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

2. PROBLEMS OF METASCIENCE AND METHODOLOGY IN CLINICAL PSYCHOANALYTIC RESEARCH[1]

Additional remarks some 30 years later

> … Nobody has ever denied scientific status to psychoanalysis on the ground that it is not like physics. For we would then have to rule out the whole of biology as a science, which would be absurd. (Hook, 1959b, p. 214)

On re-reading this now more than 30-year-old paper, we are pleased to note that it has remained current and has received significant attention, as for example in Rubovits-Seitz' (1998) substantial work *Depth-Psychological Understanding: The Methodologic Grounding of Clinical Interpretation*. Such studies from years past have been of vital help in clarifying our position as clinicians and researchers. The following argument of John Wisdom (1970), a philosopher close to the Kleinian school, is one we have taken to heart:

> It seems clear that a clinician cannot handle research into clinical hypothesis without having his area demarcated from the rest. More importantly, a psychoanalyst who wishes to test his theories empirically ... cannot begin his work, until the morass of theory, ontology, and Weltanschauung has been 'processed' by philosophy of science. (p. 360-361)

Without being aware of it, therapeutically successful clinicians are continually testing—in the broadest sense of the word—their theories. The problems of empirical therapy research on the single case are commonly underestimated. Hypothetically assumed causal connections between symptoms and their unconscious causes follow statistical probabilities and therefore cannot be deduced from scientific laws. This is one of the reasons why the Hempel-Oppenheim (1953) schema on the parity of postdiction and prediction is not applicable to the human sciences (see section "Description, Explanation and Prognosis in Psychoanalysis". This was pointed out already by the mathematician von Mises (1939; engl. 1951), with whose work we were not yet familiar in 1973:

---

[1] Helmut Thomä and Horst Kächele. Preparation of this paper was aided by the Deutsche Forschungsgemeinschaft (German Research Foundation). This paper was originally published in *Psyche,* XXVII (1973); pp. 205-236, 309-355. The English translation, by Hilda Spiegel, was published 1975 in the Annual of Psychoanalysis 3: 49-119.

> it seems justified to point out that the totality of the observations in this field seems to correspond more to the assumption of a statistical than of a strictly causal correlation. (p. 238).

In this sense we were and remain empiricists and "idiographic nomotheticists" in Freud's tradition. In order to avoid misunderstanding, this paradoxical formulation requires some explanation. First, it must be emphasized that psychoanalysts are not lawgivers. The psychoanalytic method cannot be based on laws even if it is true, as Fonagy (2003) believes, that "facing the logical weaknesses of our position, we have tended to raise the status of 'clinical theories' to laws" (ibid, p. 19). It is misleading to deduce the behavior and experience of our patients from pseudo-laws. Freud discovered complex probabilistic explanatory schemas, knowledge of which deepens and enriches our understanding of psychopathology as a whole.

The probability that similar diseases will take similar courses makes it possible to establish a typology. Still, following the principle of trial and error, the single case lies at the center of a ongoing research process. Initially one can only base oneself on uncertain diagnostic and prognostic assumptions, and a degree of uncertainty always remains. With increasing life experience and specialized analytic knowledge, probabilistic assumptions made in the course of an analytic treatment gain in reliability and certainty. In this limited sense we regard ourselves as "nomotheticists of the single case" in our striving to find typical regularities given like cases. Psychoanalysis has made an essential contribution to overcoming the historical opposition of understanding and explanation in the human sciences.

To fully present our current position we must first comment on the work of Ricoeur (1970). It was only quite recently that we recognized the overwhelmingly powerful influence that Ricoeur has had on many adherents of French psychoanalysis. To our mind, the controversies between Green (2000) and Stern (2000) and Green (2005) and Wallerstein (2005a and b) would remain incomprehensible without an awareness of Ricoeur's influence. Green "maintain[s] that as yet there is no serious study of Freudian thought by psychoanalysts. We had to wait for Ricoeur, a philosopher and a non-psychoanalyst to read such a work" (2005, p. 631).

Clearly Green considers Ricoeur's reading of Freud's *oeuvre* to be the only legitimate one. We share the view of Welsen (1987) that in its content Ricoeur's "reading of Freud" is borne by the thesis that psychoanalysis is an intertwining of energetics and hermeneutics; like

a human science it reveals the meaning of psychic phenomena, while like a natural science it explains these by reducing them to conflicts of psychic forces. In this sense Ricoeur asserts that "...Freud's writings present themselves as a mixed ... discourse, which at times states conflicts of force subject to an energetics, at times relations of meaning subject to a hermeneutics" (Ricoeur, 1970, p. 65). Ricoeur attempts to prove that the dichotomy of energetics and hermeneutics dominates Freud's entire *oeuvre*—from the "Project for a Scientific Psychology" (1895) right to the "Outline of Psychoanalysis" (1940a). Thus Ricoeur's "energetics" contains essentially the economic aspect of metapsychology. This results in an intimate entanglement, a closed circle in fact, because the interpretation of latent, unconscious meaning is linked with metapsychological energy displacements. Ricoeur's failure to pose critical questions either regarding metapsychological energetics or the interpretation of meaningful connections results in the downright research-hostile position of many French psychoanalysts influenced by him (Perron, 2006). Research always begins with critical questions that arise out of everyday professional practice. Analysts work as therapists. For this reason, differing attitudes have great repercussions on the therapeutic process. In this connection there seem to be commonalities between Ricoeur and Lacan, to which Welsen (1988) has pointed: "Both Lacan and Ricoeur fail to know Freud in his own self-understanding, which is indebted in no way to linguistics or hermeneutics but to the natural scientific tradition of the 19th century" (p. 308). Freud saw in metapsychology "the consummation of psycho-analytic research" (1915e, p. 181). On the other hand Freud could not avoid a monistic utopia and even expected that as biology progressed, psychoanalytic hypotheses would one day be "blow[n] away" and replaced by physiological and chemical terms (Freud, 1920g, p.60). In sum, it can be said that Ricoeur's hermeneutics is intimately bound up with economic assumptions without his having sufficiently come to terms with Habermas' (1971a) verdict on Freud's "scientistic self-misunderstanding."

What we as research-oriented clinicians criticize in particular are Ricoeur's erroneous judgments regarding the scientific position of psychoanalysis as therapy. The fundamental flaw in Ricoeur's argumentation is that he bases himself on a behaviorism that has now become obsolete even in modern behavioral therapy, a behaviorism that reduced psychology as a whole to the observable stimulus-response schema. This enables him to set up the thesis that psychoanalysis is neither a factual nor an observational science. Thus even a modified or revised form of operationalism, upon which the publications of Ellis (1956), Frenkel-Brunswik (1954) and Madison (1961) were based, can be designated by him as downright treasonous to the essential core of psychoanalysis. Many of Ricoeur's arguments, which form

the basis for his strict separation of observed facts and their "meaning," coincide with the death of a primitive behaviorism. With the "cognitive turn" in behavior therapy came an acknowledgment of introspection and the problem of the "psyche of the other." Since then this "turn" has moved beyond the point of lip service and there has been a further rapprochement between the cognitive sciences and psychoanalysis (Bucci, 1997a). Ricoeur proceeded from a behaviorism that regarded the psyche as a "black box."

On one point we can agree with Ricoeur: analysts do not operate on the plane of behavioristic axioms, nor do they accept the methodology so constituted. They are concerned with observation and interpretation of the probability of certain reactions based on unconscious conditions which determine how a stimulus receives its meaning. But in our estimation all psychoanalytic statements are at some point connected with observable facts, among which we also count verbally communicable experience. To this extent we concur with Ricoeur's opinion:

> If we grant that the analytic situation as such is irreducible to a description of observables, the question of the validity of psychoanalytic assertions must be reexamined in a context distinct from a naturalistic science of facts. ... [N]o art of interpreting would be possible if there were no similarities between cases and if it were impossible to discern types among these similarities. (1970, p. 373-74).

The insight that observation statements are theory-dependent means that no sharp dichotomy can be made between observation language and theory language without rendering distinctions impossible. In psychoanalysis as in everyday life, descriptions are made of phenomena that exist in a context. It is the context that changes with different points of view. With an altered perspective, different aspects of the phenomena also become visible. The more one distances oneself from the observable phenomena in depth-hermeneutics, the more difficult it becomes to justify interpretations. The associated methodological difficulties were pointed out by the above-mentioned British philosopher:

> ... [T]he unconscious is more like a root of a tree, and however much you develop the root into actual shoots, it can never be identified with the sum of the shoots that break through the soil. The unconscious always has more potential and is more than its manifestations. Its scientific status is like those high-level concepts in physics which are *never* open to checking by direct observation. (Wisdom, 1984, p. 315; italics in original).

Ricoeur raised a number of questions in the assumption that their empirical resolution was beyond the power of psychoanalysis:

> "However, on what conditions is an interpretation valid? Is it valid because it is coherent, because it is accepted by the patient, because it improves the condition of the patient? But a given interpretation must first be characterized by objectivity; this means that a number of independent inquirers have access to the same data obtained under carefully standardized circumstances. Next, there must be some objective procedures to decide between rival interpretations. Further, the interpretation must lead to verifiable predictions. But, psychoanalysis is not in a position to meet these requirements: its data are enmeshed in the individual relationship of the analyst to the analysand; one cannot dispel the suspicion that interpretations are forced upon the data by the interpreter, for want of a comparative procedure and statistical investigation. Finally, the allegations of psychoanalysts concerning the effectiveness of therapy do not satisfy minimum rules of verification; since the percentages of improvement cannot be strictly established or even defined by some kind of "before and after" study, the therapeutic effectiveness of psychoanalysis cannot be compared with that of some other method of treatment, or even with the ratio of spontaneous cures. For these reasons, the criterion of therapeutic success is unusable" (Ricoeur, 1970, pp. 346-347)

In contrast to this catalogue of allegedly unanswerable questions, a bit further on one encounters a series of requirements set by Ricoeur which he himself appears to regard as satisfiable:

> It is perfectly legitimate, therefore, to require the analyst to compare his percentage of improvements with the ratios obtained by different methods, or even with the ratio of spontaneous improvement. But it should be realized that one is at the same time requiring that a 'historical type' be transposed into a 'natural species"; in doing this, one forgets that a type is constituted on the basis of a 'case history' and by means of an interpretation that in each instance arises in an original analytic situation. Again, psychoanalysis cannot sidestep, any more than exegesis, the question of the validity of its interpretations; nor even that of a certain sort of prediction (what is the probability, for example, that a patient be accepted for therapy, or that he can then be successfully treated?). Comparisons must surely enter into the analyst's field of consideration; but it is precisely as a problem of historical science, and not of natural science, that analysis encounters and poses the problem. (Ricoeur, 1970, p. 374-375)

As can be gathered from a commentary by Grünbaum (1984, p. 48), Ricoeur (1981 p. 248) remained enmeshed in contradictions. On the one hand he adhered firmly to his conviction that "facts in psychoanalysis are in no way facts of observation," while on the other hand we read in the context of this passage:

> What is remarkable about psychoanalytic explanation is that it brings into view motives which are causes. ... In many ways his [Freud's] explanation refers to 'causally relevant' factors. ... All that is important to him is to explain ... what in behaviour are 'the incongruities' in relation to the expected course of a human agent's action. ... It is the attempt to reduce these 'incongruities' that ... calls for an *explanation* by means of causes. ... To say, for example, that a feeling is unconscious ... is to say that it is to be inserted as a causally relevant factor in order to explain the incongruities of an act of behaviour. ... From this ... it follows ... that the hermeneutics of self-understandings take the detour of causal explanation. (Ricoeur, 1981, p. 262-264)

Here Ricoeur obviously acknowledged that psychoanalytic explanations are c*ausal* and are simultaneously intended to explain different types of behavior.

At this point we reproduce Grünbaum's comprehensive commentary in its entirety, because it contains considerable consequences for clinical research and for the most comprehensive documentation possible of it:

> Now, the imperative to furnish cogent evidence of the purported causal linkages invoked to explain the patient's case history is not lessened by the injunction (Ricoeur 1981: 266-268) to fulfill the 'narrativity criterion' as well. The latter requires that the 'partial explanatory segments of this or that fragment of behaviour are integrated in a narrative structure' reflecting the individual analysand's etiologic life history (p. 267). But, as Ricoeur emphasizes, the psychoanalytically reconstructed scenario not only must be a 'coherent story' (p. 267)—made 'intelligible' by the explanatory segments—but must also aspire to being true, rather than merely persuasive and therapeutic. Quite properly, therefore, he enjoins that 'we must not give up our efforts to link a truth claim to the narrativity criterion, even if this claim is validated on a basis other than narrativity itself' (p. 268). (Grünbaum, 1984, p. 47)

In our opinion, in recent decades many psychoanalysts have endeavored in their case reports to optimize the connection between the claim to truth and the narrative criterion in the sense of Ricoeur's admonition. It should be particularly emphasized that Grünbaum, the

sharpest living critic of psychoanalysis, here approves of a Ricoeur whom he otherwise excoriates:

> Indeed, he elaborates (pp. 268-269) on 'what makes a narration an explanation in the psychoanalytic sense of the term' as follows: 'It is the possibility of inserting several stages of causal explanation into the process of self-understanding in narrative terms. And it is this explanatory detour that entails recourse to non-narrative means of proof. (Grünbaum, 1984 p. 48)

Our own clinical efforts are thoroughly documented in volume II of the Ulm Textbook (Thomä and Kächele, 1994b). The empirical studies on the model case of Amalia X published in the present research volume have it as their goal to arrive at a comprehensive validation.

Regarding Ricoeur's reading of Freud and his influence on French psychoanalysis we could have formed an opinion 30 years ago. It is another matter with the work of Adolf Grünbaum, which did not yet exist in 1973. Grünbaum's auspiciously entitled book, *The Foundations of Psychoanalysis: A Philosophical Critique,* did not come out until 1984—close to the same time as the first German volume of the Ulm Textbook (Thomä and Kächele, 1985). The subtitle of his book carries no hint of the crushing conclusion at which his critique arrives. From the perspective of Grünbaum's theory of science, psychoanalysis has no reliably secured foundations. For the title to reflect the thrust of the book, it would at least require a question mark and would perhaps read: "Is Psychoanalysis Scientifically Founded?" The title as chosen and the laudatory blurbs piqued the curiosity of a readership reaching far beyond the precincts of psychoanalysis, psychotherapy and psychiatry. Grünbaum's devaluation of the clinical experiential foundation unsettled many psychoanalysts.

Faced with this theoretician's criticism that it is impossible to test the validity of psychoanalytic interpretations, analysts—according to Mitchell's (1998) observation—developed the following "Grünbaum Syndrome[1]:

> …several days of guilty anguish for not having involved oneself in analytic research ... And may (also) include actually trying to remember how analysis of variance works, perhaps even pulling a twenty-year old statistics off the shelf and quickly putting it back. There may also be a sleep disturbance and distractions from work. (Mitchell, 1998, p. 5).

We remained unaffected by this syndrome, as we had long been acquainted with the themes discussed by Grünbaum. These same themes had been the subject of an historically prominent symposium of American philosophers and psychoanalysts that took place at the New York University Institute of Philosophy in 1958. In his talk there, Hook (1959b) raised the familiar issue of falsification and asked the analysts "… what kind of evidence they were prepared to accept which would lead them to declare in any specific case that a child did not have an Oedipus complex" (p. 214). The analysts in attendance were amazed and their answers were in part rather odd. Hook himself came to the conclusion that the oedipal phase is by no means universal:

> Many normal children do not manifest it. This would seriously invalidate one of Freud's central hypotheses. It would tend to indicate that the absence of the oedipal phase as well as variations in the extent, intensity, and mode of its expression are determined by social and cultural institutions. It suggests that the *significance* of the child's unlearned behaviour depends upon the responsive reaction of adults and the institutional framework within which it is interpreted and channelled" (Hook, 1959b, p. 217/218).

That this description came from Hook and not from one of the attending representatives of psychoanalysis is astonishing in retrospect. Almost 50 years later in a book review, E. Kafka (2004) quotes Arlow, who held a lecture at this symposium himself and had the impression that the Hartmann period was coming to an end because the question raised by Hook could not be convincingly answered. Grünbaum was an active participant at this symposium and made brief remarks on the discussion. As is evident from the *festschrift* in honor of his 60th birthday, edited by Cohen and Laudan (1983), in 1958 he was still far removed from psychoanalysis. He was viewed as a theoretical physicist specializing in space and time issues and was dubbed "Mr. Space and Time of American Philosophy." As can be gleaned from the bibliography of the *festschrift,* it was in two papers that Grünbaum launched his vehement science-theory critique of psychoanalysis.

Within a short time of its publication, his *Foundations...* had elicited 39 responses. The author in turn responded to these with his article "Précis of the foundations of psychoanalysis" (Grünbaum, 1986a). Finally, in 1993 a collection of pertinent papers appeared under the title "Validation in the Clinical Theory of Psychoanalysis".

Looking back over his publications we find it noteworthy that Grünbaum has maintained his position almost unchanged for many years. He concedes only "... that I am no more inclined to put a cap on the ingenuity of intraclinical investigations than on that of

extraclinical ones" (Grünbaum, 1993, p. 112). In the context of Grünbaum's convictions, this opaque sentence is essentially meaningless. He continues to praise Freud's brilliance to the skies while simultaneously denying his ideas the foundation of scientifically assured experience. In this connection a typical sentence of Grünbaum's deserves quotation:

> In the first place, I do *not* rule out the possibility that, granting the weakness of Freud's major clinical arguments, his brilliant theoretical imagination may nonetheless have led to correct insights in some important respects. Hence, I allow that a substantial vindication of some of his key ideas may perhaps yet come from well-designed extraclinical investigations, be they epidemiologic or experimental. Conceivably, it might even come from *as yet unimagined* new clinical research designs…" (Grünbaum, 1993, p. xi).

With great reservations Grünbaum accepted the objection of Holt that tape-recorded and transcribed analyses could make possible a separation of valid from invalid data, to achieve a decontamination (1993, p. 111).

Grünbaum's logical exegesis can be reduced to a small number of concepts. He comes to the conclusion that Freud's "master proposition," the "necessary condition thesis" (NCT) for the genesis of neuroses, namely the causal role of repression, is unproven. The concept of repression represents the comprehensive theory of defense mechanisms. The NCT is coupled with the "tally argument" of treatment technique. According to Grünbaum, the complex psychoanalytic setting is so contaminated that it is impossible to make scientifically founded statements about the genesis and healing of psychological suffering. This judgment results from Grünbaum's critique of Freud´s tally argument::

> The solution of his [i.e., the patient's] conflicts and the overcoming of his resistances can only be successful if one has given him *expectations* that are *in accordance* with his inner reality. Whatever was inaccurate in the physician's suppositions will fall away in the course of the analysis; they must be withdrawn and replaced by more correct ones. (author's italics). (Freud, 1917, p.453)

It should be noted that the *Standard Edition* translates Freud's phrase "die mit der Wirklichkeit in ihm *übereinstimmt*" (literally, "that *accord* with the reality in him") as "…*tally* with what is real in him." At this point Freud expresses the opinion that the therapy is successful only if the patient attains an accurate insight into the truth of his biographical and pathological history. The tally argument describes a problem of correspondence and not a claim to truth, as Freud had assumed.

Grünbaum (1984), who explored the problem of testing psychoanalytic theory on the couch in some depth, refers to the assertion that veridical insight leads to therapeutic success as the "necessary condition thesis." This thesis is the most important assumption for the "tally argument" ─ the argument that therapeutically successful analyses speak for the truth of the analytic (dyadic) knowledge that is gained in these analyses and transmitted to the patient. Against the therapeutic effect of veridical insight, Grünbaum asserts the following doubts: The therapeutic effect could also stem from suggestion on the part of the analyst, being based for example on nonveridical insights and pseudoexplanations; the therapeutic effect could be a matter of a placebo effect evoked by faith on the part of analyst and patient in the truth and efficacy of the insight generated through interpretation; the therapeutically desired changes could also derive from other aspects of the analytic setting, as for example from the experience of a new kind of human relationship, and not from the factor of "veridical insight."

Edelson (1984), in contrast, maintains the claim that objectively true (veridical) insight on the part of the patient is the necessary prerequisite for changes assessed as therapeutically positive in the framework of a psychoanalysis. At the same time however he concedes that veridical insight is not a sufficient precondition for achieving therapeutic changes in analysis. Edelson argues that analysis-specific goals and changes are all tied to the patient's veridical insight and that it is possible to speak of a successful and effective psychoanalytic treatment only if these goals and changes have been achieved.

It is not hard to recognize that the controversy over the necessary condition thesis is really about the question whether Freud's assertion of an "inseparable bond between cure and research" is valid for psychoanalysis or not. Someone who simply accepts the "inseparable bond" thesis into his argumentation as an undisputed fact (e.g., in the form of the tally argument) treats it as a natural law, forgetting that the role of "veridical insight" has not been adequately studied in empirical research into the therapeutic process (see chapter 5) and that the concept of insight is fraught with methodological difficulties. Hence it would be premature to accept assertions of a connection between veridical insight and therapeutic success as fact (and thus comparable to natural law). Such caution is also justified in view of the fact that empirical process research has recognized a whole array of other conditions beyond veridical insight that play a significant role (Orlinsky et al., 2003).

The question whether Grünbaum's contamination thesis is justified or not must be decided on the basis of empirical process research and not within the framework of philosophical discussions. The same is true of the charge of suggestion, the legitimacy of

which would have to be established empirically in regard to psychoanalytic practice before it is raised with the certainty often associated with it (Thomä, 1977). It must therefore be demanded that the forms of changes specific to psychoanalysis be exactly described and distinguished from other processes; further, that research seek indicators of the changes in question, since as dispositions they are only indirectly observable by way of these indicators; and finally, that not only the conditions for veridical insight be specified and investigated, but also what is needed beyond "veridical insight" in order to achieve the kind of personality changes envisioned by the goals specific to psychoanalysis (Edelson, 1984).

Grünbaum forcefully defends Freud's scientific position against philosophical and psychoanalytic hermeneuticists such as Ricoeur, Habermas and Gadamer on the one hand and Klein, Schafer and Gill on the other. Against Popper he argues convincingly in favor of the scientific status of psychoanalysis. Popper regarded psychoanalysis and Marxism as unscientific because, since both of them can be verified by anything at all, they fail to meet Popper's criterion of demarcation: falsifiability. On the basis of Freud's case histories, Grünbaum counters Popper's argument, asserting that there have indeed been refutations and falsifications of earlier hypotheses in the history of psychoanalysis, and these have been based on clinical experience and findings. The reader will share our surprise: In the controversy with Popper, findings have suddenly gained validity, although Grünbaum has denied them any force of proof. To speak with Grünbaum's own words, these modifications of theory are an eloquent testimony that Freud was responsive to *adverse* clinical and even extraclinical findings that *contradict* his theory. He quotes the lesson that Freud learned in 1926 from the circumstances of Wolf Man and Little Hans:

> It was anxiety which produced repression and not, as I formerly believed, repression which produced anxiety. ... It is no use denying the fact ... that I have on many occasions asserted that in repression the instinctual impulse is transformed into anxiety. *But now an examination of phobias, which should be best able to provide confirmatory evidence, fails to bear out my assertion; it seems, rather, to contradict it directly'*. (Freud, 1926d, p. 109)

> Grünbaum gives a number of proofs on Freud´s readiness to alter his view: Furthermore, we need only recall the very theme of Freud's 1937d paper "Constructions in Analysis," namely, just how he assures the intraclinical falsifiability of those clinical reconstructions that are avowedly the epistemological lifeblood of his whole theory! When Popper asks, 'what kind of clinical responses would refute to the satisfaction of the analysts...psychoanalysis itself?' I ask in return: what is 'psychoanalysis itself'? Is it

> the theory of unconscious motivations, or the psychoanalytic method of investigation? As to the former, Freud stressed its conjectural nature by espousing Poincaré's view that the postulates of the theory are evidently undetermined free creations of the human mind. (Grünbaum, 1984, p. 281)

In his controversy with Popper, however, in the last section of his book Grünbaum makes an about-face in a complicated chain of argument that at first appears to run in favor of psychoanalysis. He gangs up with Popper on the back of psychoanalysis:

> Since Freud's Tally Argument failed and no substitute for it is in sight, Popper is quite right that contamination by suggestion does undermine the probative value of clinical data. But I have argued that insofar as his case against the clinical confirmability of psychoanalysis *is* sound, it does *not* redound to the discredit of inductivism qua method of scientific theory validation. And I have documented that Freud had carefully addressed—albeit unsuccessfully—all of Popper's arguments against clinical validation... (Grünbaum, 1984, p. 285)

With these few concluding sentences Grünbaum tacitly takes back the passionate defense of psychoanalysis as a science that he had offered Popper. The two philosophers, though at odds regarding inductivism, are in agreement in their verdict that Freud's efforts towards a clinical confirmation of the defense theory were without success. Thus Grünbaum places the founder of psychoanalysis in the ranks of failed geniuses.

