

Freud's Case Studies and the Locus of Psychoanalytic Knowledge (*)

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INTRODUCTION: THE QUESTION OF PSYCHOANALYTIC KNOWLEDGE

Reading through Sigmund Freud's case studies in chronological order is a most instructive experience for anyone interested in the intellectual history of psychoanalysis. One quickly sees, for example, how Freud's technique evolved from the methodical and rather intrusive attempts to dispel particular symptoms that we observe in *Studies on Hysteria* (1895) to the method of free association that he describes in his report on the case of Dora (1905). Or one sees how Freud's conception of the transference developed, from the primitive notion of transference as something to be avoided or dispelled, to the mature conception of transference as the invaluable center of analysis, the very phenomenon that has to be analyzed (again in the analysis of Dora). In this paper I shall review Freud's case studies with still another trajectory in mind: how Freud's sense of the significance of his written case studies themselves evolved. I shall show how his initial embarrassment over the necessity of describing his cases in great detail was eventually supplanted by a confidence that his case studies were important vehicles of psychoanalytic knowledge. As his attitude changed, his initial anxiety about how other medical and natural scientific readers would adjudicate his cases, was displaced by a willingness to assert delicate, tenuous hypotheses in his cases, largely for the benefit of the psychoanalytic community itself. And on a more abstract and perhaps less conscious level, the positivistic belief in the importance of theory and the objective verifiability of hypotheses that Freud had subscribed to from the time he

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had decided to study medicine was at least temporarily challenged by another view of knowledge, a view that I shall try to clarify in this paper.

My examination of Freud's case studies is necessarily an interrogation of only a part of the Freudian *oeuvre*. But it leads inexorably to the question of psychoanalytic knowledge, its locus, character, and epistemological status. For we cannot examine the cases without considering their relationship to Freud's other writing. We cannot help wondering whether Freud wrote his case studies for a specific readership, for example, or whether the cases communicate to their readers in a unique fashion. These questions will lead us to delineate an important fissure in Freud's thinking and to speculate upon how Freud's most peculiarly psychoanalytic knowledge differs from other forms and kinds of knowledge.

Two notions taken from the discourse of philosophy of science will assist us in our examination of Freud's case studies: «exemplars» and «personal knowledge». We must not reify these notions, however, or permit them to distract us from the question of psychoanalytic knowledge. Let it be clear from the outset that I am not trying to defend psychoanalysis or to answer the (tedious) question of whether or not psychoanalysis is a science. Nor am I trying to demonstrate the validity of these notions of «exemplar» and «personal knowledge» in the discourse of the philosophy of science. I am using these notions in connection with Freud's case studies for the much more modest heuristic purpose of illuminating the place of case studies in the discourse of psychoanalysis and speculating upon the nature of psychoanalytic knowledge, a peculiar sort of knowledge that I believe remains to be adequately described.

This is not the place to elaborate upon how the term «exemplar» has been used by descriptive philosophers of science, but I must admit that my understanding of the way in which Freud's (and later psychoanalysts') case studies constitute exemplars in psychoanalysis diverges in one very important respect from the meaning of the term given by Thomas Kuhn. In his attempt to show that shared theory and correspondence rules are not sufficient to explain disciplinary coherence in certain natural sciences, Kuhn used the term «paradigm» in one of its senses to mean «shared examples of successful practice», which students of these disciplines learn, and which then serve to maintain the coherence of their research community (1). In Kuhn's later essays, these

(1) In his later essays Kuhn uses the term «exemplars» for this sense of his original term «paradigm». Cf. *The Structure of Scientific Revolutions*. Chicago, University of Chicago Press,

are termed exemplars. My impression is that psychoanalytic case studies—Freud's in particular—serve the purpose of helping psychoanalysts identify the problems posed by their patients and permit psychoanalysts all over the world, sitting alone in their consultation rooms with their patients, to proceed in parallel fashion, *even though the examples are not always successful*. The case of Dora comes to mind once again: the failure of Freud's attempts to reason with Dora and her abrupt termination of analysis before therapy could be effected does not seem to detract from the value of the case. The presentation of such an awkward case as an exemplar might well give a descriptive philosopher of science like Kuhn pause, to say nothing of a neo-positivist. But this seems unproblematic here, precisely because I am not trying to defend psychoanalysis as a science or demonstrate the validity of the notion of exemplars.

Whatever one thinks of psychoanalysis, it is a fact that Freud's case studies have been extremely important vehicles of the tradition of psychoanalytic knowledge. Somehow Freud's cases still communicate essential knowledge to psychoanalysts. It is much easier, however, to say what they are *not* than to specify what they are and how they work in the discourse of psychoanalysis. Although they are by no means devoid of theoretical considerations, for example, they are all highly detailed investigations of quite idiosyncratic cases. Freud seldom even claimed for them the value of illustrations of theory, and as time went on he ceased to make such claims altogether. And as I have already suggested, they are not quite like exemplars in that they obviously do not depend upon being examples of *successful* practice. Many conversations with psychoanalysts and a reading of the memoir literature of psychoanalysis suggest, however, that what psychoanalysts derive from their study of Freud's cases is a sense of how Freud thought, and, more particularly, *how he thought with his patients*. Even to the non-psychoanalytic reader Freud's case studies seem to communicate how it feels to do psychoanalysis and learn from patients. In contrast to his theoretical writings like the *Three Essays on the Theory of Sexuality* or expository-didactic ones like the *Introductory Lectures on Psychoanalysis*, Freud's case studies may be the locus of intersubjectivity in psychoanalysis. It is even tempting to think that the communication between Freud and the psychoanalytic readers of his case studies is modeled on the kind of communication that obtains between analyst and patient in psychoanalysis proper. If this is approximately true, it suggests Freud's exemplary cases function

1962, and more specifically *The Essential Tension: selected Studies in Scientific Tradition and Change*. Chicago, University of Chicago Press, 1977, pp. 306 and 318-319.

as a kind of «personal knowledge», borrowing now from Michael Polanyi (2). Studying them, psychoanalysts tacitly learn to think like Freud.

The question foremost in my mind while writing this paper is *how the status Freud accorded the knowledge deposited in his case studies evolved in his own thinking*. But other questions can never be far from the surface of inquiry: how, for example, have Freud's case studies been utilized by his followers in psychoanalysis? How have later psychoanalysts reinterpreted these case studies? And how have the new cases reported by other psychoanalysts reshaped the corpus of psychoanalytic knowledge? Although I shall not be able to answer these questions in the brief compass of a paper on Freud's own thinking, I believe my present inquiry is given greater significance by the fact that psychoanalysts have indeed studied Freud's cases as exemplary solutions to problems. They have too, I believe, learned more and more consistently from the case studies than from Freud's more traditionally formulated theoretical propositions. The whole process of psychoanalytic education enshrined in the institutes of psychoanalysis suggests this, although again it would take a separate paper to demonstrate it conclusively. It is very largely the case studies, Freud's and the cases published subsequently in the same spirit by his followers, that have enabled psychoanalysis to maintain its great methodological coherence for so long, and during a period when most of Freud's formal theoretical statements have been shown to be at least partially false (3).

Psychoanalysts are of course not all fully aware of the fact that case studies play such an important role in their discipline. Many if not most psychoanalysts are still consciously wedded to a positivistic conception of science, and they place great store in the theory of psychoanalysis when they represent it to the public at large. If I am right, however, they

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- (2) See POLANYI, M. (1958), *Personal Knowledge: towards a post-critical Philosophy*. Chicago, University of Chicago Press.
- (3) On psychoanalytic education see: GOODMAN, S. (ed.) (1977), *Psychoanalytic Education and Research*. New York, International Universities Press, and LEWIN, B. D.; ROSS, H. (1960), *Psychoanalytic Education in the United States*. New York, International Universities Press. For evidence of the role of case studies in maintaining the coherence of the psychoanalytic discipline, one should also consider the periodical literature of psychoanalysis and the prominence of case studies in it. Two other publications are also interesting in this context: one, didactic, is Hedda Bolgar's essay on The Case Study Method, in WOLMAN, B. B. (ed.) (1965), *The Handbook of Clinical Psychology*. New York, McGraw-Hill; the other, historical, is Harold Greenwald's edition of *Great Cases in Psychoanalysis*. New York, Ballantine Books, 1959, where the editor explicitly suggests that these most frequently studied and cited cases constitute a more significant history of psychoanalysis than a recounting of the evolution of theory or technique.

are much more defensive about the fragility of this body of theory than they need to be. For when they communicate with their patients and with each other, they are demonstrably more interested in the subtleties of insight found in particular cases generated by their method than they are in the validity of general theoretical propositions.

