seductions he heard from his patients were not correct, and then correctly inferred that they were "fantasies" produced by the patient's neurotic condition, and that these fantasies themselves were part of the cause of neurosis. This is the account of matters that Freud himself published much later in his life, but the second part of the account does not square with the historical facts. An alternative account, proposed by Jeffrey Masson (1984), is that Freud knew that the reports of seduction were in fact veridical, but renounced them for two reasons. First, because his theory was very unpopular with his medical colleagues and he wished to regain respectability; second, because he wished to make one of his patients and students into a kind of liar. The first of Masson's reasons is unconvincing; the second is incoherent and at points hilarious.

I wish to tell a different story. I think Freud came genuinely to suspect that the seduction tales were not correct; at the time he was not under great professional pressure, although his friend Fliess was. I think that Freud suspected that his psychoanalytic methods were un-

reliable and that the seduction tales were the result of his own suggestions to his patients. He tried, rather equivocally, to test this hypothesis, and came to reject it. He had every interest in rejecting the idea that his methods were unreliable, for he had repeatedly boasted of them in print and in public lectures, and he knew full well that he, like Fliess, would become a laughingstock if he now publicly admitted that his methods were suspect. Freud had every reason to believe that his methods were responsible for producing unreliable testimony from his patients, for the methods were extraordinarily directive. Freud's rejection of the idea that his methods were unreliable was a kind of self-deception, and it was accompanied by a public deception. In the late 1890s Freud did not know whether his seduction theory was true or not, but while he published arguments about the causes of hysteria, he did not disclaim his theory, or state any of his doubts. I think he did not because he feared, rightly, that such an admission would cast doubt not just on his seduction theory, but also on his methods. When, *nine years* after the letter to Fliess admitting his doubts, he published a renunciation of the seduction theory, he did so in the context of misrepresenting in an essential way the content of that theory and the history of his claims. In the meanwhile, he sought for some alternative etiology for psychoneurosis. Contrary to the first conventional history—Freud's own—he did *not* directly replace the seduction theory by the hypothesis that childhood fantasies of sexual intercourse could later cause neurosis in adulthood. Instead, he first supposed that the tales of seduction were displacements of memories of real events that took place in adolescence. Subsequently he supposed that sexual trauma in childhood was in fact the real cause of neurosis in adulthood, but the trauma could be self-imposed, generally in the form of early masturbation. Only in 1909, in the Rat Man case, did he introduce the hypothesis that childhood fantasies of sexual encounters could be etiological factors. He did so in order to save his account of psychosexual development from the counterexample that the case study seemed to pose. By that time the psychoanalytic movement was well established, his early theory long since forgotten, and Freud could safely give up a hitherto central pillar without bringing down the theoretical house.

The story is entangled with Freud's friendship with Wilhelm Fliess. They became acquainted in 1887, and formed a friendship that lasted until after the turn of the century. Freud sent to Fliess his personal

news, drafts of manuscripts, long letters discussing his hypotheses and doubts, professional news, and responses to Fliess's own work. Fliess was a Berlin nose and throat specialist. He was also a crank, and Freud became devoted to most of his crankery. Fliess held that the nose was the organ responsible for neurosis, or for at least a widespread kind of neurosis, the "nasal reflex neurosis" as he called it. Parts of the nose affected parts of the body, and difficulties with parts of the nose caused neurotic symptoms that affected the corresponding parts of the body. Recommended procedure was to treat the offending nasal part with cocaine, or in extreme cases to perform surgery on the nose. Fliess liked to remove the turbinal bone.

Freud not only endorsed the theory in word, he endorsed it in practice, and Fliess operated on Freud's nose. Fliess is one source of a contemporary crankery, "reflexology," which differs only in holding the feet rather than the nose responsible, and in generalizing the scope of afflictions accounted for. Fliess was also the inventor of biorhythms. He held that all people were subject to two cycles, a female cycle of 28 days and a male cycle of 23 days, and these cycles determined their mental and physical health. Freud calculated lots of cycles.

Frank Sulloway (1983) has argued that Fliess was not a crank but a man of his time. No doubt he was something of both. Certainly he was widely regarded as a crank, and the views he developed in several books were the subject of considerable ridicule and criticism. A review of his book on the nasal reflex neurosis, for example, concluded that it was a disgrace from which the otherwise estimable publishing house would no doubt recover. Freud reported angrily to Fliess in 1897 that friends of Breuer's were busy applying probability theory to Fliess's published data, and claiming that the coincidences Fliess found were due to chance. Any criticism of Fliess, no matter how well posed, offended and angered Freud.

Freud's personal life was troubled in these years. His last child was born in 1896, after which he ceased (according to his letters to Fliess) having sexual relations with his wife. His father died in the same year, and the death came increasingly to distress Freud. He detected symptoms of hysteria in himself and began to treat himself through "self-analysis." He was addicted to cocaine in this period and he suffered various symptoms of heart disorder which he may have counted among his hysterical symptoms. His mood in his letters alternates between depression and buoyancy. His professional reputation was not,