Grünbaum, until recently (2001), failed to consider that all the essential insights of psychoanalysis were gained in the scientifically impure clinical setting ─ including those observations that prompted Freud to renounce previously accepted causal connections. It should be added: precisely *because* Freud did not succeed in creating the "social null situation" (de Swaan, 1980) that is a given in the experiments of natural science. It is to Freud's credit that he shied away from fully introducing the subject and its attendant scientific problems into medical practice and therefore swayed throughout his life between psychoanalysis as science and as therapy. Like Grünbaum, he was disturbed by contamination of the findings through the personal influence of the analyst, because of which "the therapy [could] destroy the science" (1927a, p. 254). The suggestion problem troubled him his entire life. In the Anglo-American literature this problem is referred to as "Fliess's Achensee question," as in Meehl's (1983) publication under the title "Subjectivity in psychoanalytic inference: the nagging persistence of Wilhelm Fliess's Achensee question."[3]

In his search for contamination-free data, Grünbaum the scientific philosopher of physics, radically bypasses the methodological problems of psychoanalysis. These are rooted in the fact that in a practical-therapeutic human science there can be no pure data. The seemingly neutral analytic stance intended to assure objectivity was accordingly incapable of excluding the "disturbing" influence of the observer in order to achieve objectivity. The current acknowledgment of contamination makes possible the distinction of different ways in which influence is exerted and different intersubjective processes.

Psychoanalysis is the only systematic psychopathology on the foundation of human conflicts (Binswanger, 1955). These latter cannot be simulated; their investigation and therapy must be conducted in a human relationship. In our judgment, the practical and scientific problems this entails can be solved more appropriately today than at Freud's time. The problem of contamination is soluble in modern psychoanalytic research. Due to his physicalistic orientation, Grünbaum declared scientific investigations of causal connections in the psychoanalytic setting to be impossible, and displaced them to the outside. As experimental investigation of unconscious processes and dream research (Shevrin, 2005; Leuschner and Hau, 1995; Holt, 2002, 2005) have shown, "extra-clinical" research has its own independent significance. But this cannot of course replace investigations of the "native soil" of therapy, which are so productive in both scientific and practical terms.

Some years ago an intensive exchange of ideas with Grünbaum helped clarify our own position. He drew our attention to a careless formulation in our earlier study (Thomä and Kächele, 1975) in which we had written:

> We agree with Rapaport's (1960) that proving the validity of psychoanalytic theory is a task of the scientific community, which has to agree on the practical procedure of empirical science. Contrary to the restrictive limitation of general interpretations, psychoanalytic research and practice cannot be satisfied with a concept of the self-formative process that is as philosophically vague as it is rich in content, and from which confirmation of the theory should result. In any case, the logic of explanation through general interpretations points towards the specific way in which the confirmation of psychoanalytic statements can alone be obtained: this becomes clear in the linking up of hermeneutic understanding with causal explanation: "Understanding itself gains explanatory power" (Habermas, 1971a, p. 328). With regard to symptoms, constructions take the form of explanatory hypotheses with the aim of analyzing modes of behavior in causal terms. The *dissolution* of a 'causal coherence' through interpretative efforts illustrates the efficacy of psychoanalytic therapy. The

> constructions are to be applied to the single case; they this become theoretical statements from which singular prognoses can be derived. Generally speaking, these prognoses identify the conditions causally responsible for the neurotic state and claim that the therapeutic process must dissolve these conditions in order to induce change. The disappearance of the efficacy of the supposed internal conditions e..g. pathogenic unconscious phantasies – demonstrated itself in changes of symptoms and behavior. (Thomä and Kächele 1975, p. 86, italics added by authors in quotation)

Grünbaum was irritated by this careless formulation of ours, which he quotes out of context as follows: "The dissolution of a causal connection by means of the work of interpretation [in the treatment situation] illustrates the efficacy of psychoanalytic therapy." We were first made aware of this careless formulation in an oral communication with Grünbaum. Later, in the German translation of his major work (Grünbaum, 1988), he confirmed for us that in the Ulm textbook (Thomä and Kächele, 1985) we had assessed the matter correctly:

> In the meantime Thomä and Kächele have assessed the matter correctly... 'In the end, the specific causes of the repression can fall away, i.e. become ineffective. This change dissolves the determined patterns and not the causal nexus as such—the latter, as Grünbaum (in *The Foundations of Psychoanalysis*) emphasizes, is in fact confirmed by the dissolution as a correctly surmised connection' (cit. Thomä and Kächele, 1985, p. 27[2]). (Grünbaum, 1988 p. 33).

Such proofs form the scientific foundations of psychoanalysis and we believe they have been abundantly supplied. Presumably Grünbaum would not allow this argument to stand. For example, he might counter: What is possible in principle collapses due to the contamination of all data in the execution. At this point Grünbaum could take recourse to his thesis of "necessary condition" and his "tally argument." As an advocate of unified science, he could maintain his position by claiming that it is impossible to apply the logic contained in it due to the inevitable contamination in the therapeutic situation. We honor this opinion within the context of the motto with which we have prefaced these remarks.

---

[2] The English translation can be found in Thomä & Kächele 1994a, p. 27

Psychoanalysis as science?

Since many years a voluminous literature about the ranking of psychoanalysis among the sciences has been produced. The planning and carrying out of our research projects necessitated the clarification of our own ideas concerning the scientific status of psychoanalysis by discussing essential controversies. Here we want primarily to take up those points of view that influence methods of clinical research. The integration of psychoanalysis with the nomothetic or ideographic sciences, with natural, social, behavioral sciences, or the arts, must be considered an unimportant academic question, unless this coordination results in relevant consequences for research and practice.

There are many reasons for the fact that psychoanalysis has become a focal point for discussion. We would like to specify some of these psychoanalyses by sharing its theoretical problems with all the other sciences that examine human behavioral and its psychological and social motivations in the field of human relations. It was Freud who discovered the role of the participant-observer and its far-reaching influence on the observational situation. Since it goes beyond "understanding" in its description of phenomena and its proposed theories of "explanation" about the observations it has obtained, psychoanalysis is situated in a border region of scientific theory.[3]

This anomalous position might account for the fact that there is hardly a modern philosophical movement that has not been concerned with psychoanalysis and its methodology of research. The exponents of "unity of sciences," those of the logical-empirical theory of analytical science, as well as the followers of the dialectic and hermeneutic movements in philosophy and sociology, all find psychoanalysis to be interesting matter for discussion. It is remarkable that psychoanalysis will not fit into the hermeneutic claim of universality, nor will it be forced into the Procrustean bed of the uniform scientific method of "unity of science." It is not surprising that the exponents of "unity of science" cast doubts on psychoanalytic explanations because these can only be proven in an interpretive context, while on the other hand psychoanalysis as an "explanatory" theory is not hermeneutic enough. It would be appropriate to react to the critical courting of psychoanalysis with a question in the manner of Gretchen of Goethe's *Faust*: Why should one believe in a unity of science of one or the other kind?

---

[3] In this translation the terms „understanding " and „explanation" are set in quotation marks when it is important to indicate their terminological rather than their colloquial character. See also Eissler (1968) and Hartmann (1927, 1958).

We do not, however, intend to psychoanalyze the claims of the unity of science movement in order to have the last word in all matters of scientific truth. Rather, we will endeavour to turn the many valiant efforts and disputes concerning psychoanalysis to its own advantage. The application of scientific criteria (in the sense of the positivistic theory of science) such as the ability for replication, objectification and validation poses special problems that have been discussed for a long time within psychoanalysis. The discussion of these problems encompasses a field between two extremes, which in their distribution and valuation, can be seen to belong either to the Anglo-American or the French-German sphere. While we often too flippantly dismiss as positivism the effort to see psychoanalysis as an empirical and verifiable science, the group of behaviouristic social scientists rejects "understanding" as a constituting element of the dialogue. If in psychoanalysis "understanding" is attained by the way of explanation, as stated by Radnitzky (1973, p. 244ff), there is the danger that its model will be distorted by exaggerated emphasis of one aspect to the detriment of the other. These diverse attitudes have considerable practical consequences for research and treatment in psychoanalysis because it is a behavioral science with highly theoretical implications. The history of psychoanalysis itself, up to the most recent arguments among psychoanalysts, shows how vulnerable and unsure it is in regard to "understanding" itself as a science.

Hermeneutics and Psychoanalysis

We will critically illuminate those aspects of hermeneutics that are important for the interpretative technique of psychoanalysis. In this we will proceed along the lines of Apel (1955, 1967, 1971), Gadamer (1965, 1971a, 1971b), Habermas (1967, 1971a, 1971b), and Radnitzky (1973). Our theme will be limited to the relation between the hermeneutic and the psychoanalytic *technology of interpretation*; this limitation will determine our selection and our critical attitude toward its literature. We have arrived at our conclusions by including arguments from philosophy and the theory of science that have also entered into the dispute on the metascientific basis of sociology.[4] They prove useful for the resolution of certain methodological problems in psychoanalysis.

Within this fixed framework we will content ourselves with examining, from a historical point of view, those aspects of hermeneutics that are close to the interpretative technique of psychoanalysis in its psychology of "understanding." In order to facilitate communication, we would first like to give a definition in accord with Radnitzky's

[4] This debate is called "Der Positivismusstreit in der deutschen Soziologie" (Adorno et al., 1969). It originated during an International Sociological Congress in Tübingen, 1952. Among others, Adorno and Popper were the prominent proponents.

explanation. The term "hermeneutics"[5] dates from the seventeenth century. It was formed from *hermeneutike techne* and means a procedure to interpret texts ("the teaching of the art of text interpretation"). In the Greek *technai logikai (artes sermonicales),* hermeneutics was in close relation to grammar, rhetoric and dialectic. Hermeneutics is the science of interpretation according to which interpretation is based on a previous comprehension of the complete meaning of what is to be interpreted and proceeds to the exploration of presumed situational contexts. It is thus circular theory. It indicates an indissoluble interplay between an "understanding" of the whole and an "understanding" of a part, or between a subjective pre-comprehension and an objective comprehension of the object. This circle implies a correction based on the feedback between the preliminary "understanding" of the whole text and the interpretation of its parts. The development of hermeneutic was essentially influenced by the exegesis of the Bible to which the theological background of our present discussion should be ascribed. The theological debate over the theory of hermeneutics is documented in (among others) the principle of Schleiermacher: initially one does not usually arrive at an "understanding," but rather at a misunderstanding; so the problem of "understanding" becomes a theme of epistemology (the theory of knowledge), namely, that we have to already know – that is, we have to have a previous comprehension – in order to investigate something. The clearest expression of the hermeneutic procedure can be found in the arts, in the philology of the text interpretations. There, the basic question is: what meaning, what significance did and does this text have?

The step from the interpretation of old texts to the question of their present meaning adds a historical dimension to hermeneutics. Instead of practising a pre-critical, normative, and dogmatic transfer and passing-on of tradition, the art of hermeneutics nowadays claims to promote the mediation of traditions within a critical "understanding" of self and history.

Hermeneutics has thus become the instrument of the arts. Albert (1972, p. 15) affirms that it is a technology of interpretation having at its base unspoken assumptions about the regularity and legitimacy of insights in art. Only Heidegger and his followers raised hermeneutics to a "universal outlook with its own peculiar ontological claims" (Albert, 1971, p. 106). This has significantly influenced the humanities.

---

[5] Hermeneuo = I denote my thoughts by words, I interpret, I explain, I expound, I translate. We assume that there was an etymological relation between hermeneutic and Hermes because Hermes, the god of commerce, messenger of the gods, had the duty of an interpreter: he had to translate their messages. Prof. K. Gaiser of Tübingen University, whose help in this and other matters we gratefully acknowledge, has given us the philological opinion that the coupling of Hermes and hermeneutics is based on popular etymology, i.e., on the fortuitous resemblance of these words. *Hermeneuo* actually derives from a root with a meaning identical to "speaking."

From philological, theological and historical hermeneutics, a line leads to "understanding" psychology. The common denominator that connects the psychology of "understanding" with the arts is the claim of putting oneself into somebody else's place, of empathy with a text or with the situation of the other. The sharing of experiences of another person is one of the prerequisites that make psychoanalytic treatment possible. Introspection and empathy are essential characteristics of the technical rules of "free association" and of "evenly hovering attention," which supplement each other.

The sentence, "Each understanding is already an identification of the self and of the object, a reconciliation with the one who would be separate, if he were outside of this understanding; what I don't understand remains different and alien to me," when translated into contemporary language, could have been written by a psychoanalyst who concerns himself with the nature of empathy (compare, for example, Greenson, 1960, Kohut, 1959). The quoted sentence is from Hegel, as quoted by Apel (1955, p. 170). Kohut (1959, p. 464) affirms that Freud utilized introspection and empathy as scientific instruments for systematic observation and discovery.

The relation between general hermeneutics and psychoanalytic situations works in two ways. The analyst starts to understand the patient's (up to now) incomprehensible behavior by retracing its development. Historically genetic "understanding" is thus accomplished that is the "understanding" of psychological or psychopathological phenomena in the greater context of a life history. Consequently, the problem of the relation of the parts to the whole and vice versa, as well as its interpretation, has to be solved. According to Gadamer (1965), the interpretation starts "where the sense of a text is not immediately comprehensible. One has to interpret wherever one does not trust the immediate appearance of things. The psychologist interprets, not by evaluating life histories in their intended meaning, but by asking about what happened in the unconscious. The historian interprets the data of tradition in the same way, in order to grasp their true meaning, which is expressed and at the same time hidden by them" (p. 319).

In this statement, Gadamer seems to visualize a psychoanalyst; his description highlights depth-psychological questioning. It was precisely the incomprehensible, the seeming senselessness of psychopathological phenomena, which would yield to understanding when it was traced back to its unconscious roots in childhood. The problem Gadamer presents is more than an unimportant little detail: it is the problem of disguised and encoded writing, one of the most difficult ones within hermeneutics. Here hermeneutical philology seems to arrive at a frontier similar to the one that the psychology of

"understanding" could not transcend. It is a fact of scientific history that neither static nor genetic "understanding" — in Jaspers' sense — has made essential contributions to the psychogenesis of neurotic or psychotic symptoms, or to their psychotherapy. We have to inquire, therefore, by what means the psychoanalytic method has achieved a larger degree of "understanding." Is the method of psychoanalysis a special, at times partially complemented, hermeneutical, interpretive science? Were the old traditional rules of interpreting only adapted by a special technique in order to conform to the conditions of psychopathology, or the psychotherapeutic relation of physician and patient? Do we have to look for the difference that first created the new technical means of interpretive understanding in practice, or is novelty a new theoretical, explanatory paradigm, as the historian of science, Kuhn (1962), put it?

There is no doubt that by accepting the unconscious, these new technical means, especially the ones concerned with treatment, have added another significant dimension; the dimension of depth to the philological and historical rules of interpretation. One could therefore call the interpretive technique of psychoanalysis "depth hermeneutics". According to Habermas (1971a), psychoanalytic interpretation is concerned with those symbolic connections through which a subject deludes himself. *Depth hermeneutics,* which Habermas contrasts with the philological hermeneutics of Dilthey (1900), is concerned with texts that show the *self-delusions* of the author. Besides the obvious content and its connected indirect but intended comments, the texts document a latent part of the orientation of their author that is not available to him and from which he is alienated; that is nevertheless his own. Depth hermeneutics appear in this context as a process that marks the lifting of alienation, but Habermas states that the real task of hermeneutics, which does not limit itself to philological procedures, consists of combining the analysis of language with the psychological investigation of causal connections.

As we will show later, the subject and method of psychoanalysis, and especially its empirical steps of confirmation, are essentially very different from philological-theological or language-analyzing hermeneutics. Thus the concept of "depth hermeneutics" suggests too close a relationship between them. Surely, Freud had taken an understanding attitude:

> He had talked to patients he believed what they told him, instead of using objective methods. But what did he do? He developed methods, while looking at phenomena, that suited these phenomena, and these methods proved to be teachable: this means that a scientific method was created here which would never have originated if the phenomenon had not first been observed by a person endowed equally with the

> wonderful gift of apprehending phenomena, on the one hand, and understanding them, on the other, with a very critical and very methodical intellect. (von Weizsäcker, 1971, p. 301)

The Limits of the Hermeneutical Point of View

The digression into hermeneutics served to put the interpretive technique of psychoanalysis into a larger scientific and historical context. We have disregarded the fact that the psychoanalytic situation implies very special rules of interpretative technique. This accounts for the fact that its interpretive art is very different from all hermeneutical movements and schools. Philological and historical hermeneutics do describe the relationship of interpreter and text as a sort of dialogue, a kind of talk. It is evident however that the text, unlike the patient who interacts with the physician, can neither talk nor take an active position pro or con.

This difference becomes equally clear when one considers the methodological difficulties of psychoanalytic biography. In this field, the problem is that a solution of the biographical riddles, as Helene Deutsch (1928) called them, must be found "not by the psychoanalytic method, which can only be used directly and on the living person, but by being armed with analytic knowledge of the processes of mental life" (p. 85). The basic difference between a textual interpretation and a psychoanalytic situation could be defined by the fact that between physician and patient there exists not only an imaginary interaction as the one in the hermeneutic circle, but also a mutual and real interchange of a very unique kind. From this arises the claim, we believe, that a psychoanalyst not only gives plausible interpretations, but also develops an explanatory theory from which one could derive recommendations for actions that have the power to change behavior. As a result, the perception of the psychically alien, the "understanding", becomes integrated into a new function. No consequences for the text arise from its interpretation, be it right or wrong; the interpreter remains to the end bound to his separate world. But for the patient, who has to be understood, the consequences of a correct or incorrect interpretation of the psychically alien are far-reaching.

Ricoeur (1970) has underlined the psychological "understanding" aspects of hermeneutics from the point of view of philosophy. In his work the difference between interpretation of texts and psychoanalytic technique runs the danger of being wiped out. The German psychoanalyst from the Frankfurt Sigmund-Freud-Institut Lorenzer (1970) also tries to put reliable insight into the psychically alien on a hermeneutical and psychologically

"understanding" basis. His thesis incorporates a fruitful revision of the psychoanalytic theory of symbols and an attempt to reinterpret psychoanalytic work as work about language which tries to comprehend the origins of symptoms and the deformation of language as "excommunication" of private contents from consciousness.[6] Such attempts to attach the psychoanalytic method unilaterally to *szenisches Verstehen*[7] (scenic understanding) and to hermeneutics are doomed to fail. It is by chance that psychoanalysis has always been used against the claim of universality by the philosophical advocates of hermeneutics (Gadamer). The "radicalization of the hermeneutical point of view" by Lorenzer (1970, p. 7) leads us to the limits of hermeneutics where its principal weaknesses become apparent. A discussion of Lorenzer's ideas will afford a special opportunity to further analyze the relation between interpretive practice and explanatory theories in psychoanalysis.

In the following discourse we assume that the psychoanalyst fulfils certain basic conditions and that the process of comprehension is achieved by empathy with the psychically alien. The importance of imagination in the process of insight can hardly be overestimated. As Paula Heimann (1969) states:

> We can imagine what and how somebody else feels and thinks; how he experiences anxiety, hope, desperation, vengeance, hate, love and murderous impulses; what kind of imaginations, fantasies, wish-dreams and impressions, physical pains, etc., he has and how he fills these with psychic content. (p. 9)

The psychoanalyst however would not only want to understand the thoughts and feelings of the other with the help of his ego functions, which Heimann believes to be the most essential parts of a soberly defined concept of empathy. Rather, he finds himself seeking for insights that are reliable. He is confronted here with a crucial problem of research into the processes of psychoanalysis. How to arrive at a *reliable* knowledge of the psychically alien is, in our opinion and that of Lorenzer, a question of life and death for psychoanalysis as a scientific discipline.

Our preliminary answer to this question is that the psychoanalytic process has to be carried by empathic understanding because it would not otherwise occur. The question of the reliability of the understanding brings us to the problem of validation or falsification in the framework of explanatory theories. There is the question of how it will be decided if psychic and psychopathological phenomena and their genetic significance have been rightly or

---

[6] Compare Stierlin's (1972) extensive review.

[7] "Scenic understanding" = a kind of intuitive perception of the whole gestalt by way of the findings of situations that are conserved in the unconscious in their original gestalt.

wrongly "understood." Is it the "understanding" itself that assumes the decisive validating or falsifying function?

We know that according to its principal proponents (cf. the psychiatrist and philosopher Karl Jaspers, 1948) the psychology of *Verstehen* (understanding), though it has not developed a method of systematic observation similar to that of psychoanalysis and does not lay down any general or specific theories of psychogenesis, has to give proof through objective data: "Not by subjective or intersubjective evidence do we ascertain a 'comprehensible context,' but through objective data" (Jaspers, 1948, p. 251). In contrast to Jaspers, Lorenzer (1970) believes that, by transforming static to scenic understanding, the experiencing of evidence can be introduced as a decisive scientific test of reliability. Unlike most other psychoanalysts, by omitting explanatory theories from the treatment situation, he reduces the test of reliability almost completely to the intuitive experience of evidence.

According to Lorenzer the 'scenic understanding' and the evidence in the psychoanalytic knowledge of the psychically alien occupy a special place next to the logical understanding and empathy. Indeed, in the course of a discussion about the process of psychoanalytic conceptualization, one really arrives at matters that cannot be resolved by any logical or psychological understanding of the psychology of consciousness. 'Scenic understanding' embodies a large number of intrapsychic processes in both the analyst and patient as well as the interpersonal processes of transference and counter-transference. In 'scenic understanding' unconscious processes are involved and are described according to the principles of established models of interaction (Lorenzer, 1970, p. 109). The analyst makes sure of his understanding by the same psychic mode that occurs in a logical and psychological understanding under the name of experiencing of evidence. The experiencing of evidence in 'scenic understanding' is attached to the models of interaction. These models of interaction make it possible to recognize the most varied events as the expression of one and the same scenic arrangement.

These concepts need closer scrutiny, inasmuch as Lorenzer makes them the guide to his treatment process and even uses them to ascertain the reliability of the recognition of the psychically alien. Since he denies the assumption that explanatory steps are integral parts in the formation of analytic understanding, he is the most exemplary and weighty exponent of putting psychoanalytic knowledge on a basis of pure "understanding" psychology. Lorenzer is convinced that his thesis — that psychoanalytic practice consists in a pure and self-contained process of "understanding" without explanatory steps — will be proven absolutely

right by a discussion of the conceptual innovation of 'scenic understanding'. This concept can undoubtedly incorporate elements of psychoanalytic insight into the mental life of others.

'Scenic understanding' finds its conclusive certainty as follows: "It proceeds in analogy with logical comprehension and empathy; it is guaranteed to the analyst through the experiencing of certainty" (Lorenzer, 1970, p. 114). "Experiences of evidence are brought to correspond to the perception of good *Gestalten*." By the help of the view of Gestalt psychology, which Devereux (1951), Schmidl (1955), and even earlier, Bernfeld (1934) had applied in order to explain the successful closure of interpretations, Lorenzer tries to prove the reliability of experiences of evidence. Indeed, there are some experiences that end up in a persuasive, possibly common "Aha" experience (a "co-variant of action") (Bühler, 1927, p. 87). Did the "Aha" experience end all doubt because an insight had become a meaningfully significant gestalt? But precisely what is such a gestalt that leads up to reliable evidence in the dialogue? Freud's analogy (1896c, p. 205), in which he compared the interpretive construction of an infantile "scene" with fitting the pieces of a "child's picture puzzle" together, could perhaps be inserted into some theory of Gestalt psychology.[8] For Freud the crucial step of the scientific procedure in psychoanalysis does not rest upon the subjective evidence, though it might be accompanied, for example, with convincing reconstruction of a traumatic scene; but instead it is rather that of "therapeutic proof," which is an observable change of behavior. The additional understanding of the "scene" — in 1896 this consisted of sexual traumas in childhood — could not by any means be justified as correct by itself, but had to prove itself through the hypothetically required resolution of the symptoms and by "objectifying of the trauma." Lorenzer's (1970) abstention from securing more supportive evidence has serious consequences for the requirement of reliability. Sometimes he has doubts about the reliability of 'scenic understanding' (pp. 150, 163). Thus the question arises: when 'scenic understanding' tries to pinpoint the original incident through all the falsifications of meaning, on what is it based?

