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.

7. A SUMMARY AND IMPLICATIONS FOR PSYCHOANALYTIC PRACTICE[1]

The Ulm Psychoanalytic Process Research Study Group, embedded within a university department and thanks to the longstanding support of the German Research Foundation (DFG), started, developed and differentiated the multi-dimensional project of research on the course of a single psychoanalytic case. Now we can look back on the successful implementation and discuss the implications of this program for clinical work and consider further perspectives.

The studies of Amalia X, probably one of the most intensive empirical examinations of the materials of one patient ever conducted, reliably identified numerous indicators of change in directions that were specified a priori. Does this allow us to say we identified mutative factors? Working with conditional predictions of the format "If this patient will be treated sufficiently long, working through her core conflictual problem areas using a patient-oriented technique then specific changes in various areas are to be expected", we are now in a position to positively answer that under these conditions of a long-term intensive treatment with an experienced psychoanalyst the patient showed clear unequivocal signs of improvement as specified beforehand.

We acknowledge the unresolved epistemological problem of psychoanalysis, that we have no consensually – agreed, independent criteria for psychoanalysis. Lacking that, the judgment of the treating senior psychoanalyst that the treatment is psychoanalysis, is the closest approximation any research group can provide. This criterion was also used by Schachter (2005a) when selecting the cases for his clinical reports on how analyst and patient view the power of the psychoanalytic treatments that transformed their lives. We think that short-term intervention which makes up the bulk of today´s clinical practice would not have been able to free this patient from her characterological constrictions, although we cannot prove it. In any case, a clinical case report of this specific analytic

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treatment, unaided by empirical studies, given the patient's and analyst's uniqueness, could not have specified these change processes with the same degree of certainty.

In order to map what the naked eye cannot see in chapter five we looked at the pre-post change of emotional insight, recorded the systematic change of self-esteem, analyzed the patient's suffering, traced the patient's intrapsychic development in her dream material, identified the fate of her Core Conflictual Conflicts across the treatment, and followed the patient's capacity to overcome separation issues. Studying the patient's unconscious plan for dealing with personal and social experiences added to our psychodynamic understanding and generated a partially alternative view of the patient's pre-oedipal experiences. Applying Blatt's (2004) distinction of analytic and introjective personality organization we would classify Amalia X as belonging to the introjective type and thus are in agreement with his recent conclusion (Blatt, 2006) that psychoanalytic work in contrast to supportive treatment is optimal for this personality organization. These approaches using the technique of guided clinical judgments were enriched by the availability of a descriptive map on the clinical course of the analysis – a map that could have been even more detailed, but was good enough to convey a thorough and detailed understanding of the clinical development (chapter four).

In chapter six, after a short introduction on the relationship of psychoanalysis to linguistic research, we have presented the ULM TEXTBANK as the first instrument of its kind. It is no longer unique, which demonstrates that such a tool is a sine-qua-non to further basic treatment research. Various studies of exploratory character testing out the potentials of computer-based approaches point to the possibilities of studying microprocesses where we always felt in good company with researchers like Dahl, Spence and some others. When the patient returned twenty five years after having terminated her analysis she allowed us to assess the impact of her parents´ death on her attachment representations, a topic that is fairly new in the field.

The question of determining on which level of the material so-called mutative factors operate, remains a difficult one. Our tentative work on the linguistic level points to the potential richness of such material that cannot been seen by the unaided eye. Current microscopic analysis in anatomy and pathology has moved to the level of cells and their infrastructures in order to identify causal mechanisms. Clinical concepts are but

stakeholders waiting to be dissected into microscopic processes (Luborsky and Crits-Christoph (1988). Today familiar concepts like “helping alliance” (Luborsky, 1976, 2000) raise new issues: how is this experience generated, by what mechanisms on what level? They may reflect the outcome of bits of interactive behavior that intuitively has to be staged by the two participants in order to generate a productive therapeutic process (Hatcher and Barends, 2006). Research has to go “beneath the surface of the therapeutic interaction” (Bucci and Maskit, 2007). Working with unconscious material like dreams may lead to subtle change in mentation reflecting activation of right brain processes as Schiffer (1998) and Mergenthaler (2008) point out. The regulation of verbal and non-verbal activities in itself may be responsible for a satisfying experience for the patient who feels contained in a precise fitting interactive synchronization (Knoblauch, 2000).

