Kächele, H., Schachter, J., & Thomä, H. (Eds.). (2008). *From Psychoanalytic Narrative to Empirical Single Case Research. Implications for Psychoanalytic Practice.* New York: The Analytic Press.

3. THE SIGNIFICANCE OF THE CASE HISTORY IN CLINICAL PSYCHOANALYTIC RESEARCH[1]

Freud's Case Histories as a Methodological Paradigm

The discussion of psychoanalysis as a discipline has generated a host of quite controversial philosophical debates as we have sketched out in the preceding chapter. The more it enters into general awareness that psychoanalysis as a psychological system has exerted and will continue to exert a tremendous influence on the psychosocial profession and on contemporary culture generally, the more remarkable it seems that decades after its inception, some of the most basic concepts of this theoretical and practical system remain controversial (cf. Meehl, 1973, p. 104). Yet surely it would not be an exaggeration to speak of Freud's first attempt to explain neurotic symptoms in a fundamentally different way than his contemporaries, as a scientific revolution. Before Freud's attempts, hysterical symptoms were regarded by psychiatry as the result of a "degenerate constitution," the consequence of a somatic predisposition. Freud's critical contribution to the development of psychological research consisted in his formulation of two assumptions: that hysterical symptoms should be regarded primarily as psychic phenomena — though not necessarily conscious ones — and that as such they are to be viewed as comprehensible psychic structures. As Mayman (1973b) emphasizes, these postulates of psychologism and determinism remain the two most important postulates upon which psychoanalysis is based today.[2]

The introduction of these two assumptions, which went hand in hand with the development of a corresponding method of observation, represents a decisive turning point, a new methodological paradigm (Kuhn, 1962). It is one of the central paradoxes in the development of psychoanalytic theory and practice that while Freud has gone down in the

[1] Horst Kächele & Helmut Thomä. Adapted from Kächele (1981)
[2] Cf. Rapaport's discussion in his little-known lectures on psychoanalytic methodology (1944).

history of scientific theory as a significant and incisive methodologist,[3] the yield of empirical psychoanalytic research has only recently begun to bear fruits.

The fact that the insights of psychoanalysis were caught in the critically assailed cross-fire from the philosophy of science (see chap. 2) undoubtedly has to do with the nature of Freud's approach: the search for new hypotheses was far more important to him than painstaking examination of clinically verified information using empirical methods.

The continuous development of psychoanalytic theory over the forty-year course of psychoanalytic research that began with Freud himself, can be traced most clearly by following the history of central clinical concepts, such as that of anxiety (Compton 1972). Of course, not all concepts have always evolved to the current level of development in the field: some, like the theory of dreams, have remained almost unaltered over long stretches of time (Edelson, 1972). This lack of consistency first became apparent during initial attempts at systematization, as those of Rapaport (1960), and it has remained a peculiarity of psychoanalytic theorizing. What is generally known as "psychoanalytic theory" is in fact more like a research program comprising many loosely connected theories whose status must be evaluated quite variously in terms of the philosophy of science. There are, for example, psychoanalytic theories of memory, perception, attention, consciousness, action, feeling, concept formation and biographical development, to name but a few of the fundamental ones. These form the basis of the clinical theories, which themselves are conceived in a very loose fashion (compare, e.g., the theory of anxiety with that of narcissism or the theory of treatment, which would have to distinguish a theory of course from a theory of outcome). Moreover, the testing of each of the different components — the different sub-theories — is a separate task that must be approached with the most varied methodological approaches. In regard to the clinical theory of psychoanalysis — and it is only in this regard that we will deal with the relevant questions here — quite divergent views still exist on the methodology of hypothesis-testing research.

The point of contention here, between psychoanalysis and academic psychology, is how the classical psychoanalytic method is to be evaluated as a research instrument. Its clinical significance is not in the same measure at issue, nor is it so controversial in the theoretical discussion. In terms of scientific logic however it is apparent that the meeting place of research and therapy (Freud, 1926e) is still a living issue, inasmuch as the testing of hypotheses is still an aim that is pursued. Sarnoff (1971) unequivocally formulates the

[3] See also Kaplan (1964), whose textbook on scientific theory incorporates Freud's argument against premature formalization and strict definition of the central conceptual bases of a theory.

experimental psychologist's response to the frequent assertion that the psychoanalytic situation is a quasi-experimental one:

It does not logically follow that the conduct of psychoanalytic therapy is an ideal, necessary or sufficient method for the scientific testing of deductions from his (Freud's) conceptions of personality. Indeed, owing to the multitude of uncontrolled events that occur as patient and analyst interact within any psychoanalytic session, one can safely assert that such sessions cannot even minimally satisfy the scientific principle of control required to test a hypothesis deduced from a Freudian variable of personality. (p. 8)

It might seem logical to conclude from this that no single assertion based on the experience of the psychoanalytic setting can be accepted as valid until it has been verified experimentally. This view however is quite bluntly rejected by Kubie (1952):

> Many of these laboratory charades are pedestrian and limited demonstrations of things which have been proved over and over again in real life . . . Experimental facilities should not be wasted on issues which are already clearly proved and to which human bias alone continues to blind us. The experimentalist should rather take up where the naturalist leaves off. (p. 64)

Kubie goes on to compare this situation to the introduction of the microscope by Leuwenhoek, arguing that it is sufficient to look through the microscope of analysis to convince oneself of the validity of the contested questions. Also there is little doubt that certain elementary phenomena, upon which psychoanalytic theory is built, do not require experimental testing. The fact that there are two kinds of mental processes, primary and secondary, requires little or no interpretation; it can easily be made evident that dream states or drug-induced states "bring to the fore mental processes which do not abide by the laws of ordered logical thought" (Rapaport, 1960, p. 112). As soon one wishes to pass from these initial observations to more precise statements however, one must strike out upon new methodological paths. The great number of sometimes contradictory schools of psychoanalysis makes it obvious that the analytic method, as an observational instrument in the discipline of the social sciences, cannot readily be compared to the microscope or other natural science observational instruments. According to Rapaport (1960), the major body of positive evidence for psychoanalytic theory lies in the field of accumulated clinical observations:

> The first achievement of the system was a phenomenological one: it called attention to a vast array of phenomena and to the relations between them, and for the first time made these appear meaningful and amenable to rational consideration. (p. 111)

On the phenomenological plane of ordering and establishing relationships, Rapaport (1960) sees the accumulated clinical evidence as eminently positive testimony for psychoanalytic systems. In regard to the theoretical propositions of the system, however — e.g. the special theory of neurosis — there is no such assurance:

> Because a canon of clinical investigation is absent, much of the evidence for the theory remains phenomenological and anecdotal, even if its obviousness and bulk tend to lend it a semblance of objective validity. (1960, p. 111)

Thus the absence of an experimental canon of clinical investigation — not to be confused with clinical interpretative technique — appears to remain a central weakness in the testing of clinical research in psychoanalysis. "This makes it urgent to reinvestigate Freud's case studies with the aim of clarifying whether or not they can yield a canon of clinical research at the present stage of our knowledge" (1960, p. 111).

The present chapter takes up this call and examines Freud's case histories in terms of the didactic and scientific principles in their presentation. Our attempt will be to show that Freud aims simultaneously at ideographic and nomothetic aspects which lead to the creation of clinical types. In conclusion, the historical development of psychoanalytic scientific reporting will be characterized as a transition from case histories to individual case studies.

In spite of Rapaport's demand, little attention has been given to the case presentation as a means of scientific communication in psychoanalysis. For this reason it is of particular interest that several studies have turned to the Freudian case history in an attempt to clarify the scientific status of psychoanalysis. In Sherwood's (1969) logical analysis of the explanatory principles in psychoanalysis, the story of the Rat-Man Paul Lorenz occupies a central position. At the same time Sherwood does not fail to point to the peculiarity that "in perhaps no other field has so great a body of theory been built upon such a small public record of raw data" (p. 70).

Perrez (1972) analyzed the presentation of the Wolf-Man's infantile neurosis as to the formal logic of its structure. Both authors examine the validity of the steps of argumentation in the presentation of the cases (not questioning for the moment the validity of their content). While Sherwood is more interested in discovering which kind of logic[4] is appropriate for psychoanalysis generally, Perrez accepts only a generalizing nomothetic approach. Not surprisingly, in the process he finds gaps in the presentation, incomplete

[4] Schalmey's study (1977) appears to remain poorly known. Taking the Schreber case as an example, it analyzes the logic of argumentation and proof in psychoanalysis.

derivations and sketchily outlined explanations instead of complete explanations that would satisfy the requirements of the Hempel-Oppenheim schema (cf. Stegmüller, 1969; see chap. 2). This limited fulfillment of scientific requirements is also surely due to the fact that Perrez based his investigation on a case presentation. The implicit assumption that a published case history would provide a representative reflection of the actual occurrence and hence, that the scientific status of psychoanalysis could be determined by critical analysis of a *single* case history, appears problematic. No one has ever systematically investigated the relationship of the case presentation to the course of treatment it portrays. Hence it remains unclear if the incompleteness of a case history is due to the summary of the treatment itself or if the observational material in the treatment was insufficient. Furthermore, the choice of a case presentation that focuses on "refuting" the views then being put forth by Jung and Adler, forced a selective presentation in which more attention was given to the contested points while other uncontested assumptions were employed without examination.

Yet these objections do not invalidate Perrez' fundamental criticism. Rather we need to inquire how a description of the psychoanalytic process might be constituted to avoid the deficiencies of the classical case histories. From the start Freud himself was aware of the imperfections of his case histories. In his "Studies on Hysteria" we detect a note both of amazement and of self-justification in his remark that his case histories "read like short stories" (1895d, p. 160) and "lack the serious stamp of science." Yet in the very next sentence he also rejects any artistic ambitions: "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" (ibid). Even if Freud occupies a high rank as a writer of scientific prose — this is underscored by his receipt of the Goethe Prize in 1930 and in Walter Muschg's essay of the same year (1930) — the fact that he was in the position of portraying life histories and human destinies did not blind him to the huge gap that divided him from the poet:

> "I must now consider a further complication to which I should certainly give no space if I were as a man of letters engaged upon the creation of a mental state for a novel, instead of being a medical man engaged upon its dissection." (1905e, p. 50)

Freud's talents as a writer certainly contributed decisively to the development of the case history in the psychoanalytic context. Wittels, among others, reports in his biography of Freud: "Stekel informed me that Freud told him he would like to be a novelist someday so that he could bequeath to the world what his patients have told to him" (1924, p. 13). Freud, as Kris (1954) emphasizes, was in an intellectual conflict.

> A new and unpredented vista was opening before him - that of stating in scientific terms the conflicts of human psyche. It would have been tempting to base his excursion into this territory on intuitive understanding, to trace all case histories to their biographical roots, and to base all the insight on intuition, 'of the kind we are accustomed to having from imaginative writers'. (Kris 1954, p. 15)

The literary self-assurance Freud demonstrates in the presentation of biographical material, which first came into its own in his "Studies," inevitably made this temptation real and immediate. We know from his correspondence — the Freud-Fliess letters — that he was already able to penetrate literary motif-development psychologically. His analyses of two short stories by Conrad Ferdinand Meyer are the earliest attempts of this kind (Kris 1954, p. 15). The opposition of intuitive understanding and scientific explanation can be called the crux of the aforementioned conflict, and is by no means mitigated in theory or practice today. In 1928, Freud speaks of himself as one of those "who have to find their way through tormenting uncertainty and with restless groping," and compares himself with others to whom it is "vouchsafed . . . to salvage without effort from the whirlpool of their own feelings the deepest truths" (1928b, p. 133).