'Scenic understanding' relates to psychoanalytic-drive, for example, motivations theory, even if Lorenzer (1970) rejects the concept of motivation for psychoanalysis. He sees it as an alien element within psychoanalysis, especially because of its relationship to behavior. He even fears that the concept of motivation excludes the particular essential task of

[8] Kurt Lewin's (1937) gestalt theory is especially close to psychoanalytic theory. It seems very dubious to us that experiences of evidence could gain more reliability through gestalt psychological descriptions (see Bernfeld, 1934).

psychoanalysis (p. 27).[9] We don't have to further substantiate here that these theses cannot be sustained.

Loewald (1971, p. 71) further developed the psychoanalytic theory of drives into a theory of motivation, proposing the thesis that personal motivation is the basic assumption of psychoanalysis (p. 99). We believe that in 'scenic understanding', motivations and their unconscious antecedents are created figuratively through the imagination. It is through his imagination that the psychoanalyst, as Heimann described it, puts himself into the scenes the patient calls forth. However, since Freud's discovery of certain contents of psychic reality, we know that the scenes did not really happen in the way the patients could remember them if he could remember them at all.

Lorenzer seems to have this problem in mind when he speaks of falsification of meaning. In this context: what is meant by the thesis that the psychoanalyst should approach the original event through 'scenic understanding'? We first have to assume the validity of the theory of trauma in its original and uncut form, "an original event". Out of this arise, among other things, several questions for empirical research: if one defines original events, which mean traumas, according to exterior characteristics, then one should endeavour to objectify these events after one has found them (Freud, 1896c; Bonaparte, 1945). If, on the other hand, one considers the inner, the psychic side, in shaping and displacing highly affective events or experiences, then the 'scenic understanding' of these events ought to be proven by a newly created edition of these events in the treatment situation that are revealed also by a close scrutiny of written records of treatment until finally the full scene would be reconstructed through "try-out" interaction and language games in the psychoanalytic situation. However, the search for the original events, in the sense of the old trauma theory or of later psychoanalytic theories, is not an end in itself.

Rather, psychoanalytic theories advance the hypothesis that a change in behavior should occur after the release of repression and the working through of an incestuous wish and an imaginary castration threat in the transference neurosis. There is a definite "if-then" hypothesis implied; therefore psychoanalytic theories can be clinically validated or falsified. The proverb *tertium non datur* is valid for a successful analysis. Here lies the possibility of validation through empirical studies of process, which will prove to be a stronger assurance against errors than Gestalt psychological, weakly supported "experiences of evidence." The

[9] Lorenzer cannot help but speak of "the unconscious determinants of behavior" (p. 165) and so abolishes his own argument against the use of motivational and behavioral concepts.

latter have a more heuristic function, making possible the formation of hypotheses rather than the corroboration of them.

Dilthey (1894) ascribed the formation of hypotheses to "descriptive" as well as to "explanatory" psychology, if only in the various stages of the process of cognition. "Descriptive psychology ends with hypotheses, while explanatory psychology starts with them" (p. 1342). How far descriptive psychological or psychopathological-phenomenological comprehension is already governed by hypotheses and whether theoretical preconceptions have not always and predominantly directed the description and influences the selection of the phenomena to be described, are questions that have no importance here.[10] Along the lines of Dilthey, Kuiper (1965) would also like to incorporate hypotheses into a decisive stage of the psychoanalytic process of cognition and in this way assure its examination.

The problem then shifts to the question whether psychoanalysis is an "explanatory" or rather an "understanding" psychology (Eissler, 1968, p. 187). Because of its methodological consequences, we will here discuss the kind of relationship that exists between *understanding* and *explanation* when they intermingle in psychoanalysis. Kuiper (1964) also regards his critical-historical works and those in the philosophy of science about "understanding" psychology and psychoanalysis as a contribution toward a methodological consideration of psychoanalysis. He writes:

> Without first accounting to oneself which form of psychology to use, one uses all sorts of methods, explanations and modes of thought indiscriminately. Empathic insight is employed alternatively with constructs that contain models; psychologically empathic connections are not sufficiently differentiated from speculation about the theory of drives: one proves hypotheses in one area with arguments that are taken from the other. (Kuiper , 1964, p. 32)

Kuiper believes it especially dangerous if "experiences of evidence" are regarded as decisive (p. 19). Psychological connections are not proven by a feeling of certainty, as is so often stated. Some have wanted to reserve empirical proof for basic correlations – for example, organic brain disease and dementia – and they have thought, in a narrow sense, that

---

[10] Freud (1915a) gives a remarkable example of his scientific thinking in which he describes the interplay between ideas and empirical trial and error:
"The true beginnings of scientific activity consist rather in describing phenomena and then in proceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material on hand, ideas derived from somewhere or other, but certainly not from the new observations alone. Such ideas — which will later become the basic concepts of the science — are still more indispensable as the material is further worked over. They must at first necessarily possess some degree of indefiniteness; there can be no question of any clear delimitation of their content. So long as they remain in this condition, we come to an understanding about their meaning by making repeated references to the material of observation from which they appear to have been derived, but upon which, in fact, they have been imposed " (p. 117).

the experiencing of evidence would be sufficient for other psychological connections. This is, of course, false. The fact that we deem a connection to be evident does not at all mean that this connection is also valid for the patient whose attitude, or rather whose experience, we try to fathom. Material proof has to be given for satisfactory explanation; in any case, our opinion has to be supported by empirical inquiry. If we regard the "experience of evidence" as sufficient reason to accept a connection, then "understanding" psychology becomes a source of error. The "intuitively 'understood connection' stays hypothetical until proven in a definite case" (p. 19).

Kohut (1959), another author who has focused particularly on the special importance of introspection, affirms that the insights gained through empathy need many safeguards. We believe that for the same reason Eissler emphatically defines psychoanalysis as an explanatory theory because questions of validation of hypotheses as well as general scientific dialogue would cease with the acceptance of subjective certainty, just as all decisions would be attributed to individual and subjective infallibility. Although Eissler defines psychoanalysis as *psychologia explanans* and not as *psychologia comprendens,* a position opposite to Kuiper's strong affirmation of understanding, we find the two authors in agreement on most of the methodological points. Both ask for objectifying proof that has to go beyond the descriptive understanding of feelings or certainty. Eissler seems to think about this kind of understanding by saying that it could become the antagonist of scientific explanations. So long as understanding psychological statements claim that the proof of hypotheses is already rendered by exact descriptions, further scientific investigations would indeed be superfluous because the process of cognition would be ended. In our view, Eissler ranks psychoanalysis as a *psychologia explanans*. Like Kuiper, he asserts the provisional quality of descriptive understanding statements and affirms the necessity of proving hypotheses. As a result of the possibility of falsification of psychoanalytic theories Eissler predicts their reconstruction, which also means a partial refutation. That is why Eissler, as well as Rapaport, ascribes to various parts of psychoanalytic theory a longer or shorter life expectancy.[11]

In our opinion it is now clear why in the history of psychotherapy and psychoanalysis the question of whether psychoanalysis belongs to the explanatory or to the "understanding" psychologies recurs over and over again. For Freud and important theoreticians after him,

---

[11] That Eissler (1971), on the other hand, tried to revive the generally discarded death drive, fits in well with his prognosis, because the ontological assumptions, hidden in the hypothesis of the death drive, are explained in their psychological significance. Briefly, Eissler is concerned with the psychological-existential meaning of death and not with its reduction to a drive.

such as Heinz Hartmann, David Rapaport and many others, the claim that psychoanalysis is an explanatory theory implied a "mental science" (Hartmann, 1927, p. 13) thus requiring first of all the strict proof of its hypotheses along the lines of "natural science." As a result of the fact that the natural sciences and their contemporary norms were to be the principal models for proof, the methodological originality of the empirical, specifically psychoanalytic line of argument was neglected. On the other hand, the radicalizing of the hermeneutical point of view has not enlarged the empirical basis of psychoanalysis at all; on the contrary, it has narrowed it extremely. Abstention from proving hypotheses has been replaced by the autarchy of an intuition that confirms itself by *evidence*.

According to Albert, the theological past of hermeneutics is obvious here, as it is with Heidegger. It is indisputable, according to authors of such different backgrounds as Abel (1953), Albert (1968, 1971, 1972) Jaspers (1948), Kuiper (1964, 1965), Stegmüller (1969), Weber (1949), that understanding has a heuristic or helpful effect on treatment. But 'scenic understanding' also requires additional proof; hence Lorenzer cannot support his extreme thesis.

How Lorenzer himself views the failure of his hermeneutic radicalism and at which point of his argument he lets the explanatory theories of psychoanalysis enter into the concept of scenic intuition is typical. His argument, reduced to its bare essentials (1970, p. 12), is that there is one place inviolate from all errors of theoretical language: psychoanalytic practice (p. 198). Here 'scenic understanding' would be rounded off with a self-contained, flawless, ideal operation, if the inevitable scotoma of psychoanalysts would not disturb their intuition. It is then assumed that there is one absolutely certain place for recognizing the psychically alien: psychoanalytic practice — if only the blind spots of psychoanalysts would not cloud their 'scenic understanding'. The psychoanalyst, completely liberated from his scotoma, would know with absolute assurance, and therein lie the consequences of the theory of cognition of this psychological utopia, which experiences of evidence are true. Since in ordinary practice the ideal operation of the closed understanding circle is never achieved, the experience of certainty will prove more or less right. That way it would be left completely up to subjective judgment if an understanding curve has come to a convincingly right or wrong closure.

According to Lorenzer, the psychoanalyst tries to fill the gaps in his understanding by bringing in explanatory theory as compensation. This helps him find the thread of understanding again. No doubt, theory can serve as a help in orientation whenever it functions from the beginning and not as we believe at the end or as compensation. The

theoretical crutch could only help to point to the right way of perceiving the psychically alien if it no longer had to be examined concerning its empirical proof. According to Lorenzer, it seems to be sufficient that the explanatory theories of psychoanalysis can smooth over blind spots and close interrupted understanding curves. In this way the validity of theory is either presumed or is left to be proven by continued subjective scenic understanding. However, in order to make psychoanalytic practice the crucial place of where the proof of its explanatory theories is to be rendered — and we would not know where else they could be fully tested — one cannot rely on one single and, as one has seen, uncertain criterion. The radicalization of the hermeneutical point of view and the absolute refusal to objectify anything connected with it can serve neither as a practical nor as a scientific guide.

On the Relationship between the Interpretive Practice of Psychoanalysis and its Explanatory Theories

The closing remarks of the preceding paragraph have a rather large implication: we said that explanatory theories could find their decisive scientific proof nowhere but in psychoanalytic practice. If the psychoanalytic method is not employed and the process takes place outside of the treatment situation, only those parts of a theory can be tested that do not need a special interpersonal relation as a basis of experience and whose statements are not immediately related to therapeutic practice.[12] So here, when we speak about explanatory theory, we mean clinical explanatory theory.

When clinical theories are concretely proven by a given dyad (patient-psychoanalyst), special problems result because method and theory have an especially close connection in psychoanalysis. From here on we will base our argument on the assumption that there is a close connection between practice and theory; we believe that the psychoanalytic art of interpretation needs theories as a guide. Paraphrasing Popper (1959, p. 423), we would say: interpretations of facts are always made in the light of theories. That the light of psychoanalytic theories can only illuminate each given case very insufficiently especially at the outset of a treatment is attributed not to the weaknesses of the theories but to the inevitable lack of information. However, hypothetical assumptions by which the interpretive action is directed come into play at once. But there are other, even contradictory, opinions. MacIntyre (1958) claims that psychoanalysis as psychotherapy is relatively autonomous in

[12] Rapaport (1960) believes that experimental proof of psychoanalytic theory for the most part is dubious because "the overwhelming majority of experiments designed to test psychoanalysis propositions display a blatant lack of interest in the meaning, within the theory of psychoanalysis, of the propositions tested. Thus most of them certainly did not measure what they purported to; as for the rest, it is unclear whether they did or not" (p. 113).

relation to psychoanalytic theory. He adds for emphasis: “Freud’s method of treatment is not altogether dependent — and this may be an understatement — on his theoretical speculation” (p. 86).

When one looks at the reasons given for the relative or even absolute autonomy of the technique, one finds a *mixtum compositum,* which is made up of presumably practical experiences and of judgments about the state of the theory. We will first present some condensed arguments for the first category — the practical.

Thesis No. 1: There are successes in psychotherapy by physicians whose theoretical knowledge is minimal, not to say zero.

Thesis No. 2: In the course of a treatment psychoanalysts often grope their way in the dark. In spite of insufficient and in some situations even completely absent theoretical orientation, they do the right thing intuitively, as is so often said.

Both theses seem to be applicable. But the question arises: what are they advocating? Their arguments are not, as we will show, in favor of the autonomy of practice. Such observations, which are not by any means systematically researched, show that actions that are unconsciously influenced by theory also exist. In every interpersonal relationship the right word can appear at the right time without the necessity of any further theoretical derivations or deliberations. Psychotherapeutic interactions are no exception. To put this in psychoanalytic terms, as much can “happen pre-consciously” in these interactions as in the psychotherapeutic learning process itself. Practical knowledge can also be gained during training because psychotherapy is not concerned with imparting theoretical knowledge but on imparting immediate experience, by which it might appear as if theory were abandoned. It is said, for instance, that no theoretical knowledge of neurosis or psychopathology is imparted by the training course in Balint groups. If this would be true, it would support the thesis of autonomy of practice because the undebatable therapeutic success of doctors instructed in Balint groups would by definition have been independent of theory. But the appearance is deceptive.

Anybody who has participated in Balint groups for a while, and especially anybody who has seen Balint himself in workshops, knows that theoretical psychoanalytic models were created there in such an effective manner that they were transformed into “prescriptions for action”. The most important element in the process of learning in the Balint groups is the fact that one’s own action and its continuous correction are the focal point. A continuous effort toward trial and error is maintained in this manner, although its relation to theory is covered up. We would like to remark in passing that it is a rather poor process of learning when

theories are only covertly transmitted to students, as if they are being implanted into the "preconscious," in the hope that they can be called forth to action at the right moment. The "preconscious" is neither the proper place of proof, nor has it the proper criteria for showing what constitutes error in the trials and where proof lies.

The untenable thesis of the relative or absolute autonomy of practice from theory contains the old theme of the role of intuition in technique. However, proof of theories by psychoanalytic methods does not require a previous clarification of the question of how the psychoanalyst arrives at interpretations in the course of his practical technique, whether or not they came about rationally or intuitively. The deciding factor is that the treating psychoanalyst or competent colleagues can agree on, the basis of consensus (see Seitz, 1966), whether or not theoretical guidelines are apparent in the interpretations given.[13]

Theory-testing research into process is further complicated by the combination of general and special variables. We make this distinction in order to separate the typical variables of the psychoanalytic process from non-specific factors. Research in psychotherapy demonstrates that the mere expression of empathy and interest for the patient can be helpful and beneficial by itself. An understanding attitude toward the patient, as is demanded by the basic rule of psychoanalysis, can itself have a favorable effect, as we know from the investigations of the Rogerian school.

Empathy, "evenly suspended attention," and other typical patterns of ideal behavior, which the psychoanalyst should be able to adopt, are highly susceptible to disturbances. Counter-transferences are unavoidable. An insurmountable counter-transference can exert an unfavorable influence on the process of treatment, so that success or failure would not be attributable to theory in that particular case. It is completely conceivable that the psychoanalyst in such a case can explain the psychopathology of the patient very well and can give interpretations with correct content. The idea seems to be justified that therapeutic success or failure cannot be cited in the service of validation or falsification of a theory.

In the psychoanalytic situation, the light of the "theory" is deflected by the occurrence of subjective influences and by favorable or unfavorable therapeutic and patient variables; not to speak of external factors, which also can impede treatment. Therefore, it seems justifiable to think that successes or failures cannot be used for validation or falsification of theory. This frequently voiced opinion is partly right and partly wrong. Psychoanalytic theories can only be proven in their subjective form, which they take on in each dyad. Here, "understanding" in the commonly understood sense of the word comes to the fore. Without

---

[13] "Scientific objectivity can be described as the inter-subjectivity of scientific method**"** (Popper, 1944, Vol. II, p.217)

empathy, the situation would be so transformed, that it would be a completely unsuitable setting for testing psychoanalytic hypotheses.

These reflections show that in psychoanalytic research, concerning treatment processes, those situational variables have to be encompassed, which co-determine the course of the treatment in a non-specific way. In order to give validity to the psychoanalytic data gained, scientific research has to be directed especially at the processes of interaction, for instance, phenomena of counter-transference, which Perrez (1971, p. 226) has pointed out. If, because of counter-transference, the psychoanalyst deviates too widely from the typically ideal behavior pattern that the basic rule prescribes, then the ground of psychoanalytic technique has been abandoned and neither falsification nor validation of psychoanalytic theory can be derived from this study of the process.

The struggle to observe the basic rule (A. Freud, 1936), which marks one side of the psychoanalytic interaction, is not lost as long as the interaction is continued. This means that the minimal conditions are fulfilled, which means that the patient arrives and the psychoanalyst is there for him. The highlights of the struggle show in particular that the psychoanalytic situation serves essentially to clarify the disturbances of communication. Radnitzky's (1973, p. 235ff) stylized and pure dialogue, which proceeds by understanding alone, does not exist in practice.

Radnitzky, along the lines of Apel (1967), speaks about quasi-naturalistic phases in psychoanalytic treatment, which should start at the limits of understanding. He believes that when the dialogue is interrupted, explanatory operations set in, which enlarge the understanding of the self and of the other. This artificial dismemberment seems to have contributed to the idea that explanatory operations, which prove hypotheses, find a satisfactory conclusion and corroboration only through understanding and resumption of an interrupted dialogue. Actually, the dialogue is disturbed from the first moment on, since psychoanalytic situation is asymmetrically designed in order to make the hidden distortions of communication clearly visible.

Psychoanalytic theory as a scientific system is, of course, already operative for the psychoanalyst at the outset of a dialogue with a patient. It offers a special terminology for causal connections and affords a comprehension of the modes of behavior that cannot be grasped without explanatory schemata.

We will now explore the question of the special means psychoanalytic theory employs. No doubt, the light of the theory shines where interpretations are given in the psychoanalytic situation. It is in the art of interpretation that psychoanalytic hypotheses become

instrumental. We want to add a few qualifying remarks to these statements in order to avoid misunderstanding. We do not mean to say that theoretical explanations are given in the process of interpretation. In spite of the great variations in individual techniques in psychoanalysis, there is general agreement that theoretical explanations are not effective in therapeutics. Theory itself offers explanations for this fact, but we cannot take these up here.

It would certainly be simpler to prove the scientific reliability of theory if the derivation of interpretations could be easily recognized, if they were pure hypotheses. Thomä and Houben (1967) have discussed the theoretical and practical difficulties in the employment of interpretations as a means of validating psychoanalytic theories. Our efforts and reflections since then have shown that the problem is even more complex than we had originally thought. It is the instrumental character of interpretations that complicates their functions in proving theory: "We intervene by interpretation in an existing setup with the intention of bringing about certain changes" (p. 681).

Due to the instrumental character of interpretation, Farrell (1964) denies this hypothetical basis. He tried to substantiate his argument by referring to Freud's (1909b) statements that psychoanalysis is not an impartial scientific investigation but a therapeutic measure. Its essence is not to prove anything, but merely to alter something (p. 104). Actually, for a real change in the patient, there is no better proof for the theory than empirical findings; thus, Freud's words imply a true clinical as well as a scientific aim. Consequently, Farrell has to abolish his extreme point of view: he eventually concedes that an interpretation

> retains hypothesis-stating and hence declaration features. But these are apt to be overlaid by, and lost in, the complicated instrumental context in which this sort of statement functions. Consequently, even though such a statement has declaratory features, it may be difficult on many occasions to discover from its context just what its truth criteria are. (Farrell, 1964, p. 321)

Indeed, it is most difficult, especially because it is not enough to test an interpretation within a session (Wisdom, 1967); each series of interpretive repetitions in the psychoanalytic process must be evaluated (p. 681).

It does not mitigate against the central role of theory in scientific proof that interpretations as communications always contain more than their, at best, discernible guideline. Interpretations as verbal communications also have unspecified content, which might in a given case outweigh the special psychoanalytic point of reference. Therefore, empirical investigations show that many statements of an analyst cannot be considered interpretations in a narrower sense. In order to demonstrate the kind of conditions that have

to be met in order to derive theoretical proof from interpretations, let us say that proof should be shown that prognosticated changes in a patient occur by interpretations referring to the hypothesis in fear of castration, but not by using interpretations referring to the hypothesis of separation anxiety. This way, falsification or validation would be possible only in individual cases. The proof would be limited by the special conditions of trial and error relative to the examination of two alternative hypotheses during a protracted phase of treatment.

These limitations result from the structure of psychoanalytic theory, which we will discuss later. We are also omitting here the problem of circularity. This problem exists because proof has to be established of just those theories from which hypotheses are to be derived with the help of interpretations, which in turn contain these hypotheses. We shall discuss the problem of circularity and the question of suggestion in the final part of this chapter. Here we want to remark that the proof has to be oriented to a standard of the prognosticated change in the patient. In this procedure, the role of resistance has to be considered in advance and not retrospectively. It does not have to be predicted, but it must be defined. Similarly, in other fields of medicine, one expects no change in a patient if he sabotages therapy.

For this kind of theoretical proof, it does not matter how the interpretations originate in the mind of the psychoanalyst. Along the lines of Levi's (1963) work, Loch (1965) presented a schema that emphasizes the rational root, the theory-related planning of interpretations, while fully considering the emotional relation to the patient. Lorenzer, who wants to reduce his arguments to a common denominator, affirms in opposition to this that intuition is the origin of interpretations. Cautioned by the controversy between Reik and Reich, one is well advised to take into consideration as valid factors the personal bias of the psychoanalyst. Nothing needs to be added to the work of Kris (1951), who clarified the long-standing controversial issues of "intuition and rational planning" in psychoanalytic psychotherapy. Moreover, neither planned nor intuitive interpretations can take a preponderant place in the studies of process and interaction. Both have to prove themselves through given prognoses and by their effects, which can be objectified.

To this end, we presume that certain phases of treatment and their predominantly interpretive working-through can be recognized by the analyst himself, and/or consensual validation of other experts. If psychoanalyses are recorded, the psychoanalyst who interprets intuitively can afterward recognize the presumably theoretical and practical points that relate to his intuitive perception. We do not want to hide our own personal bias and would like to express our scepticism about an intuition that thinks that it can work on objective data and

continuous validation without reassurance. Even retrospective explanation, after the analysis as a whole and after each session, remains in many instances hypothetical and is subject in the further course of analysis to "trial and error." We believe that Freud had the same idea when he cautioned analysts not to draw scientific inferences about a case before the treatment is ended. Freud even advised against interim reports so as not to limit either therapeutic or scientific openness. Freud seemed to fear that provisional theoretical explanations of the origin of symptoms could, once they are formulated, assume a status they cannot merit:

> The distinction between the two attitudes would be meaningless, if we already possessed all the knowledge (*or at least the essential knowledge*) about the psychology of the unconscious and about the structure of the neuroses that we can obtain from psychoanalytic work. At present we are still far from that goal and we ought not to cut ourselves off from the possibility of testing what we have already learnt and of extending our knowledge further. (Freud, 1912e, pp. 114-115, our italics).

All this is concerning the provisional nature of theoretical assumptions and the creation of the best conditions for their proof. Besides the danger that premature theoretical explanations of neuroses, psychoses, and psychosomatic syndromes can amount to fixed prejudices, there exists another danger, equally unfavorable to therapy and science; this is a technique that overlooks its hypothetical nucleus and thereby the necessity of continuous practical and scientific validation. Technical interpretations in the course of treatment, because of their latent hypothetical component, are just as provisional as are theories. Practice reflects the imperfection of theory. At best, it can have the same reliability as theory; otherwise practice would be better than theory.

We see in Freud's "Methodology" (Meissner, 1971) that the advice to postpone explanatory synthesis until the end of treatment cannot be taken literally. Even during his education the future psychoanalyst learns something else. Interim reports that present unsystematic clinical proof of theory are currently given in the technical seminars of psychoanalytic institutes. Supervision also has, as its aim, to try alternative strategies of interpretation according to the behavior of the patient. It is the changes in the technique of interpretation, whether they have been intuitively or rationally arrived at, that in the course of a treatment or in relation to various symptoms afford the possibility of giving the clinical theoretical proof that Freud demanded. One should strive to focus on a systematic approach analogous to the aims of brief psychoanalytically oriented therapy (see Malan, 1963). The awareness of the danger that Freud described, furthers clinical flexibility. Moreover, the

repetitions of the transference neuroses will also help to prevent random interpretation and promote the use of a flexible system that can adjust to changes in the patient.

Keeping in mind the previously discussed limiting factors, pertaining to interpretations containing a possible hypothetical nucleus, we now take up the question of which kind of theories can be proven clinically.

Empirical inquiry of this type has to confront the problem of falsification. When and why does a psychoanalyst give up one "strategy of interpretation" (Loewenstein, 1951), in favor of another? Are the underlying theoretical explanations already refuted in this case only, or in general? The behavioral and the social sciences have special problems of proving and refuting, which arise from their subject matter and which psychoanalysis confronts in an exemplary way: the combination of method and theory and the mediation by a subject have made it a paradigm for other disciplines (Kuhn, 1962). All this has made psychoanalysis the butt of criticism by theoreticians of science.

MacIntyre (1958, pp. 82-83) describes the difference between an experimentalist and a clinician as follows: the experimentalist would like to conduct experiments in which his hypotheses would be falsified and in which situations would arise that would show false hypothesis to be unserviceable. Since he is looking for flaws in his hypothesis, it constitutes a victory for him when he discovers a situation in which his hypothesis breaks down. In contrast to the experimentalist, the clinician's only interest is to promote healing, but it is not true that the clinician is only interested in matters that further the healing process. To the contrary, he is also very much occupied by the question of which factors stand in the way of healing. Thus the psychoanalyst looks for alternative hypotheses in a given case, even if these cannot be isolated in a way that would permit strict experimental disposition and proof, independent of the subject. MacIntyre then raises the question of what kind of refutation would be valid for psychoanalysts and what would move them basically to change theoretical conceptions. He answers along the lines of Glover (1947) that nothing would move psychoanalysts to change their conceptualizations. But a closer look at Glover's statements shows the reason for MacIntyre's error:

> The basic ideas of psychoanalytic theory could and should be employed as a discipline to survey all theoretical reconstructions of mental development and all etiological theories, which cannot be verified immediately by clinical psychoanalysis. ... It is often said, that Freud was ready to change his formulations, if this was necessary for empirical reasons. This is true for some parts of his clinical theory, but in my estimation not for his basic ideas. (Glover, 1947, p. 1)

It is illuminating that MacIntyre has left out a large part of the original. In the missing part Glover gives some examples of basic ideas: the mobility and quantity of energy of drives and memory traces. Glover is of the opinion that dynamic, economic and topographic, namely the metapsychological points of view, can be reduced to three basic ideas. These are the ideas that, according to Glover, cannot be immediately proven empirically by the clinical method, and unlike clinical theory, have not been changed. It is not true however that the basic ideas, the metapsychological points of view, have never experienced any changes (see Rapaport and Gill, 1959). Even if these ideas had proved to be rather resistant empirically one would, first of all, have to explain the reason for it.

It is a fact that the psychoanalytic method can only indirectly examine the metapsychological points of view empirically. These are in no way the basis of psychoanalytic practice or clinical theory, but are rather their "speculative superstructure" (Freud, 1925d, p. 32). Freud (1915b, p. 77) characterizes metapsychology throughout his whole work in this way, but the "witch" keeps on exercising a singular fascination on his whole thought. We believe we can attribute this to the fact that Freud never gave up the idea that the day would come when the psychological and psychopathological observations of psychoanalysis could be traced back to universal laws. Speculations about mental economy in particular show that Freud never completely abandoned "his audacious thought" of the 1895 "Project for a Scientific Psychology … of fusing the theory of the neuroses and normal psychology with the physiology of the brain" (Kris, 1954, p. 33).

Freud's expectations that one day all scientific theories, including the ones of psychoanalysis, could be reduced to micro-physical theories also can be seen in the fact that his formulation of specific economic metapsychological assumptions is couched in such physicalistic terms such as *energy, displacement, charge.* The farther metapsychological speculations move away from the plateau of observation of the psychoanalytic method, the less such observation will be able to substantiate or to refute the speculative superstructure. The distance between practice and theory can be measured by the terminology: the richer the physicalistic-neuro-physiological language of metapsychology becomes, the more difficult it is to determine its psychological nucleus. The metapsychological points of view can nevertheless be of practical help in orientation, depending on how explicit the rules of correspondence have been made.

It could be said in general that metapsychological assumptions have an empirical scientific significance only if they can be linked to observations by rules of correspondence (Carnap 1950). Such rules do not furnish a complete definition of the theoretical concepts

through the language of observation, but they give an empirical content that is good enough for applicability and examination. When one considers the dynamic, topographic, structural, genetic or economic assumptions of metapsychology, along the lines of Rapaport's summary (1960), it becomes clear that their proximity to observation varies widely. Their "survival potential" (Rapaport) depends on their nearness to the plateau of observation; without rules of correspondence they atrophy, even if they seem to be unchanged. Their unchanged state can be a sign that they are not at all basic, but on the contrary, have been discounted in practice or have not been current or proven practical from the beginning.

The clinical research that led to the Hampstead Index (Sandler 1962; Sandler et al. 1962) showed how important it is to establish rules of correspondence. The task of relating to the observational data of an individual case with the clinical theory of psychoanalysis, and possibly with its metapsychology, makes conceptual precision mandatory as a prerequisite for validation or falsification studies. The therapeutic flexibility of the psychoanalyst will not be narrowed by this; to the contrary, it will rather be widened because alternatives will be defined and systematized. But mainly it will become possible to determine more accurately, which observational data agree with a clinical hypothesis and which ones refute it. Though the testing of alternative hypotheses is the mark of the psychoanalytic interpretive process, it is not its aim to definitely to refute one or another clinical-theoretical explanation of a given case. The analyst, for technical reasons of treatment alone, has to keep himself open to the possibility that psychodynamic hypothesis considered as refuted in the present phase of the treatment could be revalidated later. Freud's *A Case of Paranoia Running Counter to the Psycho-Analytic Theory of the Disease* (1915f) points up casuistically some problems of falsification of theory in a single case, from which general refutations have to be derived.

The problems of falsification gave rise to an informative discussion between psychoanalysts and theoretical scientists (Hook 1959a), in which Waelder (1962) later took part with a critical review. Hook (1959b, p. 214) asked some psychoanalysts what kind of evidence they would deem valid for ascertaining that a child has no Oedipus complex. Hook's question derives from a position within the theory of science that Popper (1959, 1963a) introduced as "falsification theory." In his arguments with the logical positivism of the early Vienna Circle, Popper arrived at the conclusion that inductive logic does not provide a "criterion of demarcation" that would facilitate differentiation of the empirical, the metaphysical, the scientific and the unscientific systems. On the basis of detailed arguments that we cannot take up here (nor can we go into critical considerations of the falsification theory by Kuhn, 1962), Popper concludes that the "criterion of demarcation" is not the

verification, but the falsification of a system. Popper demands that the logical form of the system "shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience"* (1959, p. 41; italics Popper's).

Psychoanalysts can agree with this definition of empirical sciences, as shown by a representative quotation from Waelder's (1962) critical review: "if no set of observations is thinkable that would disprove a proposition, what we have is not a scientific theory but a prejudice or a paranoid system" (p. 632). In the light of this agreement in principle, it is rather surprising that psychoanalytic theory has been scientifically criticized from the point of view of the theory of falsification. This comes from demands for the creation of experiments of falsification. The theory of falsification grants scientific status only if *experimenta crucis* can be performed. Thus the criterion for falsification consists in the fact that only those theories are empirically valid that expose themselves to the risk of experimental refutation. Those would be theories that would "permit" only a genuine subclass of all possible experimental results, while they would "forbid" all the others. Though Popper has shaken the foundations of scientific theories of the logical positivists of the Vienna Circle with the theory of falsification, he has — although in critical distance from them — followed the same interests, namely, to enthrone the method of experimental natural science as the only valid one. "The 'explanatory theories' or the 'theoretical explanations' of empirical science, according to Popper, should be proven empirically independent from the experiences that they explain. The type of theory that satisfies this requirement is the statement of universal laws. Statements of universal law can be useful for the deduction of limited prognoses that can be proven by planning new experiences, independent from any former ones.

We are coming back to Hook's question and hope that by our remarks on the theory of falsification we can explain why the answers of the psychoanalysts could not satisfy his requirements for scientific theory. The given, fictitious, diagnostic description of a child without any signs of an oedipal experience or behavior possibly still contains a minimal percentage of the Oedipus complex. Waelder rightly pointed out that the scientifically and experimentally oriented falsification theory, neither recognizes the logical structure of the Oedipus complex as a concept of types[14] (see also chapter three) nor values the possibilities of clinical refutations of theories, because of its restrictively normative conception of science.

[14] Compare the explanations of Hempel, 1952

Besides absolute refutations there exist other ones, especially in the applied sciences which are so highly probable that for all practical purposes, one can call them refutations.

The clinical theory of psychoanalysis, particularly in its special part, contains descriptions of pathogenesis in autistic children or in pre-oedipal disturbed grownups, who "practically refuted the Oedipus complex." So, one could say that the Oedipus complex was already refuted by the psychoanalytic method before Hook formulated his question on the basis of the theory of falsification. In fact, in testing clinical alternatives of pathogenic connections, considerations develop for conceiving a scale along which the Oedipus complex dissolves itself into its components and can thus be conceptualized as having zero effectiveness, as in the case of a paranoia of jealousy, "which went back to a fixation in the pre-oedipal stage and had never reached the Oedipus situation at all" (Freud, 1933a, p. 130). It is evident that in a diagnostic and prognostic evaluation of the case, for example, in the clinical validation of theory, positive and negative signs are compared and weighed against each other. Therefore, Hook's question is highly relevant because it could lead to a thoroughly necessary and desirable increase in the precision of the theory by its demand to provide a negative definition. It is, at any rate, not easy to explain, because of the different levels of abstraction in psychoanalytic theory, which one of its regions can be proved valid by interpretive practice.

In conclusion, we shall give a summary of the different levels of psychoanalytic theory, in order to mark the regions that are most relevant in empirical testing of the psychoanalytic theory. We will use Waelder's schema (1962) because of its clarity.

The schema differentiates:

1. *Data of observation.* These are the data the psychoanalyst receives from his patient and which generally are not available to others. These data form the level of observation. They become subject to interpretation relative to their connection with each other and their relation to other modes of behavior or to conscious or unconscious contents. Here we are at the plane of *individual clinical interpretation* (Freud's individual "historic" interpretation, 1916-1917, p. 270).

2. *Generalizations.* From the individual data and their interpretations derive generalizations, which lead to certain assertions in regard to patients, formation of symptoms and age groupings. This is the level of *"clinical generalization"* (Freud's typical symptoms).

3. *Theoretical concepts.* The clinical interpretation and their generalizations permit the formulation of theoretical concepts, which can already be contained in the interpretations or which could lead to interpretations of, for instance, such concepts as

repression, defense, return of the repressed, regression, etc. Here we have the *clinical theory* of psychoanalysis before us.

4. Metapsychological concepts. Beyond this clinical theory, without being able to draw a sharp line, are more abstract concepts like cathexis, psychic energy, Eros, death wish, constituting psychoanalytic *metapsychology.* Especially in metapsychology, or rather behind it, is Freud's personal philosophy (see Wisdom, 1971).

The schema makes a hierarchy of psychoanalytical theories visible; their respective values for scientific theory vary in empirical content. Interpretations relate mainly to clinical theory. They contain explanations, which permit prognoses, as we will point out later. How far the technological aspect of this theoretical area and its theoretical and scientific position apply to more abstract elements of psychoanalytic theory will be discussed in the following chapters.

In conclusion we want to say that the phenomena that were discovered and interpreted in the therapeutic dialogue have been objectified in a verifiable description by Freud and put into a causal, historical, and genetic connection.

General and Historical Interpretations

Whereas in our last arguments we stressed the explanatory character of psychoanalytic theories, the prominent German sociologist of the "critical school of Frankfurt", Habermas, presents quite a different view of Freud's scientific achievements.

What Habermas sets as his task in his book *Knowledge and Human Interests* (1971a) is concisely summarized by Nichols (1972) in a short review of that book[15]:

> first, to provide a critique of science on the basis of self-reflection — a critique he develops by tracing the various alternatives to positivism provided by idealism, historicism, and phenomenology; and second, to lay some of the foundations for an epistemology which satisfactorily connects knowledge (as theory) with human interest". (p. 18)

One of the sciences Habermas is dealing with in great detail is psychoanalysis, as a representative example of a social science that has not yet found its proper philosophical underpinning. Right at the beginning of his chapter "Self-Reflection as Science," Habermas

[15] During the preparation of this English translation of our paper, we realized that we are in considerable agreement with most of Nichols' critical remarks.

characterizes the traditional self-understanding of psychoanalysis as a scientistic self-misunderstanding (p. 246).[16]

This wrong and misleading understanding of the metascientific status of psychoanalysis would especially concern the evaluation of psychoanalytic theory rather than its practice, for example, it would regard in particular the research efforts to validate the theory. The origin of this misunderstanding is reconstructed by Habermas in the following way: the basic categories of psychoanalysis were "first derived from experiences of the analytical situation and the interpretation of dreams" (p. 252). The assumptions regarding the functional relations of the psychic apparatus and the origin of symptoms, are "not only *discovered* under determinate conditions of a specifically safeguarded communication," but " they cannot be displayed independently from these" (p. 307). From this, it follows that "psychoanalytic theory formation is embedded in the context of self-reflection" (p. 252). The connection of the structural model, which originally was derived from the communications between doctor and patient with the model of energy distribution, would then constitute the decisive and misleading step: that Freud "did not comprehend meta-psychology as the only thing it can be in the system of reference of self-reflection: a general interpretation of self-formative processes" (p. 252).[17]

According to Habermas, "it would be reasonable to reserve the name metapsychology for the fundamental assumptions about the pathological connection between ordinary language and interaction" (p. 254). A metapsychology thus conceived would not be an empirical theory but a methodological discipline which, as metahermeneutics, would have to explicate "the conditions of the possibility of psychoanalytic knowledge." Whether Habermas has any use at all for the classical metapsychological points of view, remains obscure.

We have already dealt with the role of metapsychology in the process of psychoanalytic insight (*Erkenntnis*) and with the question of clinical verification of metapsychological viewpoints. The notion that, for many metapsychological viewpoints, it is impossible to set up rules of correspondence implies that vast areas of metapsychology belong to the

---

[16] *Scientism* means, according to von Hayek, "the slavish imitation of the method and language of science" (Popper, 1957, p. 105).

[17] A self-formative process signifies the cultural correlate to the biological process of development from childhood to adulthood. It comprises the process of education, of training and of growing self-awareness that can be summarized also in a concept of growing psychosocial identity. The category of *Bildung* (self-formation) is central to the philosophical idea of enlightenment and played a great role in Germany's cultural development during the last centuries.

speculative superstructure of psychoanalysis, which can hardly be verified by empirical-clinical methods.[18]

In any case, between the various chapters in the building of psychoanalytical theory, there exist as we have seen, a great number of indirect connections, so that from the observations that can be made on the "ground floor," accessible to all, conclusion can be drawn for what is supposed to occur on higher or lower floors. Thus on the one hand, metapsychology plays a much smaller role than Habermas ascribes to it and also it can be scientifically verified to a limited extent, though it belongs mostly to the speculative superstructure. In this state of affairs metapsychology does not lend itself at all to being used as a metahermeneutic approach.

The methodological discipline proposed by Habermas is not affected by this criticism of the misunderstanding which, in our opinion, crept into Habermas' reception of the concept of metapsychology. We believe that the methodological position of general interpretations[19] would gain little if one gave it a superstructure (*such as* meta-hermeneutics) that is in some way related to metapsychology. To this superstructure would adhere, in our opinion, all those obscurities that characterize the relationship between clinical theory and metapsychology. The methodological significance of general interpretations is sufficiently independent. With these, Habermas has described strategies of research that are simultaneously self-reflective.

On the level of self-reflection, as distinguished from the logic of the natural sciences and humanities, something like a methodology separated from its content is not possible because the structure of the context of knowledge is one and the same as the object under examination. The general interpretations however are also distinguished by Habermas from meta-hermeneutical statements:

> For, like theories in the empirical sciences ... general interpretations are directly accessible to empirical corroboration. In contrast, basic meta-hermeneutical assumptions about communicative action, language deformation, and behavioural pathology derive from subsequent reflection on the conditions of possible psychoanalytic knowledge. They can be confirmed or rejected only indirectly, with

---

[18] Cf. Freud (1914c): "But I am of opinion that that is just the difference between a speculative theory and a science erected on empirical interpretation. The latter will not envy speculation its privilege of having a smooth, logically unassailable foundation, but will gladly content itself with nebulous, scarcely imaginable basic concepts, which it hopes to apprehend more clearly in the course of its development, or which it is even prepared to replace by others. For these ideas are not the foundations of science, upon which everything rests: that foundation is observation alone. They are not the bottom but the top of the whole structure, and they can be replaced and discarded without damaging it" (p. 77).

[19] As we will specifically show later, the concept of *general interpretations* comes from Popper, who introduced it for historical explanations.

> regard to the outcome of, so to speak, an entire category of process of inquiry. (Habermas, 1971a, p. 255).

Habermas thus characterizes those laws, the meta-scientific status that we questioned in the beginning, as "general interpretations." It would be wrong to understand these to be psychoanalytic interpretations (*Deutungen*) in the technical sense in which the word is used in treatment. On the contrary, they can be conceived of as patterns of early childhood development that can be applied as interpretive schemas for individual life histories. They consist of "assumptions about interaction patterns of the child and his primary reference persons, about corresponding conflicts and forms of conflict mastery, and about the personality structures that result at the end of the process of early childhood socialization, with their potential for subsequent life history. These personality structures even make possible conditional predictions" (p. 258).

In this framework, general interpretations are developed that are the result of various and repeated clinical experiences. They have been derived according to the elastic procedure of hermeneutic anticipations (p. 259). The basic outline of the whole proposition developed here by Habermas, which alone makes possible the experiences outlined so far, is the consideration of the life history[20] as a self-formative process (*Bildungsprozess*) which, in the case of a patient, is characterized as disturbance. In line with this, the object of psychoanalytic treatment is "the interrupted self-formative process" which, by the experience of self-reflection, is brought to its end.

Regarding general interpretations we now must keep in mind that, contrary to interpretations in the technical sense used in treatment as soon as an interpretation claims the status of "general," it is removed from the hermeneutic method of continuous correction of preliminary understanding by the text. Therefore, it is true of general interpretations that they are fixed, as distinguished from the hermeneutic anticipation of the philologist. By this Habermas means that general interpretations have a theoretical anchorage, insofar as they imply at least generalizing statements which must be demonstrable in the individual case and which therefore are exempt from the permanent change through the hermeneutic circle. Therefore, general interpretations must be verified by derived prognoses. If further, one takes into account that the reconstructive postdictions (statements after the fact) — which, with the model of the general interpretation, can as narrative forms be derived for the individual case — also have for Habermas the character of hypotheses, which are fallible, then we have

---

[20] Habermas places the reconstruction of the life history entirely in the center of his discussions. In fact, however, the working-through of the transference neurosis in the here and now plays a much greater role therapeutically than the reconstruction of the past.

found thus far in these discussions clear indications that the aforementioned sentence of Popper (*"it must be possible for an empirical scientific system to be refuted by experience"*) is also valid for psychoanalysis (1959, p. 41; see also p. 73).

So far, Habermas' clarification of the metascientific position of psychoanalysis seems to offer the following advantages: uncovering of the scientistic misunderstanding leads to the question of how far an imitation in psychoanalysis of the methods of the natural sciences which are not appropriate for their object has brought empirical research to an impasse. Insofar as the verdict of scienistic self-misunderstanding concerns many a metapsychological viewpoint — the model of energy distribution,[21] for instance — Habermas' critique corresponds well with similar conceptions held by quite a few psychoanalysts (i.e., Rosenblatt and Thickstun, 1970; Holt, 1962, 1965). The problems involved in the concept of "psychic energy" are, of course, discussed by many more authors than we can refer to in the context of this chapter (see Shope, 1971).

From Habermas' argumentation, it follows that it would be misleading to look for the great X of psychic energy, which, as Freud (1920g) said, enters as an unknown into all our equations, by way of psychology. The conclusion that psychoanalysis belongs to the humanities and not to the natural sciences could contribute to the stimulation of empirical research appropriate to the object of psychotherapy. Following Habermas, this research should refer to the general interpretations covering the realm of the clinical theory of psychoanalysis.

The characterization of psychoanalytic clinical laws as "general interpretations," as systematized historical knowledge, facilitates the understanding of the specific situation of psychoanalysis. Moreover, if one sees as central that the general interpretations must be tested against derived prognoses, then a clear dividing line to the philological-hermeneutic procedure has been drawn and empirical research has been secured to the extent of establishing expected behavioral changes — hopefully, in accordance with the theory. It is tempting to turn with this understanding to the verification of psychoanalytic theses. Habermas would then, with a difference in terminology aside, come close to Popper. To be sure, Habermas moves again in another direction when he deduces the degree of validity only from the patient's self-reflection.

---

[21] We would have before us a law of natural science, if one succeeded in verifying empirically the psychoanalytic model of energy distribution, in showing measurable conversion (*Wandlung*) of energy, and in deducing prognoses with the knowledge of specific border conditions. There were fundamental reasons for the fact that the efforts undertaken by Bernfeld and Feitelberg in this direction had to fail. "The energy-distribution model only creates the semblance that psychoanalytic statements are about measurable transformations of energy" (Habermas, 1971a, p. 253).

In contrast to the instrumentalistic viewpoint of the purposive-rational organization of means or of adaptive behavior, the elementary events of a psychoanalytic dialogue are processes in a drama: the functional relationship of disturbed self-formative processes and neurotic symptoms must be understood in the light of a dramatic model.

> That is, the elementary processes appear as parts of a structure of interactions through which a 'meaning' is realized. We cannot equate this meaning with ends that are realized through means, on the model of the craftsman. What is at issue is not a category of meaning which is taken from the behavioral system of instrumental action, such as the maintenance of the state of a system under changing external conditions. It is rather a question of a meaning that, even if it is not intended as such, takes form in the course of communicative action and articulates itself reflectively as the experience of life history. This is the way in which 'meaning' discloses itself in the course of a drama. (Habermas, 1971a, p. 260)

In the drama of the self-formative process, the subject is at once both actor and critic. The goal of the process is the capacity of the subject to relate his own history and comprehend the inhibitions that blocked the path of self-reflection.

> For, the final state of a self-formative process is attained only if the subject remembers its identifications and alienations, the forced upon him objectivities and the reflections it arrived at, as the path upon which it constituted itself. (p. 260)

While on the one hand Habermas restores the relation to Freud's empirical-scientific thinking through the concept of general interpretations borrowed from Popper and on the other hand certain romantic elements, which are far removed from Freud's sober notion of education, seem to enter into the goal-conception of the self-formative process. Albert's (1971, p. 55) plea for a critical rationalism could include Freud's intention insofar as he justly indicates a certain linking of hermeneutics and dialectics as "German ideology," and opposes this to Freud's natural-scientific maxims. In the following discussion, we shall take a look at the consequences that result from Habermas' argument for the verification of general interpretations. The minuteness with which we refer to Habermas' philosophical exegesis of psychoanalysis is justified by the radical consequences of the announced verification of the "general interpretations" which, according to Habermas, result from it.

The metapsychological founded, systematically generalized history of an infantile development enables the psychoanalyst to make "interpretive suggestions for a story the patient cannot tell" (Habermas, 1971a, p. 260). Because of this, the interpretation of a particular case proves itself "only by the successful continuation of an interrupted self-

formative process" (p. 260). On the basis of this, Habermas can conclude that "analytic insights" possess validity for the analyst only after they have been accepted as knowledge by the analysand himself. For, the empirical accuracy of general interpretations depends not on controlled observation and the subsequent communication among investigators, but rather on the accomplishment of self-reflection and the subsequent communication between the investigator and his "object" (p. 261). By this, the general interpretations are marked off from statements regarding an object domain which are made in the context of general theories. While the latter remain exterior to the object domain, the validity of the former depends on the fact that "statements about the object domain [are] applied by the 'objects,' that is, the persons concerned, to themselves" (p. 261). The distinction between the empirical validity of general interpretations and that of general theories is characterized by Habermas as follows: in the behavioral system of instrumental action, the application of assumptions to the reality remains the concern of the inquiring subject. In the behavioral system of self-reflection, the application of statements is possible only via the self-application of the research object that participates in the process of insight. In short: general interpretations have validity only to the degree "that those who are made the object of individual interpretations know and recognize themselves in these interpretations" (p. 261).

Only now it becomes evident how clearly Habermas tries to draw the dividing line between general theories, which can be falsified — and general interpretations — which must be tested by the reflexivity attained by the patient. This effort to draw a dividing line cannot however be maintained by Habermas himself, nor are psychoanalytic practice and research in agreement with him. The contradictions in which Habermas becomes entangled can be traced back to the fact that the general interpretations on the one hand move too far from such evidence as is required for general theories, and on the other hand must prove their value in the distribution of clinical success and failure. These, however, following Habermas, evade inter-subjective evaluation:

> The criterion in virtue of which false constructions fail does not coincide with either controlled observation or communicative experience. The interpretation of a case is corroborated only by the successful *continuation of a self-formative process*, and not in any unmistakable way by what the patient says or how he behaves. Here, success and failure cannot be inter-subjectively established, as is possible in the framework of instrumental action or that of communicative action, each in its way. (p. 266; our italics).

We cannot understand how Habermas relates the distribution of clinical success and failure to the patient´s experience of reflection alone. Introspection and reflection are, precisely as psychoanalysis has shown, subject to serious self-deception. Whether the force of an unconscious motive is broken reveals itself objectively exactly there where it can be ascertained inter-subjectively: in symptoms and changes in behavior. Besides, free association at first leads away from goal-oriented introspective reflection and expands it when it overcomes resistances. There is probably no analyst who bases the way in which he conducts his treatment only on the reflections of the patient, on his self-formative processes, or takes it as the only proof of interpretive hypotheses. The experience of the patient, which he accumulates in the course of a psychoanalytic treatment and as a result of which he arrives at a new interpretation of his life situation, is one aspect in which the success of the treatment manifests itself to the patient. However, there is an evaluation of the success of the treatment in the sense of objective proof of a successful psychic change, which can be fairly well operationalized and subjected to scientifically controlled testing (Fonagy, 2002a, b).

Habermas' discussion introduces the leading utopian idea that an enlightened subject disposes of the history of his "becoming himself"; this is, in our opinion, overestimation of self-reflection. It is easily overlooked that the emancipatory character of psychoanalysis is documented not only by the gained or regained insight into oneself, but also by changes in the capacity for human relationships. Many patients are unable, at the end of psychoanalytic treatment, to give an account of which changes and which self-formative processes have taken place in them: they are aware of a change in the immediacy of their experience and actions, without being able to reflect philosophically on it in an adequate way (see Leuzinger-Bohleber et al., 2003).

The maxim "Where id is there shall be ego" cannot be understood to mean that the dynamic unconscious, the repressed, and that which unfolds its power behind the back of the subject, lies permanently at the conscious disposal of the subject after the analytic treatment. We find Gadamer's (1971a) criticism concerning this matter to apply here: "The idea of the elimination of a natural determination in rational, conscious motivation, is in my opinion a dogmatic exaggeration inappropriate to the *´condition humaine'* " (p. 312). Habermas fails to appreciate the necessity that psychoanalytically, the developmental process of the individual consists basically of psychic structures and functions that safeguard the ability to work and love. By this we do not mean a conforming adaptation to an ahistoric reality principle. This principle in our view has a regulatory function and is prone to historical change, which finds its respective sociocultural content in historical change.

Therefore, in the practice of psychoanalysis, we aim at a reasonable equilibrium between those poles that can be characterized as the pleasure principle and the reality principle. Ideally, the blind autoplastic subjection to the contents of the reality principle that are passed on by sociocultural tradition and its internalization in ego and superego functions should be replaced by reasonable alloplastic solutions. Here, a concept of the theory of therapeutic technique assumes significance, namely, "acting out." Acting out signifies such alloplastic, outward-directed efforts of change as are unconsciously drive directed. Insofar as the demands that only the environment should change are not accompanied by the willingness and ability to change oneself, one can usually assert psychoanalytically that in these one-sided alloplastic actions we often are dealing with acting out. That such acting out can often have vast social and historical consequences is one of the tragic paradoxes of the history of mankind. One could almost say that often petrified situations can only be changed when through certain misunderstandings of reality forces of acting out are liberated, which do not seem to know any limits. The tragic fact is that the changes then regularly take place through aggressive-destructive forces, which soon lead to similar disturbing counter-movements (see Waelder, 1967). Thus, important insights into collective processes can be gained from the psychoanalytic method, since one can clearly discern in the acting out of the individual the disharmony in society: it is fought there instead of beginning with the individual's own self-formative processes.

The consequence of Habermas' effort to present psychoanalysis as the only tangible example of a science incorporating methodical self-reflection that furthermore should be a model for social reflection would be that the technology of clinical interpretative work thus would have to be rejected. Its methodological particularity however — that it can be an explanatory science as well as an emancipating reflection — must be in our opinion, the central issue determining its epistemological status. The multiplicity of psychotherapeutic-intervention techniques that can be derived from psychoanalytic theory and practice indicates an instrumental aspect, of which no clinician is ashamed. "The very fact that, since Freud's time, the psychoanalytic method has been used in treating both children and psychotics — to neither of whom Habermas could really grant the capacity for self-reflection — would seem to substantiate this in an important way" (Nichols, 1972, p. 267).[22]

Habermas' assertion that success and failure cannot be ascertained inter-subjectively in treatment and that justifications based on the disappearance of symptoms are not legitimate,

---

[22] As for the rest, the devaluating qualification that presents instrumentalism as the only knowledge of interest to the "real" sciences is pointed out by Albert (1971). According to him, such a reproach has, in the history of knowledge, always served the screening of specific articles of belief against criticism made possible by the natural sciences (p. 110).

fails when confronted with psychotherapeutic practice. Also Freud's emphasis that only the process of the analysis can decide the usefulness or uselessness of a construction, does not exclude the confirmative force of changes in symptoms and behavior, but comprises an expression of the self-formative process more than just the self-reflection of the patient.

Habermas himself says elsewhere (1963, p. 482) that one of the suppositions for the testing of theories is that repetitive systems can be made accessible to controlled observation. However, just such repetitive systems are present, for instance, in stereotypes of behavior which, through the repetition compulsion, manifest themselves in the various forms and contents of transference neuroses. Repetition and change both manifest in behavior are observed; these observations are reflected in the practice and theory of psychoanalysis. He admits that "single hypotheses can be taken out of the metapsychological context of interpretation and be tested independently" (Habermas, 1967, p. 189).

> Herewith is needed a transposition into the theoretical frame of strict empirical sciences. ... In any case, Freud's theory contains assumptions which can be interpreted as lawful hypotheses in a strict sense; from this it follows that it also comprehends causal relations. (p. 190)

What Habermas seems to admit here is the content of the general and specific theory of neuroses; its confirmation by the experience of the reflection of the patient alone however seems to us insufficient. By this there is a task assigned to self-reflection that patients, again according to clinical experience, cannot accomplish.

We agree with Rapaport (1960) that proving the validity of the psychoanalytic theory is a task of the scientific community, which has to agree on the practical procedure of empirical science. Contrary to the restrictive limitation of the confirmation of general interpretations, psychoanalytic research and practice cannot be satisfied with a concept of the self-formative process that is as philosophically vague as it is rich in content and from which confirmation of the theory should result. In any case, the logic of the explanation through general interpretations points toward the specific way in which the confirmation of psychoanalytic statements can alone be obtained: this becomes clear in the linking up of hermeneutic understanding with causal explanation: "Understanding itself gains explanatory power" (Habermas, 1971a, p. 328).[23]

---

[23] The reconciliation of the methodological antithesis between understanding (*Verstehen*) and explaining (*Erklären*) can be found *in statu nascendi* in Max Weber, in an "understanding explanation" or an "explaining understanding." According to Albert (1971), Weber tried, with his concept of theoretical sociology as an understanding science, which aims at an understanding explanation of the phenomena of cultural reality, to overcome the long-standing antithesis of explaining and understanding and with it the position of extreme historicity as represented by Dilthey (p. 137).

Transgressing the methodological antithesis of understanding and explaining by an <understanding explanation> or an < explanatory understanding> already can be found in the work of Max Weber. Following Albert (1971) Weber had tried by his conception of theoretical sociology as a understanding science that is directed to a understanding explanation of cultural realities, to overcome this antithesis (p.137). In our view the relevance of psychoanalysis in the history of science resides in this overcoming of the antithesis of understanding and explaining (Wright, 1994).

In regard to symptoms, constructions take the form of explanatory hypotheses with the aim of analyzing modes of behavior in causal terms. The dissolution of a "causal coherence" through interpretive effort illustrates the efficacy of psychoanalytic therapy. The constructions are to be applied to the single case; they thus become theoretical statements from which singular prognoses can be derived. Generally speaking, these prognoses identify the conditions causally responsible for the neurotic state and claim that the therapeutic process must dissolve these conditions in order to induce change. The disappearance of the efficacy of the supposed internal conditions — e.g., pathogenic unconscious fantasies — demonstrates itself in changes of symptoms and behavior.

> In its logical form however explanatory understanding differs in one decisive way from explanation rigorously formulated in terms of the empirical sciences. Both of them have recourse to causal statements that can be derived from universal propositions by means of supplementary conditions: that is, from derivative interpretations (conditional variants) or law-like hypotheses. Now the content of theoretical propositions remains unaffected by operational application to reality. In this case we can base explanations on *context-free laws.* However, in the case of hermeneutic application, theoretical propositions are translated into the narrative presentation of an individual history in such a way that a causal statement does not come into being without this context. General interpretations can abstractly assert their claim to universal validity because their derivatives are additionally determined by context. Narrative explanations differ from strictly deductive ones in that the events or states of which they assert a causal relation are further defined by their application. Therefore general interpretations do not make possible context-free explanations (Habermas 1971a, p. 272, our italics).

The contextual dependency of psychoanalytic explanations (so called narrative explanations) argued by Habermas relativizes causal statements and renders impossible strict deductive derivations from laws. Among the few analysts drawing practical and scientific conclusions due to the probabilistic nature of all psychodynamic statements beyond the mere

phenomenological descriptions Benjamin Rubinstein (1980) deserves special mention. His simple but disquieting message is that psychodynamic statements contain hypotheses in need of confirmation; they may even be wrong in single instances. Recently Fonagy (2003) drew the same conclusion: “Facing the logical weaknesses of our position we tend to ascribe the clinical theories the status of laws“ (p. 19) and tend to deduct behavior and experience from them. Actually clinical typologies allow only probabilistic statements. In a particular instance results can deviate from the probability statement which requires single case studies despite the well known problems of generalizations from them. The formalized empirical evaluation of treatment reports moves beyond the heuristic, hypothesis generating function of clinical descriptions and is able to secure objectively identified correlations by statistics (Schaumburg et al. 1974). Obviously the transferability of findings is limited to similar cases. Assuming that all analysts are searching for explanations, in order to understand their patients, then the dividing line is not between hermeneutic science and empirical social science, but in the attitude towards causality. In clinical practice only probability statements, only inductive statistical explanations are possible, but not deductive-nomological conclusions (von Mises 1939/ 1951; Ruben 1993). Recognizing that (unconscious) motives function as causes, then enlightenment, in the sense of „causality of destiny“, that Habermas took from Hegel, is rightfully a core issue of psychoanalysis.

From this follows in our opinion, in regard to the methodology of research, that it is of the utmost importance to examine the individual case in its concreteness. Both the self-formative process, as experienced by the subject of treatment, and his objectively recorded changes in conduct and behavior must and can be examined on verbal and preverbal levels and thus become the criteria for testing the clinical hypotheses.

In order to clarify still further the concept of general interpretation, which plays such a central role in Habermas’ conceptualization, we shall now look for its original frame of reference. Popper (1944) introduced the term to distinguish between scientific and historical theories to make a qualitative difference.

> Now it is important to see that many “historical theories” (they might perhaps be better described as “quasi-theories”) are in their character vastly different from scientific theories. For in history (including the historical natural sciences, such as historical geology) the facts at our disposal are often severely limited and cannot be repeated or implemented at our will. And they have been collected in accordance with a preconceived point of view; the so-called “sources” of history only record such facts as appeared sufficiently interesting to record, so that the sources will, as a rule, contain

> only facts that fit in with a preconceived theory. Since no further facts are available it will not, as a rule, be possible to test that or any other subsequent theory. Such historical theories, which cannot be tested, can then rightly be charged with being *circular* in the sense in which this charge has been unjustly brought against scientific theories. I shall call such historical theories in contradistinction to scientific theories, "general interpretations. (Popper, 1944, pp. 265-266)

The verifiability of these historical general interpretations is restricted insofar as there are no *experimenta crucis* in historical research and in psychoanalysis as there are in the natural sciences. Popper gives an elaborate argument for this, which leads him to give up the naive view "that any definite set of historical recordings can ever be interpreted in one way only" (p. 266). Hereby it becomes clear how closely Popper's falsifications theory is connected with the axiomatic sciences. He then introduces a number of relative proofs for historical interpretations, which suffice to determine probable and relative validity.

(1) There are false interpretations that do not agree with the acknowledged recordings.

(2) There are interpretations that need a number of more or less plausible auxiliary hypotheses to avoid falsification by the data.

(3) There are interpretations that do not succeed in connecting a series of facts which are connected by another interpretation and are explained to that extent (p. 266).

(4) Accordingly, considerable progress would also be possible in the area of historical interpretations. Besides, all sorts of intermediate stations between more or less general points of view and specific or singular historical hypotheses would be possible which, in the explanation of historical events, play the role of hypothetic initial conditions and not the role of general laws (see Klauber, 1968).

It is obvious that the considerable qualitative distinction Popper makes between scientific theories and general interpretations is no longer present in Habermas, to whom general interpretations claim the same degree of validity as general propositions in the empirical sciences. Their decisive difference lies in the logical procedure of validating research. In order to become more acquainted with these differences, we now consider the problem of which relations exist between the general model of scientific explanation, general interpretations and single forms of explanation as they occur in psychoanalytic work and research.

Description, Explanation, and Prognosis in Psychoanalysis

Allport (1937) characterizes scientific activity as the effort "to understand, predict, and control." Of this triad, the role of understanding is likely to be underestimated; it bears too close a relationship to philosophical speculation, whereby one easily overlooks the fact that understanding as a hermeneutic principle is in every scientific activity the condition of further progress. In the preceding pages we have already dealt at length with the role that understanding has in the scientific process. The procedures of prediction and control as represented in Allport's viewpoint presuppose explanations. In its daily decisions, clinical practice deals with this immanent coherence as a matter of course. For our discussion however it seems useful to clarify here once again the principle of this coherence, before we continue with a discussion oriented toward psychoanalysis. From a logical point of view, scientific predictions have the same structure as explanations. The event to be expected is deduced logically from given laws and auxiliary conditions, whereas explanations are a sort of post-hoc reconstruction of how an event has come about. This deduction of the prediction goes back to Popper's (1959) description of the logical structure of causal explanations. Hempel and Oppenheim (1953) have systematized the relationship between prediction and explanation in the model of scientific explanation named after them (the HO-model of scientific explanation). In order to facilitate the discussion we will repeat the relationships.

In an explanation, an *explanandum* — that is, a specific fact that has occurred — is presented. In order to explain it, one has to look for (at least) one law and the accompanying initial conditions. In a prognosis however, the explanandum is not given; we know only the laws and the initial conditions. The following outline clarifies this difference in graphic form:

Explanation		*Prognosis*
sought	law	given
sought	initial conditions	given
given	explanandum	sought

In the explanation as well as in the prognosis, an explanandum is deduced from (at least) one law and the initial conditions belonging to it. The only difference is that in each case different elements are sought and given. On the basis of our discussion in the section on "General and Historical Interpretations," it is clear that the HO-outline implies a type of explanation, which in psychoanalysis is only applicable by corresponding extension of the definition. Before we occupy ourselves with other forms of explanation, which according to

Stegmüller (1969) can likewise come under the concept of scientific explanation, we must come to grips with a contrary position.

It is often said from various quarters that a great deal of Freud's achievements lie in his brilliant description of many aspects of human behavior. The most prominent representative of this position must be Wittgenstein, who, according to Moore (1955), emphasized:

> There are so many cases [in Freud's writings] in which one can ask oneself how far what he says is a hypothesis, and to what extent [it is] only a good manner of presenting the facts. (p. 316).

MacIntyre (1958) arrives, in his effort to explain the concept of the unconscious, at a similar conclusion in this matter: "For Freud's achievement lies not in his explanations of abnormal behaviour but in his redescription of such behavior" (p. 61). When one tries to fathom where the basis for such judgements lies, as Sherwood (1969)[24] has done, then one finds that Wittgenstein refers to *The Psychopathology of Everyday Life,* and that MacIntyre predominantly considers *The Interpretations of Dreams*. Both works indeed contain anecdotic material given to illustrate ways in which the psychic apparatus functions. Causal remarks, taken out of the clinical context, appear thereafter only as ways of presenting the facts and easily lose their explanatory character. If however the clinical context is restored, then what Sherwood says is true:

> It is of course true that Freud described certain acts of the patient in a new way. But the important thing is that he tried thereby to explain them. ... To give a new description in given contexts can indeed come close to an explanation. The distinction between these two procedures is not always sharp and in each case it depends on the context, the situation in which it takes place. (Sherwood, 1969, p. 187)

Even though MacIntyre acknowledges elsewhere (1958, p. 79) that a clarifying description can indeed count as a way of explaining, he feels that he has to again deny the title of explanation to Freud's efforts to explain the significance of dreams, which have to do more with a deciphering than with explanation (p. 112). The discrepancy that here clearly comes to the fore concerns the scope, the concept of explanation. Different types of explanation are certainly at the base of Freud's clinical presentations.

Sherwood points to the fact that Freud's explanations in the case histories of patients — he illustrates extensively with the example of the Rat Man – always concern, first of all, an individual patient, a specific case history (see chapter three). The object of research is not a class of certain psychiatric symptoms and not a class of people who have a certain illness, but

---

[24] See also the discussion of Sherwood's book by Eagle (1973), Rubinstein (1973), and Sherwood (1973).

a single person. As the historian, Freud is interested in the particular outcome of events in order to perceive the typical. Accordingly, Freud uses generalizations about compulsion neurotics as a class. In the same way, there is a general theory of human behavior beyond the explanation of particular life histories (see Waelder, 1962).

A presupposition for generalization is that the explanations have been tested in a particular case. The other condition is self-evident: that the explanations tested in the particular case are present in a group of cases, whereby they become typical. The typical coherences are always only part of a case history; the particular explanations are woven into the whole. This context, which represents the comprehensive integrating moment, is characterized as "psychoanalytic narrative." Within this narrative various types of explanations can be isolated, which occur in different distributions. Hereby the narrative is to be regarded not simply as the sum (total) of these various explanations, but as the integrating framework: "In short, giving an account of the resolution of a single symptom would in fact amount to the task of relating an entire case history" (Freud, 1896c, p. 197).

According to Danto (1965), representations that present events as elements in a history are called "narrative statements." Since psychoanalytic explanations lie within the totality of a life history, the denotation "psychoanalytic narrative" — which as far as we know was first used by Farrell (1961) in philosophical discussion — underlines the historical character of psychoanalytic explanations, which soon led Freud to remark that it was not his fault if his case histories read like short stories (1895d, p. 160).

The notion of scientific explanation

In the German-Austrian philosopher Stegmüller's (1969) fundamental elaborations on the concept of scientific explanation — still the most complete overview on the status of analytic philosophy in the German language — he first of all singles out scientific explanation from a multiplicity of everyday usages of the word. The explanation of the meaning of a word, which can also be called "definition," the explanation as text interpretation or as instruction on how to act, as detailed description and moral justification — these numerous meanings of the concept of explanation show hardly anything in common and Stegmüller calls them at best a concept family in Wittgenstein's sense. For the analytic philosophy of science only the explanation of a fact has the rank of scientific explanation.

In the psychoanalytic models of explanation, as Sherwood (1969) shows, all those forms and explanations that are known from everyday language occur. It is a question of discovering the source of a feeling of "explaining" as if it were the origin of something

strange. Herewith nothing is yet explained in the sense of the HO-explanation; only a more precise knowledge of the facts is achieved. The explanation of the genesis (origin) of a symptom already poses difficult problems of demarcation. If the observed transference behavior is reduced to an infantile attitude to the mother, then not only are facts that appear as disparate brought together, but also by way of trial, genetic explanations are accepted that must prove themselves as retrodiction. The multitude of phenomena and processes in the psychoanalytic situation requires different explanatory operations, which should not be, *a priori,* designated as scientific or unscientific. Sherwood concludes his illustration of the various types of explanation with examples from the case history of the Rat Man: "A psychoanalyst is called upon to answer a wider range of questions on human behavior, and his explanations can therefore be of very different sorts" (Sherwood, 1969, p. 202).

Indeed, the demarcation of different types of explanation from explanation in the strict sense of the HO-model, as is partly reflected in Sherwood, does not take into regard the fact that, according to Stegmüller, "the concept of scientific explanations was introduced in such a way that it could claim for itself general applicability in all *empirical* sciences (p. 336). To be sure, the form of the construction of the concept of explanation decides whether it is applicable: a narrow conception corresponds to the HO-model as we have briefly outlined it in the passages above (under an explanatory argument should be understood a deductive conclusion, among the premises of which is at least one deterministic or statistic law hypothesis); if however, the concept is taken in a larger sense, that of Stegmüller, then not only the search for grounds in reality or causes, but also quite generally the search for a basis in reasons can enter into the search for an explanation.

This enlargement of the concept of scientific explanation draws particular historical and consequently, also some psychoanalytic explanations into its scope. The language of the historian, as well as of the psychoanalyst who reports on his case, is full of expressions that indicate an effort to explain. Logical or inductive arguments of many theses, instead of causal arguments, are often given. The selective description of the historian becomes an initial explanation because the description is governed by hypotheses. To be sure, in historical explanations regularities that have a statistic or trivial character are often drawn; the explanatory argument is therefore often not mentioned. Stegmüller lists other qualities of historical initial explanations, which cause the historical scientist not to interpret his statements as explanations in the sense of the HO-model. As a superior point of view he introduces thereby, following Hempel, the incompleteness of such explanations. Incomplete

explanations, which are also called “explanation outlines,” can be reduced to the following four roots:

(a) The explanation has dispositional character (see below).

(b) The explanation contains self-evident generalizations from the commonplace, which are not specifically mentioned.

(c) The explanation is incomplete because further derivation of a law must be explicitly renounced because its range would be exceeded.

(d) The explanation is incomplete due to insufficient experiential material.

According to Stegmüller (1969) for the aforementioned reasons high demands on historical explanations cannot be materialized; he therefore proposes a broadly conceived definition of historical explanation in the sense of the HO-outline:

> An explanation of *E* on the basis of antecedents’ data A_1. A_n would accordingly be present, when the event-to-be explained is to be expected on the basis of its antecedents’-event and to be expected either in the sense of a purely intuitive and not further defined, or in the sense of a formally specified ‘confirmative concept’ [*Bestätigungsbegriff*]” (p. 348).

The genetic statements of psychoanalytic theory can be covered, that is to say, with this form of historical explanations the present efforts to explain psychoanalytic developmental psychology can be so classified, at least formally. That hereby the degree of confirmation is attained with different precision is clearly shown in the various results of the longitudinal studies by Benjamin (1950), Escalona (1952) and others. Interestingly enough Langer, as early as 1957 when he was president of the American Historical Association, advocated “the use in the future, for the purpose of historical explanations, to a much greater extent than before, of ideas of psychoanalysis and related theories of depth psychology” (quoted in Stegmüller, 1969, p. 423). Langer pleaded in particular for the use of dispositional explanations, because the model of conscious-rational behavior could not suffice for the historian.

The concept of dispositional explanation

It is worthwhile to discuss in greater detail the concept of dispositional explanation, because, like functional explanation, it is of great importance for psychoanalysis. Statements such as “the glass breaks because it has quality X” are dispositional explanations. Because the dispositional quality of an object or individual has consequences in the nature of a law, Ryle (1949) classifies such explanations as “law-like” statements. Dispositional explanations concern that “category of cases in which the activity of the acting persons should be

explained with the help of character traits, convictions, goal projections and other dispositional factors" (Stegmüller, 1969, p. 120). The patient brings to treatment certain modes of behavior and certain qualities based on subconscious conflict constellations, which we explain by dispositions. Since the patient unconsciously seeks a repetition of his infantile traumata, he constructs the transference situation in an analogous manner. The formation of the transference neurosis can be interpreted as the transposition of such dispositions in object relationships that are experienced anew. The overcoming of the transference neurosis will then lead to the dissolution of the unconscious conflicts that previously determined his behavior and with it, of the disposition of those conflicts as a lawful way of reacting. Dispositional statements are often not regarded as explanations, because their relation to basic laws is, as a rule, not made explicit.

The logic of functional explanations must still be discussed separately. Freud speaks of the dream as the guardian of sleep. Are we dealing here with a scientifically legitimate mode of explanation, or is the finalistic consideration here only a veil over an as-yet-unknown causal phenomenon? Or does the functional presentation represent only a descriptive coherence, without the claim of explanation? As a prototype of a functional explanation in psychoanalysis we propose Freud's (1926d) theory of symptom formation:

> Since we have traced back the generating of anxiety to a situation of danger, we shall prefer to say that symptoms are created in order to remove the ego from a situation of danger. (p. 144)

The manner in which Freud expresses himself here is teleological. It seems almost as if what happens in symptom formation should be included in the outline of conscious goal-oriented action. But, as Stegmüller shows, the logical outline of the functional analysis provides an appropriate representation of the relations. System *S* is the individual in whom pathological symptoms form. Disposition *D* is the pattern of compulsive behavior that impresses like a symptom. The effects of disposition *D* can be indicated by *N*, which is in the case of symptom formation, the binding of anxiety. Herein, the functional explanation would be that, condition *N* is deemed necessary for a normal functioning of *S*, which in this case means that *S* can continue to live without serious psychic crises. As Stegmüller shows in his further examination, the testing of the empirical significance of such functional explanations presents considerable difficulties. These lie in the exact definition of the various parts of the explanation model. For the purpose of verification, *that* class of individuals must be specified for whom a defined disposition *D* lawfully has the effects *N*; that is to say, an empirical difficulty lies in the empirically meaningful definition of the system *S* for which the

functional explanation is claimed. A further difficulty arises for empirical testing procedures when not only the disposition D_1, but also another disposition D_2 shows the same effects of the nature *N*, for the system is thus functionally equivalent to disposition D_1.

Let us put this problem in psychoanalytic terms. Not only the defense mechanism of denial, but also that of isolation, of reversal, etc., can be utilized for the binding of anxiety of someone suffering from compulsion neurosis. The introduction of additional dispositions weakens, however reciprocally, the explanatory value of the original one. Thus for instance, Malinowski's thesis that the effect of magic is necessary for the functioning of primitive society is reduced in its explanatory value because no prove is given that only this magic enables primitive man to overcome existential anxiety. The weakness of the functional analysis thus lies in its greater range of descriptive applications, concerning which the heuristic character is easily overlooked. If, in psychoanalysis, it can be shown that different dispositions are effective for different categories of individuals, then the functional explanation can also claim explanatory value.

Prognosis

After this orientation regarding different forms of explanations and their use in psychoanalysis, we ask ourselves what the position of prognosis is in psychoanalytic theory and research. Although not all of science lies in proof, and prognosis is not the only purpose, the prognostic power of a theory has acquired an important place in psychological research. In the history of psychoanalysis, prognosis has not been held in high esteem, neither as instrument nor as goal. Indeed, we must here distinguish between unreflexive-automatic clinical everyday use and theoretic reflection. "Every interviewer who exercises any kind of interpretative technique, predicts from one moment to the next," writes Meehl (1963, p. 71). Thus in the practice of psychoanalysis, clinical experience and suggestions of therapy derived from it are from the very beginning practiced as applied prognostics. "To be sure, we know very little about the frequency of their success and their reliability and in how far the course of the interview depends on them," Meehl continues. The theoretical skepticism of the psychoanalyst was based on an opposition, pointed out by Freud (1920a), between analysis and synthesis, which proved to hamper the adequate reception of the prognosis as an instrument of scientific effort.

> But at this point we become aware of a state of things which also confronts us in many other instances at which light has been thrown on by psychoanalysis on a mental process. So long as we trace the development from its final outcome backwards, the chain of events appears continuous and we feel we have gained an insight, which is

> completely satisfactory or even exhaustive. But if we proceed the reverse way, if we start from the premises inferred from the analysis and try to follow these up to the final result, then we no longer get the impression of an inevitable sequence of events which could not have been otherwise determined. We notice at once that there might have been another result and that we might have been just as well able to understand and explain the latter. The synthesis is thus not as satisfactory as the analysis; in other words, from knowledge of the premises, we could not have foretold the nature of result. ... Hence the chain of the causation can always be recognized with certainty if we follow the line of analysis, whereas to predict it along the line of synthesis is impossible" (Freud,1920a, pp. 167-168).

This presentation from the case report on female homosexuality appears to demonstrate convincingly that it is in principle impossible to predict the future development of a personality, hence the range of genetic psychoanalytic statements is reduced to the *post-festum* analysis of the development of the personality. When at this point we apply the outline on explanation and prognosis reported above, then the question presents itself whether in fact, with exactly the same border conditions, the explanandum could have been something other than (as in the case under discussion) female homosexuality. We believe that if one follows the patho-etiological road in the directions Freud has recorded, other possibilities of development emerge in retrospect because other border conditions occur at the horizon of thought. Then it seems as if the development did not necessarily have to lead to female homosexuality. Moreover, the completing series, the etiological outline developed by Freud (1905d, pp. 239-240), contains border conditions which, if they are or were known, permit explanation. We are therefore confronted here with a problem that can perhaps hardly be solved in empirical ways but is not insoluble in principle.

Freud's formulations can lead to misunderstanding insofar as the knowledge of the presuppositions or, more precisely, the knowledge of all presuppositions must make the nature of the event predictable. In the aforementioned article, Freud himself explains the pessimistic result — that synthesis is impossible with the lack of knowledge about further causes. These causes are however nothing other than alternative border conditions, which, of course, looking back at the pathogenesis can never be known. Only a psychoanalyst endowed with the *Weltgeist* of Laplace could perhaps name all possible border conditions retrospectively. An illustration of the relation between the knowledge of possible border

conditions and the prognostic results is given by Benjamin (1959) in his excellent work on the role of prediction in developmental psychology.

Although Freud's resignation concerns prediction only in the context of genetic psychology, we must remember that the claim of conditional predictions has been formulated with much reservation in other fields of psychoanalytic theory and practice as well. According to Rapaport (1960), this is related to the central position of the principle of overdetermination in psychoanalytic psychology:

> The psychoanalytic concept of *overdetermination* implies that one or several determiners of a given behavior, which appear to explain it, do not necessarily give its full causal explanation. This is not per se alien to other sciences, though a *principle of overdetermination* did not become necessary in any of them. Psychoanalysis' need for this principle seems to be due partly to the multiplicity of the determiners of human behavior and partly to the theory's characteristic lack of criteria for the independence and sufficiency of causes. The determiners of behavior in this theory are so defined in that they apply to all behavior and thus their empirical referents must be present in any and all forms of behavior. Since there is usually no single determiner which constantly assumes the dominant role in a given behavior, other determiners can hardly be neglected while a dominant determiner is explored. When favorable conditions make one determiner dominant, the investigator is tempted to conclude that he has confirmed a predicted functional relationship; as he indeed has. Regrettably, the attempt to repeat the observation or experiment in question often fails, because in the replication either the same behavior appears even though a different determiner has become dominant, or a different behavior appears even though the same determiner has remained dominant. (Rapaport, 1960, pp. 66-67)

On the basis of such considerations it seems logical to Rapaport that Freud overestimated the role of postdiction, and underestimated the role of prediction in the construction of the theory. Waelder (1963) has subjected the principle of overdetermination to a critical analysis, which brings a logical as well as a semantic clarification. With reference to a weighty place in Freud's (1910a) text, he points out that the principle of psychic determinism and of overdetermination must be understood as a heuristic concept, which for methodological reasons requires for all psychic processes — whether they appear as unpretentious, arbitrary or accidental — sufficient motivation:

> As you already see, psychoanalysts are marked by a particularly strict belief in the determination of mental life. For them there is nothing trivial, nothing arbitrary or haphazard. They expect in every case to find sufficient motives ... (Freud, 1910a, p. 38)

The introduction of determinism had therefore, first of all, the function of providing a secure methodological foundation for Freud's analysis. From the "belief" in the determination of psychic life, a series of methodological principles of the psychoanalytical technique of research can be derived. Besides, it follows from the citation that "to be determined" was for Freud equivalent with "to be motivated;" this permits Waelder (1963) to reject the philosophical debate about the question of determinism and free will. From here on the farther-reaching concept of overdetermination must also be considered. Let us first examine those places where Freud introduces the concept of overdetermination; in the *Studies on Hysteria* we find in the discussion of etiological questions the following references:

> Almost invariably when I have investigated the determinants of such [hysterical] conditions what I have come upon has not been a *single* traumatic cause but a group of similar ones. (Freud, 1895d, p. 173)

What he illustrates here casuistically in the case of Elisabeth von R. is further explained in the theoretical chapter "The Psychotherapy of Hysteria":

> He [the physician] is aware of the principal feature in the etiology of the neuroses — that their genesis is as a rule overdetermined, that several factors must come together to produce this result. (p. 263)

In the same way it is true for the symptoms of hysteria:

> We must not expect to meet with a *single* traumatic memory and a *single* pathogenic idea as its nucleus; we must be prepared for *successions* of *partial* traumas and *concatenations* of pathogenic trains of thought. (pp. 287-288)

The clearest definition of the extent of the concept is found in Freud's (1895f) discussion of Löwenfeld's critique of the anxiety neurosis: "As a rule the neuroses are *overdetermined*; that is to say, several factors operate together in their etiology" (p. 131). What can be summarized from these citations and what functions as "the principle of overdetermination" is therefore the idea that there is for the neurotic disorders and their symptoms not a single cause, but many causes working together, the relationship among which cannot be seen as simply cumulative. The structural totality of this set of causes produces together the necessary and sufficient conditions. This principle of a multifactor

genesis was new, neither in philosophy (see, for instance, John Stuart Mill) nor in psychology.

In *The Psychopathology of Everyday Life*, Freud (1901b), in the discussion of promises, quotes Wundt, who in his *Völkerpsychologie* (psychology of nations) claims for slips of the tongue, a series of psychic influences that raise doubt about a single causal motivation of promises:

> 'In some cases too, it may be doubtful to which form a certain disturbance is to be assigned, or whether it would not be more justifiable in *accordance with the principle of the complication of causes,* to trace it back to a concurrence of several motives [Wundt, Völkerpsychologie, 1910, 380-381]. I consider these observations of Wundt's fully justified and very instructive. (Freud, 1901b, pp. 60-61)

Even if the principle was not new and is, especially today, recognized in all sciences that occupy themselves with more complex systems, it is nevertheless of special credit to psychoanalysts who as pioneers have consistently applied it. Sherwood's (1969, p. 181) criticism of psychoanalysts who claim to have "newly discovered" this principle and who want it understood as an essential concept that distinguishes psychoanalysis from other sciences, misses to that extent the heart of the matter. Psychoanalytic explanations have too often been criticized for their plasticity and vagueness. To no small degree, these criticisms stem from efforts on the part of psychoanalysts to take into account the multiple conditions and functions of psychic acts.

In any case, Sherwood justly indicates a misunderstanding of the concept of overdetermination to which Waelder also addresses. If one thus means that there are several causal "constellations" independent of each other and necessary and sufficient, as Guntrip (1961) seems to say, then the result is a logical impossibility.[25] Waelder (1963) tries to clarify the content of the concept of being overdetermined, which starts from the above-mentioned logical untenable nature. The historical perspective Waelder gives in his reference to the origin of the concept is interesting. Freud's effort to conceive of psychic processes and results in neurophysiologic concepts, brought the model of psychic causality into analogy with the processes of a single neuron: stimulus-accumulation with threshold values was an adequate concept for the manner in which neurological processes are effective. The overdetermination necessary for neurological processes — namely to reach threshold values — was borrowed for psychic processes. Waelder corrects the basic misunderstanding by

[25] Anyway, as Stegmüller (1969) indicates, self-directed behavior-flexible systems can reach a similar goal along roads that are causally independent of each other (p. 5).

bringing out the meaning of the situation and introducing a new concept: the principle of the multiple function of a psychic act implies no contradiction in regard to logic causality; it expresses the psychoanalytically central fact that any psychic act can simultaneously serve different needs and problem solutions.

While the confusing "overdetermination of the psyche" was one limitation of the possibility of prediction, so even after clearing away this misunderstanding, the question remains why we are incapable of predicting the nature of the result from knowledge of the presuppositions. In answer to this, Freud alleges that only qualitative and not quantitative etiological relationships are known. Only at the end of developmental process, one could say which of the psychic forces were the stronger, because only the outcome can inform us about the relationship between the forces. Particularly obscure relationships are present when human behavior is the result of a conflict of almost equal inner forces thus making different end results possible. Conflict solutions and steps of development are therefore decisive processes. The greater the number of border conditions, the more degrees of freedom exist and the factors of uncertainty in the prediction increase proportionately. However, predictions become reliable in those cases in which there is no conflict or in which one side is clearly stronger than the other.

In regard to this, Waelder (1963) mentions two marginal cases that render predictions possible: first, those in which the behavior is exclusively governed by the mature ego; or second, those under completely opposite conditions in which the governing by the mature ego is practically entirely eliminated and the action is therefore exclusively ruled by biological forces (drives) and the primitive efforts of solution of the immature ego — that is, when the wealth of determinants of human behavior is diminished (pp. 90 f.).

Anna Freud (1958) further pointed out that predictions are possible not only in these two extreme cases, but also in the numerous cases in which the components, primitive inner forces and sense of reality, exist in a stable relation characteristic for the individual concerned. Such stable mixtures would then constitute the essence of character (p. 92). More or less stable relations, that is, limited "degrees of freedom," always exist in the circumscribed range of psychic disturbances within the total personality. Psychoanalytic explanations and predictions concern these relatively closed systems.

Conceptual vagueness or principal objections

With respect to the difficulties mentioned so far of deriving the possibility of prediction from the theory of psychoanalysis, the question now arises whether we here have conceptual

obscurities or fundamental objections. The question is of particular interest in regard to practical necessity:

> Thus prediction, or predictability, is in the analysis not accidental but belongs to its essence. And it is obviously true ... that our technique is constantly based on such tentative predictions. Without it a rational technique would be impossible. (Hartmann, 1958, p. 121)

For clarification we should first distinguish between different areas, in which prediction can be used, in order to examine in each case whether and to what extent predictions are possible. In its present form, psychoanalytic theory has hypothetic explanations ready for a wide range of social phenomena. Systematic verification of such explanatory efforts with the help of predictive techniques will be discussed here only for the therapeutic situation.

Escalona's (1952) skepticism of whether prediction is possible in clinical psychoanalytic research finds its origin in two considerations: -

- the one, which refers to the conclusive force of an applicable prediction, does not directly belong here and will later be discussed separately;

- the other consideration compares the psychoanalytic therapeutic situation to the laboratory experiment and finds that in the therapeutic situation, for instance, the environmental variable cannot be controlled sufficiently to be able to make meaningful predictions concerning the behavior of the patient. Escalona overlooks that in psychoanalysis one has to do with *relatively* stable and permanent structures, which guarantee a high degree of evenness in the reaction to stimuli. Bellak and Smith (1956), in a pioneering experimental study, were able to show that the importance of the environmental variables is, in fact, considerably reduced by the reaction-readiness of the patient.

From the effort to make predictions concerning the next step in treatment — short-term predictions — one can reasonably distinguish the effort to make prognostic assertions concerning the outcome of treatment. For this, goals of treatment have to be formulated and written down at the beginning. To illustrate this with the model of prediction study of the Menninger Clinic, changes in behavior, adaptive changes (in Hartmann's sense), intrapsychic changes such as insight, changes in drive defense, constellations, or structural changes of the ego can be indicated. As Sargent and her co-workers (1968) have shown in detail, the use of predictions as scientific instruments requires, in any case, a more precise explanation of the formal nature of predictions. Based on Benjamin's (1950, 1959) fundamental longitudinal studies of children, in which he specified prediction as an instrument to validate psychoanalytic-genetic assertions, a prediction model was outlined that permitted empirical

testing of predicted changes after psychoanalytic treatment (see also Luborsky and Schimek, 1964, for a thorough study of these issues).

As we have been able to show, without specific reference to any of the studies mentioned, prediction as an instrument of examination can also be used in psychoanalytic therapy. The stability of neurotic processes permits us to regard the psychoanalytic treatment situation temporarily as ahistoric, even if it is embedded in the framework of systematically generalized history.

Structural identity of explanantion and prognosis

In conclusion, one question must still be raised that puts the significance of prediction in a larger context. Hempel and Oppenheim's outline of scientific explanation (1953) leads to the plausible conception to which we referred to above: "that explanatory and prognostic arguments are similar in regard to their logical structure" (Stegmüller, 1969, p. 153). This would mean that we can only be content with an explanation when we can turn it around, as it were, and use it as an instrument of prediction. On the other hand, we know examples of correct predictions in which the explanatory coherence was not always already known. This theoretical self-evidence, as suggested by the HO-model, was annulled by Scriven (1959). In his analysis of the role of explanation and prediction in the theory of evolution, he shows that the explanatory force of Darwin's hypotheses is not reduced by the lack of prognoses of similar scope:

> Darwin's success lay in his empirical, case by case, demonstration that recognizable fitness *was* very often associated with survival and that the small random variations *could* lead to the development of species. He did not discover *an exact universal law* but the utility of a *particular indicator* in looking for explanations. (Scriven, 1959, p. 478)

To a great extent, similar conditions seem to exist for psychoanalysis. Complete explanatory sets of laws and border conditions are seldom available enough so that they could be transformed into valid predictions. But very often psychoanalysis can demonstrate the explanatory power of particular indicators that sediment into well-known "rules of thumb" of which the daily clinical work draws its predictive capacity.

Circularity and Self-Fulfilling Prophecy

Preface

The expression *self-fulfilling prophecy* was coined by R. K. Merton in 1957. He refers to the theorem of W. I. Thomas, the Nestor of American sociologists, which is basic to the social sciences: "If men define situations as real, they are real in their consequences." Merton adds:

> Were the Thomas theorem and its implications more widely known, more men would understand more of the workings of our society. Though it lacks the sweep and precision of a Newtonian theorem, it possesses the same gift of relevance, being instructively applicable to many, if indeed not most, social processes. (Merton, 1957, p. 421)

In any discussion on predictions in psychoanalysis, the question must be examined whether interpretations fulfil themselves therein. Therefore, we have to occupy ourselves with the problem of circularity. To make the theme explicit, let us look in our text for references to circle and circularity. First we hit upon the hermeneutic circle, then on circularity in historical explanations. We can further perceive a circular movement in the psychoanalytic art of interpretation, where certainly, to pick up a thought of Dilthey, one could speak of a "circularity of experience, understanding, and representation of the mental world in general concepts: — if we include under the latter the clinical theory of psychoanalysis" (Dilthey, *Collected Works,* VII, 145, as quoted by Apel, 1967).

Let us first maintain with Apel that the hermeneutic circle signifies, "that we always already must have understood, in order to understand at all and to be able to *correct*, however, our preliminary understanding through the *methodic* endeavor of understanding" (p. 147, our italics). In this definition the demand for methodic correction of preliminary understanding seems to us essential, because the common bond of scientific proceedings is assured by it.

Apel thus sees in hermeneutics a "methodic" circle. With Gadamer, who follows Heidegger, the circle has lost this meaning. If one simplified somewhat, one could say that in the philosophical hermeneutics of Gadamer and Heidegger the incomplete preliminary understanding is replaced by the "anticipation of completeness." In this anticipation of completeness the totality always seems to be already known, so that parts become understandable only when they appear in a complete unity of meaning. The philosophical-hermeneutic anticipation of completeness (Gadamer, 1965, p. 277) presupposes that hermeneutics is freed from the restrictions of the scientific concept of objectivity, as Gadamer emphasized (p. 250). The important thing for us is a correction of the psychoanalytic-psychotherapeutic preliminary understanding, which is in agreement with the

empirical sciences and can be made objective. Thus Gadamer's anticipation of completeness takes the place of an antithesis that cannot be regarded in an empirical-scientific way, because it is from the very beginning outside of its terrain and therefore enjoys a kind of extraterritorial immunity. Here, to simplify, one could say that the circle is completely closed from the beginning.

Circularity in a general sense exists in every scientific inquiry because a selective preliminary understanding enters into the formation of hypotheses. Radnitzky (1973) has discussed those aspects of the circle that one can render visible outside of hermeneutics (p. 215). In the natural sciences, descriptions are governed by anticipated explanations. Before something can be explained, that which is to be explained (the explanandum) must be expressed in the language of the theory with which one hopes to achieve the more exact explanation. For instance, in order to explain planetary movements with Newton's theory, one must set the descriptions in a relevant form, but in order to do this, one must possess a certain preliminary understanding.

Preliminary understanding and correction, formation of hypotheses and verification, characterize every science and therefore cannot imply circularity in the sense of a vicious circle. Also, the process of knowing is in itself a circular process. It proceeds from ideas (hypotheses) to the facts and back again. To conceptually distinguish general circularity from its incorrect forms, we indicate the latter from now on as *vicious circle*, as faulty conclusion or the like. Then, when does preliminary understanding become faulty circularity? When is the reproach of circular conclusion justified? What proof can be found in the assertion of Popper (1944, p. 265) that it is unjust to accuse scientific theories of circularity, while in general interpretations, therefore, in historical explanations, circularity can be present in the pejorative sense of the word.

With Popper's compilation of the correction possibilities of historical interpretations through manuscripts and other source material, delimitation has been expressed. It is a question of eliminating faults which necessarily still characterize the preliminary understanding, by testing hypotheses with facts. Hereby one should be careful that the immanent faults in the preliminary understanding do not remain hidden by a pre-established choice of material, which would lead to an apparent confirmation. The fact that theory and method move in the same frame of reference would have to lead to a vicious circle only when the research directions were such that they could give answers that are already given by the theory. Theory and method must therefore be independent from each other to the extent that the observations can say "no" to the theory (see Meehl, 1973, pp. 114 ff.). A theory

constructed according to the well-known proverb: "When the rooster crows, the weather changes or it remains the same" cannot be contradicted.

That it is possible for theory and method to move in the same frame of reference, while sufficient independence remains, is illustrated by Popper in a comparison of research and legal processes. In investigating a specific problem that need not concern us here, namely the establishment of so-called basic sentences, Popper shows, by the example of a classic trial by jury, the jury members' and judges' dependence on and independence from the penal system. Thereby the rules of procedure and jurisdiction, one could say plural controls, protect against errors (1959, pp. 109-110). The rules of procedure, according to which the verdict is reached, are however not identical with the legal norms to be applied to the case, but both belong to the legal system. To this extent dependence exists on the legal system and the process moves within this circle.

It is not surprising that precisely this analogy of the research process with a legal process, likewise discussed by Radnitzky (1973, p. 216), played a role in the discussion between Habermas (1969) and Albert (1969) on the occasion of the so-called dispute on positivism (see Adorno et al., 1969, pp. 242, 278). Albert refers to the fact that in the relationship of rules and manner of procedure in the legal system one does not find a circle "in the relevant sense of the word." A "relevant circularity," as we may in any case understand Albert, would be a faulty conclusion implicit in the system or procedure. However, of greater importance is what Habermas concludes from the analogy between research and legal process: "Something like experimentally established facts by which empirical-scientific theories could fail, are constituted first in a preliminary context of interpretation of possible experience" (1969, p. 243).

The reason we presented Popper's analogy[26] and the following discussion between Albert and Habermas is because the all-around relationship to the legal system must result just as little in faulty judgments there, as do faulty conclusions in psychoanalysis. They occur because their interpreting practice depends on its explaining theories. To the contrary: all precautions serve to avoid, respectively correct faulty judgments in the one cases and faulty conclusions in the other.

The psychoanalytic Easter-Eggs

Since in the section on general interpretations we have already established that the testing of psychoanalytic theory takes place by the standard of changes that can be predicted

[26] Since Popper (1972) otherwise illustrates the methodology of the empirical sciences almost exclusively with the natural sciences, this analogy has special significance: it shows that Popper himself cannot maintain the restriction of the concept of empirical science.

under certain conditions, we can now turn to a further more fascinating problem. Let us assume that a patient suffering from anxiety neurosis would, in the course of psychoanalysis, show changes in his symptoms conforming to the theory. Since the theory, as we have shown, has influenced the technique of interpretation, the self-confirmation could be produced along this way (self-fulfilling prophecy). At this point one usually quotes what F. Kraus[27] is supposed to have said: The psychoanalyst finds the Easter eggs that he has first hidden himself (as quoted by Wyss, 1961, p. 372). Thus, it is supposed that psychoanalytic observations are not related to the real facts, but owe their existence to the imagination of the psychoanalyst. Here, one attributes to the imagination a power which in fact it does possess: it produced reality long before Sigmund Freud discovered its constructive and destructive potential and it was illustrated with a document that was completely independent from psychoanalytic technique — the Oedipus saga as described by Sophocles. As we read in Jones' biography (1953), Freud's discovery was connected with the fact that he had recognized oedipal wishes and fears in a personal form.

The discovery that the theme of self-fulfilling prophecy is explicit precisely in the Oedipus complex is obvious, not only because of its central position in psychoanalytic theory. After all, the Oedipus myth proves that the power of prophecies extends to their tragic fulfilment. It is for this reason that Popper (1963a, pp. 35, 38, 123) proposed to always speak of an "Oedipus effect" in those cases where one wants to indicate the influence of a prognosis on the predicted event. Popper substantiates his proposal with the oracular pronouncements that set the "causal chain" (thus Popper) of events in motion precisely by their prophesizing: Laius arranges for Oedipus to be murdered after having his heels pierced, to prevent the prophesied patricide and incest. We may here assume familiarity with Sophocles' *Oedipus Rex* and turn to the context on which Popper founds his proposal. He emphasizes that the compelling force of the oracle's pronouncement has escaped the psychoanalysts and he believes he can prove this. According to Popper, Freud has overlooked the influence of the psychoanalyst on the patient and his communications, as well as the related methodological problems in theory testing, in the same way as he overlooked the role of the oracle in the Oedipus saga. Thus Popper suggests that psychoanalytic interpretations come close to the pronouncements of the oracle. At the same time, he diagnosed a partial reduction of the field of vision of the psychoanalyst, which prevents him from recognizing the proper interpretations of the "causal function."

---

[27] Although this ironical remark could as well have come from the antipsychoanalytic and anti-Semitic mind of K. Kraus, who allegedly said that psychoanalysis is the illness that it pretends to cure, it was the German internist F. Kraus who, on the whole, was less antagonistic to psychosomatic medicine.

This much is true: oracular pronouncements are not set at the beginning of the causal chain in psychoanalysis. Insofar as one cannot credit the oracle with omniscience, one will have to raise the question where then the oracle can have received his information. We do not hesitate to answer: from Laius, Jocasta and Oedipus. It is not the oracle that sets the law of destiny in motion: it is father, mother and son who speak through the oracle. But how does Laius know that Oedipus may kill him? From himself, and his own unconscious destructive desires, directed against his son. At the hand of Laius', Jocasta's and Oedipus' fate, Freud has illustrated that human reality can be determined by conscious and *unconscious* psychological wishes, to a degree of complete necessity. In the first discussion of the Oedipus complex, on dream interpretation (Freud, 1900a, p. 263), one can however also read that oedipal conflicts can have a different outcome and that the complex in question is then structured by different specific initial conditions, for instance, in the area of family and social culture. One could say in short: that man, on the basis of his psychophysical constitution in the oedipal phase, lawfully gets into conflicts whose outcome is decided by initial conditions. In discovering the Oedipus complex in his patients, Freud was impressed by the biological lawfulness of its structure, although it dissolution took various forms.

He described the psychodynamic efficacy of these various forms of dissolution as registered in the experience and behavior of man. That the "initial conditions" responsible for these conflicts occupy so important a place, was then revealed in experiences with neuroses, perversions and psychoses in the various diagnostic categories and, last but not least, in anthropological field research (see Hall and Lindzey, 1968). Besides, in psychoanalytic therapy, it is not of primary importance to dissolve the particular form of the Oedipus complex into its components and to provide historical-genetic explanations. Rather, its influences on ways of feeling and behaving should be delimited from those of other unconscious dispositions. For instance: inferiority feelings and ideas of insignificance, as well as impotence representing possible forms of a fear of castration that has become unconscious, can be distinguished from the development of the same triad on the basis of disturbances in the oral phase or on the basis of narcissistic disorders. We have here one of those certainly still insufficiently solved problems of the clinical theory of psychoanalysis, namely to determine typical pathogeneses more precisely. It is here that the difficulties that we discussed in the section on general interpretations operate. It is a matter of indicating or refuting covariance in those areas for which, according to the theory, a broader context must exist, for example, repetition compulsion and its dissolution. Whatever wishes and fears related to the total complex are discovered means little at first. The decisive criterion in a

given case is whether the hypothesis of a causal relationship between unconscious oedipal death wishes and experiences, for instance, and apparently unfounded and totally unintelligible guilt feeling can be proven or not (if *X*, then probably *Y*). Similar or content-wise different correlation statements are of the greatest importance for clinical theory and practice. In the steps from secured descriptive correlations to explanations, motives in their dissolution prove to have been causes that operated. While correlation statements about typical symptom or character configurations are not prognoses in the scientifically relevant sense, their dissolutions are predictable under certain initial conditions and therefore they are not *ex post facto* explanations. The former, namely the correlations statements, make a diagnostic orientation possible and follow the proverb *ex ungue leonem* [by his claw we know the lion]. To conclude from the claw to the lion is therefore, as Waelder (1962) notes — in opposition to Arlow — not a prediction, because from the occurrence of a specific sign one can only conclude the existence of another symptom, while predictions concern future changes in a situation. These are determined by conditions, which is why one also speaks in short, of "conditional prognoses."[28]

Scientific prognoses are conditional in contrast to prophecies (Popper, 1963b). Albert (1968, p. 130) has given, in line with the distinction stressed by Popper in particular, the following summary of the logically contrasting structures of prognosis and prophecy: a presupposition for the prognostic application of a theory would be an appropriate description of the end situation of the event-to-be-predicted (including the different interventions possible for the acting person) in the language of the theory in question. Such a description of the initial conditions of the proceedings would result in specific statements which, in contrast to the general hypotheses of the theory itself, concern a well-defined area in space and time.

Let us to this end consider once again the extremely simplified psychoanalytic example that has been given. End situation: guilt feelings. Explanatory hypothesis: unconscious oedipal death wishes. Determination of specific initial border conditions namely forms of resistance, which could annul the influence of psychoanalytic "interference" (interpretations), that is to say, make them ineffective. (The resistance argument obviously does not serve the *correctness* of the psychoanalyst, but it qualifies various end situations with different prognoses.) The positive or negative result of the prediction has, first of all, significance only for this particular case at this particular time.

---

[28] The opposite of conditional prognoses are unconditional prophecies, while unconditional prognoses are those in which the conditions can with certainty be regarded as fulfilled. Popper (1963) mentions the following example: "If a physician has diagnosed scarlet fever then he may, with the help of the conditional predictions of his science, make the unconditional prediction that his patient will develop a rash of certain kind" (p. 339). Here, however, it appears rather to be a variation of *ex ungueleonem*.

We have dealt generously with the concept of initial conditions, which refers to the validity of a universal natural law and concerns its specific application. There is now no need for us to clarify which psychoanalytic assumptions can have nomological character. The deductive method of causal explanation is, according to Popper (1959, p. 146), also applicable when, in the uniqueness of events — and the psychoanalyst has to deal with these first of all — the typical can be discerned as it is generalized in psychoanalytical theory. Thus statements of probability can be derived from the theory and can be tested. For the rest, Albert (1972) does not hesitate either to grant to the alternatives of action, that is, to the possible interferences, the role of causally relevant circumstances, or to designate them as initial conditions (p. 130). When it is a matter of determining the influence of these initial conditions, of the operations of the acting person on the proceedings, then alternative influence can be checked against the presuppositions; they can be either verified or falsified. To apply this logical structure in an empirical scientific manner means to test, in the context of the particular theory after the principle of trial and error, alternative interventions against the predictions. The psychoanalytic procedure follows this rule whereby the place of manipulative interventions in experimental arrangements, which are independent from the experimenter as a person, is taken over by technical interpretations that are insolubly connected with the participating person.

Our comparative discussions can be summarized as follows: that psychoanalysis as technique and theory fulfils presuppositions to interrupt apparent vicious circles, that is, to recognize faults in the definition of the initial conditions (psychodynamic situational diagnosis) as well as in the influencing operations (border conditions — technique of interpretation). One could even say that the course of treatment is characterized by a constant correction of these faults. Since in every case the conditional prognosis is changed accordingly, a systematic testing of it is possible only when the conditions remain somewhat constant over a certain period of time. Sudden blows of fate, totally independent of the psychoanalytic process, can create a new situation, just as intervening exterior events can be suitable to call forth a fluctuation of themes in psychoanalytic sessions. However, sooner or later, those relatively stable situations with which psychoanalytic theory concerns itself in particular will again exist, because they constitute the core of nosologically and psychopathogenetic different disorders; we mean the repetition compulsion. That the repetition compulsion is a superordinate essential characteristic of psychic disorders is unquestionable. No theory deserves to be taken seriously that does not present testable hypotheses for the psychogenesis of the repetition compulsion, which characterizes all

psychopathological symptoms. Freud's greatest methodological discovery is, in our opinion, that he has discerned the repetition compulsion in the transference neurosis. In this connection Popper (1963a) cannot escape expressing his agreement with psychoanalysis:

> Psychoanalysts assert that neurotics and others interpret the world in accordance with a personal set pattern which is not easily given up, and which can often be traced back to early childhood. A pattern or scheme which was adopted very early in life is maintained throughout, and every new experience is interpreted in terms of it; verifying it, as it were, and contributing to its rigidity. (p. 49)

Popper then gives his own explanations, based on his theory of neuroses, for the repetition compulsion; most neuroses come about through the prevalence of a dogmatic attitude because a partial fixation of the development of a critical attitude has taken place. Their resistance against changes could perhaps on some basis, by which Popper terminates his considerations of his theory of neuroses, be explained as follows: on the basis of an injury shock, anxiety emerges and there is an increased need for confirmation and security. This process would be analogous to the injury of a limb. From anxiety one no longer moves it, and it becomes stiff. One could even maintain that the case of a stiff limb is not only similar to the dogmatic reactions, but an example of it.

We must deny ourselves the opportunity to translate Popper's theory of neuroses into psychoanalytic concepts and subject it to Popper's own demands for refutations. This much can parenthetically be mentioned: the trauma to the limb[29] implies castration anxiety and the stiffness refers (in Popper's own words) to character deformation, this is, to the results of unconscious, defensive processes. Here it is essential to note the agreement regarding the presupposition for psychoanalytic explanations and prognoses. Their presupposition is that in the repetition compulsion a repetitive system is present in which the conditions of its origin are conserved and strengthened — even via feedback (Popper here appropriately described psychoanalytic experiences).[30] At the pivotal point of the transference neurosis, repetitions can be observed as nowhere else. This pivotal point is methodologically of particular interest. Given the case wherein the explanatory hypothesis says that a dogmatic attitude has come about as a protection against castration fear. From the hypothesis a technique of interpretation can be deduced which has the purpose of making the unconscious castration fears conscious. With this abbreviation of technical terminology, a complicated procedure is

---

[29] We invite the reader's attention to the German pun: limb = Glied, Glied = Penis. Furthermore, *stiff* and *stiffness* are the most frequently used German terms for erection. Certainly Popper knows this pun, perhaps without "knowing" what role it unconsciously played in his theory of neurosis.

[30] An interruption of the repetition compulsion can therefore be effected by psychotherapeutic work on the strengthening of the ego.

described that leads to an intrapsychic change of the, thus far, operative motivations. The conditional prediction that the dogmatic attitude will loosen when fears of castration no longer have their causal (motivating) power confirms or refutes the explanatory hypothesis concerning this relation. The psychoanalytic interventions address themselves to causes in order to change them and lead to a peculiar situation. Their disappearance becomes proof of their previous causality. With the annulment of the repetition compulsion, psychoanalysis justifies itself therapeutically and scientifically. This thesis means that explanations of psychopathological phenomena in neuroses, perversions, addictions, psychoses and character disorders are verified and falsified (proven to be true or false) by the predicted change.

If one tries to arrange the explanatory steps formally according to the many possible meanings of explanation, we can say that the repetition compulsion on the observational level refers to a latent (unconscious) disposition as a theoretical concept; then we can describe the repetition compulsion in the first place as an essential characteristic of a disposition. This description provides, if confirmed by the case, the presupposition of a dispositional explanation. In the therapeutic dissolution of the disposition for a "repetition compulsion," typical relationships as they are systematized in clinical theory become observable relationships which, according to their logical structure, belong predominantly to the historical-genetic and probabilistic-genetic explanations, as well as to the functional analysis (see above section).[31]

In historical explanations circular errors can be particularly great, in Popper's opinion. For psychoanalysis however these problems should be easier to solve than for historical science, as Freud (1937d, p. 259) showed in a comparison with archaeology. It is the repetitions in the transference of reactions from life history, originating in the early years, that permit the psychoanalyst to correct his explanatory outlines. This correction is accomplished in the practical application of life-historical constructions in the present and in prognostic testing, as we have described earlier. Historical interpretations are not verified by the fact that men in the present learn a lesson from history or do not. Genetic-psychoanalytic constructions, on the contrary, address themselves to the repetitive systems of man, who himself represents his history. If the goal of a limited change in the empirically examined case (symptom-bound repetition compulsion) is not reached, and if this was deduced historically and genetically from an unconscious fear of castration, then the construction must be regarded as refuted for this case and during this phase of treatment.

Psychoanalysis and the problem of suggestion

[31] To avoid misunderstanding, we draw attention anew to the fact that although psychoanalysts in general do not give patients a logical explanation of one kind or another, their rational manner of conducting treatment does indeed observe logical laws.

We conclude with a few remarks concerning the problem of suggestion (see Thomä, 1977). In the context of circularity and self-fulfilling prophecy, we must first set straight Popper's assertion that psychoanalysts have overlooked their own influence on the patient in the same way as the role of the oracle in the Oedipus saga has been overlooked. The opposite is true: Freud (1917, p. 448; 1921c, p. 89) has frequently concerned himself with the theme of suggestion. That the objectivity of the findings that are brought out can be questioned because of possible suggestive influencing has been denied with good reason. The psychoanalytical method itself, as is known, originated in the failure of suggestive practices and of cases wherein these had proven to be ineffective. Most patients who come into psychoanalysis have behind them frustrating autosuggestive efforts, as well as all kinds of unsuccessful influences from others against their symptoms. It can therefore not be the usual suggestions that lead to a change in a structure that so far has remained stable (repetition compulsion).

Besides, the "suggestions" of the psychoanalyst are not aimed at the symptoms, but at their motivations. For this reason, Freud has distinguished hypnotic and other kinds of suggestions from the psychoanalyst's sphere of influence, though he has stressed that the latter obviously also depends on the capacity of being influenced as an essential characteristic of man; if such were not the case, psychoanalytic interference would also be impossible. Technical interpretations in treatment can be compared to operations in experimental arrangements without which the theory cannot be verified. In the objection that the psychoanalyst finds the Easter eggs that he himself first has hidden, one supposes a *vicious circle*, a self-fulfilling prophecy. Now, nobody will contest that symptoms are real and manifest themselves as the consequences of a psychopathogenesis.

We allude to Merton's theory and maintain: the patient defined his emotional experiences, wishes and fears as "real" long before a psychoanalyst appeared on the scene. The psychoanalyst discovered the definitions, he did not create them. It seems to us that otherwise, one must make an absurd assumption: one would have to start from the fact that, in connection with the predicted symptomatic changes, freshly discovered pathogenesis was neither operative nor did it remain operative in the present via repetition compulsion; in other words, that the elimination of the repetition compulsion takes place independently from its pathogenesis through suggestions of one kind or another. Nobody will seriously want to maintain such a complete separation. The fact that the psychoanalyst as a person has positive and negative influences on his patient, should not be indicated by the loaded term *suggestion*.

Freud's often misunderstood recommendation that the psychoanalyst should conduct himself in regard to his patient as a mirror which only reflects is in particularly directed against uncontrolled suggestions. It is an invitation to observe countertransference and to burden the patient neither with one's own personal problems nor with one's own ideologies. To this extent, the recommendation serves the interest of the patient; in it, however, is also expressed the scientific ideal of the experimenting researcher who would have his method entirely independent from the person. The precise quotation and its context are the basis for the following assumption: "The doctor should be opaque to his patients and, like a mirror, should show them noting but what is shown to him" (Freud, 1912e, p. 118). Freud wanted to purify the psychoanalytical method of all undesirable elements, and if one takes the quotation to the letter, of all personal elements. It is clear that this summons should not be taken literally. All witnesses tell us that as a physician Freud himself provided another example. If the psychoanalyst behaves only like a mirror and adds nothing to what is shown, then the psychoanalytic process can never get started (see Stone, 1961). The explanatory psychoanalytic theories pass their tests of verification in as far as the elimination of the repetition compulsion. That it is interrupted must be attributed to *new* experiences which the patient has in communicating with the psychoanalyst and which he tries out and enlarges. Verification and falsification of the theory are thereby complicated, particularly since the conditional prognoses depend on the question of whether or not new experience takes place.

Thus no testing of psychoanalytic theory is possible without considering that the method is embedded in human interaction. The transference onto the mirror characterizes *one* side of this interaction. What takes place in the psychoanalytic situation is more than the testing of a theory that refers to the psychopathogenesis up to the immediate present. The very title of the study on technique, "Remembering, Repeating, and Working Through" (Freud, 1914g) permits us to perceive that the working-through leads via remembering (past) and repeating (present) to the future. That the psychoanalyst, precisely in this working-through, acts as mediator to new experiences and makes positive identifications possible is self-evident. This is essential and constitutive for therapy, though it complicates the testing of the theory. There is no reason however to speak of suggestion where the psychoanalyst is acting as a person.

Summary

In preparing our empirical research, we have reviewed the discussion concerning the scientific-systematic position and about the logical status of psychoanalysis in order to

determine our own position within these controversies. Our work mediates between the attempts at methodological clarification, which have been made by psychoanalytic authors and the debate about the character of psychoanalysis — whether it is science or hermeneutic-dialectic procedure — that has been carried on by non-analysts. The conception of psychoanalysis as "depth hermeneutics" has been criticized along the lines of Popper and Albert. In our opinion, the grounding of all psychoanalytic knowledge on the basis of a strict psychology of *Verstehen* would limit the empirical basis of psychoanalysis. Objectifying methods are an indispensable corrective in this regard. We have considered the relationship between psychoanalytic theory and therapy. Psychoanalytic data collection must be made reliable, the theoretical concepts sharpened and the rules for translating them into empirical tests of falsification defined. According to Freud, metapsychological concepts belong to a "speculative superstructure;" its relevance diminishes with increasing distance from clinical experience. In agreement with Waelder (1962) and Wisdom (1971, 1972), we distinguish the following steps in psychoanalytic theory: (communicated) observational data; clinical generalizations; clinical theory; metapsychology, Freud's "personal philosophy." Objectification and falsification apply chiefly to "clinical theory."

We have discussed the dovetailing of general theories — chiefly the theory of neurosis — with interpretations as they occur in psychoanalytic therapy and with the theory of such interpretations. The concept of repetition compulsion refers to a psychic apparatus as a relatively closed system that is embedded in life history and in its frame motives become effective in the guise of causes. The proof of any hypothesis under consideration consists in the elimination of those initial conditions that potentiated the repetition compulsion. Whereas Habermas contends that the patient's self-reflection is the sole criterion for the revision of disturbed formative processes, we criticize this view as a utopian-dogmatic overestimation of the role of knowledge. We have discussed the role of description, explanation and prediction in psychoanalysis, and have dealt with the problem of circularity of reasoning and self-fulfilling prophecy in psychoanalytic practice and its consequences for clinical research.

Adorno, T. W., Dahrendorf, R., Pilot, H., Albert, H., Habermas, J., & Popper, K. (1969). *Der Positivismusstreit in der deutschen Soziologie (The battle on positivism in German sociology)*. Neuwied / Berlin: Luchterhand.

Albert, H. (1968). Theorie und Prognose in den Sozialwissenschaften (Theory and prognosis in social sciences). In E. Topitsch (Eds.), *Logik der Sozialwissenschaften*. Köln: Kiepenheuer & Witsch. pp 126-143

Albert, H. (1969). Im Rücken des Positivismus? (The backside of positivism). In T. W. Adorno, R. Dahrendorf, H. Pilot, H. Albert, J. Habermas, & K. R. Popper (Eds.), *Der Positivismusstreit in der deutschen Soziologie*. Neuwied & Berlin: Luchterhand. pp 267-

305
Albert, H. (1971). *Plädoyer für kritischen Rationalismus (Plea for a critical rationalism)*. München: Piper.
Albert, H. (Eds.). (1972). *Theorie und Realität (Theory and reality).* Tübingen: Mohr / Siebeck.
Allport, G. W. (1937). *Personality: A psychological interpretation*. New York: Holt.
Apel, K. O. (1955). Das Verstehen (Understanding). *Archiv für Begriffsgeschichte*, *1,* 142-199.
Apel, K. O. (1965). Die Entfaltung der "sprachanalytischen Philosophie" und das Problem der "Geisteswissenschaften" (The development of analytic philosophy and the problem of the "humanities"). *Philosophisches Jahrbuch*, *72,* 239-289.
Apel, K. O. (1966). Wittgenstein und das Problem des hermeneutischen Verstehens (Wittgenstein and the problem of hermeneutic understanding). *Zeitschrift für Theologie und Kirche*, *63,* 49-87.
Apel, K. O. (1971). Szientistik, Hermeneutik, Ideologiekritik (Scientism, hermeneutics, critique of ideology). In (Eds.), *Hermeneutik und Ideologiekritik,* _. Ed. Frankfurt am Main: Suhrkamp. pp 7-44
Bellak, L., & Smith, M. B. (1956). An experimental exploration of the psychoanalytic process. *Psychoanalytic Quarterly*, *25,* 385-414.
Benjamin, J. D. (1950). Methodological considerations in the validation and elaboration of psychoanalytical personality theory. *American Journal of Orthopsychiatry*, *20,* 139-156.
Benjamin, J. D. (1959). Prediction and psychopathological theory. In L. Jessner, & E. Pavenstedt (Eds.), *Dynamic pathology in childhood*. New York: Grune & Stratton. pp 6-77
Bernfeld, S. (1934). Die Gestalttheorie (Theory of gestalt). *Imago*, *20,* 32-77.
Binswanger, L. (1955). Zur Problematik der psychiatrischen Forschung und zum Problem der Psychiatrie (On the problem of psychiatric research and on the problem of psychiatry). In L. Binswanger (Eds.), *Ausgewählte Vorträge und Aufsätze (Selected papers and lectures. vol. 2)* (Vol. 2. Bern: Francke. pp
Bonaparte, M. (1945). Notes on the analytic discovery of a primal scene. *Psychoanalytic Study of the Child*, *1,* 119-125.
Bucci, W. (1997). *Psychoanalysis & Cognitive Science*. New York: The Guilford Press.
Bühler, K. (1927). *Die Krise der Psychologie (The crisis of psychology)*. Jena: Fischer.
Carnap, R. (1950). *The logical foundations of probability*. Chicago: University of Chicago Press.
Caws, P. (1991). Das Gerüst der Psychoanalyse (The framework of psychoanalysis). In A. Grünbaum (Eds.), *Kritische Betrachtungen zur Psychoanalyse. Adolf Grünbaums "Grundlagen" in der Diskussion (Critical comments on psychoanalysis. A. Grünbaums "foundations" in discussion)*. Berlin Heidelberg New York: Springer. pp 39-41
Cohen, R., & Laudan, L. (Eds.). (1983). *Physics, philosophy and psychoanalytic essays in honor of Adolf Grünbaum.* Dordrecht: Reidel.
Compton, A. (1972). The study of the psychoanalytic theory of anxiety. *Journal of the American Psychoanalytic Association*, *20,* 3 - 44, 341 - 394.
Danto, A. C. (1965). *Analytical philosophy of history*. Cambridge: Cambridge University Press.
Deutsch, H. (1928). Ein Frauenschicksal - George Sand (A woman´s fate - George Sand). *Imago*, *14,* 334-357.
Devereux, G. (1951). Some criteria for the timing of confrontations and interpretations. *International Journal of Psychoanalysis*, *32,* 19-24.
Dilthey, W. (1894). Ideen über eine beschreibende und zergliedernde Psychologie (Ideas on a descriptive and dissecting psychology). In W. Dilthey (Eds.), *Gesammelte Schriften*: 5). Leipzig: Teubner. pp 139-240

Dilthey, W. (1900). Die Entstehung der Hermeneutik (The origin of hermeneutics). In W. Dilthey (Eds.), *Gesammelte Schriften Bd 5*: Bd.5). Leipzig: Teubner. pp 317-338
Edelson, M. (1984). *Hypothesis and evidence in psychoanalysis*. Chicago: University of Chicago Press.
Eissler, K. R. (1968). The relation of explaining and understanding in psychoanalysis. Demonstrated by one aspect of Freud`s approach to literature. *Psychoanalytic Study of the Child*, *23,* 141-177.
Eissler, K. R. (1971). Death drive, ambivalence, and narcissism. *Psychoanalytic Study of the Child*, *26,* 25-78.
Ellis, A. (1956). An operational reformulation of some of the basic principles of psychoanalysis. In H. Feigl, & M. Scriven (Eds.), *Minnesota studies in the philosophy of science I. The foundations of science and the concepts of psychology and psychoanalysis*. Minneapolis: University of Minnesota Press. pp 131-154
Escalona, S. (1952). Problems in psychoanalytic research. *International Journal of Psychoanalysis*, *33,* 11-21.
Farrell, B. A. (1961). Can psychoanalysis be refuted? *Inquiry*, *4,* 16-36.
Farrell, B. A. (1964). The criteria for a psychoanalytic interpretation. In D. F. Gustavson (Eds.), *Essays in philosophical psychology*. Garden City: Doubleday. pp
Fonagy, P. (Eds.). (2002a). *On open door review of the outcome of psychoanalysis. 2nd ed.*, 2. Ed. London: International Psychoanalytic Association.
Fonagy, P. (2002b). Epistemological and methodological background: Reflections on psychoanalytic research problems - an Anglo-Saxon view. In P. Fonagy (Eds.), *An open door review of outcome studies in psychoanalysis*. London: International Psychoanalytic Association. pp 10-29
Fonagy, P. (2003). Some complexities in the relationship of psychoanalytic theory to technique. *Psychoanalytic Quarterly*, *72,* 13-47.
Frenkel-Brunswik, E. (1954). Meaning of psychoanalytic concepts and confirmation of psychoanalytic theories. *Scientific Monthly*, *79,* 203-300.
Freud, A. (1936). *The ego and the mechanisms of defense*. London: Hogarth.
Freud, A. (1958). Child observation and prediction of development. *Psychoanalytic Study of the Child*, *13,* 92-116.
Freud, S. (1895). A project for a scientific psychology. In M. Bonaparte, A. Freud, & E. Kris (Eds.), *The origins of psycho-analysis*. London & New York: Hogarth Press 1954. pp
Freud, S. (1895d). *Studies on hysteria*. Standard Edition II:
Freud, S. (1895f). *A reply to criticisms of my paper on anxiety neurosis*. In Standard Edition II : 123-139:
Freud, S. (1896c). *The etiology of hysteria*. In Standard Edition III : 191-221:
Freud, S. (1900a). *The interpretation of dreams*. In Standard Edition IV-V:
Freud, S. (1901b). *The psychopathology of everyday life*. In Standard Edition VI:
Freud, S. (1905d). Three essays on the theory of sexuality. *Standard Edition*, *VII,* 135-243.
Freud, S. (1909b). *Analysis of a phobia in a five-year-old boy*. In Standard Edition X : 5-147:
Freud, S. (1910a). *Five lectures on psycho-analysis*. In Standard Edition XI: 7-55:
Freud, S. (1912e). *Recommendations to physicians practising psycho-analysis*. In Standard Edition XII : 111-120:
Freud, S. (1914c). *On narcissism: an introduction*. In Standard Edition XIV: 73-102:
Freud, S. (1914g). *Remembering, repeating, and working-through*. In Standard Edition XII : 147-156:
Freud, S. (1915a). Observations on transference-love. *Standard Edition*, *XII,* 157-171.
Freud, S. (1915b). *Thoughts for the times on war and death*. In Standard Edition XIV: 275-300:
Freud, S. (1915e). The unconscious. *Standard Edition*, *XIV,* 159-215.

Freud, S. (1917). Introductory lectures on psychoanalysis. *Standard Edition*, *XV/XVI*.
Freud, S. (1920a). *The psychogenesis of a case of female homosexuality*. In Standard Edition XVIII: 147-172:
Freud, S. (1920g). *Beyond the pleasure principle*. In Standard Edition XVIII: 7-64:
Freud, S. (1921c). *Group psychology and the analysis of the ego*. In Standard Edition XVIII: 69-143:
Freud, S. (1925d). *An autobiographical study*. In Standard Edition XX: 7-70:
Freud, S. (1926d). *Inhibitions, symptoms, and anxiety*. In Standard Edition XX: 87-172:
Freud, S. (1933a). *New introductory lectures on psycho-analysis*. In Standard Edition XXII: 5-182:
Freud, S. (1937d). *Constructions in analysis*. In Standard Edition XXIII: 257-269:
Gadamer, H. G. (1959). Vom Zirkel des Verstehens (On circularity of understanding). In (Eds.), *Festschrift für M. Heidegger*. Pfullingen: Neske. pp 24-34
Gadamer, H. G. (1965). *Wahrheit und Methode. Anwendung einer philosophischen Hermeneutik (Truth and method. Application of a philosophical hermeneutic)*. Tübingen: Mohr.
Gadamer, H. G. (1971a). Replik (A replique). In (Eds.), *Hermeneutik und Ideologiekritik*. Frankfurt: Suhrkamp. pp 283-317
Gadamer, H. G. (1971b). Rhetorik, Hermeneutik und Ideologiekritik (Rhetoric, hermeneutics and critique of ideology). In (Eds.), *Hermeneutik und Ideologiekritik*. Frankfurt: Suhrkamp. pp 57-82
Gill, M. M. (1983). The point of view of psychoanalysis. Energy discharge or person. *Psychoanalysis and Contemporary Thought*, *6,* 523-551.
Glover, E. (1947). Basic mental concepts: Their clinical and theoretical value. *Psychoanalytic Quarterly*, *16,* 482-506.
Green, A. (2000). Science and science fiction in infant research. In J. Sandler, A.-M. Sandler, & R. Davies (Eds.), *Clinical and observational research: Roots of a controversy*. London: Karnacs Books. pp 41-72
Green, A. (2005). The illusion of common ground and mythical pluralism. *International Journal of Psychoanalysis*, *86,* 627-632.
Greenson, R. R. (1960). Empathy and its vicissitudes. *International Journal of Psychoanalysis*, *41,* 418-424.
Grünbaum, A. (1984). *The foundations of psychoanalysis. A philosophical critique*. Berkeley Los Angeles London: University of California Press.
Grünbaum, A. (1986a). Précis of the foundations of psychoanalysis. A philosophical critique, and author´s response to 39 reviewers: "Is Freud´s theory well-founded". *Behavioral and Brain Sciences*, *9,* 217-284.
Grünbaum, A. (1986b). The notion of placebo in psychotherapy. *Psychological Medicine*, *16,* 19-38.
Grünbaum, A. (1987a). *Psychoanalyse in wissenschaftstheoretischer Sicht. Zum Werk Sigmund Freuds und seiner Rezeption (Psychoanalysis in epistemic perspective. On the work of Sigmund Freud and its reception)*. Konstanz: Universitätsverlag.
Grünbaum, A. (1988a). *Die Grundlagen der Psychoanalyse (The foundations of psychoanalysis)*. Stuttgart: Reclam.
Grünbaum, A. (1988b). The role of the case study method in the foundations of psychoanalysis. In H. Vetter, & L. Nagl (Eds.), *Die Philosophen und Freud*. Wien: Oldenburg. pp 134-174
Grünbaum, A. (1993). *Validation in the clinical theory of psychoanalysis. A study in the philosophy of psychoanalysis*. New York: International Universities Press.
Guntrip, H. (1961). *Personality structure and human interaction*. New York: International Universities Press.

Habermas, J. (1963). Analytische Wissenschaftstheorie und Dialektik (Analytic metascience and dialectics). In M. Horkheimer (Eds.), *Zeugnisse. Theodor W. Adorno zum sechzigsten Geburtstag*. Frankfurt am Main: Europäische Verlagsanstalt. pp 473-501

Habermas, J. (1967). *Zur Logik der Sozialwissenschaften (On the logic of social sciences)*: Beiheft 5), Philosophische Rundschau.

Habermas, J. (1968). *Erkenntnis und Interesse (Knowledge and human interest)*. Frankfurt am Main: Suhrkamp.

Habermas, J. (1969). Gegen einen positivistisch halbierten Rationalismus (Against a positivistic splitted rationalism). In T. W. Adorno, R. Dahrendorf, H. Pilot, H. Albert, J. Habermas, & K. Popper (Eds.), *Der Positivismus Streit in der deutschen Soziologie*. Neuwied und Berlin: Luchterhand. pp 235-268

Habermas, J. (1971a). *Knowledge and human interests*. Boston: Beacon Press.

Habermas, J. (1971b). Zu Gadamers 'Wahrheit und Methode' (On Gadamer´s "Truth and method"). In (Eds.), *Hermeneutik und Ideologiekritik*. Frankfurt: Suhrkamp. pp 45-56

Hall, C. S., & Lindzey, G. (1968). The relevance of Freudian psychology and related viewpoints for the social sciences. In G. Lindzey, & E. Aronson (Eds.), *Historical introduction. Systematic positions*: The Handbook of Social Psychology, 2nd ed). Reading Menlo Park London Don Mills: Addison-Wesley. pp 245-319

Hartmann, H. (1927). *Die Grundlagen der Psychoanalyse (The foundations of psychoanalysis)*. Leipzig: Thieme. Neuauflage 1972.

Hartmann, H. (1958). Discussion of A. Freud. *Psychoanalytic Study of the Child*, *13,* 120-122.

Heimann, P. (1969). Gedanken zum Erkenntnisprozeß des Psychoanalytikers (Thoughts on knowledge formation of the psychoanalyst). *Psyche - Zeitschrift für Psychoanalyse*, *23,* 2-24.

Heimann, P. (1977). Further observations on the analyst´s cognitive process. *Journal of the American Psychoanalytic Association*, *25,* 313-333.

Hempel, C. (1952). Problems of concept and theory formation in the social sciences. In American Philosophical Association - Eastern Division (Eds.), *Science, language, and human rights*. Philadelphia: University of Pennsylvania Press. pp 65-86

Hempel, C., & Oppenheim, P. (1953). The logic of explanation. In H. Feigl, & M. Brodbeck (Eds.), *Readings in the philosophy of science*. New York: Appelton. pp 319 - 352

Holt, R. R. (1958). Clinical and statistical prediction. *Journal of Abnormal and Social Psychology*, *56,* 1-12.

Holt, R. R. (1962). A critical examination of Freuds concept of bounds vs. free cathexis. *Journal of the American Psychoanalytic Association*, *10,* 475-525.

Holt, R. R. (1965). A review of some of Freud´s biological assumptions and their influence on his theories. In N. S. Greenfield, & W. C. Lewis (Eds.), *Psychoanalysis and current biological thought*. Madison: University of Wisconsin Press. pp 475-525

Holt, R. R. (2002). Quantitative research on the primary process: Method and findings. *Journal of the American Psychoanalytic Association*, *50,* 457-482.

Holt, R. R. (2005). A lifelong attempt to understand and assess personality. *Journal of Personality Assessment*, *84,* 3-13.

Hook, S. (1959). *Psychoanalysis. Scientific method and philosophy*. New York: International University Press.

Hook, S. (1959). Science and mythology in psychoanalysis. In S. Hook (Eds.), *Psychoanalysis. Scientific method and philosophy*. New York: International University Press. pp 212-224

Jaspers, K. (1948). *Allgemeine Psychopathologie (General psychopathology)*. Berlin: Springer.

Jones, E. (1953). *The life and work of Sigmund Freud, vol. I*. New York: Basic Books.

Kafka, E. (2004). Book review: Bergmann M (Ed) The Hartmann Era. *Psychoanalytic Quarterly*, *73,* 836-851.
Klauber, J. (1968). On the dual use of historical and scientific method in psychoanalysis. *International Journal of Psychoanalysis*, *49,* 80-87.
Klein, G. S. (1976). *Psychoanalytic theory. An exploration of essentials*. New York: International Universities Press.
Kohut, H. (1959). Introspection, empathy, and psychoanalysis. An examination of the relationship between mode of observation and theory. *Journal of the American Psychoanalytic Association*, *7,* 459-483.
Kris, E. (1951). Ego psychology and interpretation in psychoanalytic therapy. *Psychoanalytic Quarterly*, *20,* 15-30.
Kris, E. (1954). Introduction. In F. A. Bonaparte M, Kris E (Eds.), *S. Freud: The origins of psycho-analyis*. London: Imago Publishing Company. pp 1-47
Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
Kuiper, P. C. (1964). Verstehende Psychologie und Psychoanalyse (Hermeneutic psychology and psychoanalysis). *Psyche -Zeitschrift für Psychoanalyse*, *18,* 15-32.
Kuiper, P. C. (1965). Diltheys Psychologie und ihre Beziehung zur Psychoanalyse (The psychology of Dilthey and its relation to psychoanalysis). *Psyche - Zeitschrift für Psychoanalyse*, *19,* 241-249.
Leuschner, W., & Hau, S. (Eds.). (1995). *Traum und Gedächtnis (Dream and memory)*. Münster: LIT Verlag.
Leuzinger-Bohleber, M., Stuhr, U., Rüger, B., & Beutel, M. (2003). How to study the quality of psychoanalytic treatments and their long term effects on patients well-being. A representative multiperspective follow-up study. *International Journal of Psychoanalysis*, *84,* 263-290.
Levi, L. H. (1963). *Psychological Interpretation*. New York: Holt, Reinhart and Winston.
Lewin, K. (1937). Psychoanalysis and topological psychology. *Bulletin of the Menninger Clinic*, *1,* 202-212.
Loch, W. (1965). *Voraussetzungen, Mechanismen und Grenzen des psychoanalytischen Prozesses (Preconceptions, mechanisms and limitations of the psychoanalytic process)*. Bern Stuttgart: Huber.
Loewald, H. W. (1971). On motivation and instinct theory. *Psychoanalytic Study of the Child*, *26,* 91-128.
Loewenstein, R. M. (1951). The problem of interpretation. *Psychoanalytic Quarterly*, *20,* 1-14.
Lorenzer, A. (1970). *Sprachzerstörung und Rekonstruktion. Vorarbeiten zu einer Metatheorie der Psychoanalyse (Language destruction and its reconstruction. Studies on a metatheory of psychoanalysis)*. Frankfurt am Main: Suhrkamp.
Lorenzer, A. (1985). Das Verhältnis der Psychoanalyse zu ihren Nachbarsdisziplinen (The relationship of psychoanalysis to its neighbouring disciplines). *fragmente*, *14/15,* 8-22.
MacIntyre, A. C. (1958). *The unconscious. A conceptual analysis*. London: Routledge.
Malan, D. H. (1963). *A study of brief psychotherapy*. London: Tavistock.
Meehl, P. E. (1963). *Clinical versus statistical prediction*. Minneapolis: Univ. of Minnesota Press.
Meehl, P. E. (1983). Subjectivity in psychoanalytic inference: The nagging persistence of Wilhelm Fliess's Achensee question. In J. Earman (Eds.), *Testing scientific theories. Minnesota Studies in the Philosophy of Science, Vol 10*. Minneapolis: University of Minnesota Press. pp 349-411
Meissner, W. W. (1971). Freud´s methodology. *Journal of the American Psychoanalytic Association*, *19,* 265-309.

Merton, R. K. (1957). The self-fulfilling prophecy. In R. K. Merton (Eds.), *Social theory and social structure*. New York: The Free Press of Glencoe. pp 421-436

Merton, R. K. (1968). The self-fulfilling prophesy. In R. K. Merton (Eds.), *Social theory and social structure,* Enl. Ed. New York: The Free Press of Glencoe. pp 475-490

Mises, R. v. (1939). *Kleines Lehrbuch des Positivismus*. The Hague. Reprint !951: Stockum & Zoon.

Mitchell, S. A. (1998). The analyst´s knowledge and authority. *Psychoanalytic Quarterly*, *67,* 1-31.

Moore, G. E. (1955). Wittgenstein´s lectures in 1930-1933. In G. E. Moore (Eds.), *Philosophical papers*. London: Allen & Unwin. pp 252-324.

Nichols, C. (1972). Science or reflection: Habermas on Freud. *Philosophy of the Social Sciences*, *2,* 261-270.

Orlinsky, D. E., Rønnestad, M. H., & Willutzki, U. (2004). Fifty years of psychotherapy process-outcome research: Continuity and change. In M. J. Lambert (Eds.), *Handbook of Psychotherapy and Behavior Change,* 5. Ed. New York: Wiley. pp 307-389

Perrez, M. (1971). Zur wissenschaftlichen Theoriebildung und zum Bewährungsproblem in der Psychoanalyse (On the formation of scientific theories and on the problem of validation in psychoanalysis). *Zeitschrift für Klinische Psychotherapie*, *19,* 221-242.

Perron, R. (2006). How to do research? Reply to Otto Kernberg. *International Journal of Psychoanalysis*, *87,* 927-932.

Popper, K. R. (1944). *The open society and its enemies*: 2). Princeton: Princeton University Press.

Popper, K. R. (1957). *The poverty of historicism*: 2. Ed. Boston: Beacon Press.

Popper, K. R. (1959). *The logic of scientific discovery*. New York: Basic Books.

Popper, K. R. (1963). *Conjectures and Refutations*. London: Routledge and Kegan Paul.

Popper, K. R. (1963). Prediction and prophesy in the social sciences. In K. R. Popper (Eds.), *Conjectures and refutations*. London: Routledge & Kegan Paul. pp 336-346

Rapaport, D. (1960). *The structure of psychoanalytic theory. A systematizing attempt*. New York: International Universities Press.

Ricoeur, P. (1970). *Freud and philosophy: An essay on interpretation*. New Haven: Yale University Press.

Ricoeur, P. (1981). *Hermeneutics and the human sciences*. New York: Cambridge University Press.

Rosenblatt, A. D., & Thickstun, J. T. (1970). The concept of psychic energy. *International Journal of Psychoanalysis*, *51,* 265-278.

Ruben, D. H. (Eds.). (1993). *Explanation.* Oxford: Oxford University Press.

Rubinstein, B. B. (1973). On the logic of explanation in psychoanalysis. In B. B. Rubinstein (Eds.), *Psychoanalysis and Contemporary Science*: 2). New York: International Universities Press. pp 338-358

Rubinstein, B. B. (1980). The self and its brain. An argument for interactionism. By Karl Popper and John C. Eccles. *Journal of the American Psychoanalytic Association*, *28,* 210-219.

Rubovitz-Seitz, P. (1998). *Depth-psychological understanding. The methodological grounding of clinical interpretations*. Hillsdale, NJ: The Analytic Press.

Ryle, G. (1949). *The concept of mind*. New York: Barnes & Noble 1960.

Sandler, J. (1962). The Hampstead Index as an instrument of psychoanalytic research. *International Journal of Psychoanalysis*, *43,* 289-291.

Sandler, J., Kawenoka, M., Neurath, L., Rosenblatt, B., Schnurmann, A., & Sigal, J. (1962). The classification of superego material in the Hampstead Index. *Psychoanalytic Study of the Child*, *17,* 107-127.

Sargent, H. D., Horwitz, L., Wallerstein, R. S., & Appelbaum, A. (1968). *Prediction in*

psychotherapy research. Method for the transformation of clinical judgements into testable hypothesis. New York: International Universities Press.
Schafer, R. (1976). *A new language for psychoanalysis*. New Haven: Yale University Press.
Schmidl, S. (1955). The problem of scientific validation in psychoanalytic interpretation. *International Journal of Psychoanalysis*, *36,* 105-113.
Scriven, M. (1959). Explanation and prediction in evolutionary theory. *Science*, *130,* 477-482.
Seitz, P. (1966). The consensus problem in psychoanalysis. In L. A. Gottschalk, & A. H. Auerbach (Eds.), *Methods of research in psychotherapy*. New York: Appleton Century Crofts. pp 209-225
Sherwood, M. (1969). *The logic of explanation in psychoanalysis*. New York: Academic Press.
Shevrin, H. (2005). Toward a theory on consciousness based on recent developments in subliminal research. In P. Giampieri-Deutsch (Eds.), *Psychoanalysis as an empirical, interdisciplinary science. Collected papers on contemporary psychoanalytic research*. Wien: pp 54-74
Stegmüller, W. (1969). *Probleme und Resultate der Wissenschaftstheorie und analytischen Philosophie. Bd II: Theorie und Erfahrung (Problems and results of the theory of science and of analytical philosophy)*. Berlin Heidelberg New York: Springer.
Stern, D. N. (2000). The relevance of empirical infant research to psychoanalytic theory and practice. In J. Sandler, A.-M. Sandler, & R. Davies (Eds.), *Clinical and observational research: Roots of a controversy*. London: Karnacs Books. pp 27-31
Stierlin, H. (1972). Review of "Sprachzerstörung und Rekonstruktion " von A. Lorenzer. *International Journal of Psychoanalysis*, *53,* 422-425.
Stone, L. (1961). *The psychoanalytic situation. An examination of its development and essential nature*. New York: Int Univ Press.
de Swaan, A. (1980). On the sociogenesis of the psychoanalytic situation. *Psychoanalysis and Contemporary Thought*, *3,* 381-413.
Thomä, H. (1977). Psychoanalyse und Suggestion. (Psychoanalysis and suggestion). *Zeitschrift für Psychosomatische Medizin und Psychoanalyse*, *23,* 35-56.
Thomä, H., & Houben, A. (1967). Über die Validierung psychoanalytischer Theorien durch die Untersuchung von Deutungsaktionen (On the validation of psychoanalytic theories by the investigation of interpretative actions). *Psyche - Zeitschrift für Psychoanalyse*, *21,* 664-692.
Thomä, H., & Kächele, H. (1975). Problems of metascience and methodology in clinical psychoanalytic research. *Annual of Psychoanalysis*, *3,* 49-119.
Thomä, H., & Kächele, H. (1985). *Lehrbuch der psychoanalytischen Therapie* (Vol. 1: Grundlagen). Berlin, Heidelberg, New York, Tokyo: Springer.
Thomä, H., & Kächele, H. (1994). *Psychoanalytic practice. vol 1 Principles*. New York: Jason Aronson.
Thomä, H., & Kächele, H. (1994). *Psychoanalytic practice. vol. 2: Clinical studies.* New York: Jason Aronson.
Waelder, R. (1962). Psychoanalysis, scientific method and philosophy. *Journal of the American Psychoanalytic Association*, *10,* 617-637.
Waelder, R. (1963). Psychic determinism and the possibility of predictions. *Psychoanalytic Quarterly*, *32,* 15-42.
Waelder, R. (1967). *Progress and revolution. A study of the issues of our age*. New York: International Universities Press.
Wallerstein, R. S. (2005). Dialogue or illusion? How do we go from here. *International Journal of Psychoanalysis*, *86,* 633-638.
Wallerstein, R. S. (2005). Will psychoanalytic pluralism be an enduring state of our discipline. *International Journal of Psychoanalysis*, *86,* 623-626.

Weber, M. (1949). *The methodology of social sciences*: 2. Ed. New York: Free Press.
Weizsäcker, C. F. v. (1971). *Die Einheit der Natur (The unity of nature)*. München: Hanser.
Welsen, P. (1987). "Freudlektüre" und "philosophische Freudinterpretationen" (Reading Freud and philosophical interpretations of Freud). *Psyche - Zeitschrift für Psychoanalyse*, *41,* 699-716.
Welsen, P. (1988). "Subjekt und sein Anderes" - Subversion und Wiederaneignung des Subjektes bei Paul Ricoeur und Jacques Lacan ("The subject and the other" - Subversion and regaining of the subjects in Ricoeur and Lacan). *Philosophisches Jahrbuch*, *95,* 307-321.
Wisdom, J. O. (1967). Testing an interpretation within a session. *International Journal of Psychoanalysis*, *48,* 44-52.
Wisdom, J. O. (1970). Freud and Melanie Klein. Psychology, ontology, Weltanschauung. In C. Hanly, & M. Lazerowitz (Eds.), *Psychoanalysis and philosophy*. New York: International Universities Press. pp 327-362
Wisdom, J. O. (1972). A graduated map of psychoanalytic theories. *Monist*, *56,* 376-412.
Wisdom, J. O. (1984). What is left of psychoanalytic theory? *International Review of Psychoanalysis*, *11,* 313-326.
Wright, G. v. (1994). *Normen, Werte, Handlungen (Norms, values, actions)*. Frankfurt: Suhrkamp.
Wundt, W. (1910). *Völkerpsychologie (Psychology of nations)*. Weinheim 1987: Beltz.
Wyss, D. (1961). *Die tiefenpsychologischen Schulen von den Anfängen bis zur Gegenwart (The depth-psychological schools from the beginnings until present)*. Göttingen: Vandenhoeck & Ruprecht.