We did not attempt to explore the various roles of the analyst's suggestion. In order to clarify the issue we have to remind the reader of Freud´s discussion of suggestion. He referred to the English meaning of the word which is equivalent to the German “anregen“– to stimulate (Freud 1921c, p. 89). It may be feasible to study the impact of direct and indirect suggestive elements in the analyst's activities, to identify moments of his tonality where his subjective convictions may have played an overriding role and we invite potential researchers to examine our audio-records for such subtle effects. Although it is very likely that personal influence - for example the analyst's position as a university professor - played some role in Amalia X´s analytic treatment - as it does in all other medical treatments – research on such impact would have to focus on micro-analytic interaction patterns and require innovative research designs. This is certainly not to deny that the analyst's optimism and confidence may have contributed to the patient's therapeutic benefit from treatment.

The same is true for suggestion. One possible clue that the analyst, outside of awareness, may be shaping the patient's productions by implicit suggestions, would be that none of the patient's productions in the sessions 152 and 153 seem a surprise to the analyst. This should give the analyst pause, and lead him to explore whether he may have been making covert suggestions, perhaps outside of his awareness. The methodological approach to research this issue could be using symptom-context technique as described by Luborsky (1996).

The role of the placebo effect deserves a different discussion. For good reasons, there has never been an empirical study of this mechanism as an explanatory concept for psychoanalytic treatments, which is in contrast to the study of time-limited psychotherapies (Prioleau et al., 1983). The explanation for some of the impressive findings in those placebo-studies is that what serves as placebo–therapy is in fact minimal treatment groups, for which certain treatment benefits may be expected (Luborsky et al. 1993, p. 505). Obviously it would be hard to construct a convincing alternative treatment modality lasting five years without the patient recognizing the true nature of its being only a control condition. Therefore Grünbaum (1986b) rightfully doubts whether the placebo concept has a place in psychotherapy at all, since social interaction cannot be circumvented; no "empty pill" is available. Therefore the placebo effect is confined to pharmacological therapies and ultimately makes sense only in connection with the possibility of double blind controlled studies. In any kind of psychotherapy to imagine a double blind control condition is absurd.

Grünbaum's (1984) philosophical challenge is that psychotherapy sessions cannot be used "probatively", meaning that data for sessions cannot be used to prove any hypothesis, but only to suggest hypotheses. Luborsky et al. (1993) pointed out that Grünbaum´s thinking has not been sufficiently influenced by the probability theory that forms the basis of most current statistics" (p. XXIII). There is a significant probability that the patterns we identified are not entirely based on suggestion by the analyst. In order to explore "what can change in a good analysis" (Fonagy, 1999b) we quoted the analyst's conviction based on his understanding of the initial situation:

> The analyst offered to treat this woman, who was hard working in her career, cultivated, single and quite feminine despite the way she felt about her stigma, because he was relatively sure and confident that it would be possible to change the significations she attributed to her stigma. (see chapter four)

We have shown that such research is feasible, provided that enough devotion, passion and financial resources are provided. Psychoanalytic clinical work can be the subject of objectifying and methodologically sophisticated research. The inspection of the analytical process from an external view leads to empirical results that a treating analyst cannot achieve. Extra-clinical or so-called off-line-research can contribute to an

understanding of change mechanisms that cannot be gained in any other way. We recommend that the analyst should be involved adding his clinical perspective in such a research process, articulating his subjective responses, participating in clinical examinations or bringing in critical comments to the formal findings.

On the basis of our experiences, however, we once recommended that during the course of the treatment neither the analyst nor the patient should expect to participate in anything additional to, and external, to the treatment. The experience of other researchers, however, has shown that establishing a parallel domain by postal interviewing a patient in analytic treatment by a research group has been demonstrated not to negatively influence the process (Grande & Rudolf, 2003; Huber & Klug, 2003). The often heard argument that interventions during treatment would be necessarily deleterious, has not been confirmed. It seems more appropriate to experiment with such additional research parameters and evaluate whether they are damaging or helpful to the dyad. Our long-term experiences with tape-recorded treatments have shown us that the initial approval of the patient for additional research interventions is absolutely necessary for legal reasons; but the patient and the analyst must feel free to revoke this decision at any time. The more intrusive such interventions are, such as placing patients before, and during, analytic treatment in a neuro-scientific research framework, the more a careful clinical recording of its potential impact on the process is to be recommended. In our most recent study patients with chronic depression (Buchheim et al., 2007) are investigated at the opening of their psychoanalytic treatments and in regular intervals three times more by EEG and fMRT in a laboratory environment while presenting highly structured cues distilled from diagnostic interviews with the patients. A study group of the involved analysts share and discuss their clinical experience, functioning as a reflective environment to understand the responses of patients and analysts to this challenge (Taubner et al., 2007).