however, in any difficulty. Certainly his views were controversial and not widely accepted, but there is no evidence that he was, like Fliess, widely regarded as a crank. Freud had formed a hatred for his former collaborator and patron, Josef Breuer, to whom he owed a considerable sum of money in repayment for loans Breuer had made to him while he was a medical student. Breuer in turn did what he could to continue to promote Freud's career. In November of 1895, Breuer lectured to the College of Physicians defending Freud's general approach and arguing against the claim that Freud's results were due to suggestion. Breuer continued to send patients to Freud, as did other eminences, such as Carl Wernicke. In May of 1896 Freud wrote to Fliess that he was isolated, "Word was given out to abandon me, for a void is forming all around me . . . this year for the first time my consulting room is empty" (Masson 1985, 185). But in November Wernicke sent him a patient. In February of 1897 Freud wrote to Fliess that Freud was the only one in Vienna who believes Fliess's series, but in the same letter Freud revealed that Nothnagel and Krafft-Ebing had nominated Freud for a professorship at the University of Vienna. He wrote in May of 1897 that he had several new students, including a physician who had come from Berlin to study with him. At the end of May of 1897, he remarked that Breuer had sent him a new patient. Freud was not universally celebrated, and no doubt he had his professional difficulties at the time, but he was respected by some of the most distinguished medical people in Vienna. If anyone had real troubles with his reputation, it was Fliess.

Reputation is one thing, clinical success another. There is reason to think that Freud was not having much of the latter. It is difficult to count cases from publications and correspondence, and Freud did not do any systematic follow up of his patients. In 1892 Freud published an account of a successful treatment of an hysterical patient by hypnosis. There are four of Freud's cases discussed in *Studies on Hysteria,* but one of them consisted of a single informal session. Of the three remaining cases, Freud believed two resulted in cures. In *Obsessions and Phobias* (1953–1974, vol. 3, 69–84), published in 1895, Freud mentioned eleven cases, perhaps eight of them distinct, and explicitly remarked that in two of the cases cures resulted. Presumably, the others did not although one cannot be sure. His correspondence with Fliess in the middle of the 1890s seems to indicate that many of Freud's cases did not go well.

Freud's two theories were announced in several papers in 1895 and 1896 and the theory of the actual neuroses was discussed again in 1898. The development of his views can be traced through his correspondence with Fliess in those years and, beginning in 1897, we can also find Freud indicating that he had doubts about the theory. The doubts came to a head in a letter written on 21 September 1897:

> And now I want to confide in you immediately the great secret that has been slowly dawning on me in the last few months. I no longer believe in my *neurotica.* This is probably not intelligible without an explanation; after all, you yourself found credible what I was able to tell you. So I will begin historically where the reasons for disbelief came from. The continual disappointment in my efforts to bring a single analysis to a real conclusion; the running away of people who for a period of time had been most gripped; the absence of the complete successes on which I had counted; the possibility of explaining to myself the partial successes in other ways, in the usual fashion—this was the first group. Then the surprise that in all cases, the *father,* not excluding my own, had to be accused of being perverse—the realization of the unexpected frequency of hysteria, with precisely the same conditions prevailing in each, whereas surely such widespread perversions against children are not very probable. The [incidence] of perversion would have to be immeasurably more frequent than the hysteria because the illness, after all, occurs only where there has been an accumulation of events, and there is a contributory factor that weakens the defense. Then, third, the certain insight that there are no indications of reality in the unconscious, so that one can cannot distinguish between truth and fiction that has been cathected with affect. (Accordingly there would remain the solution that the sexual fantasy invariably seizes upon the theme of the parents.) Fourth, the consideration that in the most deep-reading psychosis the unconscious memory does not break through, so that the secret of childhood experiences is not disclosed even in the most confused delirium. If one thus sees that the unconscious never overcomes the resistance of the conscious, the expectation that in treatment the opposite is bound to happen, to the point where the unconscious is completely tamed by the conscious, also diminishes.
>
> I was so far influenced that I was ready to give up two things; the complete resolution of a neurosis and the certain knowledge of its etiology in childhood. Now I have no idea of where I stand because I haven not succeeded in gaining a theoretical understanding of repression and its interplay of forces. It seems once again arguable that only later experiences give the impetus to fantasies which [then] hark back to childhood, and with this the factor of a hereditary disposition regains a sphere of influence from which I had made it my task to dislodge it—in the interest of illuminating neurosis.

If I were depressed, confused exhausted, such doubts would surely have to be interpreted as signs of weakness. Since I am in an opposite state, I must recognize them as the result of honest and vigorous intellectual work and must be proud that after going so deep I am still capable of such criticism. . . .

It is strange, too, that no feeling of shame appeared—for which, after all, there could well be occasion. Of course I shall not tell it in Dan, nor speak of it in Askelson, the land of the Philistines, but in your eyes and my own. I have more the feeling of a victory than defeat, (which is surely not right). (Masson 1985, 264–65)

The letter contains five arguments. Freud's therapy is unsuccessful, his patient's flee him and his partial successes can be explained "in the usual fashion"—which means, I take it, as the result of the self-limiting character of the disorders, or placebo effects. Second, it is improbable that the seduction of children is as widespread as his hypothesis would require. Third, Freud's own father would have had to have been a seducer, since Freud and his sisters have hysterical symptoms. Fourth, the belief that the recollections of the unconscious are not generally veridical, whether about seduction or about anything else. And finally, that lunatic ravings do not include stories of seduction.

The first argument may seem a non sequitur, but Freud was still gripped by the idea of catharsis and by his physiological account of hysteria and repression; he firmly believed that once the hidden causes had been brought to the attention of the patient, and the patient had come to accept them, the formerly unconscious thoughts would no longer threaten to release quantities of excitation that would generate hysterical symptoms. He says later in the letter that while he doubts the seduction theory he does not doubt his psychology. The second argument is odd, because Freud had raised the same objection in the paper in which he first announced the theory and he had there rejected the objection without bothering to argue against it. But we must also bear in mind that Freud was coming to believe that hysterical symptoms were extremely common, if only in minor forms, and therefore that according to his theory a large fraction of the population must be the victims of sexual abuse in childhood.