Is the essay form of presentation merely a consequence of the "nature of the object" of psychoanalysis?

In spite of the appearance that the case histories may give, in Kris' opinion there could never be any doubt on which side Freud stood: "He had been through the school of science, and it became his life work to base the new psychology on scientific methods (Kris 1954, p. 15). In his studies of Freud's methodology, Meissner (1971) describes clinical psychology as a science of subjectivity, as an attempt to grasp experience and its modification in a controlled way (p. 281). In the same vein Sherwood (1969) rhetorically asks his readers: "What is its (i.e., psychoanalysis' — author's note) subject matter; what is the principle focus of interest in this case" (p. 188)? According to Sherwood, Freud's attempt to explain the case history of the Rat-Man characterizes this patient as an individual human being:

> Freud and ourselves as latter-day observers are confronted by a single sick individual whose life story presents a variety of incongruities — events and attitudes demanding to be explained, to be brought within the framework of understandable human behavior. Freud, like the historian, is interested in a particular course of events, namely, an individual's history". (Sherwood 1969, p. 188)

However, this systematic determination of the aim of the individual case histories does not completely coincide with Freud's own intentions, for each case history contains unmistakable references to other patients with similar conflicts. Similarly, throughout we encounter comments regarding the general applicability of findings, as in this instance in "Wolf-Man":

> In order to derive fresh generalizations from what has thus been established with regard to the mechanisms and instincts, it would be essential to have at one's disposal numerous cases as thoroughly and deeply analyzed equally to the present one. (1918b, p. 105).

The decisive point in favor of Sherwood's accentuation seems to be that any new gain in knowledge about the individual case is possible only out of its totality. In this way the special methodological nature of clinical investigative technique, as it has developed, converges on the single case history — a fact that Meissner (1971) also emphasizes: "Analytic methodology is ultimately forced to rest upon the single case history" (p. 302). This insight determines the function of the case history as the explanation of singular events, in this way thematizing the ideographic element of the psychoanalytic narrative (Farrell 1981). The problem of determining the theoretical position of psychoanalysis is rooted in this complication, which was created by the introduction of the subject. This was already noted by Hartmann in the introduction to his historically significant book *Die Grundlagen der Psychoanalyse* (Foundations of Psychoanalysis, 1927):

> Historically, psychoanalytic psychology is characterized by having grown out of the seemingly unbridgeable gap that separated a scientific, chiefly experimental psychology of elementary psychical processes from the "intuitive" psychology of the writers and philosophers. The historical significance of psychoanalysis for psychology consists in its having made accessible to scientific contemplation those regions of psychical life that formerly had been relegated to occasional observation and to the psychological apercu, which is not only scientifically more or less irresponsible but also tends to make value judgments. (Hartmann, 1927, quote from 1972, p. 8)

To understand the significance of the case history in clinical psychoanalysis, it must be recalled that in the early decades of psychoanalytic research it was also an important medium of communication for psychoanalysts practicing essentially in isolation. This didactic aspect apparently had a much stronger conceptual influence on psychoanalytic

training programs and thus also on the training of later researchers than is generally realized (Tuckett, 1994).

The centrality of the case history in psychoanalytic training can easily be confirmed by studying the catalogue of lectures at a variety of psychoanalytic institutes. The six case histories that Freud presented in greater detail function here as introductory material to clinical practice in psychoanalysis; material that is worked through again and again by new students. As Jones says regarding the Dora case: "This first case history of Freud's has for years served as a model for students of psychoanalysis ..." (1955, p. 257). The close ties between therapy, research and training led to the creation of a traditional form of communication so that the short case report came to seem a natural form; initially its relevance to research was certainly brilliantly confirmed.

For this reason the problems that were gradually systematized by the developing empirical research of the social sciences, the problem of the reliability of clinical observation to name but one, were addressed only belatedly and hesitantly by the psychoanalytic research community. The six detailed Freudian case presentations had been raised to the level of paradigmatic models: "But these six essays of Freud's far excel, both in presentation and original content, anything any other analyst has attempted" (Jones, 1955, p. 255).

Even without this idealization it seems incomprehensible why at least the thoroughness and exactness of Freud's studies did not inspire a large number of further case histories that might be considered a treasury of psychoanalytic observation today. There were only a few attempts made to compose comprehensive clinical studies. Before we take up Rapaport's suggestion and consider several of Freud's case studies from the methodological point of view, we will mention several biographical points that we believe were of great significance in the development of the case history in Freud's work.

Freud's own training at first completely followed the paths dictated by his natural scientific studies at Brücke's Laboratory. His further training as a neuropathologist initially strengthened his empirical experimental orientation. Then, he began his theoretical separation from the Helmholtz school, in particularly starting with his "Aphasia" (Jones, 1953, p. 215). Jones goes on to point out however that while Freud had proven himself a good clinician, an extremely skillful histologist and an independent thinker, he was essentially unsuccessful in experimental physiology.

Charcot may be taken as the model for Freud's emphasis on well-rounded description[5]. Freud himself writes of him:

> As a teacher, Charcot was positively fascinating. Each of his lectures was a little work of art in construction and composition; it was perfect in form and made such an impression that for the rest of the day one could not get the sound of what he had said out of one's ears or the thought of what he had demonstrated out of one's mind. (Freud, 1893f, p. 17)

In his obituary, Freud especially stresses Charcot's clinical thrust, which he had particularly developed through his unique talent:

> He used to look again and again at the things he did not understand, to deepen his impression of them day by day, till suddenly an understanding of them dawned on him. In his mind's eye the apparent chaos presented by the continual repetition of the same symptoms then gave way to order; the new nosological pictures emerged, characterized by the constant combination of certain groups of symptoms. The complete and extreme cases, the "types," could be brought into prominence with the help of a certain sort of schematic planning and, with these types as a point of departure, the eye could travel over the long series of ill-defined cases — the *formes frustes* — which, branching off from one or other characteristic feature of the type, surrender to indistinctness. He called this kind of intellectual work, in which he had no equal, "practicing nosography," and he took pride in it. (ibid, p. 12)

Thus to Freud, Charcot was "not a reflective man, not a thinker: he had the nature of an artist, a *visuel*, a man who sees" (ibid. p. 12). And in this description of the man he revered, we get a hint of the traits that Freud, probably not yet very consciously, may have seen as central to himself.

References to Freud's failures in experimental studies that he conducted during his student years, in contrast to his descriptive histological studies of the same period, address this distinction: "There are two sides to this preference of the eye over the hand, of passively seeing over actively doing: an attraction to the one and an aversion to the other. Both were present" (Jones, 1953, p.52-3). This orientation might have been one of the factors that prompted Freud to turn away from the various active therapeutic techniques such as electrotherapy or hypnosis: "He preferred to look and listen, confident that if he could perceive the structure of a neurosis he would truly understand it and have power over

[5] Frommer and Langenbach (1991) follow in this evaluation our earlier study (Kächele, 1981)

the forces that had brought it about" (p. 53). It can be only roughly gauged how great Charcot's influence may have been on Freud's clinical research; Charcot who never tired of "defending the rights of purely clinical work, which consists in seeing and ordering things, against the encroachments of theoretical medicine" (Freud, 1893f, p. 13). A letter from Paris to his fiancée permits the conclusion that Freud's switch from neurology to psychopathology can be largely ascribed to Charcot's influence.[6] From Charcot Freud adopted not only the clinical method, but also the rehabilitation of hysteria and its significance in researching neurotic disease pictures:

> The first thing Charcot's work did was restore its dignity to the topic; little by little people gave up the scornful smile with which the patient could at that time feel certain of being met: she was no longer necessarily a malingerer, for Charcot had thrown the whole weight of his authority on the side of the genuineness and objectivity of hysterical phenomena. (Freud, 1893f, p.19)

There is little doubt that Freud's empirical but non-experimental approach developed on the model of the great master Charcot: when Freud went to Paris, his anatomical interests at first felt closer to him than clinical questions. According to Jones (1953), the decision to quit working at the microscope in Paris was essentially taken for personal reasons and because of Charcot's scientific influence (p. 211)[7].

As far as we know, no exact comparison has been made of Charcot's and Freud's descriptions of their patients. Nevertheless, the description of Charcot's nosographic method could easily be applied as well to the form in which Freud presented his clinical work. After all, the linking of typical processes in the life of the psyche is central to analytical work. The focus of attention has shifted from the symptoms to the psychic mechanisms; this is Freud's decisive step beyond descriptive psychopathology.

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[6] … "I believe I am changing a great deal. [Let me tell you what it is that is affecting me.] Charcot, who is both one of the greatest physicians and a man whose common sense is of the order of genius, simply demolishes my views and aims. Many a time after a lecture I go out as if from Nôtre Dame, with a new [sense of the Perfect.] …Whether the seed will ever bring forth fruit I do not know; but what I certainly know is that no other human being has ever affected me in such a way" (letter of 10/21/1885 to Martha Bernays. Quoted after Jones, 1953, p. 185). [Brackets include portions of German text omitted by Jones. – translator]

[7] There is an historical point in relation to Freud's quitting working at the microscope. He was unable to get an academic appointment in Vienna because he was a Jew, and since he was married and about to have children, he needed to earn an income, and for that reason he went into neurological practice - he later acknowledged that he wasn't interested in therapy (to Kardiner). I find it ironic that if he had not been a Jew and had gotten an academic appointment - he was certainly bright enough - he would have continued with his neurological research and might never have discovered psychoanalysis (personal communication by J. Schachter).

We have already mentioned Freud's justification of the special character of his case history (1895d, p. 124), with which he prefaces the discussion of Elisabeth von R. The first of the case histories presented in the "Studies" (Frau Emmy von. N.) still is very far from resembling a short story. Formally it is much like a continuous record of treatment presented almost without revision. The language is sober and objective and keeps largely to the observational level. Many years later, the author of this case history himself looks back on this presentation with compassion for the novice:

> I am aware that no analyst can read this case history today without a smile of pity. But it should be borne in mind that this was the first case in which I employed the cathartic procedure to a large extent. (addendum 1924 to Freud, 1895d, p. 105)

Whether the "novice" really needs our pity is another question. A thorough study, conducted by a Chicago research group, of Freud's scientific style at the time of his "Studies on Hysteria," makes it plain that even these early case histories are exemplary:

> Freud presented clinical evidence and theoretical propositions at various levels of abstraction, which could be derived from the observational data by inductive reasoning. His hypothesis formation through deductive logic was clearly labeled and sparingly employed. He used deduction to validate his theories by making clinical predictions, which could then be tested in the consulting room. (Schlessinger et al. 1967, p. 404)

Statements found in Freud's work about the methodological difficulties of his case presentations show that he was fully aware of the problems associated with the use of case histories as a form of scientific communication and that he always emphasized the heuristic nature of these communications. We shall now examine several of these points as they appear in the various great case histories.

Dora

Thus, Freud introduces his "Fragment of an Analysis of a Case of Hysteria" with the following words:

> In 1895 and 1896 I put forward certain views upon the pathogenesis of hysterical symptoms and upon the mental processes occurring in hysteria. Since that time, several years have passed. In now proposing, therefore, to substantiate those views by giving a detailed report of a case and its treatment, I cannot avoid making a few introductory remarks, for the purpose partly of justifying from various standpoints the

> step I am taking, and partly of diminishing the expectations to which it will give rise. (1905e, p. 7)

His guiding objective was to "bring forward some of the material" on which his conclusions were based and "make it accessible to the judgment of the world." However, he immediately admits that considerable technical difficulties in the process of reporting will have to be overcome. The doctor must not make notes during the actual session with the patient "for fear of shaking the patient's confidence and of disturbing his own view of the material under observation. Indeed, I have not yet succeeded in solving the problem of how to record for publication the history of a treatment of long duration" (1905e, pp. 9-10).

In the Dora case, two fortunate factors for reporting came together:

> In the first place the treatment did not last more than three months; and in the second place the material which elucidated the case was grouped around two dreams (one related in the middle of the treatment and one at the end). The wording of these dreams was recorded immediately after the session, and they thus afforded a secure point of attachment for the chain of interpretations and recollections which proceeded from them. (1905e, p. 10)

Thus it was possible for Freud to wait to set down these case histories until the treatment was concluded. As a motivating element for this feat of memory he does not omit to emphasize that his memory was enhanced by the interest in publication.

The Dora-case has become the object of many clinical secondary analyses (f.e. Deutsch, 1957, Erikson, 1962, 1964, Kanzer, 1966, Langs, 1976, Levine, 2005, Mahony, 2005). From a methodological point of view Marcus (1976) pointed to the poetic qualities of the text and its persuasive powers; Spence (1987) qualifies it as a tour-de-force in the art of persuasion:

> The appeal of the Dora case and its undoubted standing as a literary masterpiece make us aware of the influence of what might be called rhetorical craft and the subtle power of the clinical narrative. (p. 123)

This author also refers to a study by Hertz (1983) who uncovers a disturbing parallel between Freud and Dora: "They were both reticent; neither told the whole story; and finally we find a certain vagueness about the source of both Freud´s and Dora´s knowledge" (p. 133).

Little Hans

Freud´s next case history deals with the "Analysis of a Phobia in a Five-Year-Old Boy" known as "Little Hans" (Freud, 1909b, p. 5-149). Here the presentation is based on stenographic notes taken by the patient's father who, as we know, conducted the treatment himself. Freud himself makes comments on the treatment and follows it with a discussion in which he examines the series of observations from three points of view:

> In the first place I shall consider how far it supports the assertion which I put forward in my *Three Essays on the Theory of Sexuality* (1905d). Secondly, I shall consider to what extent it can contribute towards our understanding of this very frequent form of disorder. And thirdly, I shall consider whether it can be made to shed any light upon the mental life of children or to afford any criticism of our educational aims. (1909b, p. 101)

In the context of our present methodological questions regarding the significance of the case history as a practical and scientific means of communication, the report stands out for its relatively clear separation of observation and explanatory commentary. This is due to the allocation of roles in which the father — as the therapist — reports, while Freud — as the control analyst — provides the commentary. While the father's interest in the analysis apparently supports attentiveness to the material being sought, at the same time a clear distinction remains in the text. It may be partly owing to this circumstance that this case of horse phobia lent itself to different interpretations by psychologists of different provenance. It speaks well of a case presentation in that it allows for alternative explanations at all. Among the psychoanalytic commentaries and alternatives that have been proposed are those of Baumeyer (1952) and Loch and Jappe (1974): using a number of indications scattered throughout the text of the case history of Little Hans, they revealed more about the close connection between symptom formation and early suppression. However the same case also has served to criticize the way psychoanalytic evidence has been generated (Wolpe and Rachman, 1960). In any case Gardner (1972) praised Little Hans "as the most famous boy in child psychotherapy literature". Recently centennial reviews and reconsiderations have reexamined the case in the light of newer theory (Blum, 2007; Fingert Chused, 2007; Munder Ross, 2007; Stuart, 2007; Wakefield, 2007).

The Rat Man

In the same year Freud published another comprehensive case history. In fact, his "Notes upon a Case of Obsessional Neurosis" (1909d) contain far more than the modest title might lead one to expect. The case of the Rat-Man, Paul Lorenz, is the only one of the six long case reports to present a complete and successful treatment. This case presentation

can be called exemplary in many respects. The technical difficulties in reporting, about which Freud was still complaining in the Dora case — how a lengthy treatment could possibly be retained in memory — were resolved. The case report is based on the daily notes that Freud was in the habit of setting down each evening. Interestingly, it is precisely in this case that Freud warns against

> the practice of noting down what the patient says during the actual time of treatment. The consequent withdrawal of the physician's attention does the patient more harm than can be made up for by any increase in accuracy that may be achieved in the reproduction of his case history. (note 2, p. 159)

Yet the daily notes form the indispensable fund on which subsequent scientific processing can draw. Nevertheless, as Freud was in the habit of destroying both the manuscript and the preparatory notes and also warned against settling on explanations before conclusion of a treatment, the opinion is often heard that psychoanalytic case histories can rightly emerge from the head of the analyst at the conclusion of treatment like Athena from the head of Zeus. The tacit assumption here is that the entire relevant material will have gathered and taken form in the analyst's "head" (= unconscious). However, Freud preferred to make very thorough notes:

> By some odd chance, however, the day-to-day notes of this case, written every evening, were preserved, at least those for the best part of the first four months of treatment, and James Strachey has edited and published a translation of them in conjunction with the case history itself. (Jones, 1955, p. 230)

It is worthwhile studying this case history in detail, since its organization particularly reveals Freud's dramaturgic skill in structuring the dialogue between the reader and himself. In the introduction, Freud emphasizes two functions of the "following pages": first, to give "*fragmentary extracts* from a case history of obsessional neurosis"; second, in connection with this case but supported by other previously analyzed cases, to offer "*disconnected indications of an aphoristic character* upon the genesis and the finer psychological mechanism of obsessional processes…" (our italics). Freud justifies the fragmentary nature of this case history by pointing to his duty as a doctor to protect the patient from indiscrete curiosity, particularly in a capital city. On no account should it be thought that "I regard this manner of making a communication as perfectly correct and one to be imitated" (ibid., p. 155). Similarly, the aphoristic nature of the theoretical indications is not intended to function as a model, but is connected with Freud's confession that he has "not yet succeeded in completely penetrating the complicated texture of a *severe* case of

obsessional neurosis . . .” (p. 156). To help the reader follow the structure of the case history, we provide the following breakdown of its contents:

I Extracts from the Case History

A. = Beginning of the Treatment (1st session)

B. = Infantile Sexuality (1st session)

C. = The Great Obsessive Fear (2nd and 3rd sessions)

D. = Initiation into the Nature of the Treatment (4th session)

= (deepening, elucidation by Freud of the psychological differences between the conscious and the unconscious) (5th session)

= (a further childhood memory) (6th session)

= (the same topic) (7th session)

E. = Some Obsessional Ideas and their Explanation

F. = The Precipitating Cause of the Illness

G. = The Father Complex and Solution of the Rat Idea

II Theoretical

A. Some General Characteristics of Obsessional Structures.

B. Some Psychological Peculiarities of Obsessional Neurotics: their Attitude towards Reality, Superstition and Death.

C. The Instinctual Life of Obsessional Neurotics, and the Origins of Compulsion and Doubt.

The detailed development of the theme is introduced in strict chronological order. The reader is able to look over Freud’s shoulder (or through the one-way screen)[8]. As the clinical teacher, Freud stops at particular points to summarize and explain to the reader the meaning of what he has presented: “The events in his sixth or seventh year which the patient described in the first hour of his treatment were not merely, as he supposed, the beginning of his illness, but were already the illness itself” (p. 162). With these words he might then go on to introduce a critical discussion of infantile sexuality. There follow anticipated theoretical conclusions by way of explicating the knowledge gained thus far: “A complete obsessional neurosis, wanting in no essential element, [is] at once the nucleus and the prototype of the later disorder...” (p. 162). Thus Freud’s technique of presentation

[8] Mahony (1982) dedicates a whole chapter to elaborate on this aspect of Freud´s style (p.73-101).

consists in oscillating between very careful description[9], a rather short section of material and a thorough theoretical discussion of it. This theory-related clarification not only applies to the foregoing material, but also leads to hypotheses that will determine the further course of the clarification process:

> If we apply knowledge gained elsewhere to this case of childhood neurosis, we shall *not be able to avoid a suspicion* that in this instance as in others (that is to say, before the child had reached his sixth year) there had been conflicts and repressions... (p. 164; our italics)

Starting in the 2nd session the patient introduces the actual experience that prompted him to seek out Freud's help. Freud's technique of winning the reader for his presentation of the patient is once again to alternate between his function as the treating physician and as a reporter. "This 'both' took me aback, and it has no doubt also mystified the reader. For so far we have heard only of one idea..." (p. 167). The "we" draws the reader into the consulting room, into the analytic case-conference. After presenting the precipitating event in the 3rd session, Freud takes the patient's relating of another event in the 4th session as occasion to explain the nature of the treatment to him. His continuing explanation in the 5th session of the mode of action of analysis is of particular interest regarding the establishment of what is known today as the working alliance, the relationship plane that must be cultivated at the inception of treatment. The patient is so pleased by the acknowledgment that Freud shows him (p. 402) that the next session, the 6th, brings out more infantile material of great importance. The theme of his death-wish towards his father dominates the 7th session as well. After that, Freud concludes his exposition of the case history, not without explicitly noting that the course of treatment covering 11 months essentially corresponds to the sequence that he outlined in the first sessions.

At this point Freud the author changes his technique of presentation. Instead of a giving a continuous description, he first summarizes several obsessional ideas (E), explains the precipitating cause of the illness (F) and clarifies the father complex with his solution to the idea of the rat (G).

In these parts of the essay, exemplary symptoms are analyzed — pars pro toto — and traced back to their causative constellations. These examples are already inserted into a more general context. Thus, wherever the opportunity presents itself, distinctions and differentiations vis-à-vis hysteria are discussed, or references to other patients are made. At

[9] It is no accident that Freud points out in a footnote to this presentation that what he is writing is based on his notes from the day of treatment.

the same time Freud also attempts to discuss the question of whether the mechanisms he has analyzed can be generalized:

> Compulsive acts like this, in two successive stages, of which the second neutralizes the first, are a typical occurrence in obsessional neuroses. The patient's consciousness naturally misunderstands them and puts forward a set of secondary motives to account for them — *rationalizes* them, in short. But their true significance lies in their being a representation of a conflict of two opposing impulses of approximately equal strength: and hitherto I have invariably found that this opposition has been one between love and hate. Compulsive acts of this sort are theoretically of special interest, for they show us a new type of method of constructing symptoms. What regularly occurs in hysteria is that a compromise is arrived at, which enables both the opposing tendencies to find expression simultaneously — which kills two birds with one stone; whereas here each of the two opposing tendencies finds satisfaction singly, first one and then the other, though naturally an attempt is made to establish some sort of logical connection (often in defiance of all logic) between the antagonists. (p. 192).

This lengthy quotation from the case history under discussion is intended to demonstrate the degree to which Freud unites clinical demonstration with a vigorous examination of the concept. The sureness of his theoretical discussion, which is also reflected in the detail and skill of his interpretation, reminds the reader that the example under analysis here is not the only one of its kind, but that the author is using this case to test his own conceptions.

In the second part of the treatise, the relation of practice to theory is reversed. Initially the clinical deliberations were examined for their theoretical content and thus established, while now the theoretical considerations occupy center stage and the clinical example serves only to exemplify them. The regular processes of compulsion neurosis that can be abstracted from the individual case and established as having their own independent existence are presented in terms of their significance for the development of psychoanalytic theory. Here we are shown how theory can lay claim to an ability to make statements of broad validity, reaching the level of hypotheses about human culture and developmental history. From considering that "a tendency to taking pleasure in smell, which has become extinct since childhood, may play a role in the genesis of neurosis" — a tendency which he has discovered in other neurotics, compulsives and hysterics — Freud begins to wonder:

> …whether the atrophy of the sense of smell (which was the inevitable result of man's assumption of an erect posture) and the consequent organic repression of his pleasure

> in smell may not have had a considerable share in the origin of his susceptibility to nervous disease. This would afford us some explanation of why, with the advance of civilization, it is precisely the sexual life that must fall a victim to repression. (ibid. p. 248)

It is characteristic of Freud's case histories that while they perform a concrete analysis of the given case, at the same time they provide the setting for far-reaching hypotheses which bring to fruition the great riches of clinical thought.

The day-to-day notes mentioned above deserve separate consideration. In 1955 they were made available to the public in volume X of the Standard Edition. Elisabeth Zetzel however discovered them only in 1965 when she consulted the Standard Edition instead of the customary Collected Papers in preparing a paper. Her discovery led to an important addition to Freudian interpretation. For in these clinical notes there are over 40 references to a highly ambivalent mother-son relationship, which were not adequately considered in the Freudian case history as it was published in 1909 (Zetzel, 1966). These notes underscore the great importance of separating clinical observation from theory-bound interpretation. Freud himself noted with astonishment that the patient, after being informed of the conditions for treatment in the first interview, had said "I have to ask my mother." Today this reaction of the patient, though surely important, is not to be found in the case report itself. Other interesting treatments and reappraisals of the Rat-Man case incorporating Freud's notes are found in Shengold (1971), Beigler (1975), Holland (1975) and Mahony (1986). Recently Freud´s technical omissions – from today´s point of view have been critically discussed by Schachter (2005c).

The Wolf-Man

Similarly, the excerpt "From the History of an Infantile Neurosis" ― the most detailed and most important of all Freud's case histories ― deals with a relatively short period of treatment. After the analysis had been going on for 4 years without making any significant progress (Jones, 1955, p. 275), Freud set a deadline on the treatment.

> Under the inexorable pressure of this fixed limit his resistance and his fixation to the illness gave way, and now in a disproportionately short time the analysis produced all the material which made it possible to clear up his inhibitions and remove his symptoms. (1918b, p. 11)

According to Freud's own indications, the clarification of the infantile neurosis that he describes in this study derives almost entirely from these last months; from the setting of

the deadline to the end of treatment. Requested by the patient to "write a complete history of his illness, of his treatment, and of his recovery," Freud refused because he regarded this as "technically impracticable and socially impermissible." The "fragmentary" report — a bit of self-irony, since Freud surely saw how it compared in volume to his other case histories — represents a combination of a treatment and case history and is organized as follows:

I Introductory Remarks
II General Survey of the Patient's Environment and of the History of the Case
III The Seduction and its Immediate Consequences
IV The Dream and the Primal Scene
V A Few Discussions
VI The Obsessional Neurosis
VII Anal Eroticism and the Castration Complex
VIII Fresh Material from the Primal Period — Solution
IX Recapitulations and Problems

As is known, one of the aims of this publication was to combat a new form of resistance to the results of psychoanalysis. Carl G. Jung and Alfred Adler had undertaken reinterpretations aiming to "ward off the objectionable novelties … The study of children's neuroses exposes the complete inadequacy of these shallow or high-handed attempts at re-interpretation" (Freud 1918b, p. 9). As Freud's presentation in his "History of the Psychoanalytic Movement" (1914d) reveals, the polemic nature of this confrontation is noticeably subdued; instead, he attempts an "objective honoring of the analytic material." In his review of Gardiner´s anthology *The Wolf-Man by the Wolf-Man* (1971), Kanzer (1972) stressed that Freud, inspired by his experiences with the wolf dream, required his students to collect and report similar dreams indicative of early sexual experiences. The reaction to this, he says, encouraged direct observation and analysis of children. In his opinion this is to be regarded as a milestone in psychoanalytic methodology, since it has underscored the importance of collaborative research (Kanzer, 1972, p. 419). This statement is remarkable when one recalls that Freud repeatedly emphasized the impossibility of "in any way introducing into the reproduction of an analysis the sense of conviction which results from the analysis itself" (1918b, p. 13). The methodology of psychoanalytic research was by no means oriented a priori towards a successful description of individual cases. The addenda hoped for from later treatment reports on the Wolf-Man and the descriptions of adult neurosis in the famous patient of psychoanalysis, remain

disappointing. Even the Wolf-Man's own autobiographical remarks contribute little to an elucidation of a childhood, which has been charged with such a great burden of proof.[10] Mahony (1986) - specialist in matters of Freud´s literary style - dedicated a whole monograph on the Wolf Man.

Freud's sixth case history, "The Psychogenesis of a Case of Homosexuality in a Woman," can be omitted in the context of this methodological discussion, since in it Freud presents only "the most general outlines of the various events" and "the conclusions reached from a study of the case," because the requirements of medical discretion made it impossible to report it in greater detail (1920a, p. 147).

The Schreber Case

This makes it all the more interesting to broach a methodological discussion of the fourth case history of 1911. Freud's "Psychoanalytic Notes on an Autobiographical Account of a Case of Paranoia (Dementia Paranoides)" relate to a patient whom he had never seen, so in the strict sense it is not a case history at all.[11] Thus Freud seems to feel the need to provide a justification for the fact that "the analytic investigation of paranoia presents difficulties of a peculiar nature to physicians who, like myself, are not attached to public institutions" (1911c, p. 9). Because the therapeutic prospects were judged to be poor, as a rule Freud was unable to obtain sufficient analytic material to "lead to any analytic conclusions" about the structure of the cases (p. 9). A clever maneuver drawing on what was already known about paranoia changed this unfavorable situation into an excellent one:

> The psychoanalytic investigation of paranoia would be altogether impossible if the patients themselves did not possess the peculiarity of betraying (in distorted form, it is true) precisely those things which other neurotics keep hidden as a secret. Since paranoiacs cannot be compelled to overcome their internal resistances, and since in any case they only say what they choose to say, it follows this is precisely a disorder in which a written report or a printed case history can take the place of personal acquaintance with the patient. (p. 9)

What was first introduced as a justification proves to be a great advantage. Freud can tell readers to look up all the places in Schreber's *Denkwürdigkeiten* (Memoirs) that support his interpretations and read the patient's own words for themselves. The demand

[10] A very clear and didactically well-organized survey of the structure of argumentation in the Wolf-Man is given by the French authors Lebovici and Soulé (1970).

[11] cf. also Adler's case, Fräulein R. (1928).

that was previously impossible to fulfill — actually to provide the potential critic with the original data — was now met for the first time. This resulted in the English translation by MacAlpine and Hunter (1955). Dissatisfied with the therapeutic results of the then traditional thesis of a homosexual conflict of the paranoid psyche, they had turned to the "original text," which was more quoted than actually read:

> We therefore read Schreber's memoirs and subsequently published a study (MacAlpine and Hunter, 1953) in which we showed that projection of unconscious homosexuality, though playing a part in the symptomatology, could not account for the illness in course or outcome, phenomenologically or aetiologically. (p. 24)

Based on this experience they decided to translate the memoirs, not hesitating to praise this report from the methodological and didactic point of view as well:

> For all students of psychiatry, Schreber, his most famous patient, offers unique insight into the mind of a schizophrenic, his thinking, language, behavior, delusions and hallucinations, and into the inner development, course and outcome of the illness. His autobiography had the advantage of being complete to an extent no case history taken by a physician can ever be: its material is not selected or subject to elaboration or omission by an intermediary between the patient and his psychosis, and between both and the reader. Every student therefore has access to the totality of the patient's products. Indeed the memoirs may be called the best text on psychiatry written for psychiatrists by a patient. (MacAlpine and Hunter, 1955, p. 25).

Freud's report on Senate President Schreber was initially taken up by a number of psychoanalytic authors and utilized further. Abraham (1914) studied a case of neurotic light phobia, which until then had not yet been specifically treated in the literature. "And yet it contains … an important clue to our understanding of them" (Abraham 1914, p. 172). The indication refers to Schreber's delusion that he could look at the sun for minutes without being dazzled. If a delusional misapprehension of the danger of being blinded can be accepted in the psychotic, Abraham proceeds to assume in the neurotic an anxiety, an exaggerated fear of the risk of being blinded.

For the history of research it is of particular interest to see the development of an entire Schreber research program which, like MacAlpine and Hunter, did not confine itself to the sections excerpted by Freud. Until this point the Freudian report had been taken up and discussed by a number of psychoanalysts (Abraham, 1924; Bonaparte, 1927; Fenichel, 1931; Spielrein, 1912; Storch, 1922; Brenner, 1939). Starting in 1945 we observe the growth of an independent Schreber research, which in American psychoanalysis was

carried particularly by the studies of Niederland (for a summary see 1974), Katan (1959) and Nunberg (1952). A number of others participated in the discussion, among them White (1961, 1963) and Meissner (1976).[12] An unexpected development occurred in 1946 when Baumeyer became medical director of the hospital in which Schreber had been hospitalized and came upon a great quantity of new material, which he published in the following years (1956, 1970). Next to the contributions of Katan and Niederland, those of Baumeyer are the most important ones that have contributed to an understanding of the psychoanalytic aspects of the case. At Jacques Lacan's instigation a French translation of the memoirs was prepared, which was studied in the seminars of the Lacan circle. Lacan himself produced a linguistic structural analysis of the book, which especially deepens our understanding of Schreber's "basic language" (Lacan, 1959). Recently contributions by Israels (1989) and Lothane (2005) have enriched the controversial debates[13].

The fruitfulness of the decision to choose a publicly accessible case history as the starting point is further corroborated by the fact that this work became the object of scientific analyses outside of psychoanalytic circles. Thus, Elias Canetti (1962) proclaims that "there is no richer or more instructive document" (p. 434). To him, examination of this one system of paranoiac delusion leads to the conclusion that "paranoia is an *illness of power* in the most literal sense of the words" (p. 448; italics in original).

By placing Freud's analysis of the Schreber case at the end of this survey of case histories, we wished to show that a particularly favorable constellation was present here for further research: there is a clear division between the original and its interpretation, and new interpretative initiatives could be taken again and again. Certainly there are other valid approaches to studying the significance of Freud's case histories and their methodological peculiarities. As literary scientist Steven Marcus analyzed the Dora case as a work of art and found that the case histories represent a literary genre: they are "creative accomplishments that bear their own analysis and interpretation in themselves" (1976, p. @@).

Our interest here recides in the creative opportunity that is open to later researchers to correct previous attempts at interpretation and explanation using the case histories.

This discussion of the case history as a communication medium in Freud's writing has been oriented around his six lengthy case histories (Jones, 1955, p. 255ff). The

[12]The list is by no means exhaustive. Further references are found in Niederland (1974).
[13] Little known in this discussion is an effort to use computer-based content-analysis to solve some of the riddle of this case report (Laffal, 1976).

boundaries between this form and other clinical communications of Freud's are not precise and certainly have not been drawn using any explicit distinguishing criteria. The case histories in question are those that presented individual patients in a thorough way and at the same time were intended to illustrate general principles.

From the Case History to the Single Case Study[14]

The central scientific and didactic function of the case history is to bring out the type in the manner that Freud evidently adopted from Charcot´s nosographic method. "Progress in scientific work is just as it is in analysis. We bring expectations with us into the work, but they must be forcibly held back" (Freud, 1933) Thus the oscillation between conjectures / hypothesis and the testing of them is crucial. Frommer and Langenbach (1991) in their discussion of "the psychoanalytic case study as source of epistemic knowledge" follow Schwarz and Wiggings (1987) calling such early stages of knowledge which have abductive and inductive elements, 'typifications' (p.60). Amid the plethora of "formes frustes" one must be able to "read" the ideal type and then give it succinct form in an example; and this ability may account for the efficacy of a convincing case history. For this reason, we feel it is crucial for psychoanalytic and particularly clinical researchers to acquaint themselves with the concept of the type as a conceptual instrument of the highest order. The discussion below regarding the establishment of types as an ordering operation is based on the type concepts proposed by Hempel (1952).

The first and simplest type is designated by Hempel as the classification type. It arises when the individuals to be classed by type are assigned to different categories. The assignment is made according to the criteria of completeness, unambiguousness and exclusivity. Although this form of classification is very popular in the thinking of everyday clinical practice, the necessary conditions are seldom met. To characterize patients by "typical interaction patterns" or to refer to a "typical anal" or "typical suicidal" patient is misleading if the classificatory type is intended. In this kind of pragmatic type assignment for everyday use, which is especially common in psychoanalytic characterology, the genetic/dynamic aspect is ignored. Dictated by clinical needs, it is essentially a simplification of the cognitive contents that enter into the diagnostic decision-making process. According to Hempel, the classification type is most applicable during the early

[14] This section is dedicated to A. E. Meyer, whose polemic " Down with the Short Story – Long live the Patient-Therapist Interaction Story" (1994) breathed new life into our aspirations.

stages in the development of a science. Classification types function here as ordering structures by which the phenomenal world can be organized. They are present, however, only when the mentioned conditions are actually met. Yet in this respect clinical psychoanalytic phenomenology — i.e. systematic description — is more than unreliable. Indeed, it is a distinguishing mark of many theoretical discussions that their empirical basis is not unambiguously described.

A methodologically more demanding type, i.e., one belonging to a higher logical level, is designated by Hempel as the extreme type. This type is defined by two extremes that are rarely or never encountered in reality. Between the two extremes, subjects are characterized according to their closeness to or distance from one of the poles (see Rosch´s (1978) "principles of categorization"). In practice, transitional forms are conceivable between classification and extreme types, but in theory there is no gradual transition. In psychoanalytic clinical practice, this type makes little sense. While we may speak of a patient being more or less "anal," as a purely empirical feature-class "not at all anal" or "extremely anal" is not reasonably conceivable. A concept such as "anal" or "anality" is essentially an ideal type (see below) although specifically for research purposes it may be useful to make extreme-type application of certain concepts.

While the classification type and the extreme type are empirical in nature — i.e. they can be established by empirical features — the ideal type is a model that unites observable phenomena and concepts in an interpretive or explanatory schema. And herein lies its difference from the concept of *gestalt*, which embraces only empiric-phenomenal aspects. With this type, occupying the highest logical level of the three, the issues surrounding the concept of type altogether now become visible. It makes evident the amount of theory that as a rule goes into conceptualizing such types: Thus it becomes clear that the concept of the ideal type leads to examination of theory — a demand that is implicit in psychoanalytic case study.

This discussion of the significance of the type in psychoanalytic case study brings out a useful point of distinction vis-à-vis the biographical method, namely the generalizing thrust to which psychoanalytic case studies have always laid claim. Certainly the ability to discern types within the multiplicity of the phenomenal world is of tremendous heuristic value, yet it still must be asked if beyond this, the case-study approach has also been adequately worked through from a methodological point of view to permit an evaluation of clinical typology (Wachholz and Stuhr, 1999). In the following section, the research

connected with this question is examined under the theme of the transformation of the case history into the single-case study.

We now trace a development in the way clinical matters have been communicated in psychoanalysis since Freud's case histories. The first "genres" to establish themselves in the organs of scientific communication were more or less artfully drawn clinical miniatures, excerpts of treatments, single observations and dream analyses. Excellent examples of these can be found in Ferenczi's *Bausteine der Psychoanalyse* (Fundamentals of Psychoanalysis), vol. 2, *Praxis* (Practice), compiled 1927, in which the reader can still feel the enthusiasm for the newly revealed world that was now to be understood and communicated. The story of "Little Hahnemann," subliminally reminiscent of "Little Hans," dates from 1913. Another typical report for these years is a case presented by Schilder (1927) on a psychosis following a cataract operation:

> The psychoanalyst seldom has the opportunity to publish the entire material on which he bases his conclusions. The psychosis on which I wish to give a brief report presents such clear and unambiguous findings after a short period of observation that a documentary presentation is possible. This alone is reason enough to justify a detailed presentation of the case history. (p. 35)

After this introductory justification for providing a "detailed presentation" — the entire study is only about nine pages long — Schilder reports on a 53-year-old female patient who develops a condition of psychotic agitation in the aftermath of a cataract operation. Schilder describes the productive symptoms, which are dominated by images of her body being injured and portions of her own or her doctors' flesh being cut out, and then summarizes:

> This gives rise to the overall picture that the eye operation activates in the patient the general concept or general consciousness of injury to the body as a whole, within which concept an injury to the genitals is particularly dominant … The fact that it is an eye operation that evokes the psychosis is especially noteworthy inasmuch as it is well known that the eye frequently stands for the genitals. It should also be emphasized, however, that other operations, both in men and in women, will evoke a castration complex. (p. 42)

From then on the author is engaged in comparing and classifying this single case history:

> The psychosis has the type of Meynert's amentia. Formally it appears to be essentially indistinguishable from the majority of published observations of

> psychoses following cataract operations, to the extent it is at all possible to make a judgment on the basis of short case histories. (p. 43)

Now several additional types of operations are mentioned to which the literature ascribes a castrating action, and the author concludes his presentation with the following words:

> I have no doubt that the castration complex is significant in the genesis of postoperative psychoses and believe that a general significance must be ascribed to the results of the study of this case. (p. 44)

The sense of entitlement with which the author takes the step from discussion of an individual case to generalization is presumably based on a great number of other widely known experiences, but these are *not cited*. This is a characteristic of the clinical research tradition that Rapaport (1960) describes as clinically impressive yet not valid. We are left wondering if psychotic episodes occur more frequently after eye operations than after other operations — which one is inclined to assume given the towering importance of the eye as a sexually symbolic organ — or if this finding is not rather a product of wishful thinking.

With a change of style in scientific communication came the attempt to make verbatim transcripts of treatments accessible to the public. The need for this became evident as partial reports began to be published on such impressive successes that doubts seemed appropriate. Thus, in his review of a book by Sadger on *Die Lehre von den Geschlechtsverirrungen auf psychoanalytischer Grundlage* (Theory of Sexual Aberrations on a Psychoanalytic Foundation, 1921), Boehm writes:

> If the author wishes to assert that after just four sessions he has achieved a permanent cure by dissolving the mother bond (p. 96), this will raise doubts in the circles of Freud's pupils. On the other hand, if this accomplishment, standing alone, is supposed to be beyond doubt, then there is a significant omission in the text: A presentation of the technique that made it possible to dissolve the mother bond in four sessions would necessarily revolutionize the entire field of psychoanalytic therapy as it has been known. (Boehm 1923, p. 538)

The critic was fortunate in this case. Sadger had based his treatment reports on in-session stenographic notes, and these voluminous and detailed presentations made it possible for the reviewer (Boehm) to make a clear criticism of the treatment technique and thus also of the theoretical relevance of Sadger's conclusions:

> The case histories read like essays or novels that patients might write about the origin of their ailments after having read some part of the psychoanalytic literature and

> understood it poorly. They keep giving attempts at explanation, interpretations, questions, while symptoms presently manifested are simply 'traced back' to their 'source' in conscious childhood impressions and portrayed as repetitions and mere habituation: 'This might have its source in' ... is a stereotypical phrase in all of them ... It struck me that Sadger's patients use the same expressions, the same German as Sadger himself uses in his text. The longer I worked with these case histories, the stronger became my conviction that all of Sadger's patients were under a strong suggestion from the author — probably unconsciously to him — and for his sake made no resistance to 'associating' whatever attempts at explanation they assumed from their reading and from suggestive questions might please the doctor. Consequently these case histories, published as they are on the basis of stenographic records, unfortunately have no value as scientific evidence; furthermore they do not provide the uninitiated with an accurate picture of a psychoanalytic treatment. (Boehm, 1923, p. 539)

There could hardly be a clearer illustration of the advantages of providing stenographic or even verbatim accounts of treatments, as they allow for an evaluation not solely based on the evidence of the analyst, who is describing himself. Why Freud never published his notes, why he limited his discussion of technique to a small number of essays, most of which relate to the first ten years of psychoanalytic work, will not be considered here in detail.[15]

What is surely a significant development in this regard occurred in 1939: Confronted with the virtual unanimity of psychoanalysts regarding their method — after all, Freud himself had most clearly described it — Edward Glover felt sufficiently uneasy as to undertake an empirical survey within the British Psychoanalytic Society (Glover and Brierley, 1940). Using quite simple questions such as "When in the session do you engage in interpretation?", "How much interpretation do you do?" and "What do you interpret?," the results of the study revealed that as an ideal construct the psychoanalytic method allows psychoanalysts a great deal of empirical freedom, of which they take full advantage. As Balint (1950) has shown, these multifarious variations in technique stem at least partially from "changes in the therapeutic goals" of psychoanalysis, which in turn are derived from a

[15] Brody (1970, 1976) compiled a demographic evaluation of Freud's patients based on all patients mentioned in Freud's works. The assumption that this would capture a representative sample of Freud's clients, however, appears highly problematic. The simple fact mentioned by Brody that the number of published case histories declines rapidly after 1900, means that the patients presented in the "Studies on Hysteria" receive a qualitatively undue weight in the development of psychoanalysis.

differing reception of theoretical developments. Thus, Glover's attempt can also be seen in the context of the tensions that had arisen in the British Society with the development of the various schools. Even today there appears to be a considerable difference between the theory needed for the technique and the theory actually available, as can be observed in the disagreement with the French analysts. A good example of this is given in Widmer-Perrenoud's review of the study by Kestemberg and Decobert "La faim et le corps" (1972):

> Whoever turns to the case descriptions expecting to gain a better understanding of the theory and learn a specific technique for dealing with anorexics will be disappointed. …In other words, there is a discrepancy between the subtle theoretical considerations regarding the narcissism of anorexics and the application of these insights to treatment. (1975, p. 587)

As a bibliographic exercise Kächele (1981) reviewed the post-Freud psychoanalytic literature for treatment reports of a certain size, searching for presentations that cover, using a rough measure, of more than 30 pages in published form. We tabulate as a synopsis those reports that at the time of the study met this criterion (Table 3.1). Though some publications may have escaped the search, the synopsis ought to be informative and representative on the whole. Of the examples that are listed, we will be able to discuss only a part, placing emphasis on those cases that seem most important to us.

The synopsis lists the author and identifies the patients, whenever possible citing their age, sex and any names by which they might be known in the literature, the dates and length of treatment, to the extent that this could be ascertained from the reports, as well as indicating the type of record and the approximate page-count of the report in published form. Looking at the dates of publication in this sample, its incompleteness must be emphasized once again, one is struck that from 1930 to 1959 there were 6 reports, while from 1960-1979 there were 20.

Author	**Case**	**Date of Treatment**	**Duration**	**Date of Publication**	**Type of Record**	**Size (page-count)**
Adler	"Fräulein R. "	—	—	1928	after-session notes	146
Taft	7-year-old boy	—	31 sessions	1933	in-session notes	161
Wolberg	42-year-old man "Johann R."	1940	4 months	1945	after-session notes	169

Berg	young man	ca. 1940	—	1946	in-session notes	ca. 240
Sechehaye	18-year-old woman	1930	10 years	1947	after-session notes	107
McDougall/ Lebovici	9-year-old boy "Sammy"	1955	166 sessions	1960	in-session notes	270
Klein, M.	10-year-old boy "Richard"	1944	93 sessions	1961	after-session notes	490
Thomä	26-year-old woman "Sabine"	1958	304 sessions	1961	after-session notes	70
Parker	16-year-old boy	1955	200 sessions	1962	after-session notes	355
Bolland/ Sandler	2-year-old boy "Andy"	ca. 1960	221 sessions	1965	weekly report after-session notes	88
Boor	22-year-old man "Frank A."	ca. 1960	580 sessions	1965	after-session notes	30
Pearson	12-year-old boy "adolescent"	—	6 years	1968	after-session notes	140
Milner	23-year-old woman "Susan"	1943-1958	15 years	1969	after-session notes	410
Dolto	14-year-old boy "Dominique"	1968	12 sessions	1971	after-session notes	160
Balint	43-year-old man "Mr. Baker"	1961	29 sessions	1972	after-session notes	130
Dewald	26-year-old woman	ca. 1966	304 sessions	1972	in-session notes	620
Winnicott	30-year-old man	ca. 1954	—	1972	after-session notes	240
Argelander	35-year-old man	—	ca. 600 sessior	1971	after-session notes	75
Stoller	30-year-old woman	—	—	1974	in-session notes	400
Winnicott	2-year of girl "Piggle"	1964	14 sessions	1978	after-session notes	200
Firestein	25-year-old woman	—	—	1978	after-session notes	30
Goldberg	25-year-old man "Mr. I."	—	—	1978	after-session notes	108

Goldberg	31-year-old woman	ca. 1966	600 sessions	1978	after-session notes	98
Goldberg	22-year-old man "Mr. E."	ca. 1972	2 years	1978	after-session notes	134

Table 3.1 Extended case reports

These data are certainly may not be reliable statistically, but they confirm the impression that the study of the literature gave us: an increasing number of in-depth case reports are being made available to the public. It is interesting to note that in some cases treatment and publication are separated by a relatively long time. Also, 11 of the 26 reports concern children or adolescent patients; quite a high number when one considers that the quantitative proportion of child therapists is doubtless quite a bit lower. Furtherrmore, almost all of these children suffer from psychotic or pre-psychotic illnesses. The length of the reports cited here varies from the arbitrary lower limit of 30 pages to over 600 pages of text. With few exceptions these reports were written after the sessions. Verbatim transcripts were used only by Robert Stoller, although Dewald's report, which was based stenographic writing during the session, probably approaches the exactness of a verbatim transcript. It is obvious that the more recent reports have demonstrated more concern on methods of reporting clinical material (Klumpner and Frank, 1991). This preliminary overview should facilitate the subsequent discussion of several of these treatment reports, in which we shall limit our commentary to the authors' methodological approach.

We shall begin with the report of the British psychoanalyst Charles Berg who was also active at the Tavistock Clinic. Before the war he saw a young man whose unusual symptomatology struck him: he was practically normal but still felt the need to consult a psychoanalyst. Berg bases his decision to make the case report on this:

> It was on this account that I was tempted to record his analysis stage by stage in the hope that I would be able to convey to others interested in the subject the insight gained from a study of this clinical material. (p. 9)

The presentation of this treatment report is based on in-session notes and is ordered chronologically; the selection of the material is made according to clinical progress. Thus to a certain point Berg is following the exposition that Freud exemplified with the Rat-Man, but he does not explicitly refer to Freud. The preliminary interview is presented in great detail, and the first sessions even more exactly.

Gradually a condensation process sets in and the selection is largely determined by the thematic structure. Certain climaxes — the beginning of transference, the regression to childhood, the father-fixation, etc. — determine the further course of the presentation. It is a play in three acts under the overarching themes of father, mother and son, for which the report is even divided into three "books."

The fate of a treatment report by Donald Winnicott deserves to be noted. At the XVII Conférence des Psychanalystes de Langues Romains, Paris 1954, and a year later to the British Psycho-Analytical Society, he reported on the analysis of a schizoid man who experienced states of 'withdrawal and regression' during analysis, an understanding of which proved critical for the further course of this treatment. In 1972 Winnicott's notes of the last six months of this treatment appeared hidden away in a comprehensive book by Giovacchini (1972) on issues of treatment technique under the title "Fragment of an Analysis." Interestingly, the extended written version of the lecture including the case material, which appeared in German in *Psyche* in 1956, already contains the following unmistakable indication:

> It so happens that for the last four months of this part I made a verbatim report, which is available to anyone who wishes to read back over the work to-date with the patient. (Winnicott, 1956, p. 207) [Translation from German]

Surely it is an indication of a special communication problem among psychoanalysts that this offer of Winnicott's could only be taken up posthumously. Since then the report on this treatment has been published separately (French edition 1975; German edition 1982). Annie Anzieu's sensitive critique in the Bulletin of the European Psychoanalytic Federation (No. 11) immediately shows the virtue of such a publication in promoting discussion. In contrast to the admiring attitude of the American editor Giovacchini, Anzieu criticizes the analyst's penchant for interpretation, which makes it impossible to experience the unbroken speech of the patient. "The situation does not appear to be the kind to which a French analyst would usually refer" (p. 28). To the American editor, it is just this perceptible activity on the part of the analyst that they find of positive significance:

> The benefits derived from the detailed presentation of an analytic case are emphasized by this example. Not only do we learn about Dr. Winnicott's clinical-theoretical orientation, which has had and will continue to have, in our opinion, considerable impact on psychoanalytic theory and practice, but we are also made aware of how really exciting and rewarding the actual treatment of a patient can be. We particularly want to call the reader's attention to the way in which Dr. Winnicott

> has integrated fantasy and dream material with reports of routine daily activities, in the service of the analysis. (Giovachini, 1972, p. 455-6)

Given the particular personal transmission of theory and technique in psychoanalysis, we must acknowledge it as a great exception that the process notes from a treatment by a significant psychoanalyst are available at all, allowing us to get at least a step closer to a direct impression and to make independent judgments on technique and theory.

A similar legacy is found in Melanie Klein's report on a child analysis, which she compiled shortly before her death (1961). She provides an explanation of the aims of this voluminous publication herself:

> In presenting the following case history, I have several aims in view. I wish first of all to illustrate my technique in greater detail than I have done formerly. ...The day-to-day movement in the analysis, and the continuity running through it, thus become perceptible.
>
> I took fairly extensive notes, but I could of course not always be sure of the sequence, nor quote literally the patient's associations and my interpretations. This difficulty is one of a general nature in reporting on case material. To give verbatim accounts could only be done if the analyst were to take notes during the session; this would disturb that patient considerably and break the unhindered flow of associations, as well as divert the analyst's attention from the course of the analysis. (p. 11)

The short duration of the treatment is not simply due to a favorable course; in fact, as the editor informs us, it was made clear from the beginning that only four months would be available. It is important that Klein feels able to assure us that this analysis does not differ in any way from an analysis of normal duration.

One might think this report would especially lend itself to research, as it contains notes on exactly 93 sessions with an average length of 5 pages each. Yet apart from Geleerd's exhaustive discussion (1963), the only other investigation we are aware of is the recent detailed study by Meltzer (1978), which provides a systematic description of the course of this treatment.

Joyce McDougall and Serge Lebovici (1969) reported on the analytic treatment of 9-year-old Sammy that had been treated in the mid-fifties of the last century in Paris. The lad himself initiated the exact report on the treatment, since for a long period he refused to speak except if the analyst took down every word he said: "Now write what I dictate. I'm your dictator," he would shout (1969, p. 1). After eight months the treatment of this psychotic child was terminated, apparently with significant improvement; yet the reports of

the parents from subsequent years, which are included in the publication, make it clear that this fragment of a child analysis was really only a beginning:

> Sammy left for New York the following day. Thus his analysis after only eight months' treatment, still in its beginnings, came to an abrupt end. (Commentary of the analyst in her notes on the last session, No. 166 of 9/9/1955)

As mentioned before treatments of children seem more likely to be published than those of adults[16]. One, the case of 2-year-old Andy, was published by Bolland and Sandler of the Hampstead Clinic in 1965. Covering a period of 50 weeks, 271 sessions are presented in weekly summaries. In addition, this treatment report exemplifies the way the Hampstead Index works. By indexing analytic material, i.e. putting it into schematic form, the research group at the Hampstead Clinic seeks to "create something like a collective analytic memory, a store of analytic material that makes a wealth of data gathered from many colleagues available to the individual researcher or author" (Anna Freud in the preface p. X)[17].

Another report is that of Francoise Dolto (1971). Her 14-year-old Dominique is "cured" of his psychotic regression in 12 sessions. Dolto too seeks justification in referring to Freud's case histories, particularly the child case histories such as Little Hans, and expresses criticism:

> Contemporary literature offers a multitude of short or minute extracts drawn from a series of several hundred sessions. These represent selections from the dreams, the words, or the behavior of patients, and mostly serve to justify technical research or some discussion of transference or counter-transference. The clinician is left to wonder about the basis of their selection. (p. 3)

In addition, Dolto makes a plea for the presence of third parties in the treatment situation, such that "one of the psychoanalytically trained individuals present records everything that is said on either side, by both the patient and the analyst" (p. 8). As it happens, this condition was not fulfilled in the Dominique case and the process notes were made by the therapist herself.

Without these exact records, it is unlikely that the negative criticism directed at this case report by the American reviewer (Anthony, 1974) could have been so objective or so

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[16] The ethical issue of children giving "informed consent" to publication must bypassed here.
[17] The use of the Index as a research instrument is described by Sandler (1962).

outspoken in declaring that “each nation [seems] to cultivate its own psychoanalytic garden” (p. 684). The German reviewer (Haas, 1976) seems to prefer a garden à la Lacan.

This critical discussion deserves high marks because it makes it possible to reduce ideological differences to their demonstrable empiric substance This appears to be why Dolto’s demand for “extremely detailed notes” is not controversial, to the extent that they are in fact published and made available for didactic and theory-demonstrating purposes. Unfortunately, a great number of reasons speak against this, reasons that cannot be lightly dismissed. Protection of the patient and the need of the analyst for protection as well are without doubt the prime ones, which is why we often see the problematic issues surrounding a publication dwindle with the passage of time. Thus, it is probably no accident that Winnicott’s records became accessible only after twenty years, that Klein’s treatment of Richard was not published until 1961, or that Balint’s presentation of his focal therapy with the patient Baker did not appear in print until ten years after completion of the treatment. We learn from David Malan that Michael Balint decided only quite late (around 1952) to take on cases himself. The first two cases he treated, as reported by Malan (1975, p. 116), “were singularly unsuccessful”, but the third attempt led, through its literary use, to a new type of psychoanalytic treatment: focal therapy.

> This book is based on Michael Balint’s treatment of the patient Mr. Baker, written by him (Chapter 5). Unfortunately, the comments at the end of each session report are “asymmetrical.” The reader should be aware that Michael Balint dictated his notes right after each session and that originally these notes were not meant for publication. He later decided to include them in their original form with only very minor stylistic and grammatical changes here and there. (Ornstein, 1972, p. vii)

> The aim of this joint work, which took gradual form over the course of time, was: …to use the history of the treatment of Mr. Baker to study in detail the interactions between the patient’s associations and the therapist’s choice of interventions. From the theoretical point of view this interaction can be summed up not only as the study of the treatment as a process, but also as a study of the developing doctor-patient relationship. (Balint et al., 1972, p. 2)

When one considers this study from the point of view of the public nature of its observational data, several questions arise that Balint himself posed and immediately answered:

> The material for this study is the collection of session reports, which were as a rule dictated to a secretary immediately after each session. No notes were made during the

> treatment sessions. The therapist relied entirely upon his memory. We know that this method has many drawbacks, and a number of purists will find it inadequate for meaningful research.
>
> We readily admit that in a way recall from memory is not as reliable as a record on tape. On the other hand, we maintain that the internal cohesion within any single session and the whole series of sessions taken together is enough to demonstrate the validity and usefulness of this particular method.
>
> Here we would only like to indicate that the method of recording used in this treatment facilitates the clear emergence of both the patient's character and the nature of the therapeutic technique, whereas otherwise both would have to be laboriously extracted from the collection of raw data provided by tape-recordings. Furthermore, no tape-recording can give any information about 'interpretations thought of, but not given', the atmosphere of the session, the therapist's initial expectations, or his changing views regarding outcome and his afterthoughts and so on; on the other hand, all these important data are provided by the design of the method used. (Balint et al., 1972 p. 2)

Balint's argument emphasizes that in psychoanalysis, the publicly accessible raw data go beyond the verbal utterances of the patient and therapist. Only the crudest behaviorist could deny that the therapist's considerations, intentions and attitudes have a potent existence in the therapeutic process. Balint's suggestion introduces the subjective dimension of this process into research, thus opening up a great number of vital issues to it for the first time.

In this connection, an important formal aspect of the described treatment is that a structuring element enters into the description of the sessions: prior to treatment a *schema* was established, setting in advance the thematic points to be covered. In this way a relatively systematic documentation of this course of treatment was achieved.

In fact such a schema for describing a course of treatment was introduced earlier, in 1951, by Alexander Mitscherlich at the Heidelberg Psychosomatic Clinic. In 1947 in his monograph *Vom Ursprung der Sucht* (The Origin of Addiction), he had already presented three case histories in which the presentation of the course of treatment is organized almost entirely within the framework of a dream analysis.[18] This "Systematic Case History" was

[18] This may be compared to French's monograph of 1952, which bases its clinical proofs on an extensive series of dreams of a female patient (on the use of dream series, cf. Geist & Kaechele, 1979).

intended to complete the "biographic anamnesis" in order to capture the process aspect of treatments. How many such "systematic case histories" were in fact written is difficult to determine today. To date only one has been published in the Festschrift for Alexander Mitscherlich:

> Although only a small number of patients and their disease courses have been systematically studied, for many reasons it is more than justified at this moment to recall a work from the pioneer era. (Thomä, 1978, p. 254)

Doubtless inspired by this conception but not directly determined by it are the extensive case histories involving patients suffering from anorexia nervosa presented by Thomä in 1961.[19] Regarding the scope of the case Sabine B., the author writes:

> Even a report as extensive as the following one presents only a selection of the observations and considerations that were gone through in 304 treatment sessions. In order to get at what was essential, we proceeded from the experiences of transference and resistance, which became the guideline for our presentation. (1961, p. 150)

Subsequently the author apologizes for the "considerable" length of the report (approx. 70 pp.), without pointing to the dearth of thorough treatment presentations as a justification for going into such detail. The presentation of this treatment is divided into 16 sections, the longest of them covering a period of 38 sessions, the shortest 9 sessions. A methodological discussion as to how this division of the treatment was arrived at is limited to the statement that the treatment sections are described according to "main themes." It would certainly be a worthwhile undertaking to make a careful investigation of the decision-making processes that lead to such segmenting of the psychoanalytic process (cf. Knapp et al., 1975).

In Hermann Argelander's case study "Der Flieger" (The Pilot, 1971) we begin to see a noticeably more positive attitude towards comprehensive reporting. The chronological presentation of a course of treatment is expanded to include a theoretical introduction and a concluding summary with critical reflections; as to his procedure in selecting the material, the author writes:

> In documenting the analytic material I shall alternate between a summarizing report form and excerpts of verbatim transcripts, especially at points that appear important for my theme. (p. 10)

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[19] See also de Boor's monograph on the psychosomatic aspect of allergy (1965), which also contains several lengthy case histories and treatment reports.

Since the presentation was intended as a contribution to the ongoing discussion of narcissism, the theme was focused in the tradition of the psychoanalytic case history.

Argelander's explicitly formulated reflections on the form of his presentations, his chronological recording of the events in the analysis, his strictly systematic inclusion of numerous verbatim quotes and his summarizing reports, which appear very objective and free from theoretical and personal bias, all reveal the author's endeavor to allow for more transparency in his case presentation than was often true of earlier case histories. At the same time it must be noted that Argelander had to strike a selection and make descriptive summaries of long sections in order to keep down the length of the work.

The issue of the length of the case presentations studied here deserves special mention; the argument is too easily made that the importance of a work surely cannot be measured by its length. In fact, to the extent that clinical observations are being presented, the length of a treatment report does give an indication of the closeness to clinical reality of the observations presented.

It is useful in this regard to look at the case of Mr. Z. first described by Kohut (1979a) that for good reason was not included in our tabulated list of cases. In it the course of two psychoanalyses is presented, one differing considerably in technical approach from the other.

Even though the clinical details provided in the German translation of "The Restoration of the Self" (1979b. p. 172-216) are more complete than in the English journal version (1979a) a conclusion such as Kohut´s calls for a far more comprehensive presentation. For example, the conclusion, based on this case, that "the new psychology of the self is helpful in the clinical area, that it allows us to perceive meanings, or the significance of meanings, that were formerly not perceived by us, at least not consciously" (p. 26), should be supported with documentation that enable it to be tested.

Recently a biography on Kohut (Strozier, 2001) clarifies that Mr. Z´s second analysis was an artful invention, to illustrate how his second analysis should have been. In 1984 Kohut reinforced his satisfaction with the first fictive psychoanalysis worldwide. Indirectly he sharply criticized his training analyst, Ruth Eissler, whom he had chosen for tactical reasons after his application for training at the Chicago Psychoanalytic Institute was rejected. In his own appreciation he glorifies the changes by the self-psychological theory. Nothing else Kohut did illustrate more clearly his heroic sense of himself. The two analyses of Mr. Z, published as genuine case material in the profession most respected journal,

reveals his deep psychoanalytical understanding and experience (Strozier, p. 308). Even in his last work "How does analysis cure?" (1984) in a final debate with his critics he wrote:

> The case not only highlights the way theoretical changes enable the analyst to see new clinical configuration, but further demonstrates how the analysts´ apprehension of the self-object transference affects his handling of clinical material via the enpanded empathy that results from the new theoretical frame. (Kohut, 1984, p. 91)

The two analyses of Mr. Z are also a telling demonstration that even distinguished journal editors are unable to differentiate valid clinical dynamics from theoretical, hypothetical dynamics; how then can any analyst determine whether the dynamics developed with a patient are valid or theoretical?

Further detailed cases provided as support to Kohut's theory are found in a casebook edited by Goldberg in 1978. It presents relatively lengthy case reports, primarily of those patients who appear in short vignettes in Kohut's books. Thus the book represents a "response to a persistent and clear request from a large number of clinicians" who have concerned themselves with Kohut's concepts (Goldberg, 1978, p. 1). The presentation of the analyses of Mrs. I. and Mrs. A., each over 100 pages long, certainly allows for an excellent clinically oriented discussion.

Yet the need for even more detailed presentations of treatments seems to be felt by the treating analysts and the researchers alike. Thus, Paul Dewald decided to document an entire psychoanalytic treatment by making careful notes during the sessions. Introducing his intention at a workshop of the American Psychoanalytic Association in April 1972, Dewald stressed that:

> …most experienced analysts can conduct a reasonably effective psychoanalytic treatment with our current understanding of the process, and our traditional methodology of anecdotal description has resulted in the accumulation of a considerable body of knowledge. (quote in Dorpat, 1973, p. 170)

"Nevertheless," Dorpat comments, "if we are to have an impact, scientifically, beyond our profession, further research in this area is essential" (p. 171).

Dewald describes a treatment that was conducted initially without any scientific aim. Its systematic elaboration was undertaken only a year after conclusion of the treatment. Nevertheless Dewald had to make detailed, almost verbatim notes of the dialogue while treatment was in progress, and he attempted to incorporate nonverbal elements of the communication into them. It is interesting that the "verbal interventions" of the analyst are

cited separately, as if it were odd for the analyst to have a part in the psychoanalytic dialogue.

As might be expected, the note-taking becomes a technical problem. Still, Dewald assures us that as a rule his taking notes was accepted by the patient as belonging to the overall psychoanalytic situation, as part of the treatment arrangement. Furthermore, it was found that the patients' reactions to it could be analyzed exactly as other reactions to the analyst's reality. The patient whom Dewald introduces is a young woman suffering from a classical mixed neurosis with multiple phobias, free-floating anxiety, depression and frigidity. As it developed, the patient quickly grew accustomed to the analytic situation and proved an understanding partner in the work. Thus, the treatment lasted only 24 months (347 sessions) and led to improvement of the symptoms as well as a structural transformation of her personality.

About a year after the conclusion of treatment, Dewald began to transcribe his notes using a Dictaphone, making a particular effort to be true to the patient's idiomatic traits. When it came to publishing the notes however, he had to select a sample as the material was too voluminous in its entirety.

This argument points to a certain contradiction between the scientific demand for revealing the original data and the practical limitation resulting from the impossibility of publishing even relatively short psychoanalyses in toto. Psychoanalytic process research suffers between the scylla of shortening and the deep blue sea of "systematical acustical gap" (Meyer, 1981). What is "impossible," of course, is a determination that must be agreed upon by scientists. Only when such notes have been established as major data sources are these "impossibilities" likely to change.

Dewald made a decision to present unabridged session process notes of certain time blocks of the treatment. In addition he rounds out each set of in-session notes with summaries that enable the reader to gain insight into the analyst's thoughts. For those periods of treatment not presented in full, highly compressed summaries are inserted. The following table is intended to provide an overview of the distribution of the total publication into the several treatment sections and types of presentation:

Length of treatment:	24 months = 347 sessions
Verbatim records of these	107 sessions
Sample: months	1+3; 11; 13; 15; 23+24
Total volume of clinical material	656 pages
Portion of this in verbatim text	510 pages

Portion of this in summaries 146 pages

What was Dewald's aim in keeping notes of this treatment? The presenter at the workshop gives the following explanation:

> Dewald's aim in publishing this case record was to provide an overview of the psychoanalytic process from a clinical perspective, with demonstration and documentation of psychoanalytic data. Now that these data are published, it is possible to study them from a variety of different perspectives. Other researchers could try to validate through consensus on just what constituted the analytic process in this case. The same data could also be approached predictively by someone who has not previously studied the case and who would therefore not be biased by his advance knowledge of what happened in the analysis. (Dorpat 1973, p. 172)

On a par with Dewald's report in scope and importance is that of Robert Stoller (1974), who has been studying questions of psychosexual development for years: *Splitting. A Case of Female Masculinity*. In the introduction to his 400-page report on this unusual patient, Stoller too takes a position regarding the undertaking that makes room for both sides: the advocates of the classical case study who follow in the footsteps of Freud, and the experimentalists who have learned to doubt the value of individual clinical studies. The following passage exemplifies the style of the foreword to this book, which is essentially a plea for comprehensive case presentation:

> Despite the importance of discovering the psychodynamic sources of human behavior and the extensiveness of the literature to date, there is not a single psychoanalytic report in which the conclusions are preceded by the data that led to them. If such data are not available, critics can be forgiven for not being convinced of the validity of the conclusions. …You and I never know when reading someone's report whether he is right because he is brilliant, imaginative, and agrees with noted authorities, or whether he is right because his conclusions follow from his data; we cannot know because we have not had access to his data. (Stoller, 1974, p. xiii)

This foreword also strikes a now-familiar refrain. What should interest us however, is that these are not methodological outsiders speaking, but experienced clinicians who themselves have cultivated the traditional style of communication for years and decades. The question as to whether the examples gathered here will be style-setting or not hinges on whether clinical needs will require making more information on treatment available than has traditionally been the case. The desire to get a real look into the psychoanalyst's

workroom is no longer disparaged as voyeurism or infantile curiosity, but has gained clinical, didactic and scientific respectability in recent years.

By its nature, psychoanalysis can be experienced and learned only within a human relationship; for a long time this peculiarity resulted in putting a low priority on publishing treatment reports. The general feeling was that the important elements of a treatment could not yet be demonstrated or communicated. When one reads the enthusiastic reviews of treatment reports of experienced psychoanalysts however, the opposite is regularly shown to be true. Thus, James writes regarding Winnicott's "Piggle" (1978):

> Remarkably enough there are few accounts of clinical work which tell to the new, and to the learning, analyst, how others who are believed to be successful work. "The Piggle" is one of those *rare open descriptions which establish a style* (James, 1979, p. 137; our italics).

Our hope is that a style-setting influence will be felt not only from Winnicott's treatment technique, but also from the openness practiced by him and others. At the same time there does seem to be a certain risk associated with such openness. Margret Little´s personal record of her analysis with Winnicott´s in the early fifties describing psychotic anxieties and their containment was courageous (1990). Harry Guntrip's report on his two analyses with Fairbairn and Winnicott, for example, appeared only after his death (1975), and the attempt of living, particularly younger analysts to report on their "apprenticeship on the couch" (*Lehrjahre auf der Couch*, Moser 1974), cannot be an easy undertaking. In his discussion of Moser's "confessions," Lowenfeld (1975) clarifies the special requirements that must apply to a treatment report of this kind composed by a professional colleague. With reports by patients it is easier to accept each of them for its particular nature and motivation. Homages to Freud (Wortis, 1954; Blanton, 1971; Doolittle, 1976) or reports on therapeutic experiences[20] possessing special literary charm, as those of Hannah Green (1964) and Marie Cardinal (1983), more easily win our sympathy. Attempts to write a common report, in which the patient and the therapist together reflect on the treatment, are still so uncommon as to arouse curiosity (Yalom and Elkin, 1975). A recent example was provided in Schachter´s (2005a) collection of treatment reports (see his chapter 5). However Mary Barnes' *Two Accounts of a Journey through Madness* (1971), an overtly enthusiastic report from Laing´s therapeutic community Kingsley Hall in London, also

[20] Only recently the diary of a 27-year old patient on her treatment with Freud for 80 sessions in 1921 was published by the grand-daughter, the Swiss psychoanalyst Anna Koellreuter: www.werkblatt.de Nr. 58 (DIE ZEIT vom 2. 8. 2007)

raises doubts about the therapeutic function of such shared reports. In her review, Curtius speaks of a "new literary genre of the patient *Bildungsroman* or the therapist-patient novel in letters" (1976, p. 64).

Since the publication of this study in 1981 it is obvious that in recent years detailed case reports are being published more and more frequently. Some of them have even reached television audiences. The growth of public interest in what goes on in psychotherapy is paralleled by psychoanalysts' growing interest in communicating their clinical experiences to each other in greater detail. The topic of the "analyst at work" rightly moves metapsychological discussions into the background.

A more recent example underscores that the problems of adaequate reporting still carry moments of tensions. In 1981Casement reported at the International Congress in Helsinki on a technical problem ("Some pressures on the analyst for physical contact during the re-living of an early trauma") which was published in the following year (Casement, 1982). Later the author published an enlarged version of the paper in two books (Casement, 1985, 1990) and compiled important opinions from discussants of diverse theoretical orientations (Casement, 2000). In the meantime, as Boesky (2005) pointed out that more than 25 authorss have taken position to the original report; the publication of such discussions have become a kind of "cottage industry" (p. 842). Boesky´s conclusion of his methodological critique focuss on the lack of understanding of clinical evidence formation:

> If we truly wish to reap the benefits of pluralistic psychoanalysis, we are well advised to refine our understanding of what information about the patient has been used to support the conclusions reported. (Boesky, 2005, p.860)

This problem of how an analyst gains and refines his working model of the patient and of the transference-countertransference situation and how he is in a position to adaequately report about has been studied in a Hamburg-Ulm collaborative project (Meyer 1988); following up this research road into an analyst´s mentation König (1993) has delivered an unique example studying a single session by detailed cross-examination of the analyst based on the transcript of the session.

To conclude our study of the transformation of clinical vignettes and traditional case histories into formalized single-case studies of the course of a treatment, it should be pointed out that, beyond the didactic clinical advantages of the latter, they also open up possibilities for systematic studies using social science research methods. Whether they are published in complete form or simply in the form of samples, the inclusion of treatment

process notes arranged by observation and conclusion can provide a valuable and adequate fund of material for further study. As Thomä and Houben have pointed out:

> …extensive case-history material that we have collected in technical seminars over the years proves inadequate for scientific evaluation in its present form. Too often the psychoanalytic case presentations have remained on the level of 'uncontrolled' clinical description. That means that in these reports observation and theorizing are still too closely interwoven. (1967, p. 664).

Therefore to ensure systematic documentation of courses of treatment, the use of mechanical devices, whether video or audio, to record the dialog is indispensable today. This creates new problems, starting with concerns on the part of the therapists (cf. Bergmann, 1966; Gill et al., 1968; Fonagy, 2002b; Perron, 2003) and extending to the problems involved in evaluating the material that has been gathered. To solve them, new technologies have been integrated into psychoanalytic research, which provide it with the necessary help. The implementation of computer-assisted archives as it was called for by Luborsky and Spence (1971) has been put into practice for some time now in Ulm (Kächele & Mergenthaler, 1984; Mergenthaler & Kächele, 1993; see chap. 6.2) and at few other places by now (f. e Luborsky et al., 2001; Waldron, 1989) thus facilitating qualitative and quantitative text analyses.

The process of transformation that we have attempted to highlight with a series of examples was first set off by increasing criticism of the scientific value of clinical case presentations. Freud's case histories owe their life and success to a captivating synthesis of the observational material they present and the theoretical conclusions they draw from it.

Nevertheless, or perhaps *because* Freud's case histories are so captivating that they contributed to an overestimation of the methodological value of such presentations, a struggle is now taking place over this core issue: Can clinical research continue to consist solely in such informally structured treatment reports, or should it be complemented by more formalized research strategies? Our answer has been that an increased formalization and intensification of research is both desirable and necessary in course and outcome research.

Finally, let it be stressed that the traditional method of psychoanalysis, which in its original form we owe to Freud, was sufficient to earn it the highest place among the scientific endeavors that have enriched our anthropological knowledge. The German philosopher Heinz Kunz expressed this clearly:

> No other discipline in our age has concerned itself so intensively and comprehensively with the human being, his experience and behavior as psychoanalysis. (Kunz 1975, p. 45)

Yet we cannot overlook the fact that this achievement of Freud must now be reattained bit by bit in this phase of "normal scientific activity" (Kuhn 1962). As to the further development of clinical psychoanalytic research, we are convinced that the methodological requirements have grown and that intensive investigation of the single treatment case has opened new territory. This will include the "role of the psychoanalyst" (Thomä, 1974; Sandell et al., 2007), since long the dark continent of clinical-analytic research. The review presented here hopefully will further contribute to the transformation of the case history into the single-case study, thus securing the foundation for a new stage in clinical psychoanalytic research.

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