We plead strongly for a multi-dimensionality of empirical approaches to the subject of psychoanalysis; namely to conduct research on the impact of unconscious processes on conscious experience and behaviour. In relation to this research process the systematic single case study, takes it proper place — next to other ways of access.

Although a generational approach on the development of psychotherapy research (Wallerstein, 2001) is adaequate to plot main lines of research activities, we prefer a

conception in which six stages of therapeutic research are differentiated (Kächele, 2005). Then the systematic single case study is to be assigned on the one hand to the descriptive stage I in which careful, reliable descriptions are required (Messer, 2007). On the other hand, the single case study can, as the studies in this volume show, in a diverse way generate experimental data belonging to stage II that allow confirmation or disconfirmation of single case oriented hypotheses (Fig. 7.1).

{Figure 7x1 about here}

Figure 7.1 Six stages of treatment research

Therefore, we are of the opinion that the model-case Amalia X represents an example of a research-based case study which Grawe (1992) marks as an especially successful and promising way for future process research:

> …Such 'research informed case studies' …. that is case studies in which extensive process and measured change on the basis of an elaborated clinical case conception are interpreted in their entire context and in which every statement can be traced back to the base in the recorded measuring, can be viewed as a particularly promising way for future research of process. Because of the interpretation in the context of the understanding of a clinical case, the results make clinical sense; however they differ from clinical fiction in that they have a close comprehensible relation on a basis of objective measuring data, which is independent from the interpretation. (Grawe, 1992, p.140)

The studies we have reported support not only the finding that this treatment led to a diversity of changes in the experience and behaviour of the patient Amalia X, but also demonstrate the benefit of research techniques in which the findings contribute to the understanding of change processes. Research techniques provide the essential reliability

of observations that are lacking in clinical inferences. The number of descriptive dimensions, which can be examined by means of a transcribed corpus, is huge dependent only on the availability of suitable process measures. However, we can conclude that change processes exist and that these can be recorded reliably and validly. We find these in interactive dialogical exchange as well as in basic changes in the personality of the patient. These often show a linear trend. This leads us back to our meta-theoretical discussion:

> Possibly, from a logical point of view, the objection could be raised that, instead of the concept of strict causality, a statistical relation should be applied to the interdependencies indicated by psychoanalysis – perhaps in the sense that persons under the influence of certain engrams are more inclined toward Freudian slips, nervous symptoms, and dream pictures than others who are free of them, just as a dice that has been tampered with shows more sixes on the average than an unbiased one.....It is a reasonable conjecture that psychoanalytic theory would have received a more correct form, modified in this sense, if at the time of its creation the deterministic conception of all natural occurrences had not been so absolutely predominant in sciences....
>
> Psychoanalysis comprises the scientific theory of a specific area of psychological occurrences: on the grounds of objective observations it constructs a hypothetical causal connection between certain symptoms and the latent remainders of earlier experiences. Almost all objections raised against it so far are of an extralogical nature. But it seems justified to point out that the totality of observations in this field seems to correspond more to the assumption of a statistical than of a strictly causal correlation. (von Mises, 1951, p. 238)

Therewith, the statement that psychoanalytic therapy occupies itself with probabilistic states of a person is supported; in other words, the object of therapeutic efforts are the patient´s response tendencies that in the beginning show great stability (in the sense of persistent templates, chapter two in this volume), which in the course of the treatment become more and more unstable and through which changes of the system become possible. When the conditions by which a system of response tendencies is supported are known then clinically typical statements of probability are permitted. Due

to uniqueness in each individual case these conditions can also be completely different; consequently the necessity of single case studies arises as well as the known problems of generalisation (Midgley, 2006).