And finally, note that the first alternative hypothesis Freud considers is not that childhood fantasies of seduction are etiological factors, but that they are displacements from real adolescent experiences

that together with heredity cause adult neurosis. Freud offered the same hypothesis in a letter to Lowenfeld, and Lowenfeld reported it in 1900 in the introduction to a psychiatric textbook.

Masson's History

Masson takes the letter of 1897 to be dishonest. The renunciation of the seduction hypothesis was done, on his account, in order to regain Freud's damaged professional reputation and for personal psychological reasons. I have argued that Freud's professional reputation was not imperiled by his published theories, and if regaining a lost reputation were the aim, one might have expected Freud to renounce the theory publicly, which he did not. In fact, he had drawn at least as much criticism for his theory of the actual neuroses which he endorsed again in a paper in 1898. The psychological reasons Masson offers in explanation of Freud's change of mind are so bizarre and incoherent that it is hard to believe they were meant seriously or attracted any serious notice. No one ever went broke underestimating the American lite intellectual.

Freud had a patient, Emma Eckstein, who also became a disciple. Fliess visited Freud at Christmas of 1894, met Eckstein, and suggested to Freud that she suffered a nasal reflex neurosis and should be operated on. In February of 1895 Fliess returned to Vienna and performed the operation, which he botched. In the days that followed Eckstein suffered severe hemorrhaging, her nose became infected, and she very nearly died. Freud called in two Viennese surgeons who discovered that Fliess had left in the cavity more than half a meter of gauze, which had promoted infection. She continued to be ill for several months with intermittent hemorrhaging. Freud sent Fliess accounts of her health in March and April. By the end of May she had recovered.

What has this to do with Freud's renunciation of the seduction theory? According to Masson, what happened is this. Freud needed to blame Eckstein for her bleeding, so that his friend Fliess would be free of responsibility for the consequences of the botched surgery. Since she was an hysteric, the bleeding must be hysterical, and Freud suggested as much in his letters. Since Freud believed she was an hysteric, he believed she had been seduced in childhood. But it was better that Eckstein be viewed as a liar than a victim. Hence Freud abandoned the seduction theory.

This is pretty loony. It makes no sense as a psychological story. If Freud wanted to regard Eckstein's bleeding as hysterical and believed she had been seduced, then he had only to keep to his theory. Neither does it make sense of history. Freud's first suggestion of the seduction theory is in correspondence with Fliess in October of 1895, well after Eckstein's recovery, and the seduction theory was not published until May of 1896. Freud had no need to renounce the seduction theory to make Eckstein out a liar; he need only have not published the theory.

Doubt

I take Freud's criticism of his own theory in his letters to Fliess more or less at face value. It is hard to imagine what evidence as to his state of mind could defeat their testimony. What then did the recognition of the falsity of the seduction theory mean for Freud's conception of his own work? I think Freud's immediate and foremost concern was that his psychoanalytic method was untrustworthy. That, of course, would have been a real disaster, for Freud's career and whatever research he could claim to do were premised on the assumptions that the responses he got from his patients were not the effects of suggestion by the analyst, and that Freud could discriminate between veridical and false reports. His conclusions about the seduction theory would undermine any reasonable person's faith in these assumptions and that, I believe, is the principal reason Freud did not publicly acknowledge the difficulties he found with the theory.

Freud had good reason to be worried. His clinical methods were almost dictatorial, and he had repeatedly announced in print that they could not possible lead to false reports from patients. The method Freud used in the cases described in *Studies on Hysteria* included Janet's suggestion therapy. Janet's procedure was to hypnotize the patient and induce the patient to recall some traumatic event associated with a symptom. Then Janet would suggest to the hypnotized patient that the traumatic event had not occurred, or that it was in fact harmless and not frightening. Freud used hypnotic suggestion therapy on Frau Emmy Von N. One might well think that the power of suggestion under hypnosis might have given Freud reason to ponder the power of suggestion when the patient is not under hypnosis. Breuer, in his contribution to *Studies on Hysteria*, worried about the matter; Freud was more forceful:

> We learn with astonishment from this that *we are not in a position to force anything on the patient about the things of which he is ostensibly ignorant or to influence the products of the analysis by arousing an expectation.* I have never once succeeded, by foretelling something, in altering or falsifying the reproduction of memories or the connection of events; for if I had, it would inevitably have been betrayed by some contradiction in the material. If something turned out as I had foretold, it was invariably proved by a great number of unimpeachable reminiscences that I had done no more than guess right. We need not be afraid therefore, of telling the patient what we think his next connection of thought is going to be. It will do no harm. (1953–1974, vol. 3, 295)

The smugness this passage reveals continued until 1897, and so did Freud's practice of leading his patients and pressing them until they produced the answers Freud sought. His clinical method did change. By about 1894 Freud had ceased hypnosis and begun instead to press his hand against the forehead of the patient and ask the patient to recall relevant events or to associate freely, saying whatever might pass through one's mind. Just as with hypnosis, the procedure left the analyst with great scope for leading the patient and for pressing until the patient produced the expected response. Freud did both, and had no qualms:

> If the memory which we have uncovered does not answer our expectations, it may be that we ought to pursue the same path a little further; perhaps behind the first traumatic science there may be concealed a second which satisfies our requirements better and whose reproduction has a greater therapeutic effect. . . . Well, Gentlemen, this supposition is correct. If the first discovered scene is unsatisfactory, we will tell our patient that this experience explains nothing, but that behind it must be hidden a more significant earlier experience. (Ibid., 195)

And from the same period, 1896, in a paper on the seduction theory, he writes:

> Doubts about the genuineness of the infantile sexual scenes can, however, be deprived of their force here and now by more than one argument. In the first place, the behavior of patients while they are reproducing these infantile experiences in every respect incompatible with the assumption that these scenes are anything else than a reality which is being felt with distress and reproduced with the greatest reluctance. Before they come for analysis the patients know nothing about these scenes. They are indignant as a rule if we warn them that such scenes are going to emerge. Only the strongest compulsion of the treatment can induce them to embark on a reproduction of them. (Ibid., 204)

These passages show how heavy Freud's hand must have been and they also show why, when he came to doubt the veracity of the seduction reports his patients gave him, he must have begun to doubt his clinical procedures. The passages also show why Freud was reluctant to publish his doubts. Any of his contemporaries who were aware of these statements—and there were many—would likely have taken a retraction of the seduction theory as evidence that Freud's enterprise was premised on an unreliable procedure, and that Freud himself was insufficiently sensitive to the dangers of his own methods.

Almost immediately after his letter of 21 September 1897, Freud determined to conduct an experiment to test the possibility that the seduction reports were the effects of suggestion. It was just the sort of question he was not intellectually suited to address. Freud had never had any luck with experiments or much understanding of experimental design or any sensitivity to problems of experimenter effects. The same limitations of scientific temper bounded his attempt to investigate the role of suggestion in psychoanalysis. Freud understood how to get examples and counterexamples to universal generalizations but he had only the most general idea about how to test the hypothesis that some or many of the seduction reports he received were due, not to the patients history, but to his methods.

The experiment Freud conducted was to have the now recovered Emma Eckstein, who had moved from patient to pupil, treat a new patient without mentioning the possibility of seduction. On 12 December 1897, Freud wrote to Fliess that

> My confidence in paternal etiology has risen greatly. Eckstein deliberately treated her patient in such a manner as not to give her the slightest hint of what would emerge from the unconscious and in the process obtained from her, among other things, the identical scenes with the father. Incidentally, the young girl is doing very well. (Masson 1985, 286)

Eckstein, of course, knew what outcome was wanted, and Freud did not consider whether she may have influenced the patient inadvertently. Nor did he consider that *this* patient may actually have been seduced while many others who give similar reports were not.

We are left to wonder whether Freud was genuinely convinced that his procedures were reliable; perhaps there is no truth to the matter. What emerged in the following years was a pattern of discussion of the neuroses that is inconsistent with such a conviction, and a kind

of dissembling about his own views. It can be explained, as Masson prefers, as a deliberate dishonesty about the fact of seduction and a gradual withdrawal from the seduction theory in order to gain respectability. It can also be explained as a kind of deliberate dishonesty about the reliability of his clinical methods. I prefer to understand it as a kind of self-deception, carried out under the most powerful of motives and aided by the limitations of Freud's scientific capacities and temper.

Dissembling

After 1896, Freud never again endorsed the seduction theory in print, although he did not publicly withdraw it for another decade. In his correspondence through 1897 and 1898, he described one infantile sexual attack, which he took to be real. By March of 1898 he had begun to reformulate the sexual etiology of psychoneurosis: What is experienced sexually in childhood produces the psychoneuroses. But "experienced sexually" is broader than seduction by an adult and may include masturbation, which he increasingly emphasized in his letters. Toward the end of March of 1898 he wrote to Fliess that he was "completely disoriented" (Masson 1985, 304) about hysteria, and in April he wrote that he did not care to lecture on hysteria "because I lack the decisive verdict on two important points" (ibid., 307).

It is difficult to understand how after 1897 Freud could have continued in private to doubt his seduction theory without also privately doubting his methods, but in print and in correspondence he was if anything more confident and as insensitive as before to the role of suggestion or to the thought that the analyst might sometimes be deceiving himself. After the Eckstein experiment the Fliess correspondence contains no more doubts about clinical method on Freud's part. In 1898 in an article on sexual etiology of the neuroses devoted entirely to the actual neuroses, Freud wrote:

> Having diagnosed a case of neurasthenic neurosis with certainty and having classified its symptoms correctly, we are in a position to translate the symptomatology into aetiology; and we may then boldly demand confirmation of our suspicions from the patient. We must not be led astray by initial denials. If we keep firmly to what we have inferred, we shall in the end conquer every resistance by emphasizing the unshakeable nature of our convictions. . . .

Moreover, the idea that one might, by one's insistence cause a patient who is physically normal to accuse himself falsely of sexual misdemeanors—such an idea may safely be disregarded as an imaginary danger. (1953–1974, vol. 3, 269)

Not until 1905 do we find a renewed discussion of the causes of psychoneurosis, and even then Freud's account is equivocal. In *Three Essays on Sexuality* (1953–1974, vol. 3) he addresses the role of infantile sexuality on neurosis. It is striking that he still failed to mention any doubts as to the veracity of the seduction accounts his patients provided, still held that seduction is a principal cause of neurosis, but now allowed that seduction is no longer a specific cause. The role of seduction is to generate premature sexual activity, but if that happens by some other means, neurosis can still result:

The reappearance of sexual activity [in children] is determined by internal causes and external contingencies both of which can be guessed in cases of neurotic illness from the form taken by their symptoms and can be discovered with certainty by psychoanalytic investigation. I shall have to speak presently of the internal causes; great and lasting importance attaches at this period to the accidental *external* contingencies. In the foreground we find the effects of seduction, which treats a child as a sexual object prematurely and teaches him, in highly emotional circumstances, how to obtain satisfaction from his genital zones, a satisfaction which he is then usually obliged to repeat again and again by masturbation. I cannot admit that in my paper on "The Aetiology of Hysteria" I exaggerated the frequency or importance of that influence, though I did not then know that persons who remain normal may have had the same experiences in their childhood, and though I consequently overrated the importance of seduction in comparison with the factors of sexual constitution and development. Obviously, seduction is not required in order to arouse a child's sexual life; that can also come about spontaneously from internal causes. (Ibid., vol. 7, 190)

If Freud's aim in giving up the seduction theory was to reingratiate himself with Viennese morality, this new modification of the theory would not have succeeded much. Note also that the account does not in the least suggest that Freud misjudged the truth value of the reports his patients gave to him. Further, Freud's claim that in 1896 he did not realize that people who had been seduced need not become hysteric is false; the contrary claim is emphasized in his 1896 paper. The account in this passage seems disingenuous.

In 1906 Freud gave the following equivocal account of the development of his views. Speaking of 1896, he wrote:

At that time my material was still scanty, and it happened by chance to include a disproportionately large number of cases in which sexual seduction by an adult or by older children played the chief part in the history of the patient's childhood.

I thus over-estimated the frequency of such events (though in other respects they were not open to doubt). (Ibid., 274)

The parenthetical remark is just false, and represents in all probability a deliberate falsehood on Freud's part. Freud continued:

Moreover, I was at that period unable to distinguish with certainty between falsifications made by hysterics in their memories of childhood and traces of real events. Since then I have learned to explain a number of phantasies of seduction as attempts at fending off memories of the subject's own sexual activity (infantile masturbation). (Ibid.)

This passage contradicts the preceding one. The recollections of seduction cannot both have been "not subject to doubt" and also "phantasies" that were "attempts at fending off the subject's own sexual activity." Nor does he tell the reader how it is that he is now able to distinguish with certainty between truth and falsity in the recollections of his patients.

The whole business is distasteful, and it appears Freud could not bring himself to tell an honest story. These passages help persuade me that Freud was principally concerned with concealing his doubts about his clinical methods, and much less concerned with saving the seduction theory.

The Interpretation of Dreams

In the letter of 21 September 1897 in which Freud told Fliess that he no longer believed in his own seduction theory, there occurs the following paragraph; "I have to add one more thing. In this collapse of everything valuable, the psychological alone has remained untouched. The dream stands entirely secure and my beginnings of the metapsychological work have only grown in my estimation. It is a pity that one cannot make a living, for instance, on dream interpretation!" (Masson 1985, 266). Freud found a way to make a living on dream interpretation.

Freud's theory of dreams began with his *Project for a Scientific Psychology,* which is what is referred to in this passage as the "meta-

psychology." Freud included a section on dreams in the *Project,* and he claimed there that normal dreams, at least, are always wish fulfillments. He seems to have reached this conclusion entirely on theoretical grounds. The reasoning in the *Project* is roughly the following.

The psyche is run by the distribution and fluctuations of Q in the nervous system. We can distinguish two kinds of processes in the system, the "primary process" consisting of endogenously determined impulses that seek for the immediate decrease of Q, and the "secondary processes" that serve as a control structure permitting short run increases of Q in order to obtain a lower long run or average quantity of Q. The secondary processes are occasioned by the distributions of Q that Freud calls the Ego; they are brought about by experience and they are associated with conscious thought, deliberation, and belief. Objects of thoughts or images are collections of cathected neurones. They can have two sorts of cathexes, either wishful or painful. The cathexes of the primary process are always wishful; without the Ego, we are systems that desire. In sleep the cathexes of the Ego are nearly impotent and the primary processes rage mostly unhindered, save that they are somehow disconnected from motor nerves. Thus in sleep the primary process is free to activate wishful cathexes, and the wishful cathexis, sufficiently energized, is just a dream. In dreaming we do not consciously *think* of what is wished for; instead the very object of the wish appears to us as an hallucination. Freud was uncertain how or why in dreams the object of the wish becomes conscious—or at least present to the Ego—and he was also uncertain as to why the thought of the wish often remains unconscious, although he suggested protomechanisms in terms of neurones and their cathexes.

Throughout 1896 and 1897 Freud occasionally reported and interpreted to Fliess his own dreams or those of others. Sometime in this period he determined to write a book on the interpretation of dreams and began searching the literature and collecting material. In this period Freud also began trying to treat himself and analyzing his dreams was evidently part of the regimen. In March of 1898 he sent Fliess a draft of a chapter of the book on dreams and a list of titles for later chapters. Work on the book became for Freud a way of not thinking about the problems he had had with the psychoneuroses and with his clinical methods; "The habit of working on the dream [book] is—after the hysteria torture—extremely good for me" (Masson 1985, 314).

If Freud took the conclusion of the seduction episode to be that his interpretative methods were unreliable, then *The Interpretation of Dreams* would have been impossible. Equally, if he had remained wedded to the standards of argument he had learned from Brucke and Brentano, the book would have been impossible. The book was published in November of 1899, and its publication signaled that part of Freud had given up the scientific spirit.

The Interpretation of Dreams is a long, complicated and perhaps bewildering book. Despite its title, it is a grand psychology with large aims. The book has a structure, an argument, and a lot of speculation. I think the book represents a kind of revision of Freud's conception of science, a revision that resulted from his own self-analysis to be sure, but also a revision that represents the resolution of his doubts about his clinical methods. Freud had decided not to air his doubts about his clinical procedures and about his ability to discriminate between true and false reports from his patients. He retained the general Millian conception of science with which he had begun his scientific life, but his *Project* and his own self-analysis led him to an interest in dreams and to substantiating his conviction that dreams are wish fulfillments. His hypotheses about dreams as wish fulfillments could only be substantiated by cases, by examples, and since dreams do not generally *state* a wish but only, according to his hypothesis, *show* or hallucinate its fulfillment, the hypothesis could only be established by somehow detecting the wish, the desire and intent, hidden in the hallucination. Freud became convinced that by a combination of common sense, psychoanalytic hypotheses, and sheer intuition he could find the wish behind each dream. The collection of instances and examples needed to substantiate that dreams are wish fulfillments and to support his many other hypotheses about their structure had to be obtained by extending the capacities of the psychoanalytic researcher. Freud would no longer merely extract confessions and memories; he would infer intent. The inferences were not to be based on generalizations that determine the specific meanings Freud found. Instead these were to be established by a kind of extension of everyday psychology. He came to view the devices of conversation and of the theater as public versions of the private devices that direct the theater of the mind. We are accustomed to puns, to irony, to innuendo, to sarcasm, to parable and parody, to deception, to simile, to metaphor, and we routinely infer a meaning in others' words

besides the most literal. Freud would do the same for the reports of dreams, and the meanings he found would then provide the Millian instances for generalizations about dreams and their formation.

The Interpretation of Dreams divides into four parts. It begins with a survey and criticism of the literature on dreams and articulates standards for a theory of the subject. Freud then describes and attempts to illustrate and argue for his procedures for dream interpretation. The largest part of the book is the development and exemplification of generalizations about the content of dreams and the mechanisms of representation that occur in them. The final fragment is a description of the general psychological framework of Freud's *Project*, applied to the phenomena of dreams. The book is in some ways deliberately equivocal. Early claims made about terrifying dreams, for example, are taken back or qualified in the final chapters. The neurological basis of the psychology of the *Project* is suppressed and Freud warns against taking the psychological structures he describes as having specific locations in brain autonomy, but he almost immediately after introduces the conjecture that they may be spatially localized, twice lapses into talk of neurones, and writes throughout of "excitation" as a physiological process.

Freud's own announced criteria for a satisfactory theory of dreams were these: (1) the theory ought to remove the appearance that the content of dreams has no determining causes, but it ought not to do so in such a way as to make it causal claims untested or untestable; and (2) a theory that proposes that dreams have a meaning should provide a general procedure for interpreting them, and a warrant that demonstrates the trustworthiness of the procedure.

This may be good sense, but it is good sense by which Freud found it difficult to abide. Freud's account of his procedure for interpreting dreams is this: The analyst starts with a phrase in the dream report, and asks the patient to associate freely, and speak the thoughts that occur to him. When associations stop, or seem to become unproductive, the analyst takes up another piece of the original dream report and asks the patient to associate to that. If themes in a later train of associations seem related to a previously considered element of the dream report, the analyst is free to direct the dreamer's attention back to the previous phrase or association.

The process continues until the meaning of the dream emerges. There are no rules for stopping, or for when to return to a previous topic. Freud does offer some rules of thumb about choosing the initial piece of the dream report for association.

Freud illustrated his method with a dream of his own—the dream of Irma:

> A large hall—numerous guests, whom we were receiving.—Among them was Irma. I at once took her on one side, as thought to answer her letter and to reproach her for not having accepted my 'solution' yet. I said to her: "If you still get pains, it's really only your fault." She replied: "If you only knew what pains I've got now in my throat and stomach and abdomen—it's choking me"—I was alarmed and looked a her. She looked pale and puffy. I though to myself that after all I must be missing some organic trouble. I took her to the window and looked down her throat, and she showed signs of recalcitrance, like women with artificial dentures. I thought to myself that there was really no need for her to do that.—She then opened her mouth properly and on the right I found a big white patch; at another place I saw extensive whitish grey scabs upon some remarkably curly structure which were evidently modelled on the turbinal bones of the nose.—I at once called in Dr. M., and he repeated the examination and confirmed it. . . . Dr. M. looked quite different from usual; he was very pale, he walked with a limp and his chin was clean-shaven . . . My friend Otto was now standing beside her as well, and my friend Leopold was percussing her through her bodice and saying: "She has a dull area low down on the left." He also indicated that a portion of the skin on the left shoulder was infiltrated. (I noticed this, just as he did, in spite of her dress). . . . M. said: "There's no doubt its an infection, but no matter; dysentery will supervene and the toxin will be eliminated." . . . We were directly aware, too, of the origin of the infection. Not long before, when she was feeling unwell, my friend Otto had given her an injection of a preparation of propyl, propyls . . . propionic acid . . . trimetheylamin (and I saw before me the formula for this printed in heavy type). . . . Injections of that sort ought not to be made so thoughtlessly. . . . And probably the syringe had not been clean. (1953–1974, vol. 4, 107)

Irma was a patient of Freud's who had relapsed; during the evening of the dream, Freud had written out an account of the case to give to Dr. M., as Freud says, "in order to justify myself" (ibid., 106). M., Otto, and Leopold were all physician acquaintances of Freud's, and Otto had mentioned the case to Freud the previous day with, Freud thought, implied reproof. Freud's interpretation of the dream is this:

> The dream fulfilled certain wishes which were started in me by the events of the previous evening (the news given me by Otto and my writing out of the case history.) The conclusion of the dream, that is to say, was that I was not responsible for the persistence of Irma's pains, but that Otto was. Otto had

in fact annoyed me by his remarks about Irma's incomplete cure, and the dream gave me my revenge by throwing the reproach back on to him. The dream acquitted me of the responsibility for Irma's condition by showing that it was due to other factors—it produced a whole series of reasons. The dream represented a particular state of affairs as I should have wished it to be. *Thus its content was the fulfillment of a wish and its motive was a wish.*

This much leapt to the eyes. (Ibid., 118–19)

The procedure of dream interpretation is rather vaguely characterized and the Irma dream does not even accord very well with it—Freud, acting as interpreter and dreamer, mixes association with commentary and there is no clear demarcation between the two. Given the background Freud's interpretation of his dream is perfectly plausible, and that is the trouble. The burden of the discussion is that the *procedure* of dream interpretation is reliable; the example only gives us a particular dream interpretation that is plausible, largely because given the background the dream wears its meaning on its face, and the interpretation does not follow the procedure in any case. We are left to wonder why, in less obvious cases, we should believe Freud's assignments of intent. We are left to wonder whether, if a dream had no meaning, or did not express a wish, one could nonetheless be manufactured by Freud's procedure. We are left to wonder if we could not as easily make every dream express a fear or disgust or any number of other attitudes. Some of Freud's contemporary readers had the same wonders.

Freud gives a one sentence reply to these qualms, "No doubt I shall be met by doubts of the trustworthiness of 'self-analyses' of this kind; and I shall be told that they leave the door open to arbitrary conclusions. In my judgement the situation is in fact more favourable in the case of *self*-observation than in that of other people; at all events we may make the experiment and see how far self-analysis takes us with the interpretation of dreams" (ibid., 105). Lacking some independent access to the meaning of dreams, there is no experiment. Freud proceeds to consider a variety of apparent counterexamples to the thesis that dreams are always wish fulfillments. He distinguishes the manifest content of a dream from the latent content; when the manifest content does not seem to express a wish, the latent content, revealed by interpretation, does so. The details of infantile material in dreams, recent material, distortion, representation in dreams, displacement, condensation, symbolism, typical dreams, absurd dreams, and so on,

are established in much the same way. Many of the interpretations are plausible, many of them interesting, but was the method reliable?

One might charitably compare Freud's technology for dream interpretation with, say, Galileo's telescopic technology. Galileo could not, independently of what was seen through his telescope, establish that Jupiter has moons revolving about it, just as Freud could not independently establish the latent content of dreams. Galileo could only argue that when his telescope was pointed at earthly objects it revealed them enlarged but more or less faithfully. Freud could only argue that his method, when applied to homely obvious dreams, gave the obvious interpretation. The comparison is not apt, for an observer through Galileo's telescope could identify earthly objects without knowledge beforehand of what would appear in the telescopic field, but Freud's method, when applied to homely and obvious cases, was always done with the knowledge, independently of any technique, of the homely and obvious interpretation.

Freud's circumstance in *The Interpretation of Dreams* can usefully be compared with John Dalton's circumstance in his *New System of Chemical Philosophy* (1964). Freud claimed a method for locating the sense of dreams but he lacked any independent access to that sense and thus any means for testing his method. Dalton claimed a method, his rule of simplicity, for determining the atomic composition of a compound substance, but he lacked any independent method of assessing the composition, and so any means of testing his rule. Dalton had the advantage that his rule was at least clear, even if sometimes indeterminate in its application. He had the further advantage that within fifty years his followers had replaced the faulty pieces of his theory with better pieces and produced a theory that led to a wealth of new predictions and robust explanations. In the ninety years since the publication of *The Interpretation of Dreams* Freud has had no such luck.

Dora

Freud did not confine his broadened conception of science to dreams. He applied it to his neurotic patients as well. His case studies near the turn of the century show him at work inferring intent and desire not only from dreams but from all manner of other behavior. The plausibility of Freud's dream interpretations depend heavily on

context, and *The Interpretation of Dreams* gives only the tiniest glimpses of that. The case studies are heftier and offer a larger view. Freud's *Fragment of an Analysis of a Case of Hysteria* (1953–1974, vol. 7, 1–122), the case of Dora, was offered as an illustration of the application of the theory of dreams. It offers more, an illustration of Freud's broadened interpretative methods in and out of dreams. Freud began the case in 1900 and published his account of it in 1905.

Dora was brought to Freud by her father when she was eighteen, suffering from depression, irritability and suicidal impulses. She had previously suffered from incidents in which she had difficulty breathing, chest pains, and occasional loss of voice. Her parents were friends with the family of Herr and Frau K. Her father explained to Freud that two years previously, during a joint vacation, Herr K. had taken Dora for a walk by a lake and had made a sexual proposal to her. He was convinced that her more recent disorders, including a suicide note, were due to that encounter. Herr K. had denied making advances, and had claimed that his wife, Frau K., had told him that the girl was interested only in reading and talking of sexual matters, and these activities had overexcited her.

Dora told Freud a story, which he fully accepted, of a romance between two families. She had been very close to Frau K. and they had spent a great deal of time together. Frau K. and her father were lovers, although her father was impotent. Dora believed Frau K. practiced fellatio with him. Herr K. had in fact approached her twice and had on previous occasion embraced her; she had fled. After the scene by the lake she had tried to avoid him, but found him in her room when she awoke. When she discovered that the key to her room was missing, she demanded to be returned home.

Freud's interpretation of the case is partly an inference about Dora's sexual practice, and more significantly an ascription of desire. Her shortness of breath, gastric distress and other symptoms that had begun around the age of eight are attributed to masturbation as a child. Dora neither admitted nor recollected the practice, but for various reasons Freud was nonetheless confident of the conclusion. The desire is more important. Freud regarded her flight from Herr K.'s embrace as an hysterical reaction, and he ascribed her sensations of pressure against her chest as an hysterical symptom: the manifestation of the memory of Herr K.'s erect penis against her abdomen. According to Freud, despite her claims to the contrary, she was in love

with Herr K. The principle evidence for that conclusion was that she recollected that a friend had made the same remark, and that the first few occurrences of her aphonia corresponded to Herr K.'s absences. (Later attacks apparently had no such connection.) The aphonia was, according to Freud, hysterical speech, a way of saying that with Herr K. gone, no one worth talking to remained. Not only was Dora in love with Herr K., she was also in love, homosexual love, with Frau K. For she had praised Frau K.'s beauty, her "adorable white body," and had spent much of her time with Frau K. The decisive evidence for Freud, however, was that when Herr K. had defended himself with the information about Dora's reading, information obtained from Frau K., Dora had not been angry with Frau K. If further evidence were needed, there was Dora's virtual obsession with her father's relations with Frau K., a "supervalent" train of thought which covered her love for Frau K. and her resentment at Frau K.'s betrayal. But there is more. Not only was Dora in love with Herr K. and with Frau K., she was also in love with her own father. The supervalent train of thought concerning her father's relations with Frau K., and her conscious bitterness towards her father covered an unconscious Oedipal desire:

> Her behaviour obviously went far beyond what would have been appropriate to filial concern. She felt and acted more like a jealous wife—in a way which would have been comprehensible in her mother. By her ultimatum to her father ('either her or me'), by the scenes she used to make, by the suicidal intentions she allowed to transpire,—by all this she was clearly putting herself in her mother's place. If we have rightly guessed the nature of the imaginary sexual situation which underlay her cough, in that phantasy she must have been putting herself in Frau K.'s place. She was therefore identifying herself both with the woman her father had once loved and with the woman he loved now. The inference is obvious that her affection for her father was a much stronger one than she knew or than she would have cared to admit: in fact, that she was in love with him. (1953–1974, vol. 7, 56)

There is of course much more to the case, and for all I *know*, Freud's interpretation of Dora's desire may be correct. But there is a much simpler explanation of her depression, of her suicidal mood, of her anger toward her father, of the absence of comparable anger towards Frau K. or even towards Herr K., and of her insistence that her father break off her relationship with Frau K. Moreover, the essential fact upon which that explanation turns is not in dispute, at least it is not

disputed by Freud. Dora believed, and Freud believed she believed correctly, that her father, the man she most trusted and who was most obliged to protect her interests, had traded her to Herr K. in return for Frau K. Herr K. and her father had struck a deal explicitly or tacitly: Herr K. would not make an issue of her father's affair with his wife, and her father would not make an issue of Herr K.'s attempts to seduce Dora. Dora had been made an object of barter, and she was trapped in that status, cause enough, perhaps, for irritability, depression and a suicidal mood; her bitterness was directed at the responsible party: her father.

Conclusion

There are better case studies than Dora (The Rat Man), and worse (Little Hans), and in any case I was not there to see her and to see Freud with her. But Dora makes me think that Freud had long since forgotten Brentano's warning: Do not settle upon an explanation unless you have excluded all equally good alternatives. I think he had also forgotten, or pushed out of mind, the warning in the failure of his own theory of seduction and neurosis. Caught by his career, limited by his own scientific conception, fascinated by self-revelation, Freud put aside the doubts he did not know how to investigate, and could not endure to have made public. In their place he put a new conviction as to his powers, and a new conception of his science—a conception that made it something else. It is one thing to be faced with scientific problems that one's methods of inquiry are impotent to solve; all but the most fortunate investigators confront that circumstance at some time, whether they recognize it or not. It is another matter to sense such limitations only to hide them from others and, eventually, from oneself. Science is a bit of sociology, a collection of people, traditions, pecking orders, and so on. Freud left the broader scientific community sometime after 1910, in the sense that he no longer published in scientific journals not run by his disciples, and no longer attended scientific meetings other than those organized by analysts. For all the cynicism of sociologists, science is also a system of ideals. In every science, everyone not stupid knows the ideals are different from the practice, but there is an inner community who acknowledges the standards and seeks, so well as it can, to live scientific lives by their command. When this century began, Freud was no longer a scientist in his soul.

NOTES

I thank Patricia Kitcher for her comments on a draft of this essay. She just hated it.

1. This is according to scholarly consensus. The encyclopaedia article is unsigned.

REFERENCES

Dalton, J. 1964. *A New System of Chemical Philosophy.* New York: Philosophical Library.

Freud, S. 1953–1974. *The Standard Edition of the Complete Psychological Works of Sigmund Freud.* Vols. 1–18. Edited by J. Strachey. London: Hogarth Press.

Jones, E. 1953. *The Life and Work of Sigmund Freud.* Vol. 1. New York: Basic Books.

Masson, J. 1984. *The Assault on Truth: Freud's Suppression of the Seduction Theory.* New York: Farrar, Straus, & Giroux.

———. 1985. *The Complete Letters of Sigmund Freud to Wilhelm Fliess, 1887–1904.* Cambridge, MA: Belknap Press of Harvard University Press.

Sulloway, F. 1979. *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend.* New York: Basic Books.

Wollheim, R. 1971. *Sigmund Freud.* New York: Viking Press.