Psychoanalytic Practice: Clinical Studies

Case Histories

and Treatment Reports

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Table of Contents

Case Histories and Treatment Reports

Introduction

The crisis of psychoanalytic theory, which was the central topic of Chap.1 of the companion volume on the principles of psychoanalytic practice, has inevitably had some effects on psychoanalytic technique. In the last decade it has also become apparent that the perspectives of psychoanalytic therapy rooted in interpersonal theories have caused many concepts relevant to psychoanalytic practice to be reevaluated. It is now essential to distinguish between, on the one hand, the theory of the genesis or the explanation of psychic and psychosomatic illnesses and, on the other, the theory of therapeutic change and how it is brought about. Of course, all assumptions about structural changes depend on the observation of variations and alterations of symptoms.

This chapter's title, "Case Histories and Treatment Reports," reflects the discord in Freud's work between the theory of genesis and that of change. Our reconsideration leads us in the first section of this chapter to reject the notion that he gave adequate scientific consideration to both poles of this discord in his case histories. It is necessary to reformulate his famous assertion about the existence of an inseparable bond between curing and research. A

promising new source for regrounding psychoanalytic therapy is for us to take the fact seriously that the theory of repeated traumatization has significance for the structuring of the therapeutic situation.

If we attempt to apply scientific criteria to the preparation of case histories and treatment reports, it is necessary for us to experiment with different schemes for reporting our work. For about three decades we, together with many other analysts, have striven toward the goal of reproducing the psychoanalytic dialogue as precisely as possible. In Sects. 1.2 and 1.3 we refer to important stages in the development of reporting, which we elaborate on in later chapters by providing appropriate examples. We have now reached a new stage. The use of audio recordings enables us to make the verbal exchanges between patient and analyst accessible to third parties in a reliable form. Because of the significance of this technical aid for advanced training and research, in Sect. 1.4 we make the reader familiar with a controversy that has been dragging on for a long time and that the examples we give in Sect. 7.8 should help resolve.

1.1 Back to Freud and the Path to the Future

Freud's case histories frequently fulfill the function of an introduction to his work. Jones emphasizes that the Dora case—the first of Freud's exemplary case histories following his *Studies on Hysteria*

> for years served as a model for students of psycho-analysis, and although our knowledge has greatly progressed since then, it makes today as interesting reading as ever. It was the first of Freud's post-neurological writings I had come across, at the time of its publication, and I well remember the deep impression the intuition and the close attention to detail displayed in it made on me. Here was a man who not only listened closely to every word his patient spoke, but regarded each such utterance as every whit as definite and as in need of correlation as the phenomena of the physical world. (Jones 1954, p. 288)

This makes it all the more remarkable that it was precisely on this case that Erikson (1962) demonstrated substantial weaknesses in Freud's understanding of etiology and therapy (see Vol.1, Sect. 8.6). The paper he presented to the American Psychoanalytic Association marked the increasing criticism both of Freud's explanations of etiology in his *case histories* and of his technique as described in his *treatment reports* . In view of the growing flood of publications containing such criticism, Arlow (1982, p. 14) has expressed his concern about their ties to objects belonging to the past. He recommended that we should simply say

goodbye to these "childhood friends" who served us so well, put them to rest, and get back to work.

That and how Anna O., little Hans, Dora, President Schreber, the Rat Man, and the Wolf Man became our childhood friends is definitely very important, as is knowing the conditions under which each friendship developed. Training institutes mediate these friendships, in this way

familiarizing the candidates with Freud's work as a therapist, scientist, and author.

While writing this textbook we have returned to our own childhood friends and have studied several of Freud's large case histories in detail. Even though new elements can be discovered by rereading them, we have hermeneutic reservations about supporting Lacan's (1975, p. 39) call for a "return to Freud." With Laplanche (1989, p. 16), we "prefer to speak of *going back over* Freud, as it is impossible to return to Freud without working on him, without making him the object of work." In our reconsideration we do not meet these old friends in the same form as during our initial encounter with and enthusiasm for Katharina or little Hans. We have always viewed Freud's case histories in a somewhat different light and, unfortunately, have frequently shown too little concern for how Freud himself understood his texts. We were not, after all, introduced to the love for psychoanalysis through Freud alone, but also by spiritual parents who solicited support for their own views. In whom could we then place our trust and confidence in going back to Freud in order to ensure that ideas can be revitalized and point to the future that Arlow and Brenner (1988) and Michels (1988) envisage in their suggestions for reforming psychoanalytic training.

In view of the immensity of our task in determining which items belong to the past, it is impossible to rely on a single individual, not even someone of

the stature of Rapaport, who ventured (in 1960) to estimate the probable longevity of important psychoanalytic concepts. Which mediator should we turn to in attempting to master this hermeneutic task? Hermes' name did not provide the etymological source for the concept of hermeneutics, but as messenger and translator between the gods and the mortals he was also a participant in the doings and dealings of the world who always acted according to his own interests. The same is true of those interpreters who try to do justice to Freud's work without losing sight of their own interests. Practicing psychoanalysts are not the only ones who live from Freud's legacy; this is also true of the many authors for whom Freud's legacy is a playground for their criticism.

Can the analyst's acquisition of his own approach be considered a special form of translation? Uncertainty has spread since Brandt (1977) applied the play on the Italian words "traduttore-traditore" to the *Standard Edition* and thus made Strachey the translator into the traitor, and since Bettelheim's (1982) provocative book appeared. Following the criticism of Strachey's translation by Bettelheim (1982), Brandt (1961, 1972, 1977), Brull (1975), Ornston (1982, 1985a, b), Mahoney (1987), Junker (1987), and Pines (1985), nothing could illuminate the difficult situation of Anglo-American psychoanalysts who have relied on the *Standard Edition* better than the ironic title of Wilson's (1987) article, "Did Strachey Invent Freud?" The answer is obvious (see Thomä and Cheshire 1991).

The unjustified and very exaggerated criticism of Strachey's admirable achievement has in the last few years led the discussion onto a side track and distracted attention from the real reasons for the crisis of psychoanalysis. It is consequently more than naive to want to resolve this crisis allegedly caused by the *Standard Edition* with the aid of a new translation. Beyond demonstrating that Strachey made mistakes and distorted passages, which have been correctly pointed out by many authors, the criticism of the *Standard Edition* concerns the hermeneutic question of whether Strachey's translation distorted the work itself. To demonstrate mistakes in translation that distort meaning is a relatively simple matter. Yet we confront difficulties of a more principle nature—and not limited to Freud's works—because hermeneutics, i.e., the theory of the interpretation of texts, does not provide us with rules we can use as a mountain climber would a safety line while climbing a difficult mountain trail. We follow Schleiermacher (1977, p. 94) in assuming that it is possible after all for a reader to equate himself with an author both objectively and subjectively. Equating oneself with the author is one of the preconditions for being able to interpret a text and ultimately to understand the object better than the author himself (see Hirsch 1976, pp.37 ff.). According to Schleiermacher this task can be expressed as follows: "To understand the statement at first as well and later better than the author." Every reading enriches our basic store of knowledge and puts us in a better position to have a better understanding; thus Schleiermacher continues, "It is

only with insignificant things that we are satisfied with what we immediately understand" (p.95).

When we read Freud's treatment reports we naturally take our own experience as a basis for comparison, and in time we become more confident that we understand the subject better than the founder of psychoanalysis did. The growth of knowledge on our subject—in our context, the analytic technique—is fed by several sources. One factor is that the critical discussion of Freud's treatment reports has created a distance to them, so that we today view these childhood friends differently than when we had our initial experiences with them. Another factor helping us to make our own experience is the fact that creative psychoanalysts have discovered other and new aspects of the subject that have brought about changes in therapy and theory.

With a view to the many psychoanalysts and other Freud interpreters to whom we ourselves owe a debt of gratitude from our studies of Freud, we request that the reader identify with our interpretation on a trial basis. In this two-volume textbook we believe we have brought our long grappling for the foundations of psychoanalytic theory and its effectiveness as therapy to a preliminary conclusion in that we are able to ground a firm point of view. There is a lot at stake in our attempt to grasp the current crisis of psychoanalysis on the basis of Freud's works and their reception in the

psychoanalytic movement and in intellectual history as a whole. We hesitated for a long time to compress our ideas into a limited number of sentences because we are aware that this is a problem with far-reaching implications. It was Freud's grand idea to link, in an *inseparable bond* , the interpretative method he discovered for treating patients with causal explanations, i.e., with the study of the genesis of psychic and psychosomatic illnesses. Yet if proof of the causal relationship requires that the data be independent of suggestion by the therapist, then therapy destroys the science. If the analyst, on the other hand, believes that it is possible to refrain from making any suggestion whatsoever, in order to obtain uncontaminated data by means of pure interpretations, then he ruins the therapy without coming closer to a theoretical explanation if *independence* from the researcher is required. It is obvious that the analyst offering interpretations influences the patient even if he apparently only directs his interpretations to the unconscious and without any further-reaching aims, which is a self-deception as it is impossible. Instead of eliminating manipulations it opens the door to hidden manipulations.

Freud's inseparable bond thus contains a dilemma that has gone largely unrecognized because it suggested that following the rules served therapy and research equally. For decades the magic of this concept exerted a settling influence and appeared to solve the therapeutic and scientific problems of psychoanalysis with a single stroke. Only recently has it become obvious how

many methodological problems have to be solved to realize Freud's credo. It implies that therapeutic efficacy, i.e., symptomatic and structural change, as well as the truth of explanatory hypotheses are the two sides of the same coin: the gold of the pure psychoanalytic method without *direct* suggestion. Of course, the scientific and therapeutic problems are the inevitable and necessary indirect influence exerted by the analyst on the patient.

By contrasting the *case history* and the *treatment report* it is possible to demonstrate that the scientific reconstruction of the genesis of psychic and psychosomatic illnesses in the case history follows criteria that differ from those for treatment; the function of these criteria is to ground the theory of therapy and specify the conditions for cure. In Sect. 10.5 of Vol.1, we have described the individual consequences of loosening the inseparable bond and freeing the analyst from the excessive demands it places on him. To quote the concluding sentence from the first volume, "Freud's theory of technique requires that the analyst distinguish between the following components: *curing* , *gaining* new hypotheses,*testing* hypotheses, the *truth* of explanations, and the *utility* of knowledge" (p. 371).

With regard to therapeutic theory and its testing, we completely agree with Lorenzer's opinion that

> The goal of psychoanalytic understanding is to*achieve alterations in terms of the patient's suffering* ; psychoanalytic theory conceptualizes this suffering and the reactions to it. Psychoanalysis is thus a theory of the

therapeutic attitude toward suffering. (1986, p. 17, emphasis added)

One aspect of Lorenzer's definition is that it is very important to possess suitable methods for assessing change. Such investigations are part of therapeutic theory, but this theory raises questions that differ from those raised by the theory describing the etiology of psychic and psychosomatic illnesses.

Our study of the sources has convinced us that Freud grappled with this still unresolved dilemma for his entire life. Much can still be discovered in his works, and each renewed study of them enriched us. Yet the guides that Freud himself provided for satisfying the inseparable bond condition appear to us to be completely inadequate to meet the criteria for research designed to test hypotheses. For decades psychoanalysis was practiced under the cover of Freud's authority, in a manner that led to the stagnation of the therapeutic and scientific potential offered by the psychoanalytic method. It was more than unfortunate that explanatory theories were tied to metapsychology. Many pseudoscientific constructions have resulted from this union and have impeded the study of causal relationships and the attempt to solve the problems associated with the explanatory theory of psychoanalysis. Causal research cannot consist in employing metapsychological terminology to describe clinical phenomena. Grünbaum's interpretation that the study of the causal relationships surrounding the genesis of psychic and psychosomatic illnesses is not tied to metapsychological concepts is convincing. Fara and

Cundo (1983, pp.54-55) have shown in an ingenious study that different approaches are combined in all of Freud's works although the mixture of metapsychological models and art of interpretation is always different.

In the first volume we demonstrated that Freud's materialistic monism, which determined his metapsychology, was probably the cause of the subsequent mistakes and confusion. Habermas' claim, however, that Freud fell victim to a "scientistic selfmisunderstanding" not only inaccurately judges the significance of causal research in psychoanalysis, as a result of an unfortunate linkage of such research with metapsychology, but also burdens therapy with a handicap that, as we have demonstrated in detail elsewhere (Thomä et al. 1976), was made even more severe by Lorenzer. Both of these influential authors have filled old wine into new bottles that have impressive labels simply because they were renamed. As *metahermeneutics* or *depth hermeneutics* it has been possible not only for the old metapsychological points of view to survive but also to influence practice indiscriminately for the first time in the history of psychoanalysis, because they were put in direct relationship with the interpretive process. Neither Habermas nor Lorenzer seems to have recognized that large portions of metapsychology derive from the fact that Freud "psychologized" the "neurophysiological hypotheses" of his time, to use Bartel's (1976) words.

Yet of course not all "self-misunderstandings" are the same. It is

possible to distinguish between different kinds of ignorance on the part of authors. Freud was not in a position to have a clear understanding of many of the implications of the therapeutic and scientific applications of his method. In this sense his work has suffered the same fate as that of all discoverers and authors of importance in intellectual history, namely that later researchers have understood some things better than the founder, discoverer, or author himself. As far as we have been able to refer to the relevant literature, we have not found any convincing arguments to justify the thesis of a scientistic self-misunderstanding. Habermas himself has to concede that an analyst bases his interpretations on *explanatory theories* . Freud's error was not his credo in causality but that he based it on the psychophysiology of "psychic energy."

It is an especially urgent task that social science perspectives be taken into consideration in psychoanalytic research, as we point out in our "Introduction" to Chap.2. This could provide psychoanalysis a scientific foundation that leads beyond the polarization between interpretative skill and explanation. We consider ourselves, at any rate, to belong to the group of hermeneuticists whose prime precept is that their interpretations be validated. We speak of an autonomous hermeneutic technology in order to emphasize that the psychoanalytic art of making interpretations is indebted to validations that are of necessity also concerned with the question of causal relationships. Hirsch (1967, 1976), whose understanding of hermeneutics is

characterized by sober pragmatism, argues along the same lines. It is surprising that his studies have hardly received any attention in the Anglo-American psychoanalytic literature from authors following a hermeneutic approach. Rubovits-Seitz (1986) was recently the first to emphasize that Hirsch's view of hermeneutics places high demands on the grounding of interpretations.

In summary, it is possible to say that our disentanglement of the inseparable bond is not only useful for research but enables psychoanalytic practice to be innovative. One side effect of the social psychological understanding of the psychoanalytic situation has been the discovery of new aspects of transference and countertransference. Clarifying such distinctions is thus not only essential for research designed to test hypotheses, which is of increasing importance in our time, but also well suited to prepare the ground for new discoveries and new hypotheses. Freud's inseparable bond assertion belonged to a phase in which ingenious analysts were able to make discoveries about psychic relationships in almost every treatment. Today it is far more difficult to discover something truly new and to formulate it in a way meeting the demands raised by research concerned with the *verification of hypotheses* .

A cooperative effort is necessary to move Freud's paradigm into a phase of normal science. Although we definitely cannot expect philosophers to solve

our empirical problems, we no longer have any doubt that even the study of extended psychoanalytic dialogues by philosophers would prove more productive than their epistemological criticism of Freud's works. Regardless of the significance of self-reflection in therapy, it would hardly have been possible after the study of several transcripts of tape recorded sessions for Habermas to make psychoanalysis into a purely reflective science. Ricoeur, for his part, could have discovered that psychoanalysts also observe. Finally, Grünbaum would feel confirmed that psychoanalysts search for relationships that may be of causal relevance and might have even discovered that psychoanalysts are today more cautious in claiming to have found the past and unconsciously still active causes of symptoms than Freud was. On the other hand, it is impossible to uphold Grünbaum's view that the influence exerted by the analyst contaminates the data in a way that cannot be disentangled. The dialogues presented in this volume, for example, make it possible to recognize different degrees of suggestion. It is true, however, that the demands raised by Meehl (1983)—that the large spectrum of means ranging from persuasion to manipulation be registered—have not yet been met. The suggestive elements of the psychoanalytic technique of interpretation are themselves becoming the object of joint reflection, whose goal is to eliminate dependencies. It is surprising that Grünbaum (1985) himself did not point to such useful applications of his epistemological study of the placebo concept. He demonstrated that the discrimination of the

characteristic and *spurious factors* with regard to the medication for a *syndrome under investigation* depends on the particular theory of therapy. Without wanting to reopen the discussion of nonspecific and specific or general and specific factors that is included in the first volume (Chap. 8), we do want to mention that Strupp (1973, p. 35) and Thomä (1981, p. 35) have shown that the valence of the therapeutic influence exercised in a particular situation depends on that situation itself. A reliable and valid clinical classification of characteristic and spurious factors is thus difficult but not impossible. Finally, we believe that the study of the dialogues presented in this volume can also lead the epistemological discussion out of its ivory tower.

Freud (1933a, p. 151) referred to the treatment of patients as the *home-ground* of psychoanalysis. This is the source of the interpretative method of therapy that, in contrast to hermeneutics in theology and the humanities (Szondi 1975), systematically examines the unconscious psychic life of patients who come to analysts hoping for an *end* to their suffering. This therapeutic goal distinguishes psychoanalytic hermeneutics significantly from other hermeneutic disciplines. Works of art cannot in general be damaged by an interpretation, and a dead artist can only metaphorically turn around in his grave if he does not agree with an interpretation. Psychoanalytic interpretations interfere in human destinies. Patients seek help for their symptoms, and whether an improvement or cure is achieved is fundamental

to them. Texts are not affected by differing exegeses and interpretations, and cannot make critical comments of their own.

The analyst thus must not only justify his therapeutic actions in the individual case, but also has the responsibility of continuously examining the accuracy of his theoretical ideas about the unconscious and about human experience and behavior. In contrast to hermeneutics in theology and the humanities the founder of psychoanalysis linked the art of making therapeutic interpretations to explanatory theories. Freud assumed that his theory of psychogenesis had causal *relevance* and raised the demand that the analyst differentiate between the *necessary* and the *sufficient* conditions regarding the genesis and course of psychic and psychosomatic illnesses. Later reconstructions have shown them to be postdictions. For this reason Freud's concept of *restrospective attribution* (*Nachträglichkeit*) assumes a significance that has been largely underestimated, as we show in Sects. 3.3 and 6.3.

The analytic dialogue is doubtlessly concerned with words. These words mean something, and this something is nothing exclusively sensory or linguistic. The words "connection," "relation," "relationship," "synthesis" etc. appear in Freud's works for the term "explain," in accordance with the scientific usage of the time. Freud (1901a, p. 643) spoke, for example, with regard to the conditions under which the manifest dream is constituted, of its

"regular relations" with the latent dream thoughts. In principle he was concerned with clarifying causal relationships; in individual cases he was mistaken regarding the question of empirical proof and on the whole underestimated the problems posed by research concerned with verifying hypotheses.

Clinical psychoanalysis is subject to research about its course and results. Freud's explanatory theories were based on his therapy, and they in turn have had a lasting influence on the interpretive method. Therefore interpretations are wrong if they are derived from a component of the theory that has been refuted. For example, in view of the results of recent research on mother-child interaction and of epidemiology, many assumptions of the general and specific theories of neurosis are questionable (Lichtenberg 1983). It is especially essential for therapeutic theory to be revised.

In revising the technique we can proceed from several of Freud's assumptions that have been ignored. It is especially with this thought in mind that we have given this section the heading "Back to Freud and the Path to the Future." According to Freud (1937c, p.250), "the business of analysis is to secure the best possible psychological conditions for the functions of the ego; with that it has discharged its task." If we relate this statement to the treatment situation and not only to the patient's ultimate ability to master the difficulties of everyday life without developing symptoms, then it is possible

to formulate the following general thesis: Favorable conditions for the resolution of conflicts in the treatment situation are those that make it possible for the patient to transform the passive suffering from the original pathogenic traumas into independent action. This is a generalization of Freud's trauma theory; at its center is *helplessness* , at least since Freud's article "Inhibitions, Symptoms, and Anxiety" (1926d; see Vol.1, Sect. 8.7). We agree with Freud (1926d, p. 167) that "the ego, which experienced the trauma passively, now repeats it actively in a weakened version, in the hope of being able itself to direct its course. It is certain that children behave in this fashion towards every distressing impression they receive, by reproducing it in their play. In thus changing from passivity to activity they attempt to master their experiences psychically." This thesis can be generalized even further: "Through this means of going from passivity to activity [man] seeks to master psychically his life's impressions" (G. Klein 1976, pp.259 ff.). Klein has shown convincingly that the neurotic and psychotic repetition compulsion described by Freud takes place for psychological reasons, both affective and cognitive. This exacerbates the patient's feeling of passive helplessness, which continuously makes it more difficult for him to overcome past conditions of anxiety. Such unconscious expectations have the function of filtering perception in the sense of a negative self-fulfilling prophecy, so that that patient either does not have positive experiences or brackets out pleasant experiences and empties them of meaning. Sacrifices, punishments, and hurt

feelings in the distant past—in short, all traumatic experiences—are not only conserved in this way, but enlarge cumulatively in everyday life and even in therapy if the course is unfavorable. We believe that we do justice to viewing psychogenesis as an ongoing process by expanding the theory of cumulative traumatization inaugurated by Khan (1963) to apply to the entire life cycle.

The life histories of many people are structured, for unconscious reasons, in ways that lead predispositions to be confirmed and new traumatic experiences to occur continuously. For example, "jealous and persecutory paranoics . . .project outwards on to others what they do not wish to recognize in themselves . . . .but they do not project it into the blue, so to speak, where there is nothing of the sort already . . . .they, too, take up minute indications with which these other, unknown, people present them and use them in their delusions of reference" (Freud 1922b, p.226). In one of his late works Freud emphasized the fundamental significance of such processes:

> The adult's ego, with its increased strength, continues to defend itself against dangers which no longer exist in reality; indeed, it finds itself compelled to seek out those situations in reality which can serve as an approximate substitute for the original danger, so as to be able to justify, in relation to them, its maintaining its habitual modes of reaction. Thus we can easily understand how the defensive mechanisms, by bringing about an ever more extensive alienation from the external world and a permanent weakening of the ego, pave the way for, and encourage, the outbreak of neurosis. (1937c, p. 238)

In such a process symptoms can be given new contents. This age-old

discovery of Freuds (1895d, p. 133) is theoretically grounded in particular in Hartmann's concept of change in function, but its relevance to technique has not been systematically worked out. For this reason we put special emphasis, in Sect. 4.4 of Vol.1, on how symptoms can maintain themselves in a vicious circle that becomes increasingly strong on its own. Every day it is possible for situations of helplessness and hopelessness to develop whose contents are very different from the original traumas. A sure sign of this process is an increasing sensitivity to feeling offended, which enhances the patient's receptiveness to all kinds of stimuli. Finally, events that seem banal when viewed superficially can have drastic consequences for oversensitive people —and the feeling of being offended is a heavy burden on all interpersonal relations.

As a result of such repetitions, which we understand on the basis of the extension of the theory of trauma, it is also possible for a patient to feel offended in therapy. Such events, which must be taken very seriously, occur despite the analyst's efforts to create a friendly atmosphere. An unfavorable effect can even result if the analyst believes that it is possible to produce a kind of psychoanalytic incubator, i.e., constant conditions enabling undisturbed psychic growth to take place. The patient can also feel offended as a result of the setting and of the misunderstandings that inevitably occur, and the traumatic effects are stronger the less it is called by name, and recognized and interpreted as such (see Vol.1, Chap.7 and Sect. 8.4).

For a long time analysts did not recognize the severity of the trauma that can occur as a consequence of transferences that are associated with a repetition of old oedipal or preoedipal frustrations and that, moreover, can also affect the adult patient in new way. The traumatic consequences of transference were probably not discovered until late because the frustration theory of therapy seemed to justify it. In an unpublished speech held at the Budapest congress in 1987, Thomä emphasized the fact that traumatic events can be an unintended side effect of transference. At that time the profound discoveries that Ferenczi (1988) had recorded in his diary in 1932 were largely unknown. He described how professional attitudes and psychoanalytic rules can have new traumatic consequences of their own and revive traumas that analysis is supposed to help the patient overcome.

The consequences we draw from the rediscovery that traumatization is a constituent element of the analytic situation differ from Ferenczi's. We believe that our readiness to let the patient take part in the process of interpretation and, if necessary, in countertransference helps to overcome new and old traumas. Balint's two and more person psychology extended Freud's definition of helplessness to characterize the traumatic situation and drew attention to the unintentional and antitherapeutic microtraumas in the psychoanalytic situation. It could be of fundamental importance that this basic problem of technique led to the polarization into schools—on the one hand, the mirroring analyst who apparently cannot be injured or offended

and, on the other hand, the loving analyst who, as a person, attempts to compensate for deficiencies.

A new era was initiated when Weiss and Sampson (1986) refuted the frustration theory on the basis of an experimental design, in favor of the mastery theory of therapy. The analyst must display great determination in considering every possibility at his disposal for countering the patient's repeated feelings of being offended during the analysis of transference, including its unfavorable effects on the patient's self-esteem and self-confidence. First steps in this direction are Klauber's emphasis on spontaneity as an antidote to traumatization in transference and Cremerius' (1981b) detailed description of the therapeutic significance of naturalness in the analyses Freud conducted.

Some aspects of Kohut's self-psychological technique indicate how strongly the frustration theory has established itself, to the detriment of the therapeutic effectiveness of psychoanalysis and in promoting a pseudoscientific idolatry. Kohut believed it is only possible for the analyst, as part of his adherence to analytic abstinence or neutrality, to provide narcissistic gratification and not true confirmation. This retention of a misunderstood concept of neutrality by Kohut removes the emotional basis from the confirmation and encouragement that are therapeutically so important, thus not strengthening the patient's realistic self-esteem but

creating an as-if situation. According to Kohut's selfobject theory, confirmation does not even come from a significant other, but represents a kind of narcissistic self-confirmation—a reflection of the patient's own self.

The fear that acknowledgment might lead to oedipal seduction and incestuous wish fulfillment will continue to diminish in the new era of psychoanalytic therapy. Genuine acceptance substantially limits the occurrence of traumas from transference and considerably improves the therapeutic effectiveness of psychoanalysis. A constant theme running through all the chapters of this book is the question of how the analyst creates the conditions in his office that are most conducive to therapeutic change. The issue is to further the growth of the patient in ways so that he can cope and master old and new situations of helplessness and anxiety. The concept of working through conflicts must be subordinated to the comprehensive theory of mastering. Previously neglected therapeutic potential can be derived from the psychoanalytic theory of anxiety, which we recapitulate in Sect. 9.1, if the mechanisms of defense are understood from the perspective of coping in the here and now.

1.2 Case Histories

In the case histories that he published, Freud pursued the goal of demonstrating the connection between illness and life history. The conclusion

he reached was that the genesis of psychic and psychosomatic illnesses should be understood as a *complemental series* . There must be a convergence of many factors for neurotic disturbances to develop and become chronic. An individual's capacity to cope with stress in a critical phase of life depends on his disposition, which he acquires as a result of formative influences and conflicts in childhood and in adolescence, which he in turn acquires on the basis of an innate reaction readiness. Oedipal conflicts have far-reaching implications for everyone's life history because, first, it is in them that the basic structuring of *psychosexual differentiation* takes place and, second, the acceptance of the specific psychosocially defined sex role—which is subjectively experienced as a feeling of identity that refers and is tied to one's sex—is elementary. Whether and how these conflicts subside or whether they form an unconscious structure that can be diagnosed on the basis of typical forms of behavior and experiencing depends in turn on many sociocultural and familial constellations.

The phenomenon referred to as the overdetermination of symptoms and the fact that pathological processes are maintained by subsequent unfavorable events have remarkable consequences for therapy. Overdetermination makes it possible for the therapeutic effects of the analyst's interventions to spread via the network of these very conditions. These therapeutic consequences extending beyond the immediate focus are the result of the role of overdetermination in the etiology of neuroses, or in

Freud's words, "their genesis is as a rule overdetermined, [and] several factors must come together to produce this result" (1895d, p.263). Overdetermination does not refer to a multiple determination in the sense that each condition or each individual cause in itself would cause an event, a parapraxis, a slip of the tongue, or a symptom. On the contrary, it is the *convergence* of several motives in speech disturbances that Wilhelm Wundt described and that Freud incorporated in the concept of overdetermination (1901b, p. 60). The assumption of overdetermination makes it necessary, with regard to the genesis of psychic and psychosomatic illnesses, to establish a hierarchy of factors and to distinguish the conditions into those which are *necessary* and those which are *sufficient* . Accordingly, we have to start from the possibility that the causal factors can be combined in various ways, e.g., necessary, sufficient, sometimes necessary, sometimes sufficient, together necessary, together sufficient, etc. The discussion that Eagle (1973a,b) and Rubinstein (1973) initiated following the publication of Sherwood's book *The Logic of Explanation in Psychoanalysis* (1969) shows that Freud, who translated several works by J.S. Mill into German, advocated a philosophically well-grounded theory of causality (Thomä and Cheshire 1991; Cheshire and Thomä 1991). The following passage from one of Freud's early publications refers to several important concepts within a causal theory:

> (a) *Precondition* , (b) *Specific Cause* , (c) *Concurrent Causes* , and, as a term which is not equivalent to the foregoing ones, (d) *Precipitating or Releasing Cause* .

In order to meet every possibility, let us assume that the aetiological factors we are concerned with are capable of a quantitative change—that is of increase or decrease.

If we accept the idea of an aetiological equation of several terms which must be satisfied if the effect is to take place, then we may characterize as the *precipitating* or releasing cause the one which makes its appearance last in the equation, so that it immediately precedes the emergence of the effect. It is this chronological factor alone which constitutes the essential nature of a precipitating cause. Any of the other causes, too, can in a particular case play the role of precipitating cause; and [the factor playing] this role can change within the same aetiological combination.

The factors which may be described as *preconditions* are those in whose absence the effect would never come about, but which are incapable of producing the effect by themselves alone, no matter in what amount they may be present. For the specific cause is still lacking.

The *specific cause* is the one which is never missing in any case in which the effect takes place, and which moreover suffices, if present in the required quantity or intensity, to achieve the effect, provided only that the preconditions are also fulfilled.

As *concurrent causes* we may regard such factors as are not necessarily present every time, nor able, whatever their amount, to produce the effect by themselves alone, but which operate alongside of the preconditions and the specific cause in satisfying the aetiological equation.

The distinctive character of the concurrent, or auxiliary, causes seems clear; but how do we distinguish between a precondition and a specific cause, since both are indispensable and yet neither suffices alone to act as a cause?

The following considerations seem to allow us to arrive at a decision. Among the '*necessary causes*' we find several which reappear in the aetiological equations concerned in many other effects and thus exhibit

> no special relationship to any one particular effect. One of these causes, however, stands out in contrast to the rest from the fact that it is found in no other aetiological equation, or in very few; and this one has a right to be called the *specific* cause of the effect concerned. Furthermore, preconditions and specific causes are especially distinct from each other in those cases in which the preconditions have the characteristic of being long-standing states that are little susceptible to alteration, while the specific cause is a factor which has recently come into play. (1895 f, pp.135-136)

These four factors have to converge to create a complete "aetiological equation." The complexity of causes poses a difficult task because different sufficient or necessary causes can be linked or replace each other. An exception is the specific cause, which by itself is sufficient if there is a certain predisposition. The context of the Freud quotation shows that the model for this cause and effect relationship is the *specific* pathogen that is responsible for an infectious disease and that can be deduced by pathologists from very particular transformations of tissue that are also referred to as specific.

In psychic and psychosomatic illnesses the disposition that develops in the course of an individual's life history takes on a special significance as the necessary condition, in contrast to the external "stimulus" which is the precipitating factor. These two factors, i.e., the necessary conditions, therefore play a correspondingly large role in Freud's model of scientific explanation. We will return to these problems when we discuss the specificity hypothesis in psychosomatic medicine (Sect. 9.7), yet in the context of Freud's case histories it should be noted that his explanatory model has proven itself

to be exceptionally productive, even though the *validity* of many individual causal assumptions must be doubted today. The logic of the causal schema has not been refuted, rather the relationships discovered in individual cases have turned out to be wrong or have had to be relativized. We must keep this distinction in mind. Freud linked his model of *complemental series* to the causal theories of Hume and Mill (Eimer 1987). The interrelatedness of the factors makes it possible for the effects of therapeutic interventions to be reproduced via the above-mentioned network, the "nodal points" (Freud 1895d).

Freud's *causal* model of the etiology of psychic illnesses is complemented by a corresponding understanding of therapy. In order for an individual to find solutions to the problems later posed by life and to be able to discover connections between very different human activities, it may be necessary to descend "into the deepest and most primitive strata of mental development" (Freud 1918b, p. 10; see Vol. 1, Sect. 10.2).

Freud's case histories are reconstructions that proceed from an individual's present situation and attempt to find the roots and typical conditions of symptoms in the individual's past. With regard to the symptoms of psychic and psychosomatic illnesses, time does appear to stand still—the past is present. The phobic is just as afraid of a completely harmless object today as he was 10 or 20 years ago, and compulsive thoughts and actions are

repeated ritually in the same way for years.

Neurotic symptoms are so embedded in the patient's life history that knowledge of it is essential for comprehending the specific pathogenesis. "Case histories of this kind are intended to be judged like psychiatric ones; they have, however, one advantage over the latter, namely an intimate connection between the story of the patient's sufferings and the symptoms of his illness" (Freud 1895d, p. 161).

Of special significance is the case history of the Wolf Man, which Freud published under the title *From the History of an Infantile Neurosis* (1918b). A very substantial secondary literature has appeared on this patient alone, which in 1984 had amounted to about 150 articles (Mahony 1984). Despite many reservations about the demonstration or validation of psychoanalytic explanations, Perrez (1972) concludes that the description of the Wolf Man is beyond doubt a grand attempt to explain the puzzles that this case presented in the form of a narrative. The designation "narrative," introduced by Farrell (1961), acknowledges an aspect of the case histories that filled Freud with a certain sense of uneasiness—namely, "that the case histories I write should read like *short stories*" (Freud 1895d, p. 160). He sought recognition as a scientist and was concerned that his description of the fates of human beings might "lack the serious stamp of science" (Freud 1895d, p. 160). The Goethe Prize honored Freud the author, whose style has attracted students of

literature from Muschg (1930) to Mahony (1987; see Schönau 1968).

The special tension contained in Freud's case histories results, in our opinion, from the fact that all the descriptions in them have the goal of making the background of the patients' thoughts and actions plausible in order to be able to present explanatory outlines of their history.

> Of special importance is the fact that the analysis of one of Freud's case presentations clearly shows that Freud was not only concerned with *describing* the history of a neurosis. He was concerned most of all with *explaining* it, and apparently in the form of a *genetic historical explanation* . The genetic historical form of explanation not only attempts to describe a chain of events, but also to show*why* one state leads to the next. For this reason it makes use of certain laws of probability, and in the case of Freud's narratives this is not always made explicit. (Perrez 1972, p. 98)

However unclear the etiology may be in an individual case and however insufficiently statistical probabilities and laws may be validated, the general result still holds that schemata of experiencing and behavior anchored in the unconscious develop over a very long period of time. Thus there is not only the danger that repeated adverse experiences can lead to the formation and maintenance of stereotypes, but also always a good chance that positive experiences can alter motivational schemata. Freud's conversation with Katharina may have opened new perspectives for this young girl, who consulted him in passing in an alpine lodging. Noteworthy is that this conversation provides an especially precise view into how Freud conducted diagnostic-therapeutic interviews (Argelander 1978).

The uniqueness of each life history links the psychoanalytic method to the rationale of the "single case study" (Edelson 1985). *Scientific* aims, of course, go beyond single case studies and are directed at postulating generalizations; Freud therefore emphasized in his report on the Wolf Man that generalizations can only be gained with regard to certain assumptions about pathogenesis by presenting numerous cases that have been thoroughly and deeply analyzed (Freud 1918b).

Since the primary purpose of Freud's case histories was to reconstruct the psychogenesis, i.e., to demonstrate that symptoms have repressed unconscious causes, the description of therapeutic technique took second place. Freud did not discuss *technical* rules systematically in his treatment reports. He only mentioned in a rather fragmentary way what he felt, thought, interpreted, or otherwise did in a particular session.

Freud distinguished between *case histories* , which he occasionally referred to as the histories of illnesses, and *treatment histories* . We have adopted this distinction, except that we prefer the designation *treatment reports* because of the significance of the different forms of documentation. Freud pointed out the difficulties confronting suitable reporting in an early publication:

> The difficulties are very considerable when the physician has to conduct six or eight psychotherapeutic treatments of the sort in a day, and cannot 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. (Freud 1905e, pp.9-10)

He was referring in this instance to Dora, whose case history and treatment he described in the *Fragment of an Analysis of a Case of Hysteria* . His task of reporting this case was eased by two circumstances, namely the brevity of the treatment and the fact that "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" (Freud 1905e, p. 10).

Freud did not write the case history itself, the core of the publication, until after the cure; he did it from memory but claimed it had a high degree of precision. According to his own words, he accepted incompleteness with regard to the *treatment history* as a given:

> I have as a rule not reproduced the process of interpretation to which the patient's associations and communications had to be subjected, but only the results of that process. Apart from the dreams, therefore, the technique of the analytic work has been revealed in only a very few places. My object in this case history was to demonstrate the intimate structure of a neurotic disorder and the determination of its symptoms; and it would have led to nothing but hopeless confusion if I had tried to complete the other task at the same time. Before the technical rules, most of which have been arrived at empirically, could be properly laid down, it would be necessary to

> collect material from the *histories* of a large number of *treatments* . (Freud 1905e, pp.12-13, emphasis added)

Freud did not assign any special weight to the abbreviated form of this description because transference "did not come up for discussion during the short treatment," which only lasted three months (1905e, p. 13). A similar predominance of the case history at the expense of the treatment history can be found in all of the case reports Freud published.

Freud's reason for putting the genesis of neurotic symptoms at the center of his published case histories was his view that clarifying the genesis and achieving more insight are the factors that create the best preconditions for therapeutic interventions. A representative quotation reads: "We want something that is sought for in all scientific work—to understand the phenomena, to establish a correlation between them and, in the latter end, if it is possible, to enlarge our power over them" (Freud 1916/17, p. 100).

Not Freud's case histories, but his five technical works are, according to Greenson (1967, p. 17), the source from which an analyst can learn to create the best conditions for therapeutic change. Considering Freud's unique position in psychoanalysis, the fact that he did not provide a synoptic description of his technique comprising both theory *and* practice has had lasting consequences. His case histories acquired exemplary character for the psychoanalytic theories describing the conditions of genesis, and were

referred to in this way by, for example, Sherwood (1969), Gardiner (1971), Niederland (1959), Perrez (1972), Schalmey (1977), and Mahony (1984, 1986). Freud was more concerned with specifying rules of research to clarify the genesis rather than with making these rules the object of study to determine whether they provide the patient the necessary and sufficient *conditions for change* (see Vol.1, Sects. 7.1 and 10.5).

At the beginning of therapy the *neurosis* becomes the *transference neurosis* regardless of how deep its roots reach back into and are anchored in the patient's life history (see Vol.1, Sect. 2.4). Even if the domain referred to by this concept has not been sufficiently defined, as is assumed by prominent analysts in the controversial discussion edited by London and Rosenblatt (1987), we cannot overlook the fact that the analyst makes a substantial contribution to determining the nature of the transference. In this sense, school-specific transference neuroses even develop, contradicting to Freud's idea that simple observance of the rules of treatment lead transference neuroses to develop uniformly. This extension of the theory of transference and countertransference follows from the recognition of the analyst's influence. These developments were eased by the fact that it has become possible in recent years to acquire some insight into Freud's own practice, deepening our understanding of the case histories he reconstructed and also extending our knowledge of how he applied technical rules.

In Vol.1 we referred to the fact that the increasing amount of literature about Freud's practice has facilitated the critical reappraisal of the history of the psychoanalytic technique (Cremerius 1981b; Beigler 1975; Kanzer and Glenn 1980). When necessary, Freud gave patients board, lent them money, or even gave it to them. Yet it would, of course, be naive to want to find solutions to today's problems by identifying with Freud's natural and humane attitude in the consulting room as he apparently disregarded the consequences of transference.

It is characteristic of Freud's case histories that they, on the one hand, report the concrete analysis of an individual case while, on the other hand, containing far-reaching hypotheses that attempt to present the entire wealth of clinical observations in condensed form and to put them in a causal connection.

According to Jones (1954), Charcot's nosographic method exerted a lasting influence on Freud's goals with regard to the reconstruction of the genesis and course of psychogenic illnesses. Freud did not study the technical rules primarily to determine whether they provide the best conditions for therapeutic change. He instead wanted his technical recommendations to secure the scientific foundation for the psychoanalytic method: "We have a right, or rather a duty, to carry on our research without consideration of any immediate beneficial effect. In the end—we cannot tell where or when—

every little fragment of knowledge will be transformed into power, and into therapeutic power as well" (Freud 1916/17, p. 255). The rules Freud set down were supposed to guarantee the objectivity of the results and to limit the analyst's influence on the data as much as possible. The documentation of the phenomena observed in interviews was oriented around the statements made by patients that were incorporated into the case histories because of their assumed causal relevance. The material is structured by the method, according to Freud's fundamental thesis:

> What characterizes psycho-analysis as a science is not the material which it handles but the technique with which it works. It can be applied to the history of civilization, to the science of religion and to mythology, no less than to the theory of the neuroses, without doing violence to its essential nature. What it aims at and achieves is nothing other than the uncovering of what is unconscious in mental life. (Freud 1916/17, p. 389)

Of course it makes an immense difference whether the psychoanalytic method is applied to cultural history or is practiced as a form of therapy, because the patient comes to the analyst expecting a lessening or cure of his suffering. By providing therapy the analyst assumes a responsibility that does not arise in the interpretation of mythology or in other applications of the psychoanalytic method. Most importantly, however, the patient is a critical witness of the analyst's actions.

1.3 Treatment Reports

Attention was so focused on the dialogue between patient and analyst in the metamorphosis from the case history to the treatment report that the preparation of protocols according to *selective* criteria has become the object of intense interest. Freud's literarily stimulating description of the Rat Man, of which Mahoney (1986) recently presented a linguistic interpretation, owes its wealth of details to the daily notes that Freud was accustomed to making from memory. The protocols about the Rat Man were first published in 1955, in Vol.10 of the *Standard Edition* .

When Zetzel, while preparing an article, turned to the *Standard Edition* instead of to the *Collected Papers* , she found Freud's protocols, which until then had gone largely unnoticed. They are instructive particularly with regard to his therapeutic technique, yet also provide important additional information about the genesis of the symptoms. Freud's notes contain over 40 references to a highly ambivalent mother-son relationship, which were not given adequate consideration in the case history published in 1909 (Zetzel 1966). Freud (1909, p. 255 himself noted, "After I had told him my terms, he said he must consult his mother." This important reaction by the patient is not mentioned in the case history. Since these protocols have become known, the case history of the Rat Man has been reinterpreted by Shengold (1971) and Holland (1975), in addition to the others named above.

Like all psychoanalysts, Freud prepared his protocols according to some

criteria, i.e., selectively; they guided what he selected from his notes. Freud used the individual cases to describe examples of typical connections and processes in psychic life.

The notes Freud made about the Rat Man have aroused attention because the founder of psychoanalysis did not observe—neither then nor later on—the technical recommendations that were later incorporated into the system of psychoanalytic rules. Yet as we outlined above and explained in Vol.1, the solution to technical problems cannot be found in a return to Freud's unorthodox style of treatment.

We see a sign of radical change in the fact that analysts are devoting more attention to the dyadic nature of the analytic situation while preparing protocols of treatment, both for shorter and longer periods. Influential psychoanalysts from all the different schools have contributed to this change toward the adoption of an interpersonal point of view in presenting case material.

The criteria that must be applied in order to write a convincing case history, i.e., a reconstruction of the conditions of genesis, are different from those for the description in a treatment report. Treatment reports focus on determining whether change has occurred and which conditions led to the change. Freud could be satisfied with making relatively rough distinctions

that left a lot to subsequent research. From today's point of view, however, Freud's case histories are not suited to serve either as a model for a reconstruction of the etiology or as a paradigm for protocols of psychoanalytic treatment. The task of creating the most favorable conditions for change and of investigating the therapeutic process is a very challenging one. Similarly, etiological research that is designed to provide evidence to test hypotheses demands too much of the individual analyst. Following Grünbaum's (1984) criticism, Edelson (1986) drafted an ideal model according to which a case history and a treatment report would have to be written today in order to make it possible for hypotheses to be tested.

Insights can be gained into Freud's technique by reading any of his case histories. The emphasis in each of them is on reconstructing the genesis of the particular neurosis. Freud also gave some examples of therapeutic interventions, sometimes even word for word. We recommend that anyone reading one of Freud's case histories consult a representative book from the secondary literature for critical guidance.

The post-Freudian development of the preparation of case histories and treatment reports has in fact been characterized by an increase in the number of large-scale case reports (Kächele 1981). In the last few years there has been an unmistakable and growing tendency for more and more analysts to make their clinical work accessible to readers. Given adequate preparation,

this can put the critical discussion within the profession on a sound footing. However, in the psychoanalytic literature the "vignette" is still the primary form of presentation. A vignette is characterized by unity, subtlety, and refinement (see discussion in Thomä and Hohage 1981) and serves to illustrate typical psychodynamic connections. In it the implications for the analyst's therapeutic actions are secondary in comparison with this focus of interest. Greenson (1973, p. 15) has also criticized older textbooks, including those by Sharpe (1930), Fenichel (1941), Glover (1955), and Menninger and Holzman (1977), for hardly describing how the analyst actually works and what he feels, thinks, and does.

Thus we are justified in joining Spillius (1983) in complaining-as she did in her critical survey of new developments in the Kleinian therapeutic technique—about the lack of availability of representative treatment reports prepared by leading analysts. Everywhere case reports are primarily supplied by candidates in training, who submit them for admission into the psychoanalytic societies; because of their compromising character, these reports are of dubious value, as Spillius rightly emphasized. This state of affairs is confirmed by the exceptions, and we do not want to miss the opportunity of favorably mentioning a few examples of them.

Shortly before her death M. Klein completed a comprehensive treatment report of the 4-month analysis of a 10-year-old boy (from 1941), whom she

named Richard; it was published in 1961.

> 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 extensive notes I made enable the reader to observe how interpretations find confirmation in the material following them. The day-to-day movement in the analysis, and the continuity running through it, thus become perceptible. (Klein 1961, p. x)

There is hardly another treatment report in which the analyst's theoretical assumptions as clearly determine his actions as in this report, which reproduces all 93 sessions in detail. In addition to the reviews by Geleerd (1963) and Segal and Meltzer (1963), there is also a thorough study by Meltzer (1978) that contains a detailed reappraisal of the course of the case.

Another treatment report, a large one by Winnicott (1972) entitled "Fragment of an Analysis," was also published posthumously, in a volume edited by Giovacchini (1972). The interactive nature of the exchange of thoughts between this patient and Winnicott irritated the French analyst A. Anzieu (1977, p. 28) because, according to her argument, the large number of Winnicott's interpretations made it impossible to perceive what the patient had said. Analysts in Lacan's sphere of influence are often extremely reticent, one of the items that Lang (1986) criticized. Lacan himself has not provided detailed clinical descriptions, and there are no empirical studies either, especially linguistic ones, although it would seem natural for such studies to be conducted considering his particular theses. Only a few indications of

Lacan's treatment can be drawn from the published version (1982) of Lacan's diagnostic interview of a psychotic patient that was recorded. He merely explored the patient's symptoms by using the traditional psychiatric technique of clarifying the psychopathology by questions.

In strong contrast to this is Dewald's (1972) description of a psychoanalytic process. He bases his account, just as Wurmser (1987) later did, on protocols of sessions recorded in shorthand, which provided Lipton (1982) an excellent basis for his criticism of Dewald's technique (see Vol.1, Chap.9).

An ideal example is also provided by a discussion that Pulver (1987) edited under the title "How Theory Shapes Technique: Perspectives on a Clinical Study." The basis of the discussion is a collection of an analyst's (Silverman) notes. The analyst prepared a protocol containing his thoughts and feelings in addition to the interpretations he made and the patient's reactions in three sessions. This clinical material was examined by ten analysts who are prominent representatives of the various psychoanalytic schools. Shane (1987) and Pulver (1987) summarized the results of the discussion, in which each of the analysts naturally started from his own personal point of view. Silverman, the treating analyst, is known as an adherent of structural theory.

After an evaluation of the material by Brenner (structural theory), Burland (Mahler's school), Goldberg (self psychology), and Mason (Kleinian perspective), Shane concluded in resignation:

> First, we cannot help observing that each panelist found in the patient important diagnostic features best explained by his particular frame of reference . . ..In summary, I would say that the diversity of opinions regarding the diagnosis and dynamics of Silverman's patient would suggest that one's theoretical stance takes precedence over other considerations. The presentations amply demonstrate that each theory can sound highly convincing, which makes absolute judgment almost impossible and personal choice inevitable. (Shane 1987, pp.199, 205)

Schwaber (1987, p. 262) also showed convincingly that the models employed by the participants in this discussion even frequently have a distorting effect on the gathering of data. For this reason she argues that theoretical models should be used in a more appropriate manner.

> Modern science teaches us that the observer's participation is an essential and fascinating element of the data. I make no argument for an atheoretical orientation, even if that were possible. I argue, rather, for our recognition that no matter what theory we espouse, we run the risk of using it to foreclose rather than to continue inquiry, to provide an answer rather than to raise a new question . . ..Our models are not simply interchangeable, matters of personal preference. We must seek that model which best explains the data and best expands our perceptual field. (Schwaber 1987, pp.274, 275)

These critical insights into an on-going treatment illuminate the numerous problems that the participation of third parties, whether they be specialists, scientists from other disciplines, or lay people, can make apparent. It was

therefore logical for Pulver (1987) to be particularly concerned with the question of how a protocol should be prepared.

Pulver enthusiastically welcomed the frankness of the reporting analyst. It is remarkable, in fact, that analysts still deserve our special praise when they attempt to precisely record in a protocol—prepared during or after the session—what the patient said and what they themselves felt, thought, or said, fully aware that this protocol will form the basis for a discussion with colleagues from other psychoanalytic schools and approaches. There are several reasons for the increasing willingness of analysts to let colleagues look over their shoulder. Without a doubt, psychoanalysis itself is going through a phase of demystification and shattering of illusions; although psychoanalysis has played a major role in the enlightenment, for a long time it did not subject itself to the same critical self-criticism. Institutionalized psychoanalysis is in danger of transforming itself into an ideology. Freud became a mythical figure. It is consequently no coincidence that a large public eagerly absorbs everything that Freud's analysands report about his work. Thus the rhetorical question expressed in the title of an article by Momogliano—"Was Freud a Freudian?"—can be answered clearly: "He was not" (Momogliano 1987).

The fear of publicity has declined sufficiently in recent decades to encourage many analysands, whether patients or prospective analysts, to

report in one form or another about their treatments (Anzieu 1986; Guntrip 1975). In addition to the well-known stories and diaries by Anais Nin, Marie Cardinale, Hannah Green, Erica Jong, Dörte von Drigalski, and Tilmann Moser, there are also joint publications containing the individual reports of both participants, for example that by Yalom and Elkin (1974). They take the old motto—*audiatur et altera pars* —seriously that it is important to hear both sides. The psychoanalytic community makes it too easy for itself if it reduces such autobiographic fragments of varying quality prose to someone's hurt feelings, to negative transference that has not been analyzed, or to excessive exhibitionism and narcissism.

Systematically planned empirical research on therapy is one of the factors contributing, to an increasing degree, to these changes in climate in psychoanalysis that, in turn, have initiated this demystification in psychoanalysis (see for example Masling 1983, 1986; Dahl et al. 1988). This then leads to further changes, which are very valuable precisely because the arguments employed in the clinical literature have been relatively naive. For example, Pulver stated, in the article referred to above, that each of the very experienced and distinguished analysts are equally successful despite holding divergent views about a case. In fact, however, the sessions presented by Silverman took a rather unfavorable course, so the fact that the protocols were not examined for curative factors might be traced back to a case of special consideration among colleagues. Of course, it is still unclear how many

of the general and specific factors that are taken to be curative on the basis of the results of research on therapy must converge qualitatively and quantitatively in an individual case in order to achieve a significant improvement or cure (Kächele 1988). Thus it is entirely possible that the effectiveness of psychodynamic therapies is more the result of similarities with regard to a few fundamental principles than of the differences separating them with regard to the meaning of interpretations. Joseph (1979) listed some of these basic assumptions, including unconscious processes, resistance, transference, free associations, the genetic derivation of problems, the therapeutic efforts to understand and to interpret, and the assumption that there are conflicts. Pulver went even further when he said that the differences of opinion between the participants in the discussion are more apparent than real.

> The therapists may be saying essentially the same thing to the patient, but in different words. The patients, once they get used to the therapist's words, in fact *do feel understood* . For instance, this patient might feel that her ineffable feeling of defectiveness was understood by a Kleinian who spoke of her envy at not having a penis, a self-psychologist who spoke of her sense of fragmentation, and a structural theorist who spoke of her sense of castration. (Pulver 1987, p. 298)

Thus Pulver assumed that this patient could have had insights that could have been expressed in different sets of terminology, yet that the latter would simply represent metaphoric variations of the same processes. Joseph (1984) argued in a similar vein by referring to unconscious linkages; for example, an

interview covering anxiety and loss touches both on unconscious preoedipal separation anxiety and on castration anxiety. Every individual does in fact recall many experiences in response to the word "loss" that may be interrelated but that belong to separate subgroups. Which narrative develops in a treatment is therefore not arbitrary or insignificant (Spence 1982, 1983; Eagle 1984). Although it is definitely important for both participants, patient and analyst, to reach some agreements, the purpose is not to find or invent an arbitrary "language game" that metaphorically links everything together. The patient wants to be cured of his defects, after all. He would like to master his specific conflicts and their roots, not just to recognize them. Furthermore, independent persons are able to determine whether the alterations in symptoms are really there.

The phenomena that occur in analytic treatment can, as Eagle has convincingly demonstrated, make a special contribution

> to a theory of therapy, that is, an understanding of the relationship between certain kinds of operations and interventions and the occurrence or failure of occurrence of certain kinds of specific changes. It seems to me ironic that psychoanalytic writers attempt to employ clinical data for just about every purpose but the one for which they are most appropriate—an evaluation and understanding of therapeutic change. (Eagle 1988, p. 163)

From today's perspective, the summary of a course of treatment is, if for no other reason than because of its incompleteness, of problematic value for the task of scientific validation. Yet the nature of the subject itself dictates that

completeness cannot be achieved. It is possible, however, to fulfill one important demand today, namely that detailed documentation be provided at the level of observation, from which generalizations are made. The model that Mitscherlich introduced for systematic case histories was an early attempt in this direction, even if very few case histories of this kind were actually written (Thomä 1954, 1957, 1961, 1978; de Boor 1965). Important was the demand that the abstraction and conceptualization that were the basis for classification be grounded. The Hampstead Index attempted to achieve something similar, namely to make it possible to clarify the major psychoanalytic concepts by means of a systematic documentation (Sandler 1962; Bolland and Sandler 1965). The Mitscherlich model was of great didactic value because it facilitated reflection during the phase in which specific hypotheses are made in psychosomatic medicine; its systematization also eased comparison. Its design pointed the direction for subsequent developments. Mitscherlich emphasized the significance of the doctor-patient relationship for diagnosis and therapy by also adopting the interview scheme of the Tavistock Clinic for purposes of documentation. The changes in symptoms that result from the analyst's interventions became the center of interest in descriptions of the course of a psychoanalytic treatment.

Going beyond the technical aspects of interpretation and the question of what should be interpreted in what way at what time, Bernfeld (1941) was innovative in concerning himself with the topic of the scientific validity and

the truth of interpretations. This problem was further discussed in the 1950s by Glover (1952), Kubie (1952), and Schmidl (1955).

The studies of interpretations conducted at the Psychosomatic Clinic of the University of Heidelberg and in cooperation with the staff of the Sigmund Freud Institute in Frankfurt—both institutions headed for longer periods by A. Mitscherlich—in the mid-1960s pursued the ambitious goal of validating the theory that was the basis of the individual analyst's therapeutic actions. Important impulses for this attempt came from the manner in which Balint structured his seminars on treatment technique, which assigned just as much significance to the thoughts of the analyst before he made a given interpretation as to the patient's reaction.

In order to do justice to the numerous thoughts that are part of the analyst's evenly suspended attention, Balint recommended that items which were merely thought should also be included in the notes about the session. The inclusion in the analyst's protocol of what the analyst considered—in addition to his actual interventions—and of information about the emotional and rational context in which interpretations originated was an important intermediate step. It became evident from this form of keeping records that it is of great importance to let the patient take part in the thoughts that are the basis of the analyst's interventions or interpretations. This is in fact a result of experience that was first discovered long ago and that Freud (1940a, p. 178)

had already referred to. He emphasized that the patient must be made an accomplice, i.e., must have some knowledge of the analyst's constructions, specifically about how the analyst arrives at his interpretations and what the reasons for them are. According to the reports available to us today, Freud did in fact acquaint his patients with his thoughts in detail, i.e., with the context of his interpretations. According to him, it is not unusual for the case to be divided into two distinctly separate phases:

> In the first, the physician procures from the patient the necessary information, makes him familiar with the premises and postulates of psycho-analysis, and unfolds to him the reconstruction of the genesis of his disorder as deduced from the material brought up in the analysis. In the second phase the patient himself gets hold of the material put before him; he works on it, recollects what he can of the apparently repressed memories, and tries to repeat the rest as if he were in some way living it over again. In this way he can confirm, supplement, and correct the inferences made by the physician. It is only during this work that he experiences, through overcoming resistances, the inner change aimed at, and acquires for himself the convictions that make him independent of the physician's authority. (Freud 1920a, p. 152)

The danger of intellectualization that is associated with this can be avoided. Explaining the rational context of interpretations generally elicits a strong affective echo from the patient and provides additional information, giving the patient the opportunity to critically consider the analyst's perspective. The patient achieves a greater freedom to understand the analyst's views and what appeared to be his mysterious role. An exact examination of what is termed the patient's identification with the analyst's functions also depends

on whether the exchange processes are documented in a detailed manner (see Sect. 2.4).

Thomä and Houben (1967) attempted—by examining interpretations--to identify the important aspects of an analyst's technique and its theoretical foundation, and—by studying patient's reactions—to estimate its therapeutic effectiveness. While conducting these studies we slowly became aware of the problems concerning the *effectiveness* of interpretations and the *truth* of theories.

In order to systematically study interpretations, we followed a recommendation made by Isaacs (1939) and designed a report scheme. It required the psychoanalyst preparing the protocol to locate interpretations between observation and theory and to describe the patient's reactions. Periods of treatment were distinguished according to the following points:

1. Associations, forms of behavior, and the patient's dreams that led the analyst to focus on a specific topic in one period for working through (*psychodynamic hypotheses*)

2. The analyst's thoughts, based on the theory of neuroses and his technique, that preceded individual interpretations

3. The goal of the interpretation

4. The formulation of the interpretation

5. The patient's immediate reaction

6. All the rest of the analyst's interpretations and the patient's reactions (associations, forms of behavior, dreams, changes in mood and affective state, etc.) that appear to be relevant for the topic to be worked through

7. Was the goal achieved?

8. Reference to material that does not agree with the hypotheses

While working on this project it became clear that the question of validation can only be answered within the complex sphere of research into the course and outcome of psychoanalysis, which was far beyond our possibilities at the time. The reporting scheme is, however, still a suitable means for providing important information for clinical discussion, as Pulver (1987) demonstrated 25 years later. It is enormously productive for the analyst to prepare protocols of his feelings, thoughts, and interventions in a way that enables a third party to develop an alternative perspective or that facilitates this task (for an example, see Chap. 8). The clarifications we have summarized in Vol.1 (Chap. 10), are necessary to promote clinical research and to be able to reach a better scientific grounding for psychoanalytic practice.

Our special interest in the effects that interpretations have led us, in preparing those protocols, to pay insufficient attention to the role of the emotional aspects of the relationship. The loss of the emotional context

forming the background of analysis makes interpretations and reactions appear far more intellectual than they in reality are. Insight and experience, interpretation and relationship, and the verbal and nonverbal aspects of the dialogue interact (Thomä 1983; see also Vol.1, Sect. 8.6). While making or reconstructing interpretations, analysts also move into the depths of countertransference, which is easier to talk about than to write about.

These two examples of attempts to write treatment reports were concerned with obtaining data regarding what the analyst felt, thought, and did in the presence of his patient that is as accurate as possible. Glover (1955) also assigned special value to the analyst preparing a protocol of what he told the patient. This is important because many of the so-called narratives are, as Spence (1986) criticized, typical narratives constructed by psychoanalysts according to hidden psychodynamic perspectives and without it being possible to recognize the analysts' own contributions.

The tape recording of analyses has finally put the development outlined here on a firm footing, both for research into the course and outcome of treatment and for further training (Thomä and Rosenkötter 1970; Kächele et al. 1973). Almost 30 years after the introduction of the Mitscherlich model the *systematic single case study* has proven itself to be very fruitful. The methodology of such studies has been the focus of discussion for some time (Bromley 1986; Edelson 1988; Petermann 1982). Such case studies provide a

means to satisfy the demands placed today on research testing psychoanalytic hypotheses (Weiss and Sampson 1986; Neudert et al. 1987).

1.4 Approximating the Dialogue: Tape Recordings and Transcriptions

The idea of employing technical aids should be given very careful consideration. Although tape recordings document the verbal dialogue, this technological "third ear" does not register the thoughts and feelings that go unspoken or that fill unspoken space with meaning and affects. It would be superfluous to make special reference to this fact if this deficiency were not given such weight in the literature. It is possible, after all, to "hear" more of the tone that makes the music when reading transcripts or, particularly, listening to the original recordings than by reading publications based on protocols. An analyst's attention can be distracted if he takes a protocol during a session, and the analyst is more selective if he reports key words after the session, as Freud recommended. In selecting the phenomena to be described, the analyst follows his own subjective theoretical perspective, and who appreciates discovering that his own expectations and assumptions have been refuted! It is not only the patients who draw pleasure and hope from confirmation. Research testing hypotheses is a burden on all psychotherapists because it of necessity questions preferred convictions (Bowlby 1982). For this reason we like to share this task with cooperating scientists not participating directly in the therapy.

After becoming chairman of the department of psychotherapy and director of the psychoanalytic institute in Ulm in 1967, the senior author initiated the tape recording of psychoanalytic therapies. In the following years these recordings, together with those of therapies conducted by some of his associates, became the core of the transcripts of psychoanalytic therapies stored in the "Ulm Textbank," that in the meantime has been made available to a large number of scientists from around the world (Mergenthaler 1985).

It took years before we learned to sufficiently appreciate the enormously profitable effects of listening to dialogues and reading verbatim transcripts of our own clinical work for us to overcome all of our earlier reservations. The struggle to introduce corresponding technical aids into the analytic interview was begun by E. Zinn in 1933 (Shakow and Rapaport 1964, p.138). Although it is not over yet, the opportunities offered by the tape recording of analyses for psychoanalytic training and practice were first mentioned in positive terms by McLaughlin at the International Psychoanalytic Congress held in Helsinki in 1982.

In contrast to the followers of C. Rogers, psychoanalysis did not take advantage of these numerous possibilities for a long time. At the core of many misgivings was the concern that the presence of a tape recorder could have consequences similar to those of a third party, namely that the patient "would

become silent as soon as he observed a single witness to whom he felt indifferent" (Freud 1916/17, p. 18). Yet it has long been known that patients, with few exceptions, readily give their approval to having the interview recorded, discussed in professional circles, and evaluated scientifically. It is not unusual for patients to—correctly—expect to profit therapeutically from having their analyst concern himself especially intensively with their case. Of course, the patient's initial approval and his motivations are just one aspect; another and decisive question concerns the effects of the tape recording on the psychoanalytic process. In order to make a comparative study of one and the same patient it would have to be possible to treat him twice, once with and once without a recording. Yet it is possible to refer to the large number of psychoanalytic treatments that have been recorded and in which no systematic negative effect has become known. We do not employ the so-called playback technique, but according to Robbins (1988) severely disturbed patients achieve a therapeutically effective "self objectification" (Stern 1970) by listening to their recorded interviews and being able to work through the experiences they thus acquire.

Once recording has been agreed upon, we consider it to be part of the permanent framework on the basis of which everything that happens is interpreted. Of course, the patient can also retract his approval. In Sect. 7.5 we give examples of such cases; they show that it is not only possible but also very productive for these events to be made the object of precise analytic

study. At any rate, according to our own experience and the relevant literature the course of the psychoanalytic process is in general such that the recording ultimately becomes a matter of routine that only occasionally has an unconscious significance, just as lying on the couch does. Superego functions can, for example, only be attributed to the tape recording and projected onto the secretary (as a transference figure) as long as such expectations of punishment are virulent. Similarly, in the course of analysis the omnipotent fantasies a patient is ashamed of and whose publication he fears—neurotic fears that they might be identified despite being used anonymously—lose their disturbing force.

After working through, many things become simple and human that initially appeared to be characterized by a unique personal dynamism. Nonetheless, no text of a psychoanalytic dialogue is superficial even though many readers express surprise at how little the text alone says. Occasionally doubts are therefore raised whether the availability of verbatim protocols offers anything new. Yet at least the analyst in question is frequently surprised when he realizes, from hearing his own voice or reading the transcript, how far his interpretations are from what they should be according to the textbooks, i.e., clear and distinct.

It is remarkable how many problems an analyst has to cope with when he gives a colleague the data from his clinical work, in this case a transcribed

dialogue, for evaluation. Colleagues confirm more or less bluntly what one's self evaluation actually cannot overlook, namely that there can be a significant discrepancy between one's professional ideal and reality. The tape recorder is without a doubt a neutral receiver that cannot miss something or be selective! Kubie, to whose supervision using tape recordings the senior author owes a debt of gratitude, described in the following quotation painful experiences that every psychoanalyst has to get over when he is directly confronted with his statements in an analytic situation:

> When for the first time a student psychiatrist or an experienced analyst hears himself participate in an interview or a psychotherapeutic session, it is always a surprising and illuminating experience. He hears himself outshouting or outwhispering the patient, always louder or always softer. Or he hears himself playing seesaw with his patient—loud when the patient is soft, and soft when the patient is loud. Or with surprise and dismay he hears in his own voice the edge of unintended scorn or sarcasm, or impatience or hostility, or else overtender solicitude and seductive warmth. Or he hears for the first time his own unnoted ticlike noises punctuating and interrupting the patient's stream. From such data as this he and the group as a whole learn a great deal about themselves and about the process of interchange with patients and what this process evokes in them in the form of automatic and therefore indescribable patterns of vocal interplay.
>
> They learn also to watch for and to respect the subtle tricks of forgetting and false recall to which the human mind is prone. At one seminar a young psychiatrist reported that in a previous interview at one point his patient had asked that the recording machine be turned off while he divulged some material which was particularly painful to him. The group discussed the possible reasons for this, basing our discussion on our knowledge of the patient from previous seminars. Then to check the accuracy of our speculative reconstruction, the psychiatrist was asked to

> play to the group about five minutes of the recorded interview which had preceded the interruption, and then about five or ten minutes which followed when the recording had been resumed. To the amazement of the young psychiatrist and of the group as a whole, as we listened to the recording we discovered that it had been the psychiatrist and not the patient who had suggested that the recording should be interrupted. Of his role in this, the young psychiatrist had not the slightest memory. Furthermore, as we heard the patient's halting speech, his change of pace and volume, the altered pitch and placing of his voice, it became clear to the whole group that the young psychiatrist's intuitive move had been sound: that he had correctly evaluated the patient's mounting tension and had perceived the need for this gesture of special consideration and privacy. The result was that the patient's rapport was more firmly established than before, to such an extent that the psychiatrist could now recall that it had been the patient who had suggested that the recording be resumed after a relatively brief interruption, and who then, with the machine turned on, had continued to discuss frankly and without embarrassment the material about which he had been so touchy before. The illuminating implications of this episode for the data itself and for the transference and countertransference furnished the group with material for reflection and discussion throughout the remaining course of the seminars. These could not have been studied without the recording machine. (Kubie 1958, pp.233-234)

It is difficult to ignore the meaning of this story. It opens a context of discovery that illuminates the always latent danger of reductionism inherent in a condensed and selected report.

Transcripts often seem paltry in comparison to the recollections that the analyst has of the session and that are immediately revitalized when he reads the text. It is the rich emotional and cognitive context that adds vitality to the sentences expressed by the patient and the analyst. This context and the multifaceted background, which are revitalized when the treating analyst

reads a transcript, can only be assumed by the reader who did not participate in the interview; it may be possible for the latter to fill in the gaps with the aid of his imagination and his own experience. In the traditional presentation of case material, which in general contains much less original data, this enrichment is provided by the author's narrative comments. Even the use of generalizations, i.e., of the abstract concepts that are regularly employed in clinical narratives, probably contributes to making the reader feel at home. The concepts that are used are filled—automatically, as it were—with the views that the reader associates with them. If a report refers to trauma or orality, we all attribute it a meaning on the basis of our own understanding of these and other concepts that is in itself suited to lead us into an approving or skeptical dialogue with the author.

Uncommented transcripts, in particular, are sometimes rather strange material. It took us some time to get accustomed to them. Yet if you become absorbed in these dialogues and practice on your own texts and those of other analysts, you become able to recognize a wealth of detail. For example, the context clarifies how a patient understood a question and whether he took it as encouragement or criticism. Thus the verbatim transcript at least makes it possible to understand how the tone makes the music. An even more precise method for studying the emotional background is for the analyst to summarize his countertransference during specific sequences or immediately after the session or for him to be questioned afterwards.

This also makes it possible for third parties to examine the theoretical assumptions behind an interpretation. The assumptions they make about the background motives and the goals contained in an interpretation are more reliable if entire sequences can be considered in a transcript. The "thinking-out-loud" approach, which Meyer (1981, 1988) used to examine the thought processes involved in the conclusions drawn by three analysts, leads even further. Finally, listening to the tape recording makes it possible to establish very close contact with the original situation.

Missing in manuscripts of analyses recorded in their entirety are both the silence pauses, which can be very eloquent "comments" for each participant, and descriptions of the mood, which can be remembered during an oral presentation at a seminar on treatment. We would like to raise the question of why it seems to be easier for musicians to hear the music while reading a score than for analysts to make the transcript of a session come alive.

Sandler and Sandler (1984, p. 396) refer to the "major task for future researchers to discover why it is that the transcribed material of other analysts' sessions so often makes one feel that they are very bad analysts indeed." They qualify this by adding that "this reaction is far too frequent to reflect reality" and ask, "can so many analysts really be so bad?" This conclusion is a challenge for us to enlarge the size of the sample available for

examination. Apparently only the bad analysts have so far been ready to put the naked facts—the unerring transcripts—on the table. With the examples contained in this volume we considerably enlarge the previous sample size and naturally hope that we do not fall victim to the same verdict. Yet even more bad examples can serve a useful function and encourage renown analysts to finally demonstrate how it should be done by making ideal models of transcripts of dialogues available for discussion. Everyone in the process of learning looks for role models. The great masters of our time should not miss the chance to set a good example. Of course, the naked facts of the verbal exchange are not the last word. By coding intonations and other nonverbal communications it is possible to represent affects better in transcripts than in traditional publications. It requires some practice, however, to be able to follow texts of psychoanalytic dialogues containing such coded information.

Video recordings are essential to examine some issues, for example, to study how affects are expressed in mimicry and intonation (Fonagy 1983), gestures, and the overall expressiveness of posture and movement, i.e., body language (Krause and Lütolf 1988). Of course, they do not lead anywhere if the issues have not been clearly conceptualized or if there is no clearly defined method for evaluating the data. This is the reason that the films of a complete psychoanalytic treatment that have been made (Bergmann 1966) have disappeared into the vaults of the National Institute of Mental Health and have probably been destroyed in the meantime. There are less

complicated and costly means to register the nonverbal communication expressed by posture and movement for clinical purposes than to make video recordings of the patient while he is lying on the couch and restricted in movement. In several articles Deutsch (1949, 1952) has pointed to the significance of posture and movement, and McLaughlin (1987) has described how he uses simple marks in the protocol to record the patient's movements on the couch.

On the basis of our experience, we realize that transcribed psychoanalytic dialogues become more meaningful the more the reader can put himself into the situation and add vitality to it by identifying with the participants and reenacting, as it were, the dialogue. There is still a difference, however, between in vivo and in vitro. When the treating analyst reads his own interpretations, his memories add important dimensions. It simply makes a difference whether you read a drama by Shakespeare, sit in the audience and view a stage performance, or help to enact it as an actor or director. Since we will frequently confront the reader with excerpts from transcripts, we request him to make the mental attempt to dramatize the text. We believe that most dialogues can stimulate the reader to make multifaceted and imaginative identifications and, consequently, numerous interpretations. Yet this does not eliminate the difference between the producer and the recipient of a text.

So-called naked facts or raw data have always been couched in personal theories, on the basis of which the observer illuminates the individual fact and assigns it a meaning. This on-going process of attribution makes the talk about registering simple facts appear just as dubious as the related teaching of mere sensations, which William James termed the classic example of the psychologist's fallacy. Yet there are hard facts, as we inevitably discover when we believe we are able to disregard laws of nature. The pain we sense after a fall, which is in accordance with the law of falling bodies (i.e., gravity) but not with the magical belief in being invulnerable, may serve as an example that can be recognized as easily being in agreement with Freud's reality principle. In this example it is very obvious that belief was attributed a power that foundered on the reality principle. The analyst's recognition of both the metaphoric and the literal meaning and of the tension between them makes it possible for him to grasp the deeper levels of the transcribed texts. Of course, the wise Biblical saying "Seek, and ye shall find" also applies here. As an aid we supplement the dialogue with commentaries and further considerations.

The detailed study of verbatim protocols opens new approaches in training at all levels (Thomä and Rosenkötter 1970). On the basis of such protocols supervisions can be very productively organized, especially with regard to technical procedure and developing alternative modes of understanding. For this reason we dedicate an entire section to this topic (Sect. 10.1).

The issue is not to make the tape recording of treatments a routine measure. We are of the opinion that tape recording is linked to certain learning experiences that are difficult to acquire in other ways. The most important one, for us, is that the treating analyst can acquire a realistic picture of his concrete therapeutic procedure; this is only possible to a limited degree in retrospective protocols, for psychological reasons associated with our memory. This limitation has a systematic character since regular omissions creep into such protocols, as we know since the instructive studies by Covner (1942) and Rogers (1942). In the form of supervision common today, the supervisor attempts to discover the candidate's blind spots although these are customarily well hidden as a result of unconscious motives. The frequently observed procedure of participants at seminars of reading the prepared report against the grain, i.e., of searching for alternative interpretations, speaks for the widespread nature of this attitude.

Once the analyst has exposed himself to the confrontation with the tape recorder and has overcome his many inevitable hurt feelings, which regularly occur when he compares his ideals with the reality of his actions, then he can dedicate his entire and undivided attention to the patient in the session. He is not distracted by thoughts of whether and what he should note after the session or of which key words he should note during the session. The analyst's subjective experience is relieved of the responsibility of having to fulfill a scientific function in addition to the therapeutic one. One independent

task is, however, reserved for the analyst's free retrospective consideration of the psychoanalytic session, called the inner monologue by Heimann (1969); it obviously cannot be recorded at all. The manner in which analysts look back at their own experiences and ideas constitutes a field of its own in which free reports have an indispensable function; we have studied this question for many years together with A.E. Meyer (Meyer 1981, 1988; Kächele 1985).

In retrospect we can say that the introduction of tape recordings into psychoanalytic treatment was linked with the beginning of a critical reappraisal of therapeutic processes from a perspective directly adjacent to the phenomena themselves. This simple technical tool was and is still today an object of controversy among psychoanalysts; those analysts, however, who are active in research agree that such recordings have become an important instrument in research (e.g., Gill et al. 1968; Gill and Hoffman 1982; Luborsky and Spence 1978). Criticism of research methodology from within the ranks of psychoanalysis began in the 1950s and was initially not taken very seriously (Kubie 1952). Glover (1952) complained, for example, about the lack of sufficient control on the collection of data. Shakow (1960) referred to the view, derived from Freud's assertion of an inseparable bond, that every analyst is per se a researcher as a "naive misunderstanding of the research process." This inseparable bond has in fact only been made possible by the introduction of tape recordings and to the extent that the treating analyst, i.e., his personal theories and their application in therapy, can now be made an

object of scientific study. The substantial participation of independent third parties is an essential aspect of such studies to test analysts' hypotheses. Thus Stoller questioned the claim that the psychoanalytic method is scientific as long as one essential element is missing that can be found in other disciplines acknowledged to be sciences:

> To the extent that our data are accessible to no one else, our conclusions are not subject to confirmation. This does not mean that analysts cannot make discoveries, for scientific method is only one way to do that. But it does mean that the process of confirmation in analysis is ramshackle . . ..I worry that we cannot be taken seriously if we do not reveal ourselves more clearly. (Stoller 1979, p. xvi)

We think that Stoller's skepticism is unfounded today because the tape recording of sessions provides reliable data about verbal exchange. Insofar we agree with Colby and Stoller (1988, p. 42) that the transcript "is not a record of what happened" but "only of what was recorded." The verbal data can easily be supplemented by additional studies about, for instance, the analyst's countertransference (see our studies referred to above).

Since psychoanalysis quite rightly insists that the clinical situation is its home ground for acquiring clinical data to test theories, it is necessary to arrive at an improved method of observation that does not exclude the analyst as a participant observer but provides him the tools for verifying his "observations." Gill et al. (1968) recommended separating the functions of the clinician and the researcher and introducing additional procedures for

systematic observation.

Freud's (1912e, p. 113) own impressive ability to record examples "from memory in the evening after work is over" did not protect him from being selective and forgetful and does not supply sufficient justification for any psychoanalyst to make notes for scientific purposes from memory only. We need to employ some form of externally recording data as a means to support our memories, regardless of how good our unconscious memory is. Gill et al. (1968) have pointed out that the ability to remember is developed to very different degrees. It is probably impossible to "calibrate" our ability to remember in a way which would comply with the standardization of a mechanical recording method. Psychoanalytic training, and especially the training analysis, promotes the apperception and selection characteristic of a specific school more than it does a balanced and critical attitude.

Following the lead of cognitive psychology, models have recently been put forward that demonstrate the complexity of an analyst's patient-specific memory configurations; Peterfreund (1983) has called them working models (see also Moser et al. 1981; Teller and Dahl 1986; Pfeifer and Leuzinger-Bohleber 1986; Meyer 1988). The approaches described in this book suggest that we expect to encounter great variability in the personality-dependent processes of image formation, storage, and retrieval (Jacob 1981).

The method of listening that Freud recommended can facilitate the perception of unconscious processes. There have also been experimental studies that emphasize the heuristic value of nondirected listening (Spence and Lugo 1972). The point of this discussion cannot be to restructure the exclusively subjective protocol, but to acknowledge that it has a limited scope in matters related to research. A clinician working on a specific problem will have to find additional opportunities for observation in order to be able to make any systematic statements. This is exactly the purpose of introducing tape recording into treatment. This technical aid influences—as do many other factors—both the patient and the analyst; the same is true of the presentation of cases by candidates in training and of the consequences that the analyst's life history has on the patient.

We believe that the introduction of research into the psychoanalytic situation is of immediate benefit to the patient because it enables the analyst to draw many stimuli from the scientific issues that are raised. Thus we can return to items we mentioned above to better prepare the reader to study transcripts. We are all used to facts being presented in the light of theories. A transcript creates, in contrast, the impression of being one-dimensional: the analyst's interpretations and the patient's answers do not automatically reflect the latent structures of perception and thinking. Although typical interpretations disclose which school the analyst belongs to, we cannot simply throw his statements into one pot with his theoretical position. In

traditional case reports phenomena are united in a psychodynamic structure that satisfies several needs at once. One does not ask, with regard to a good report, whether the items the patient contributed were left in their original form or whether they only fit into the whole after interpretive work was done. To demand that the cognitive process and the consistency of a structure be scrutinized and that the structure be divided into its parts leads us back into the analyst's office, which obviously can only be poorly reflected in a transcript. Yet this is a means to obtain a reasonable approximation of what analysts do in order to satisfy the demands of the day, namely that the clinical practice of psychoanalysis be studied. Insofar the tape recording provides an "independent observer" (Meissner 1989, p. 207). Such an observer is a prerequisite for examining Sandler's thesis that psychoanalysis is what psychoanalysts do.

Before we conclude this chapter we still have to mention several simple facts. It is rather arduous to read a transcript of a session of treatment that has not been edited. We believe that the resulting loss of linguistic accuracy is made up for by the didactic benefit. Texts have to be in a certain linguistic form in order to entice clinically oriented readers to participate in the processes that are described.

In written form it is only possible to approximate complex relational processes. Our previous line of argument indicates which form of protocols

we will primarily rely on. We will also refer to notes and protocols made by analysts. In accordance with our basic idea we will, as a rule, dispense with extensive biographical introductions to the episodes of treatment. We want to demonstrate that it is possible to comment on the fundamental principles of therapeutic activity without providing a detailed introduction describing the patient's biography. Both theoretical considerations and therapeutic experience document that, at least in the sphere of symptoms, structures of meaning that play a causal role remain constant through time. Clichés are sustained that are the basis for repetition compulsion. It is not always necessary to resort to detailed descriptions of preceding biographical events in order to be able to understand processes in the here and now.