This suggests that psychoanalytic knowledge, the knowledge peculiar to psychoanalysis, is constituted rather differently than the knowledge of a more thoroughly theory-based science. In fact, I use the word «knowledge» in reference to psychoanalysis, instead of «science», precisely in order to suggest that the very question of science is misleading when applied to psychoanalysis (4). If so, then the great energy expended in trying to reformulate Freud's theoretical propositions to make them experimentally testable may not — in spite of the value such efforts may have for other purposes — contribute much to our understanding of what psychoanalytic knowledge really is (5). A truly descriptive

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- (4) Several other disciplines of questionable scientificity have applied for the status of special forms of knowledge in order to circumvent the question of science, which seems always to entail a comparison with physics. This tactic gives rise to the plural form, «knowledges». This seems novel but it could become as common as «sciences» is now. And since «knowledge» is so much less freighted with ideology than «science», this might facilitate more and more genuine descriptive approaches to the intellectual capitals of the anomalous disciplines of psychoanalysis, ethnography, history, etc. In following this suggestion, however, one necessarily raises the alternative question of «art» or «craft», and that two will need to be differentiated before it will be useful.
- (5) For the efforts to test Freud's theories experimentally, see EYSENCK, H. J.; WILSON, G. D. (eds.) (1973), *The Experimental Study of Freudian Theories*, London, Methuen, and SARNOFF, I. (1971), *Testing Freudian Concepts*, New York, Springer. More recently Seymour Fisher and Roger Greenberg have catalogued and analyzed the vast literature devoted to experimental study of Freud's theories in their book *The Scientific Credibility of Freud's Theories and Therapy*. New York, Basic Books, 1977; the same authors have edited a book of exemplary tests of Freudian theories, entitled *The Scientific Evaluation of Freud's Theories and Therapy*. New York, Basic Books, 1978.
- On Freud's theories more generally, their genesis, their «style», and their status as science, see ELLENBERGER, H. (1970), *The Discovery of the Unconscious*. New York, Basic Books; various papers by Robert R. Holt, e.g. Two Influences on Freud's Scientific Thought: a Fragment of intellectual Biography, in: WHITE, R. W. (ed.) (1963), *The Study of Lives*, New York, Atherton Press, pp. 364-387, Freud's Cognitive Style, *American Imago* 22, 163-179 (1965), and A Review of Some of Freud's Biological Assumption and their Influence on his Theories, in: GREENFIELD, N. S.; LEWIS, W. C. (eds.) (1965), *Psychoanalysis and current biological Thought*, Madison, University of Wisconsin Press, pp. 93-124; GEDO, J. E.; GOLDBERG, A. (1973), *Models of the Mind: a psychoanalytic Theory*, Chicago, University of Chicago Press. GEDO, J. E.; POLLOCK, G. H. (eds.) (1976), *Freud: the Fusion of Science and Humanism*, New York, International Universities Press; the work of Roy Schaefer, especially Action: its Place in Psychoanalytic Interpretation and Theory, *The Annual of Psychoanalysis*, 1, 159-196 (1973) and *A New Language for Psychoanalysis*, New Haven, Yale University Press, 1976; and most recently, Frank Sulloway's *Freud: Biologist of the Mind*, New York, Basic Books (1979).

account of psychoanalysis, I am suggesting, would have to pay much more attention to case studies as repositories and vehicles of this knowledge than has been the case thus far. Such a descriptive account would of course involve an extensive study of the periodical literature of psychoanalysis, its monographs, and of psychoanalytic education as practiced in the institutes. The present paper is only a beginning in that direction, an attempt to see how Freud came to the tentative conclusion that case studies were the genre of peculiarly psychoanalytic knowledge, and (as a corollary) to aim his more theoretical statements at the extrapsychanalytic reader. It is of course also an attempt to pose the question of psychoanalytic knowledge as a question of discourse.

Freud published his first case studies jointly with Joseph Breuer, whose patient «Anna O.» had actually invented the «talking cure» that constituted the rudimentary psychoanalytic method that Freud practiced in the 1890's. These first case studies appeared in *Studies on Hysteria* in 1895. From that year until 1915 when Freud completed his case history of the Wolf Man (*From the History of an Infantile Neurosis*, which was not published until 1918), his method was continually evolving. And especially during the later half of this period, Freud's energies were dedicated very largely to the formation of a school of disciples and institutionalizing psychoanalysis. We shall see that Freud's preoccupations with his method of investigation and with forming a community of psychoanalytic investigators governed his innovative use of case studies. I shall focus here in turn upon each of the case studies Freud wrote in this twenty year period.

I. *STUDIES ON HYSTERIA*

Studies on Hysteria is one of the least coherent works in the Freudian corpus. For one reason, it was written by two authors who did not agree entirely on its substance. For another, the principal author had not yet completed his so-called self-analysis or even begun the synthesis of his ideas that he would publish as the *Interpretation of Dreams* in 1900. Thus *Studies on Hysteria* remained a series of fragments, but its fragmentary and tentative nature permits an unhindered view of the authors' anxiety about the case studies they report in it. Freud's and Breuer's concerns were not unlike what any late nineteenth-century natural scientists would have felt when, still caught in the throes of giving birth to a new kind of knowledge, they could only report their fragmentary insights in case studies.

In his study of Anna O., for example, Breuer felt obliged to «excuse» himself for reciting the details of her case by drawing an analogy

between Anna O. and the clarity of the protoplasm of the eggs of the sea-urchin which enable the histology student to see what happens in the eggs of other species whose egg-protoplasm is opaque: «The interest of the present case seems to me above all to reside in the extreme clarity and intelligibility of its pathogenesis». Breuer's analogy was later used by Freud too, as the editors of the *Standard Edition* of his writings point out. And his general argument that pathology may illuminate the workings of the healthy personality became an important justification for the value of psychoanalytic research. But it is interesting to note that both of these ideas arose in response to Breuer's (and Freud's) anxiety about reporting in such detail on a single unusual example. Strangely enough, the sea-urchin analogy is actually inappropriate in this most important respect, since Anna O. did not constitute a typical example of a different species but an individual of the same species whose pathology might have made her wholly idiosyncratic. Thus the analogy serves principally to mask the problem which natural scientists of the time would have found most unsettling (6).

The Story of Anna O. — now thought to have been Bertha Pappenheim in real life: the social worker, philanthropist and founder of the *Jüdischer Frauenbund* — was the first and most extraordinary of the cases reported in *Studies on Hysteria*. Henri Ellenberger describes Freud's learning of the story of Breuer's patient, along with his experience with Charcot at the Salpêtrière in Paris, as one of the two personal experiences that led him to devise a new theory of the neuroses (7). Breuer had treated Anna O. in the early 1880's and for several reasons was uncom-

(6) STRACHEY, J., et al. (eds.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, London, Hogarth, vol. 2, p. 41. The analogy may seem improbable today, but in the intellectual climate of physiological psychiatry that prevailed in the 1890's it was not unusual—see SULLOWAY, F. (1979), *op. cit.*

(7) ELLENBERGER, H. (1970), *op. cit.*, p. 480. Ellenberger distinguishes these two personal experiences from the larger trends in neurology and psychiatry that also contributed to Freud's thinking about the neuroses.

The writings of later psychoanalysts on Anna O. / Bertha Pappenheim are too numerous to list. A few of the more important: ELLENBERGER, H. F., The Story of Anna O.: a critical Review with new Data, *Journal of the History of the Behavioral Sciences*, 8, 267-269 (1972); JENSEN, E. M., Anna O. - a Study of her later Life, *Psychoanalytic Quarterly*, 39, 269-293, (1970); MEISSNER, W. W. A Study on Hysteria: Anna O. Rediviva, *The Annual of Psychoanalysis*, 7, 17-52 (1979); POLLOCK, G. H., The Possible Significance of Childhood Object Loss in the Josef Breuer — Bertha Pappenheim (Anna O.)— Sigmund Freud Relationship, *Journal of the American Psychoanalytic Association*, 16, 711-739 (1968), Bertha Pappenheim's pathological Mourning possible Effects of Childhood Sibling Loss, *Ibid.*, 20, 476-493 (1972), and Bertha Pappenheim: Addenda to her Case History, *Ibid.*, 21, 328-332 (1973); and more generally, S. REICHARD, A Re-examination of "Studies on Hysteria", *Psychoanalytic Quarterly*, 25, 155-177 (1956).

fortable about returning to the case. As Ellenberger notes, the case «radically differ[ed] from other cases of hysteria [reported] at that time, but [was] analogous to the great exemplary cases of magnetic illness in the first half of the nineteenth century, ...» The case of Anna O. seems to have appeared to be a reversion to an earlier and ostensibly pre-scientific psychology. As Ellenberger reports,

To the older magnetizers, Anna O.'s story would not have seemed as extraordinary as it did to Breuer. It was one of those cases, so frequent in the 1820's, yet so scarce in the 1880's, in which the patient dictated to the physician the therapeutic devices he had to use, prophesied the course of the illness, and announced its terminal date. But in 1880, when authoritarian use of hypnosis had supplanted the former bargaining therapy, a story such as that of Anna O. could no longer be understood (8).

The anxiety of the two authors, therefore, must have been based on the kind and quality of case study they were reporting, and not only on the fact of reporting case studies. Case studies were common enough in medicine and neurology at the time. Just two years before publishing *Studies on Hysteria*, Freud had published a conventional case history illustrating how effective hypnosis could be in clearing up symptoms in a case of hysteria (9). This was quite unproblematic since the case merely illustrated a relatively common medical procedure and appeared to validate a theory. In *Studies on Hysteria*, however, Freud and Breuer were admitting the lack of medical and physiological authority over hysteria and, by implication, attributing to the patients the knowledge and power to cure themselves. As Freud noted, their cases also had more of a literary than a scientific character (10). And that, taken in conjunction with the fact that Freud and Breuer were using their cases to illustrate yet non-existent theory, was what made *Studies on Hysteria* an anomalous book on an anomalous subject.

Of course *Studies on Hysteria* consisted of more than merely case studies. The first chapter was a paper that Freud and Breuer had previously published, a «Preliminary Communication» on hysteria, defi-

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- (8) ELLENBERGER, H. (1970), *op. cit.*, p. 484. This gives interesting relevance to a remark of Robert Darnton in his book, *Mesmerism and the End of the Enlightenment in France*, Cambridge (Mass.), Harvard University Press, p. 173: «Works by nonmesmerists usually honor Mesmer as a misunderstood and sometimes heroic prophet of modern psychology. It may be that psychoanalysis developed from a line of occult scientists, linking Freud, Charcot, and Braid with Bertrand, Puysegur, and Mesmer, just as chemistry emerged from alchemy.»
- (9) A Case of a successful Treatment by Hypnosis in: STRACHEY, J. *et. al.* (eds.) (1966), *Standard Edition*, vol. 1, London, Hogarth, pp. 115-128.
- (10) See below, p. 271.

ning their place on the frontier of research and distinguishing their approach from that of other investigators. And the book concluded with a «Theoretical» chapter by Breuer and a paper on catharsis as the essential ingredient of «The Psychotherapy of Hysteria» by Freud. Thus the two authors carefully bracketed their detailed case studies by theoretical statements, giving their case studies the formal appearance of mere illustrations. But this was an obligatory formality dictated by the norms of scientific reporting, norms to which Breuer and Freud naturally subscribed. Nevertheless, the theoretical portions of *Studies on Hysteria* were the profoundly tentative portions and the case studies the real substance of it. Only in the case studies could the authors speak comfortably in the indicative mood — only there were they really sure of their ground. And there were good reasons for this, that Freud at least recognized.

In his discussion of the case of Frau Emmy von N., also published in *Studies of Hysteria*, Freud noted the difficulties arising from the fact that hysteria was a diagnostic category without rigorous or commonly accepted definition: «Unless we have first come to a complete agreement upon the terminology involved, it is not easy to decide whether a particular case is to be reckoned as a hysteria or some other neurosis.» Of course the fact that hysteria was a poorly understood but notorious class of nervous disorders is precisely what interested Freud. One might say that hysteria was an acknowledged category of anomalous mental disorders in the 1890's. Anyone who could understand and define the nature of this disorder would clear up many mysteries in neuropathology. As Freud put it, «we have still to await the directing hand which shall set up boundary marks in the region of the commonly occurring neuroses and which shall bring out the features essential for characterization.» In the meantime, however, «we are still accustomed to diagnosing a hysteria, ... from its similarity to familiar typical cases.» Thus Freud explained the necessity of reporting case studies to himself by remembering that hysteria was not then a well understood disease, but anticipated that some great scientist — and in the very fact of his writing upon the subject we can see that he already anticipated the possibility that he might be that «directing hand» — would clear up the confusion by defining the distinctive features of hysteria. Then hysteria would come in under the umbrella of medical science and case studies would no longer be necessary (11).

(11) *Standard Edition*, vol. 2, p. 85. We also note that Freud expresses the germ of an idea of «scientific revolutions» in this passage.

In another passage in *Studies on Hysteria* Freud explicitly stated his anxiety as a neuropathologist writing case studies. At the beginning of his discussion of the case of Fräulein Elisabeth von R., he confessed,

I have not always been a psychotherapist. Like other neuropathologists, I was trained to employ local diagnoses and electro-prognosis, and it still strikes me myself as strange that the case histories I write should read like short stories and that, as one might say, *they lack the serious stamp of science*. I must console myself with the reflection that the nature of the subject is evidently responsible for this, rather than any preference of my own. The fact is that local diagnosis and electrical reactions lead nowhere in the study of hysteria, whereas a detailed description of mental processes such as we are accustomed to find in the works of imaginative writers enables me, with the use of a few psychological formulas, to obtain at least some kind of insight into the course of that affection. Case histories of this kind are intended to be judged like psychiatric ones; they have, however, one advantage over the latter, namely an intimate connection between the story of the patient's sufferings and the symptoms of his illness — a connection for which we still search in vain in the biographies of other psychoses (12).

This passage speaks very much for itself and illustrates again what I have tried to suggest about the anxiety the authors of *Studies on Hysteria* had to confront. But it also reveals a characteristic of Freud's scientific mind that would enable him to press on with the solution of the problem of hysteria, while Breuer would withdraw from the controversial enterprise. Freud was able to express his anxiety about the disjunction of the expectations of the scientific community on the one hand and what he was doing on the other, and explain to himself and his readers that the path he was breaking in method by narrating his cases was appropriate to the peculiar nature of the pathology he was investigating. To the objections that he expected his contemporaries would make to his procedure he found answers, and even comparative advantages.

This may have been only a moment of bravado in 1895, however, since Freud was still far from his mature understanding of hysteria or the «talking cure» that Anna O. had invented in her conversations with Breuer. But if it was bravado, it was facilitated by the fact that Freud had as yet no idea how complicated his reports of case studies would become by the time when, two decades later, he was to write his report on the case of the Wolf Man. Here he states easily that he had «endeavoured to weave the explanations which I have been able to give of the case into my description of the course on her [Fräulein Elisabeth von R.'s]

(12) *Ibid.*, vol. 2, pp. 160-161.

recovery» (13). In fact the cases reported in *Studies on Hysteria* do read rather easily and the narrative is straightforward, not unlike, I am tempted to suggest in echo of Freud, the naturalistic narrative common in fiction in the late nineteenth century. In style too, *Studies on Hysteria* constitute both a reversion to the early nineteenth century and a harbinger of the twentieth. As Wolf Lepenies shows, when psychology was redefined as a physiological and medical science, around 1830 on the European continent, literary style become an embarrassment to psychologists and they apparently felt obliged to repudiate the important tradition of elegant descriptive investigation in which psychopathological states had therefore been made empathically plausible. Thereafter, novelistic descriptions of psychology and psychopathology such as those of Balzac were no longer considered relevant to the science of psychology. It was left to Freud, in his careful description of individual cases, to reintroduce literary style as a tool of investigation in psychology (14). Intimately connected to his abdication of medical authority over hysteria, this too was a source of anxiety to Freud. Freud's investigations would eventually become one of the principal stimuli of radically different and extremely intricate narrative styles, as is commonly known (15). The simplicity of his narratives in *Studies on Hysteria* is however, a measure of his naiveté *vis à vis* the problem of hysteria and the genre of case studies. For instead of progressively resolving themselves into scientifically acceptable theories as Freud came to understand hysteria better, his case studies were to become more and more complex and entangled narratives. But they would remain an independent genre of psychoanalytic investigation.

II. THE DORA CASE

Freud did not write another case study until after he had published *The Interpretation of Dreams* at the turn of the century. But when he did, it

(13) *Ibid.*, vol. 2, pp. 161.

(14) Wolf Lepenies, *Transformation and Storage of Scientific Traditions in Literature*, unpublished manuscript cited by courtesy of the author, 1980. The following passage from ELLENBERGER, H. (1970), *op. cit.*, p. 474, is also relevant: «One is free to speculate about the literary possibilities had Freud left medicine to develop his great talent as a writer. Emmy von N., Elisabeth von R., and young Dora would have become heroines in short stories. The obsessions of the Wolf-Man would have been made a nightmarish novel in Hoffmann's fashion, and a story about Leonardo da Vinci would have overshadowed Merezhkovsky's historical fiction. A novel by Freud about the cruel old father and the horde would have brought to perfection that literary genre of prehistoric novels that the brothers Rosny had made popular in France, although Freud would have conceived it more in the style of Hesse's *Rainmaker*. The story of Moses ...» and so on.

(15) See for example, BROOKS, P. (1979), *Fictions of the Wolf Man: Freud and Narrative Understanding*, *Diacritics*, 9, 72-81.

was the seminal *Fragment of a Analysis of a Case of Hysteria* (1905), written in 1901 but not published until four years later. In his three volume *Life and Works of Sigmund Freud*, Ernest Jones wrote that this case, commonly referred to as the Dora case, «has served for years as a model for students of psychoanalysis» (16). We may add that it has enjoyed notoriety among critics of Freud for nearly as long. And it was a case in which Freud failed to achieve his therapeutic ends, a fact that may cause us to wonder why he chose to publish it at all. Dora left Freud after only three months in analysis. Her departure surprised him and caused him to devote a great deal of thinking to an explanation of what had gone wrong. He wrote and eventually published the case for at least two reasons. First, the case turned largely upon two dreams. Since *The Interpretation of Dreams* had been published so recently, he was naturally inclined to take advantage of the opportunity to illustrate «how an art, which would otherwise be useless, can be turned to account for the discovery of hidden and repressed parts of mental life». Second, he wanted to show how the general therapeutic method he had outlined in the final chapter of *Studies on Hysteria* had led him to a far more sophisticated understanding of that anomalous disease of the mind than any other investigator had achieved. Not only was the case a therapeutic failure; Freud's interest in the case and motives for publishing it were extra-therapeutic. After Dora quit psychoanalysis, she became an even more interesting subject of research. Freud could hardly resist the opportunity Dora afforded him to display the method he had devised for the analysis of hysteria and the insight it yielded in this particular case (17).

Freud managed to convince himself that the difficulties inhering in this (therapeutically) unresolved case suited it to become «a first introductory publication» on the question of hysteria. Oddly enough, that is precisely how it has been read by several generations of psychoanalysts. Using a still very rudimentary notion of «transference», Freud argued that it was because he had failed to «master» or dispell the

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- (16) JONES, E (1955), *The Life and Work of Sigmund Freud*, New York, Basic Books, vol. 2, p. 257. Some of the most interesting things on the Dora case have been written by non-psychoanalysts. See for example, Steven Marcus's long essay, *Freud and Dora: Story, History, Case History*, in his *Representations: Essays on Literature and Society*, New York, Random House, pp. 247-310 (1975). There is also a very interesting historical reconstruction of Dora's identity in an unpublished essay by Peter Loewenberg, «Austro-Marxism and Revolution: Otto Bauer, Freud's Dora Case, and the Crisis of the First Austrian Republic», to appear as part of a forthcoming book.
- (17) *Standard Edition*, London, Hogarth, vol. 7, pp. 114-115 (1953). For a further statement on how Dora illustrated the technique described in *The Interpretation of Dreams*, see pp. 10-11 of the same volume.

transference that Dora had formed with respect to him that their therapeutic encounter was so short-lived. He felt that he had been distracted by the readiness with which Dora put considerable quantities of pathological material at his disposal. He had been unaware of the fact that the transference had already developed to shield another portion of her pathology. Had he been aware of this he might have explained the matter to Dora and their interpretations might have proceeded to the eventual resolution of her difficulties. As it was, he had the considerable materials of her two rich dreams and he also had her dramatic departure as an illustration of the transference as well as grounds for inferring what additional materials the transference has masked. This enabled him to write a rather extensive and illuminating report (18).

Freud was consequently unconcerned about the brevity and incompleteness of his therapy with Dora, but two other kinds of incompleteness did give him pause when he considered publishing his written report. First, since he wanted to focus upon hysteria rather than his interpretive technique, he had not reported «the process of interpretation to which the patient's associations and communications had been subjected, but only the results of that process» (19). And second, he explicitly reaffirmed the credo of the science of his day by stating that «a single case history, even if it were complete and open to no doubt, cannot provide the answer to all the questions arising out of the problem of hysteria» (20). We may profitably reflect upon each of these additional senses of incompleteness in the account of Dora's case, for both of these questions had concerned Freud writing *Studies on Hysteria* and would occupy his attention in later case studies too. One of the problems may be called the question of rhetorical strategy in reporting a case and the other the question of the scientific status of Freud's observations of a case.