The formalized evaluation of treatment reports goes beyond the heuristic function of clinical description and can secure statistically significant correlations. Schneider (1983) propagated this “way toward a new understanding of the psychotherapeutic process” theoretically into therapy research by utilizing on to biological change models. Our findings show that such changes of the probability of the behaviour and the experience of a patient can hardly be reliably identified in individual sessions, have instead to be observed on the macro-systematic level of multiple sessions over time. Our empirical studies of our model case Amalia X emphasize that a long-term view of the course of treatment is essential to identify structural changes of the patient. Short range assessments using a few sessions may be useful for understanding the current interactions, but they do not provide reliable information about enduring changes in feeling and behavior. In our view only a long-term perspective over the course of the treatment can be the arbiter of success. This necessity for a longitudinal approach was also demonstrated by a number of controlled single case studies that have been published in the last years (Joseph et al., 2004; Waldron et al., 2004a, b; Lingiardi et al., 2006; Porcerelli et al., 2007).

The long standing research model dominant in clinical psychoanalysis – "Testing an interpretation within in a session“ (Wisdom, 1967) – is critically undermined if one keeps in mind that one session is but part of a series of sessions in which, at each different times different conditions exist, and therewith a great openess for possible reactions of a patient exists ("like in weather conditions“). The suggestions of the Boston Change Process Study Group (1998, 2005) about “moment-to-moment changes” are presently being discussed very vigorously (Mayes, 2005; Litowitz, 2005); however, according to our experience structurally relevant processes of change only can be identified over a longer time span of the treatment. The single response of a patient to an intervention is open to a diversity of theoretical attributions. It needs not only contextual knowledge, but also „general interpretations“ (in the sense of Habermas). These are the unavoidable theoretical models in the mind of an analyst (f.e. Kleinian, Bionian,

Kohutian, Lacanian, Relational etc: see Hamilton, 1996) that back up the handling of contextual information. This bi-directional process increases the subjective plausibility of an interpretation. Following Bowlby (1979) there is no way to bypass this process for any involved clinician. In order to overcome the self-sufficience of an understanding that confirms itself in subjective evidence, we recommend a critical attitude that might be acquired in interaction with researchers or even by finding once´s own field of clinical research.

We recommend discontinuing unending discussions of the validity of specific individual interventions and interpretations as they are only part of a larger game as implied in Freud´s metaphor of chess where a single move's value only can be determined by the state of the game. Interventions derive their status from their functional utility at any moment in the process. As has been illustrated in the micro-analysis of two sessions, an analyst's intervention strategies can be demonstrated to be successful in furthering the patient's insight in a problematic area (chapter five Comparative Psychoanalysis). However, as the presentation of this material at the New Orleans International Psychoanalytic Congress in 2004 again has demonstrated (Ireland, 2004; Wilson, 2004) alternative, divergent views are easily brought foreward. Presenting the clinical material in verbatim recorded details allows comparative, even competitive discussions which do not transgress matters of opinion however sophisticated the clinical expertise of the proponents. In order to judge the success of a psychoanalytic treatment general statements about the treatment have to be measured objectively. Only on that level would we venture to estimate the probability of validity of our findings.

Long-term course observations are essential (Thomä, 1996); only then comparative examinations can be evaluated meaningfully. The individual session can, to an outside person, certainly convey quite a lot about the applied technique and the up to date standing of transference and countertransference; but, as with a magnifying glass, one easily loses the view of the whole matter. Only the systematic examination of the process generates demonstrable statements that can also withstand the critical view of outsiders.

In order to identify such effects we need sophisticated measurement techniques reaching beyond the ones Galatzer-Levy et al. (2001) listed. As Bucci (2007a) points out

we need in addition to the clinical evaluations by external observers a broad range of reliable and valid empirical process measures: “These will include measures to be applied to the verbatim transcripts and also measures applied to tape recordings that examine the nonverbal aspects of the clinical interaction, including emotive and other paralinguistic vocalizations, pausing, vocal tone and modulation” (p. 200). New technologies like phonological analysis based on voice recordings have hardly been tried; they might be attractive supplemental approaches especially for the detection of countertransference responses (Dahl et al., 1978). Recordings of facial activity only work in a face-to-face setting. In the couch setting mimic expression is hardly used as a communicative channel. Linguistic techniques in all their diversity are still the best way to tap these micro-processes (see also Blatt and Auerbach, 2003).

We quoted Habermas (1971a) stating that individual interpretations cannot be supported or rejected; they only can be applied by the patient to himself leading to that kind of narrative truth which makes up the intriguing quality of psychoanalytic experience. „General interpretative strategies“ however may fail or not in the long run. In this vein our theories like old soldiers never die; they just wither away.

For example, in the context of existing pluralism in psychoanalysis we are witnessing changes in paradigmatic frames of references. Interpretative activities based on the conception of drive psychology or ego psychology are on the decline; intersubjectivity is on the rise – for better or worse. These changes in the psychoanalytic intellectual climate are not research-based or evidence-based, but may reflect societal change. It is part and parcel of research

> „to open the questioning as to what is specific to psychoanalytic theory and technique, recognize the theoretical vacuum that still exists in psychoanalysis which Thomä and Kächele (1987), Holt (1985), and others have referred, and work through ist implications“. (Bucci, 2007a, p. 203)

Scientific results must be repeated in order to establish their value. In this sense we hope that there will be subsequent examinations of individual psychoanalytic cases. However, at this time the impact of conducting our own research efforts on our own psychoanalytic thinking has been enormous. Nothing enriched our thinking and doing as much as the discussion of our detailed reports by friendly critics and critical friends.

Investigations that relate to what happens in psychoanalytic treatment are presently highly important and "quantitative research" is no longer a stepchild of the psychoanalytic profession, as Luborsky and Spence in 1971 wrote. The successful launching of an IPA Committee on Research, research sections in our journals, annual poster session at APsaA´s meetings signal a definite change in climate towards empirical research. Still one encounters a common response to empirical research: "Does this finding agree with clinical knowledge?". This scepticism may contribute to the fact that although the leadership of the APsaA increasingly verbalizes about the importance of analytic research, the budgetary allotments for analytic research of the Association remains unchanged at 3-4%.

We do not share the position of Mijolla (2003), a historian of psychoanalysis, claiming that the phase of objective research ended when Freud began his self-analysis. Even a perfunctory view of the one hundred year old history of psychoanalytic research shows that neither training analysis, nor the subsequent self-analysis can replace scientific thinking and acting. We definitely prefer John Bowlby´s admonition which differentiates the role of the scientist and the clinician:

> „In his day work it is necessary for a scientist to exercise a high degree of criticism and self-criticism: and in the world he inhabits neither the data nor the theories of a leader, however admired personally he may be, are exempt from challenge and criticism. There is no place for authority. The same is not true in the practice of a profession. If he is to be effective a practitioner must be prepared to act as though certain principles and certain theories were valid; and in deciding which to adopt he is likely to be guided by those with experience from whom he learns. Since, moreover, there is a tendency in all of us to be impressed whenever the application of a theory appears to have been successful, practitioners are at special risk of placing greater confidence in a theory than the evidence available may justify. (Bowlby, 1979, p. 4)

Implications for psychoanalytic practice

From decades of intensive study of many facets of treatment, what stands out the most is the limitations of our clinical knowledge about analytic treatments. This is the lesson we would like to impart to practitioners. As Bowlby has noted, analysts are

inclined to place greater confidence in their theories and analytic views than are warranted; this, indeed, is risky. In conclusion, the most salient implication for psychoanalytic practice that we can identify from our empirical study case is that rather than the analyst making sweeping inferences and drawing strong conclusions, we urgently suggest that humility and tentativeness in all interventions is optimal. Analysts' need for confidence and conviction may expose them to a tendency toward arrogance, often more covert than overt, for at least hypothetically understandable reasons.

This need for certainty may arise from analysts having underlying feelings of uncertainty - probably unconscious - about the difficult work they do, with treatment guidelines less and less clear and widespread unresolved diversity of views about analytic theory and practice. Too often this uncertainty is defended against by compensatory feelings of knowing all about analytic treatment, or, as Jonathan Lear termed it, „Knowingness". This view is also supported by Casement: "The more experienced we are, we need to be able to recover a position of non-certainty. For in my opinion, it is only thus that we can keep the analytic space free from preconception" (2007, p. 1). Thus the analyst needs enough confidence to be effective in treatment, but not so much confidence that it merges into arrogance - a challenging dialectic for an "impossible profession" (Malcolm, 1980).

The current unresolved differences about what constitutes the fundamental tenets of psychoanalysis strongly suggests, and is supported by empirical data (Schachter, 2002) that none of the conflicting psychoanalytic theories have been validated. If that is the case, to view with "certainty" any particular analytic theory and to base the analyst's confidence upon that theory is misplaced and self-deceiving. