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2. Problems of Metascience and Methodology in Clinical Psychoanalytic Research

Helmut Thomä and Horst Kächele

2.1 Additional remarks some 25 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" (S. Hook, 1959 p. 214).

On re-reading this now more than 30-year-old study, we are pleased to note that it has remained current and has received significant attention, as for example in Rubovits-Seitz' (1998) recently published *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 6, "Description, Explanation and Prognosis in Psychoanalysis" (Thomä and Kächele 1973, p. 322-339). This was pointed out by von Mises (1939), with whose work we were not yet familiar in 1973 (cf. von Mises 1990, p. 345). 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. Thus, in the case of Amalia X we described the unconscious causal significance of displacement and its undoing (Thomä & Kächele 2006, also chap. 5.1 of this volume).

The probability that like disease pictures 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 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 (1969). 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: i.e., 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; cf. Welsen 1987, p. 701). Ricoeur attempts to prove that the dichotomy of energetics and hermeneutics dominates Freud's entire *oeuvre*—from the "Entwurf einer Psychologie" (Project for a Scientific Psychology) right to the "Abriss der Psychoanalyse" (Outline of Psychoanalysis), so to speak (Welsen 1987 p. 701). 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. 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" (Welsen 1988, p. 308). Freud saw in metapsychology "the consummation of psycho-analytic research" (1915e, SE XIV, 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, SE XVIII, 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' (1973) 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 1997). 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 philosopher (1984): "... [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 (English); 1969, p. 354-355, cf. also p. 383). In contrast to this catalog 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; 1969, p. 383-384). As can be gathered from a commentary by Grünbaum (English: 1984, p. 48; Ger. 1988, p. 86), 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 p. 262-264, quoted from Grünbaum 1984 p. 47).

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 (Grünbaum 1984, pp. 47-48):

> "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)."

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 (1984 p. 48):

> "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.'"

Our own efforts are thoroughly documented in volume II of the Ulm Textbook. 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 volume of the Ulm Textbook. The subtitle 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 p. 5) 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" (344/345). We remained unaffected by this syndrome, as we had long been acquainted with

[1] Mit Eagle's und Wakefield's Besprechung der Veröffentlichung von Mitchell unter dem Titel „How NOT to escape from the Grünbaum Syndrome: a critique of the ‚new view' of psychoanalysis" (Casement 2004) können wir uns hier nicht befassen.

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 (1959) 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" (1959, 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 channeled" (1959, 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 in 1986 with his article "Is Freud's theory well-founded?" Finally, in 1993 a collection of pertinent papers appeared under the title "Validation in the Clinical Theory of Psychoanalysis," which also contained an older, original publication on "The Placebo Concept in Psychiatry and Medicine."

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, i.e. 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 the tally argument, which we discuss in detail in the first volume of the Ulm Textbook (p. 465-467):

> "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).

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, who explored the problem of testing psychoanalytic theory on the couch in some depth (i.e., in practice; cf. especially Grünbaum 1984), 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"[2] 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 and that the concept of insight is fraught with methodological difficulties (cf. the survey in Roback, 1974). 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 (Garfield & Bergin, 1978).

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

[2] Freuds „Junktim zwischen Heilen und Forschen" lautet: In der Psychoanalyse bestand von Anfang an ein *Junktim zwischen Heilen und Forschen* , die Erkenntnis brachte den Erfolg, man konnte nicht behandeln, ohne etwas Neues zu erfahren, man gewann keine Aufklärung, ohne ihre wohltätige Wirkung zu erleben. Unser analytisches Verfahren ist das einzige, bei dem dies kostbare Zusammentreffen gewahrt bleibt. Nur wenn wir analytische *Seelsorge* treiben, vertiefen wir unsere eben aufdämmernde *Einsicht* in das menschliche Seelenleben. Diese Aussicht auf wissenschaftlichen Gewinn war der vornehmste, erfreulichste Zug der analytischen Arbeit (Freud 1927 a, S. 293 f.; Hervorhebungen von uns).

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. I can now add 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'* (SE XX: 109; 1926). Furthermore, we need only recall the very theme of Freud's 1937 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 (GW 10: 142; 1914; 10:210; 1915)" (Grünbaum 1988a, p. 448/449) Ger.; cf. 1984, p. 281 Eng.).

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..." (Ger. p. 554, Engl. 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 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" (SE XX: 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, Kris 1950). These latter cannot be simulated; their investigation and therapy must be conducted in a human

[3] Im Sommer 1900 trafen sich Freud und Fließ am Achensee letztmals zu einem ihrer Privatkongresse. Dort kam es, wie Fließ später schrieb, zum „Streit, weil Freud den Einfluss der Periodizität (der biologischen Bisexualität) auf die psychischen Phänomene leugnet(e)" (Freud 1986, S. 505f., Anm. 5). In seiner Verärgerung hielt Fließ Freud vor, dass er seine eigenen Gedanken in Patienten projiziere. Die Begegnung am Achensee markierte das Ende der innigen Freundschaft zwischen den beiden Männern. Freuds Sicht lässt sich anhand seiner Briefe an Fließ vom 7. 8. und 19. 9. 1901 rekonstruieren (ebd., S. 492, 494f.). Im Zusammenhang mit dem Plagiatsstreit von Fließ vs. Weininger und Swoboda (Schröter 2002), in den Freud verwickelt war, kam es später nochmals zu einem brieflichen Austausch.

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 2004; Leuschner & Hau 1995; Holt 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 a 1973 study in which we had written:

> "We share Rapaport's (1960) view that proving the validity of psychoanalytic theory is the task of an intersubjectively communicating scientific community that must reach agreement on a given practice following the rules of empirical science. In contrast 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 inclusive and by which the confirmation of the theory should take place. In any case, the logic of explanation by general interpretations points to the specific way by which the confirmation of psychoanalytic statements can alone be achieved, namely from the union of hermeneutic understanding and causal explanation: "Understanding itself gains explanatory power" (Habermas, 1968, p. 328). Applied to symptoms, constructions take the form of explanatory hypotheses... The *dissolution* of a causal connection by means of the work of interpretation illustrates the efficacy of psychoanalytic therapy. These statements are to be applied to the single case. From them prognoses are derived such that, through the therapeutic process, the original causative conditions are deprived of their foundation. The dissolution of these assumed conditions is recognizable by the changes in symptoms and behavior" (1973, p. 320, italics added by authors in quotation).

Grünbaum was disturbed 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, he confirmed for us that in the Ulm textbook (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' (Thomä and Kächele 1985, p. 27)" (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.