The dubious «objectivity» of his observations was prominent in Freud's mind when he wrote his account of Dora's hysteria. On the first page, having alluded to his earlier publications on the disease, he noted that it had been «awkward that I was obliged to publish the results of my inquiries without there being any possibility of other specialists testing and checking them» and that it was «scarcely less awkward now» (21). He attempted to forestall criticism on this score by going on at considerable length about the prurience of his contemporaries, even physicians,

(18) *Ibid.*, vol. 7, pp. 116-120.

(19) *Ibid.*, vol. 7, pp. 12-13 and again on p. 112.

(20) *Ibid.*, vol. 7, p. 13.

(21) *Ibid.*, vol. 7, p. 7.

which did not permit identifying the subject of his case report. This may have been an accurate account of the state of affairs then, but it was unrealistic for Freud to think that circumstances would ever permit disclosure of the identity of such a patient. Confidentiality was in fact a feature of medical ethics generally, and only the special nature of the materials under investigation in Freud's work made this problematic. We must, therefore, look beyond the prurience of Freud's readers, into the nature of psychoanalytic knowledge itself, as indeed Freud did, if we are to appreciate the special problems of intersubjectivity presented by psychoanalysis.

Freud made two other observations suggesting that the material he was presenting in his study of Dora could not be appreciated by readers unfamiliar with his earlier work. Pointing out that his analysis of Dora was largely an interpretation of two dreams, he said he expected his readers to study *The Interpretation of Dreams* before undertaking to read his *Fragment of an Analysis of a Case of Hysteria*. For although it was true that no other observer could check the veracity of his report of Dora's dreams, the general principles of dream interpretation were accessible to every reader in his earlier book, and «everyone can submit his own dreams to analytic examination» (22). This, as we know, was quite a request to make of his readers. It was tantamount to suggesting that his readers undergo psychoanalysis, or what is even more difficult, self-analysis, before they read his account of Dora's analysis. In another passage, Freud suggested that even familiarity with the ideas in *The Interpretation of Dreams* was not enough —one must agree with them in order to appreciate his case study of Dora: «Anyone who has hitherto been unwilling to believe that a psycho-sexual aetiology holds good generally and without exception for hysteria is scarcely likely to be convinced of the fact by taking stock of a single case history. He would do better to suspend his judgment until his own work has earned him the right to be convinced» (23). Here Freud links the problem of Dora's being a single case with the difficulty of checking his observations and suggests that these obstacles to the intersubjectivity of his insights could be overcome if the reader were sufficiently initiated into psychoanalysis. He does not, however, go beyond the implication that psychoanalysis is a specialized branch of knowledge that cannot be appreciated immediately by a novice, something that can be said about almost any scientific

(22) *Ibid.*, vol. 7, pp. 10-11. Again in the same passage Freud asserts that «since ... this case history presupposes a knowledge of the interpretation of dreams, it will seem highly unsatisfactory to any reader to whom this presupposition does not apply».

(23) *Ibid.*, vol. 7, p. 13.

discipline. Later he would acknowledge that psychoanalytic knowledge had its own peculiar difficulties intimately related to its unique value.

While we are considering Freud's anticipation of difficulties with unreceptive readers, it may be well to recall how the case of Dora struck readers who were positively inclined to psychoanalysis. It was Ernest Jones's initial encounter with Freud's work and when he wrote his biography of Freud he had this to say about it.

This first case history of Freud's has for years served as a model for students of psychoanalysis, and although our knowledge has greatly progressed since then, it makes today as interesting reading as ever. It was the first of Freud's post-neurological writings I had come across, at the time of its publication, and I well remember the deep impression the intuition and the close attention to detail displayed in it made on me. Here was a man who not only listened closely to every word his patient spoke but regarded each such utterance as every whit as definite and as in need of correlation as the phenomena of the physical world. At the present day it is hard to convey what an amazing event it was for anyone to take the data of psychology so seriously. Yet that it should less than half a century after seem a commonplace is a measure of the revolution effected by one man (24).

This seems to suggest that difficulties of intersubjectivity inherent in Freud's case studies when seen from the view-point of positivistic philosophy of science were positive advantages when these studies were read by someone favorably disposed to Freud's ideas and intuitions.

Jones's remarks reflect equally on the other reservation Freud had about the difficulty of presenting case studies: the rhetorical strategy that he was forced to adopt. Here again his ideas had advanced some since his work with Breuer. Although in writing *Studies on Hysteria* he had been distressed by the fact that he wrote something like short stories on the course of each patient's illness, he nonetheless managed to make them into vigorous and illuminating narratives. But in his *Fragment of an Analysis of a Case of Hysteria* —already an awkward title— Freud professed surprise that other scientific writers «can produce such smooth and exact histories in cases of hysteria». He had observed that «the patients are incapable of giving such reports about themselves». Furthermore, according to Freud's new view of the matter, their incapacity to give such reports is an integral part of their illnesses: the repression of memories and the expression in the form of symptoms of the conflicts associated with these memories that constitutes hysteria precludes the

(24) JONES, E. (1955), *The Life and Work of Sigmund Freud*, New York, Basic Books, vol. 2, p. 257.

possibility of an orderly recounting of a patient's life (25). Furthermore, Freud recognized that there were several stories to be told that could hardly be told simultaneously. In the Dora case he distinguished only between the narration of his interpretive technique and the reconstructed biographic story in which the patient's pathology had developed. But he no longer felt that he could weave these stories together as he had in *Studies on Hysteria*.

I have in this paper left entirely out of account the technique, which does not at all follow as a matter of course, but by whose means alone the pure metal of valuable unconscious thoughts can be extracted from the raw material of the patient's associations. This brings with it the disadvantage of the reader being given no opportunity of testing the correctness of my procedure in the course of this exposition of the case. I found it quite impracticable, however, to deal simultaneously with the technique of analysis and with the internal structure of a case of hysteria: I could scarcely have accomplished such a task, and if I had, the result would have been almost unreadable (26).

Freud had learned a great deal about the epistemological status of the knowledge he was deriving from hysterical patients in the years since 1895. Paradoxically, what he had learned made him both more and less sanguine about the prospects of communicating this knowledge. He was much more aware of the difficulties of demonstrating the validity of his observations and hypotheses to the medical world at large, but he had begun to see that other initiates and especially practitioners of psychoanalysis were in a peculiarly advantageous position to appreciate his case studies. Ernest Jones writes that we do not know why Freud hesitated so long to publish the Dora case, and strictly speaking that is true, but the way his thinking had changed since 1895 suggests a very strong reason. He simply took his reservations about the case's incompleteness very seriously. And in view of the still almost complete lack of a psychoanalytic community in 1901, it is no wonder that he did not publish it then. In 1905 things looked different and the illustrative value the case held for other psychoanalysts outweighed his reservations about the possibility of its being appreciated by the uninitiated.

(25) *Standard Edition*, vol. 7, pp. 16-18.

(26) *Ibid.*, vol. 7, pp. 12-13 and 112. Cited passage appears on p. 112. On p. 13, Freud writes that «Apart from the dreams, therefore, the technique of the analytic work has been revealed in only a very few places. My object in this case history was to demonstrate the intimate structure of a neurotic disorder and the determination of its symptoms; and it would have led to nothing but hopeless confusion if I had tried to complete the other task at the same time.» This is an even more explicit recognition of the impossibility of reconciling the naturalistic narrative with the work of analysis.

Anyone seeking to understand what psychoanalytic knowledge is should try to understand Freud's dilemma at this juncture. I believe that if we cannot appreciate Freud's motives in considering whether to publish the Dora case, we may be reasonably sure that we do not understand the way in which psychoanalytic knowledge developed and how it is constituted.

III. LITTLE HANS AND THE RAT MAN

When Freud finally decided to publish his account of Dora's case it was after four years of temporizing, as I have already pointed out. In fact, the paper had been accepted for publication as early as January, 1901 by Ziehen, the editor of the *Monatsschrift für Psychiatrie und Neurologie*, the same journal in which it actually appeared in 1905 (27). But this was the last of Freud's case studies that was to appear in a medical journal not directly associated with the psychoanalytic movement. (Jones notes that Freud gave his last lecture before a medical audience in 1904.) Of course in 1900 when Freud worked with Dora and wrote the history of her case, he couldn't be expected to have had disciples or a journal devoted exclusively to psychoanalysis. It was only in late 1902 and 1903 that the first meetings of what was to become the Vienna Psychoanalytic Society were held in Freud's waiting room, and only in 1908 that the first «international» meeting of psychoanalysts was held in Salzburg. It was at the Salzburg meeting that the decision was made to publish a psychoanalytic periodical (28). As one might suppose, the *Jahrbuch* was founded in order to give the group its own forum in which to further develop psychoanalysis without the interference of the uninitiated — a step taken by the practitioners of virtually every emergent discipline or subdiscipline (29). Thus in 1909 Freud was able to publish his next two case histories in the first two numbers of the first psychoanalytic periodical, the *Jahrbuch für psychoanalytische und psychopathologische Forschungen*. These were his reports on the cases commonly known as Little Hans, *Analysis of a Phobia in a Five-Year-Old Boy*, and the Rat Man, *Notes upon a Case of Obsessional Neurosis*. In view of the circumstances in which they were

(27) See the «Editors Note», *Standard Edition*, London, Hogarth, vol. 7, pp. 3-5 (1953).

(28) See JONES, E. (1955), *The Life and Work of Sigmund Freud*, vol 2, pp. 38-45.

(29) See also Jung's prefatory statement in the first volume, and Jones's account of Freud's letter in which he expressed satisfaction at being rid of outside interference and able to communicate with his disciples in their own publication, *ibid.*, p. 45. It is significant that this important step in creating a disciplinary matrix should have been marked on Freud's part by the contribution of two case studies (and no other communications) to the first volume of the *Jahrbuch*.

published, it should come as no surprise that Freud's accounts of these two cases should be devoid of that defensive sensitivity *vis à vis* non-psychoanalytic readers which marked his account of the Dora case. The cases published in 1909 were clearly aimed at a readership of psychoanalysts, a readership which had only emerged in the time since the writing of the Dora case (30).

The case of Little Hans especially gives the impression of a contribution *entre nous*, and it became the very first item in the first issue of the *Jahrbuch*. The sense of familiarity derives in part, of course, from the fact that the raw materials for the report were supplied by the little boy's parents, who were, according to Freud, «among my closest adherents» (31). According to Freud, too, he had «for many years been urging my pupils and my friends to collect observations of the sexual life of children» in order to compare the data of such observations to hypotheses about early sexual life derived from the analysis of adults, e.g., those that he had published in the *Three Essays on the Theory of Sexuality* (1905). The case of Little Hans is the most obvious fruit of this request, something the parents of the child shared with Freud, and which Freud then shared with his other disciples. But there were important and positive theoretical reasons for sharing this case with psychoanalysts. Freud acknowledged that the hypotheses in the *Three Essays*,

seem as strange to an outside reader as they seem incontrovertible to a psycho-analyst. But even a psycho-analyst may confess to the wish for a more direct and less roundabout proof of these fundamental theorems. Surely there must be a possibility of observing in children at first hand and in all the freshness of life the sexual impulses and wishes which we dig out so laboriously in adults from among their own débris — especially as it is also our belief that they are the common property of all men, a part of the human constitution, and merely exaggerated or distorted in the case of neurotics (32).

Here we see that Freud continued to regard the case study as somewhat inferior in significance to theoretical formulations, but accorded it an important role in the confirmation of hypotheses, i.e., in the validation

(30) The two cases constitute vol. 10 of the *Standard Edition*.

(31) The father was Max Graf, a music critic and member of Freud's early circle who attended the «Wednesday meetings».

It is interesting that this relatively simple case that Freud published for didactic reason alone, and not because it illustrated any new complexities of technique or marked out any new territory of diagnosis, has been very little the subject of later psychoanalytic investigation or writing. This would seem to confirm Freud's statements about the greater value of the cases on the frontiers of knowledge, even if they cannot be completely resolved, given in his account of the case of the Wolf Man. See below, p. 290.

(32) *Standard Edition*, vol. 10, pp. 5-6.

process (33). It is also interesting to note that he seems no longer worried about the fact that this is only *one* case.

Working now in the context of what he was later to call «the psychoanalytic movement», Freud discovered another use for the case history —the pedagogical. In his conclusion to the *Analysis of a Phobia in a Five-Year Old Boy* he wrote that,

Strictly speaking, I learnt nothing new from this analysis, nothing that I had not already been able to discover (though often less distinctly and more indirectly) from other patients analysed at a more advanced age. But the neuroses of these other patients could in every instance be traced back to the same infantile complexes that were revealed behind Hans's phobia. I am therefore tempted to claim for this neurosis of childhood the significance of being a type and a model, ... (34).

This is a particularly interesting passage, in view of the tremendous importance that case studies, and particularly Freud's case studies, have occupied in the curriculum of the psychoanalytic institutes. The account of Little Hans's phobia and its removal constitutes the first psychoanalytic case study written largely for didactic reasons (35). Of course Freud had called his account of Dora's analysis «suitable as a first introductory publication», but with the scientific reading public at large in mind. In the case of Dora Freud wanted to demonstrate his understanding of hysteria; in that of Little Hans he wanted to show psychoanalysts how psychoanalysis proceeds and that the sexual aetiology of adult neuroses could be confirmed in the analysis of a child. The different audience that Freud envisioned is crucial to the evolution of his thinking about the value of his case studies.

Freud's report on his analysis of the Rat Man, *Notes upon a Case of Obsessional Neurosis*, confirms this impression about the intended readership to which Freud now directed his case studies (36). The case of

(33) Cf. JONES, E. (1955), *The Life and Work of Sigmund Freud*, vol. 2, pp. 258-262.

(34) *Standard Edition*, vol. 10, p. 147.

(35) It was also the first child analysis, a practice later developed along slightly different lines by Melanie Klein. See her *Psychoanalysis of Children* (London, Hogarth, 1932) and especially her *Narrative of a Child Analysis* (London, Hogarth).

(36) The post-Freudian psychoanalytic literature on the case of the Rat Man is abundant. See KANZER, M. (1952), The Transference Neurosis of the Rat Man, *Psychoanalytic Quarterly*, 21, 181-189; ZETZEL, E. R. (1966), 1965: Additional Note upon a Case of obsessional Neurosis: Freud 1909, *International Journal of Psycho-Analysis*, 47, 123-129; MEYERSON, P. G. (1966), Comments on Dr. Zetzel's Paper, *ibid.*, pp. 139-142; LIPTON, S. D. (1971), Freud's Analysis of the Rat Man considered as a technical Paradigm, abstracted in *Bulletin of the Philadelphia Association for Psychoanalysis*, 21, 179-183; GEDO, J. E.; GOLDBERG, A. (1973), *Models of the Mind*, pp. 111-114; and BEIGIER, J. S. (1975), A Commentary on Freud's Treatment of the Rat Man, *The Annual of Psychoanalysis*, 3, 271-286.

the Rat Man is less an illustration of a theoretical hypothesis than the case of Little Hans, and more an attempt to explore new ground, but it is therefore even more directly oriented to the psychoanalytic readership of the *Jahrbuch*. In noting the incompleteness of his account of the case, Freud explained that he had «not yet succeeded in completely penetrating the complicated texture of a *severe* case of obsessional neurosis». Furthermore, Freud noted that obsessional neuroses were far less easy to understand than cases of hysteria. This was paradoxical, since obsessional neuroses constitute not only «a dialect of the language of hysteria», but «a dialect in which we ought to be able to find our way about more easily, since it is more nearly related to the forms of expression adopted by our conscious thought than is the language of hysteria». He suggested that the difficulty was due to the fact that fewer cases of obsessional neurosis had presented themselves for analysis, such neurotics finding it possible to «dissimulate their condition in daily life». In any case, obsessional neuroses constituted a pathology on the frontier of psychoanalysis, much like hysteria had been an anomaly in neuropathology when Freud began to study it. This is what made Freud feel justified in communicating his insights to the psychoanalytic community.

In these circumstances there is no alternative but to report the facts in the imperfect and incomplete fashion in which they are known and in which it is legitimate to communicate them. The crumbs of knowledge offered in these pages, though they have been laboriously enough collected, may not in themselves prove very satisfying; but they may serve as a starting-point for the work of other investigators, and common endeavour may bring the success which is perhaps beyond the reach of individual effort (37).

With this I believe we begin to see another pattern in Freud's thinking about his case studies: they are exemplary investigations, not necessarily solutions, to problems of psychopathology. He now thought of his case studies as parts of a scientific research literature to which other psychoanalysts would obviously contribute. Even his imperfect and incomplete case studies served to mark out the territory of future psychoanalytic investigations.

In his conclusion to the same case, he expressed his wish that «[al]though my communication is incomplete in every sense, it may at least stimulate other workers to throw more light upon the obsessional neurosis by a deeper investigation of the subject». Freud may have been given to false modesty on occasion, but this is not one of those

(37) *Standard Edition*, vol. 10, pp. 156-157.

occasions. He was concerned here with the very difficult problem of demarcating obsessional neuroses from cases of hysteria, as we see from the same passage where he also states very vaguely that «What is characteristic of this neurosis —what differentiates it from hysteria— is not, in my opinion, to be found in instinctual life but in the psychological field» (38). This postulate remained to be further specified by other workers who would analyze cases of obsessional neurosis. Thus we see that Freud's diminished anxiety about the incompleteness of his cases and his confidence in the cooperation of his psychoanalytic followers go hand in hand. The case history had by this time achieved the status of a genre peculiarly suited to communication among psychoanalysts. Freud clearly saw both of his cases published in 1909 as a part of an ongoing process of psychoanalytic research. They were not merely places where acquired insight and knowledge could be deposited (for the benefit of the world at large), but a genre which fomented further research by investigators who had already gained a conviction of the value of psychoanalysis.

Freud said little enough about the transference in these two case studies published in 1909, but that may be precisely because these are cases written for psychoanalysts and psychoanalysts understood this matter by then. There is good evidence in remarks he made for non-psychoanalysts on the subject of transference in the same year, that he believed that experience in analysis was a prerequisite for the appreciation of the validity of insights derived from psychoanalysis. In his *Five Lectures on Psychoanalysis* given at Clark University in 1909, Freud stressed that the experience of transference

plays a decisive part in bringing conviction not only to the patient but also to the physician. I know it to be true of all my followers that they were only convinced of the correctness of my assertions on the pathogenesis of the neuroses by their experiences with transference; and I can very well understand that such certainty of judgement cannot be attained before one has carried out psycho-analyses and has oneself observed the workings of transference (39).

This conviction that his followers had verified the value of his insights by their own experience with the transferences of their own patients must be seen as an important part of the basis of his new attitude about case studies.

The experience of transference that Freud now began to presuppose in the readers of his case studies can be viewed with extreme skepticism

(38) *Ibid.*, vol. 10, p. 248. Cf. JONES, E. (1955), *The Life and Work of Sigmund Freud*, vol. 2, p. 264.

(39) *Ibid.*, vol. 11, pp. 51-52.

or it can be casually accepted as specialized training. It can be viewed as no more than the psychoanalytic analogue of the prerequisite training presupposed in the scientific reports of every other specialized discipline, from psychics to statistical demography. Or, experience with the transference in the prospective psychoanalyst's own analysis and in the supervised analysis of his first patients can be taken as a rite of initiation bound to produce believers and thus a highly unscientific community of investigators. It seems to me that there is at least a modicum of truth in both of these positions, and furthermore, that there is little prospect of resolving the issue to the satisfaction of all those who adopt the opposed attitudes. This much must be admitted. In fact, the two positions form the parameters that give rise to the question of psychoanalytic knowledge. For if psychoanalytic training, analysis, etc., is *both* scientific training and religious rite of initiation, then psychoanalysis is *neither* simply science *nor* religious pseudo-science.

What strikes me as more interesting, however, is that this experience with the transference in the analytic situation that Freud prescribed might be the complement of the case studies in defining the peculiar nature of psychoanalytic knowledge. For I began by noting that psychoanalytic knowledge seemed anomalous in being located in case studies. Now in the process of demonstrating the evolution of Freud's own thinking toward this view, we have discovered that experience with the transference is a prerequisite to understanding the cases Freud reported. The transference seems to be the particular kind of «personal knowledge» required of the psychoanalyst. But unlike the many examples of personal knowledge given by Polanyi in his book by that name, the transference is only useful when the psychoanalyst is acutely conscious of it. Unlike the many skills that we learn without being able to describe them adequately or explain them theoretically, and by definition unlike tacit knowledge, transference is a kind of knowledge that depends upon the psychoanalyst's ability to articulate it. Thus it may be that the transference is more thoroughly «personal knowledge» than the skills that other scientists learn by empathy and imitation from their teachers. It seems to constitute a form of intersubjectivity that can only be defined as articulated «implicity», or, in psychoanalytic terms, the unconscious made conscious. This would seem to be what makes it both highly subjective and yet a shared medium of communication that constitutes the basis of the psychoanalytic community of investigation.

IV. *THE SCHREBER CASE*

Freud's analysis of Daniel Paul Schreber's delusion was published in the *Jahrbuch für psychoanalytische und psychopathologische Forschungen* in 1911.

This essay, *Psychoanalytic Notes on an Autobiographical Account of a Case of Paranoia*, is not a case study in the same sense as the others I am considering here, inasmuch as Freud had no personal contact with Schreber, nor did Schreber himself have any experience with psychoanalytic therapy (as even Little Hans had). Schreber had, however, written an elaborate account of his mental illness, and published it as his *Memoirs of a Mental Patient* (40). Freud used these published memoirs as the basis of his analysis. Nonetheless, the case is again presented primarily to psychoanalysts rather than to the medical world at large. Thus we may note that by this time Freud's thinking on his case studies had progressed so far that he made no rhetorical use of the fact that the patient's autobiographical account was in the public domain. He might have argued that this would give his analysis an objectivity that his earlier case studies did not possess, opening it to the review of other professionals interested in paranoia. But he was no longer concerned in his case studies with establishing his greater capacity to deal therapeutically with a particular form of mental illness. He was still very much concerned that other psychoanalysts should read the case and assimilate his analysis of it, however. In this respect, Freud's case history of Schreber bears some similarity to his treatment of Wilhelm Jensen's story «Gradiva», which also verged upon becoming a case study of its author (41).

Freud's most general aim in writing his account of Schreber's illness was to establish the nature of paranoia and its location among other psychoanalytically understood mental disorders. As he had been at pains to distinguish obsessional neuroses from hysteria in his account of the analysis of the Rat Man, in his essay on Schreber Freud wanted to establish paranoia as a diagnostic category by penetrating to its defining features too (42). All of this ought to remind us, I think, of his original anticipation in *Studies on Hysteria*, of «the directing hand which shall set up boundary marks» among the various mental disorders. But the difficulties in the area of paranoia were particularly grievous and led to the fact that Freud was compelled to base his discussion of this disorder upon a written account rather than an analysis. People suffering from paranoid delusions were not apt to submit to psychoanalysis, a process

(40) *Denkwürdigkeiten eines Nervenkranken* (Leipzig, Mutze, 1903), an English translation was made by Ida Macalpine and Richard Hunter and published with an introduction as *Memoirs of my Nervous Illness* (London, Dawson, 1955).

(41) «Delusions and Dreams in Jensen's *Gradiva*» (1907), *Standard Edition*, vol. 9, pp. 7-95.

(42) On which, see chapter three of the essay, «On the Mechanism of Paranoia», *Standard Edition*, vol. 12, pp. 59-79.

which would naturally elicit all their fears of persecution and cause them to ascribe the persecutions to the psychoanalyst. Having virtually no paranoid patients, therefore, and retaining those that he did encounter for only the very briefest periods, Freud found himself forced to rely upon the serendipitously published account of Schreber. Thus his apologies for the fact that under these circumstances he was not able to analyze the transferences of the patient in the now well established analytic context (43). In view of all of these difficulties, it is all the more interesting and revealing that Freud should have chosen to convey his insights on paranoia in the form of a case history.

The Schreber case seems to demonstrate the significance of case studies as the privileged genre of psychoanalytic knowledge in a negative way. Freud found it natural to make his arguments about paranoia in the context of this case study, even without transference, free association or even any personal contact with the subject, because no other mode of reporting constituted a plausible alternative. To be sure, the inclination Freud obviously felt to anchor his thinking on paranoia in Schreber's memoirs may stem as much from the unusually florid nature of the pathology of this particular case as from the impossibility of giving a convincing account of paranoia in a more abstract mode. But whatever the mixture of motives, the incontestible fact is that Freud's report on the Schreber case has remained the cornerstone of psychoanalytic thinking on paranoia to this day (44).

In his «Attempts at Interpretation», the second chapter of the essay on Schreber, Freud noted that his readers would «only follow him as far as their own familiarity with analytic technique will allow them» (45). But the case is a good illustration of how Freud's psychoanalytic colleagues did in fact follow him and by their own attention to Schreber's memoirs amplify the conclusions that could be drawn from a single case study. A year after his publication of the essay on Schreber, Freud noted that,

By a happy chance the same issue of this periodical [the *Jahrbuch*] as that in which my own paper appeared showed that the attention of some other contributors had been directed to Schreber's autobiography, and made it

(43) *Standard Edition*, vol. 12, p. 9.

(44) Of the enormous literature on the Schreber case, see especially the book by NIEDERLAND, W. G. (1974), *The Schreber Case psychoanalytic Profile of a Paranoid Personality*, New York, Quadrangle, which contains good bibliographies of earlier work on Schreber. There was, for instance, a Symposium on the case, papers from which were published in the *International Journal of Psycho-Analysis*, 44 (1963), edited by Philip M. Kitay. Other recent work on Schreber is represented by GEDO, J. E.; GOLDBERG, A. (1973), *Models of the Mind*, pp. 125-134; and MEISSNER, W. W. (1979), Schreber and the Paranoid Process, *The Annual of Psychoanalysis*, 7, 3-40.

(45) *Standard Edition*, vol. 12, pp. 36-37.

easy to guess how much more material remains to be gathered from the symbolic content of the phantasies and delusions of this gifted paranoid (46).

Of course Ernest Jones and other writers since him have suggested that Freud's remarks about paranoia in his account of the Schreber case sowed the seeds of the soon to be accomplished disassociation of Carl Jung and his followers from Freud, and the eventual disaffection of Sandor Ferenczi (47). But so far from suggesting that Freud's essay failed in its role of exemplar to help maintain the coherence of the psychoanalytic community of investigation, this demonstrates the power of the case study to define the boundaries of the nascent discipline. Reading Freud's case studies, it seems, not only guided and educated Freud's willing followers, but could also serve to separate less orthodox analysts from the movement. In fact, Freud's next case study would be even more important in separating errant analysts.

V. THE WOLF MAN

Freud's analysis of the case of the Wolf Man, his last lengthy case study, was written in the winter of 1914-15, entitled *From the History of an Infantile Neurosis* (1918), and published in his *Kleine Schriften*, since the war had interrupted the publication of the *Jahrbuch* for which it was intended. It quite elaborately illustrates the fact that Freud had come to use his case studies to educate and influence the thinking of his fellow psychoanalysts rather than the medical world at large. In fact, Freud used the case of the Wolf Man to arbitrate his differences with Adler and especially with Jung. As he noted in regard to the context of his writing the case,

At that time I was still freshly under the impression of the twisted re-interpretations which C. G. Jung and Alfred Adler were endeavoring to give to the findings of psychoanalysis. This paper is therefore connected with my essay "On the History of the Psycho-Analytic Movement" which was published in the *Jahrbuch der Psychoanalyse* in 1914. It supplements the polemic contained in that essay, which is in its essence of a personal character, by an objective estimation of the analytic material (48).

It should not escape our notice that in linking this essay «On the History of the Psychoanalytic Movement» to the case of the Wolf Man here, Freud calls the latter «objective» and implies that the former is more

(46) *Ibid.*, vol. 12, p. 80.

(47) JONES, E. (1955), *The Life and Work of Sigmund Freud*, vol. 2, pp. 268-269.

(48) *Standard Edition*, vol. 17, p. 7.

subjective, in spite of the fact that it constitutes a much more thorough overview of the theoretical issues separating him from Jung and Adler.

As I mentioned at the outset, the case study of the Wolf Man is Freud's most complicated and intricately argued case. It is interesting that he would use the case both to demonstrate his technique to his followers and to define the boundaries of psychoanalysis by showing how Jung and Adler had excluded themselves from the group of proper practitioners. But this only further extends our appreciation of the degree to which he relied upon case studies to establish his ideas. The language used in this introductory passage confirms another impression we have drawn from earlier case studies, namely that Freud had ceased to worry about the lack of objectivity inherent in the genre and about the fact that cases were single instances. The case as a whole, furthermore, contains some of Freud's most explicit statements on the value and significance of case studies.

One of the central issues of Freud's account of the Wolf Man's infantile neurosis is closely related to a central theme of the Dora case: the importance of «libidinal motive forces» in psychopathology. At the same time, the difference in Freud's conception of his readers and his critics in the two cases illustrates how he thought psychoanalysis had progressed in establishing itself as an autonomous scientific discipline.

People were content formerly to dispute the reality of the facts which are asserted by analysis; and for this purpose the best technique seemed to be to avoid examining them. That procedure appears to be slowly exhausting itself; and people are now adopting another plan — of recognizing the facts, but of eliminating, by means of twisted interpretations, the consequences that follow from them, so that the critics can still ward off the objectionable novelties as efficiently as ever. The study of children's neuroses exposes the complete inadequacy of these shallow or high-handed attempts at re-interpretation. It shows the predominant part that is played in the formation of neuroses by those libidinal motive forces which are so eagerly disavowed, and reveals the absence of any aspirations towards remote cultural aims, of which the child still knows nothing, and which cannot therefore be of any significance for him (49).

The criticisms Freud had had to confront at the time he published the case of Dora were based on resistance to the very idea of infantile sexuality, in the medical profession and the population at large. The critics Freud had in mind in 1914-15 were psychoanalytic initiates, prominent exponents of psychoanalysis who had indulged in reinterpretations of the data of infantile sexuality that subjected it to other

(49) *Ibid.*, vol. 17, p. 9.

factors, such as the need for power in the case of Adler, or the cultural values of the collective unconscious in the case of Jung.

Freud said substantially the same thing about his new critics in another passage of his essay on the Wolf Man, a passage that I cite for the way in which it indicates how he has abandoned the attempt to convince the skeptical.

The whale and the polar bear, it has been said, cannot wage war on each other, for since each is confined to his own element they cannot meet. It is just as impossible for me to argue with workers in the field of psychology or of the neuroses who do not recognize the postulates of psycho-analysis and who look on its results as artefacts. But during the last few years there has grown up another kind of opposition as well, among people who, in their own opinion at all events, take their stand upon the ground of analysis, who do not dispute its technique or results, but who merely think themselves justified in drawing other conclusions from the same material and in submitting it to other interpretations.

Hard on the heels of this rather literary version of his assertion of disciplinary orthodoxy *vis à vis* Adler and Jung, Freud states more explicitly than in any other place I know of that the presentation of case studies is superior to theoretical debate.

As a rule, however, theoretical controversy is unfruitful. No sooner has one begun to depart from the material on which one ought to be relying, than one runs the risk of becoming intoxicated with one's own assertions, and in the end, of supporting opinions which any observation would have contradicted. For this reason it seems to me to be incomparably more useful to combat dissentient interpretations by testing them upon particular cases and problems (50).

This passage speaks very eloquently for itself.

The disagreement over the centrality of infantile sexuality between Freud on the one hand and Jung and Adler on the other led Freud to focus almost exclusively upon the Wolf Man's infantile neurosis, the course of which had been terminated in childhood. This complicated matters, since it necessitated «taking into account the distortion and refurbishing to which a person's own past is subjected when it is looked back upon from a later period». Noting that the analysis of children might yield more convincing results, he asserted that the procedure he had been forced to adopt in the case of the Wolf Man was more instructive, precisely on account of the difficulties involved (51).

(50) *Ibid.*, vol. 17, p. 48.

(51) *Ibid.*, vol. 17, p. 9.

His concentration upon the Wolf Man's infantile neurosis thus involved him once again in reporting a severe case «only in a fragmentary manner», as he noted in a paranthetical remark at the very outset (52). This had become a regular feature of Freud's case studies, which, as we shall see, we can not think of as case *histories* in the traditional sense. As Freud noted in his final chapter, significantly entitled not «Conclusion» but «Recapitulations and Problems», «it must be recognized that everything cannot be learnt from a single case and that everything cannot be decided by it; we must content ourselves with exploiting whatever it may happen to show most clearly» (53). Freud's emphasis upon the pedagogic value of his case studies is accompanied here by a statement about why problematic and severe cases have the greatest instructive value.

Analyses which lead to a favourable conclusion in a short time are of value in ministering to the therapist's self-esteem and substantiate the medical importance of psycho-analysis; but they remain for the most part insignificant as regards the advancement of scientific knowledge. Nothing new is learnt from them. In fact they only succeed so quickly because everything that was necessary for their accomplishment was already known. Something new can only be gained from analyses that present special difficulties, and to the overcoming of these a great deal of time has to be devoted. Only in such cases do we succeed in descending into the deepest and most primitive strata of mental development and in gaining from there solutions for the problems of the later formations (54).

Here we see how Freud would explain the fact that his published «exemplars» are never completely satisfying resolutions of all the problems involved. Only from the most severe cases where the deepest strata are brought to light does one learn anything new, and such severe and difficult cases are often so complicated that one could hardly hope to resolve them completely and restore such a patient to permanently unproblematic emotional stability. The Wolf Man's later difficulties and his reanalysis are thus not a reproach to Freud's treatment of him, but a confirmation of Freud's expectations (55). For Freud clearly did not publish his account of this case because it was such a neat piece of therapy; it was rather a particularly revealing investigation of the unconscious forces in a severely pathological personality, rich in difficulty.

(52) *Ibid.*, vol. 17, p. 7.

(53) *Ibid.*, vol. 17, p. 105.

(54) *Ibid.*, vol. 17, p. 10.

(55) One interesting aspect of the case of the Wolf Man is that later psychoanalysts have not only returned to write about the case, but the Wolf Man himself was reanalyzed, first by Freud, and later by Ruth Mack Brunswick and Muriel Gardiner. See Muriel Gardiner

The case of the Wolf Man was rich in these «fertile difficulties», as Freud called them, but for that reason complicated the presentation even more than his earlier cases. In the final portion of the essay Freud acknowledged his fear that his readers would not be able to follow his account of the origin and development of the Wolf Man's illness. «The description of such early phases and of such deep strata of mental life has been a task which has never before been attacked» (56), he wrote, but preferred to make this attempt at the risk of overstraining his own expository powers in order to further the exploration of this territory of infantile sexuality. Earlier in the essay, at the beginning of his «General Survey of the Patient's Environment and of the History of the Case», Freud had more general things to say about reporting case studies.

I am unable to give either a purely historical or a purely thematic account of my patient's story; I can write a history neither of the treatment nor of the illness, but I shall find myself obliged to combine the two methods of presentation. It is well known that no means has been found of in any way introducing into the reproduction of an analysis the sense of conviction which results from the analysis itself. Exhaustive verbatim reports of the proceedings during the hours of analysis would certainly be of no help at all ...

Freud appended to this general statement a reminder that «Analyses such as this are not published in order to produce conviction in the minds of those whose attitude has hitherto been recusant and sceptical» (57); but the main point was that a psychoanalytic case study can never be a simple narration, for many stories intersect in any analysis. There were of course more stories than the history of the illness and the treatment involved in Freud's analysis. A careful accounting would have to include at least 1) the patient's actual life history, 2) the patient's narration of his life, 3) the verbatim history of the analysis, 4) the history of the psychoanalyst's interpretations, 5) the life history reconstructed by the psychoanalyst, and 6) the history of the patient's acquisition of self-knowledge.

(ed.), *The Wolf Man*, with «The case of the Wolf Man» by Sigmund Freud, and a Supplement by Ruth Mack Brunswick, and a Forward by Anna Freud (New York, Basic Books, 1971). Other studies include OFFENKRANZ, W.; TOBIN, A. (1973), Problems of the therapeutic Alliance: Freud and the Wolf Man, *International Journal of Psycho-Analysis*, 54, 75-78; BLUM, H. P. (1974), The Borderline Childhood of the Wolf Man, *Journal of the American Psychoanalytic Association*, 22, 721-742; MEISSNER, W. W. (1977), The Wolf Man and the Paranoid Process, *The Annual of Psychoanalysis*, 5, 23-74; and GEDO, J. E.; GOLDBERG, A. (1973), *Models of the Mind*, pp. 115-124.

(56) *Standard Edition*, vol. 17, p. 104.

(57) *Ibid.*, vol. 17, p. 13.

The tortuous complexity of Freud's essay on the Wolf Man illustrates the violence that Freud's investigations did to the narrative style that had been current in the late nineteenth century. The 115 page essay is divided into an unprecedented nine parts: The only part that surveys the history of the patient's illness occupies scarcely five pages. One of the nine parts is anomalously entitled «A Few Discussions», and the conclusion, as I have already indicated, «Recapitulations and Problems». This complexity and awkwardness is the result, I believe, of a long process during which Freud recognized more thoroughly in each case study he wrote that intricacy was precisely the virtue of this genre of scientific reporting that made it the ideal vehicle for psychoanalytic knowledge. But it is extremely important to remember and I must emphasize that this complexity and awkwardness was not some disadvantage inherent in case studies of psychoanalyses, a disadvantage which could be overcome if psychoanalytic knowledge could only be translated into theoretical propositions. Ernest Jones said the case was «assuredly the best of the series», and I believe most psychoanalysts would agree (58). The complexity was the very strength of the psychoanalytic case study that lent it to the propagation of new insights among those who already understood the psychoanalytic process of investigation. This complexity and intricacy was also, and not incidentally, an important part of the Freudian world-view that migrated into fiction in the early twentieth century in the form of stream-of-consciousness writing and other departures from nineteenth century narrative form (59).

One additional word must be said to link this complexity of reporting in which the «story» has been lost back to the polemical purposes of the essay. Why should Freud have produced such tortuous logic in a polemic against Adler and Jung? Wouldn't clarity and simplicity have been more to his advantage? For those of us who read Freud's cases from without the discourse of psychoanalysis, the answer to this last question would surely be yes. But if I am right in suggesting that Freud's case studies are not only psychoanalytic exemplars, but a particular sort of personal knowledge as well, then the answer for psychoanalysts is no, and his tortuous thinking is a positive advantage. For Freud seems not to have expected that he would convince anyone who did not already agree with him about infantile sexuality — he says as

(58) JONES, E. (1955), *The Life and Work of Sigmund Freud*, vol. 2, p. 274.

(59) I would not want this statement to imply an assertion that Freud invented a specifically literary genre, only that his difficulties with the narrative form were part of what he discovered as his own understanding of his insights matured. This seems at least parallel to what developed in prose fiction.

much. But he did expect to deepen the conviction and insight of those who did agree and want to follow him further in psychoanalysis. For them, the psychoanalysts willing to study Freud's cases as exemplary analyses and extend their command over the tacit knowledge that inhered in Freud's practice, the case of the Wolf Man led them once again beyond the frontier.

This concludes my review of Freud's case studies. It bears remarking that Freud did write two more briefer case studies, published in 1915 and 1920 (60), but these do not evidence any advance in Freud's thinking on this genre. It may be thought a problem, however, that Freud wrote so few case studies after he had finally come to his rather definite conclusions about their value in 1914-1915. There would necessarily be many components to an explanation of this superficially surprising fact. I shall merely list several without trying to evaluate their relative importance. By the time Freud published his analysis of the Wolf Man's infantile neurosis the psychoanalytic movement was definitely established as a discipline. Freud's followers had begun to present and publish their own case studies and Freud devoted considerable energy to criticizing and commenting upon their work. Several psychoanalytic journal had been founded to circulate such writings. The boundaries of psychoanalysis were set and the major psychoanalytic apostates definitively separated from the movement. And Freud himself, relaxing in this atmosphere of long awaited security, turned to more publicistic projects designed to secure wider recognition of psychoanalysis; and, one may surmise without deprecation, to enlarge his position in the history of occidental thinking generally. It may also be that his old affection for theoretical formulations and his desire to make his science respectable to other positivistic scientists reasserted itself then (61). In any case, the role of the case study in the psychoanalytic community itself was secure, and as I suggested at the outset, serves to this time as the methodological anchor that lends psychoanalysis its enduring coherence as a scientific community.

CONCLUSION:

PSYCHOANALYSIS, DISCOURSE, AND THE FISSURES IN KNOWLEDGE

By the time psychoanalysis grew into an organized discipline of research, Freud's case studies had taken on the status of psychoanalytic

(60) «A Case of Paranoia ...», *Standard Edition*, vol. 14, pp. 261-272; and «The Psychogenesis of a Case of Homosexuality in a Woman», vol. 18, pp. 145-172.

(61) See Freud's «Autobiographical Study», *Standard Edition*, vol. 20.

knowledge par excellence. Not that they constituted the *only* kind of psychoanalytic knowledge. As a first distinction, one might contrast this *knowledge for psychoanalysts* deposited in the case studies with the *knowledge of psychoanalysis* that Freud purveyed in his more expository and theoretical works (62). A detailed investigation would undoubtedly yield many further distinctions based upon differences in the ways in which psychoanalytic knowledge is transmitted and the uses to which it is put (63). There are, in other words, many discourses of psychoanalysis. But we do not need to begin by knowing everything; i.e., we do not need to make an exhaustive study of all the differing discourses of psychoanalysis before we can begin to distinguish psychoanalytic knowledge from other forms of knowledge. If case studies are indeed the genre of *peculiarly* psychoanalytic knowledge—the kernel of *Knowledge for psychoanalysts*—we should begin our description of psychoanalysis and psychoanalytic knowledge here.

Freud's *oeuvre* opened the discourse of psychoanalysis, but it seems that all the fissures that have developed in psychoanalytic discourse since Freud are prefigured in the fissures of Freud's writing. I have traced only one of these fissures, showing that behind the explicit difference between the genres of Freud's writing intended for different audiences there lay a tension between the positivistic notions of science that Freud had imbibed with his medical education, in which theory predominated, and another notion of knowledge that arose out of or was produced by the psychoanalytic mode of knowing itself. The view of knowledge implied in Freud's increasing reliance upon case studies, the view that can be compared to a combination of Kuhn's exemplars and Polanyi's personal knowledge, never fully triumphed over Freud's commitment to theory and its veracity. But even though this view never came to predominate in Freud's mind, we are justified in according it the status of a distinguishing feature of psychoanalytic knowledge. For it represents the most radical departure from the tradition of positivistic scientific discourse in Freud's *oeuvre*, and we may assume that Freud

(62) I do not allude to Clifford Geertz's distinction between «model of» and «model for» here. That is quite different.

(63) As far as the knowledge of psychoanalysis that is had among non-psychoanalysts is concerned, psychoanalysis is obviously one thing for the psychoanalytic patient who has come to know psychoanalysis through his or her own experience in analysis and another for the academic student of psychoanalysis who has read Freud's treatises by not experienced analysis. And similarly, the psychoanalytic knowledge acquired by psychoanalysts has many other components than merely the case studies, e.g., what they learn from their own training analyses, what they appreciate about the concerns of their fellow analysts, what they read about analysis, etc.

would not have permitted himself to be drawn into such a departure if his method and the exigency of communicating with his followers had not pulled him in that direction.

Ironically, this story of how Freud came to view his case studies as the locus of peculiarly psychoanalytic knowledge—in spite of himself and his positivistic training—is a story that could appear in an unreconstructed history of science. Without much exaggeration, we can say that the data forced Freud to this view. But Freud's data were not the inert data of the nineteenth century physical science. His data were patients, their dreams, their slips of the tongue, and ultimately their transferences and Freud's countertransferences. Freud's ambition to solve the problem of hysteria gradually led him to transform the intrusive hypnotic therapy he learned from the French into a method of intersubjectivity. In this he allowed himself to be instructed by the patients, starting with Anna O. But this method of intersubjectivity—analyzing transferences—yielded knowledge that Freud found it difficult if not impossible to convey to readers in the usual genres of scientific communication. He found himself organizing a community of investigators who experienced transference in their own analyses (often with Freud) and in their analyses of patients. For this community, the case study proved the ideal genre of communication. Thus Freud was forced to his novel view of knowledge by two strictly social configurations: his relationships with his patients and with the scientific communities, first of medicine and physiology and later of psychoanalysis.

This brings us squarely back to the assertion that the question of psychoanalytic knowledge is a question of discourse. For the two relationships that pulled Freud toward his view of psychoanalysis as personal, exemplary knowledge in the case studies turn out to be the irreducible axes of psychoanalytic discourse. The discourse of analyst and patient *is* psychoanalysis in the sense of a practice, and the discourse of analyst and fellow analysts *is* psychoanalysis in the sense of a discipline. Without these two levels of discourse any other is unthinkable. Every other discourse of psychoanalysis derives ultimately from one or both of these.

Freud's case studies (and the case studies published since) are of course not identical with these two levels of discourse. But of all the writings of Freud and his followers, it is in the case studies that the discourses of analyst/analysand and analyst/fellow analyst are most fully precipitated. The discourse of analyst/analysand cannot by its very nature be replicated for a reader. As Freud noted in his report on the case of the Wolf Man, a verbatim transcript of the words exchanged in a

whole analysis would be completely inadequate for representing that analysis to a third party (64). But rather than transcribe analyses, Freud gradually devised the case study as the vehicle and nearest approximation of the intersubjectivity that obtains in an analysis. This approximation can be deciphered by other analysts who have experienced transference in the analytic situation. Thus the discourse of analyst/fellow analyst is much more clearly reflected in the case studies, but again, the relationship is not one of equivalence. The case studies are a sediment of this discourse, whereas they are translations of the discourse analyst/analysand. But in the case studies we hear only one voice in the discourse between analysts, the voice of Freud. And we can only attempt to infer and extrapolate from that voice the nature of the discourse among psychoanalysts. With these reservations we can still conclude that the case studies constitute the most characteristic and specifically psychoanalytic knowledge in the Freudian *oeuvre* (65).

Looking back then, what does the evolution of Freud's attitude as he wrote the case studies reveal? Freud's awareness of the significance of the case study emerged hand in hand with his realization that psychoanalysis constituted a new kind of knowledge. Early in his career as a psychoanalyst, when Freud thought his insights and discoveries to be valuable merely as increments of familiar scientific knowledge, he was still oblivious to the importance of his reports on particular cases, even embarrassed by them. Only when he began to realize that his new insights might constitute a new *kind* of knowledge did he begin to realize that they required a novel sort of reportage. Thus the case studies lead us not only to an appreciation of an important fissure in psychoanalysis—the distinction between knowledge for psychoanalysts and the knowledge of psychoanalysis—they lead us to an awareness of the fissures in knowledge in general. In its most characteristic form, psychoanalytic knowledge cannot be adequately described as science or pseudoscience. As a discourse, one discourse or set of discourses among many others, it must be described as one of many kinds of knowledge. To confront the question of psychoanalytic knowledge, we must bracket the question of science.

(64) *Standard Edition*, vol. 17, p. 13.

(65) The tensions between the discourse of the analyst with himself, the analyst with the patient and analyst with fellow analyst constitute fissures within the discourse of the case studies itself. To examine these a specifically literary method would be necessary. The place to start would be with Gerard Genette, «Frontières du récit», *Figures II* (Paris, Seuil, 1969), pp. 49-69, and «Discours du récit», *Figures III* (Paris, Seuil, 1972), pp. 67-282.

Early in his career Freud was positively intoxicated by the aura of natural science and his highest ambition was to make his mark as a scientist. As he noted in his *Autobiographical Study*, and other places, Freud had never been motivated by an inclination to be a healer or therapist. Curiosity and ambition led him to science (66). The role of scientific ambition is especially clear in *The Interpretation of Dreams* where he put forward such pseudo-theories as that every dream must be the fulfillment of a wish. Formulae of this sort have enabled philosophers of science like the neo-positivist Karl Popper to categorize psychoanalysis as a «pseudo-science». Popper claims that «no description whatsoever of any logically possible human behavior can be given which would turn out to be incompatible with the psychoanalytic theories of Freud» (67). Thus many of Freud's theories appear to be untestable and unscientific. This is a criticism that psychoanalysts find difficult if not impossible to answer. But I have tried to show that it is an inappropriate criticism. Certainly it is an uninteresting one (68). If I have been right in showing that the case study gradually emerged in Freud's work as the locus of the most specifically psychoanalytic knowledge, then criticism that supposes psychoanalysis to be located in theories is surely misplaced. That is not to suggest that psychoanalysis is immune to criticism, of course. A knowledge located in exemplary case studies and communicated as personal knowledge may have grave problems of its own, but they are different problems than those of theoretical sciences.

(66) See «Autobiographical Study», *Standard Edition*, vol. 20, pp. 8 and 71-72, and «The Question of Lay Analysis», *Standard Edition*, vol. 20, pp. 253-254.

(67) POPPER, K. R. (1972), *Objective Knowledge: an evolutionary Approach*, Oxford, Clarendon, p. 8.

(68) That may be why Popper has not pursued the matter further than in a few snide remarks about psychoanalysis in passing.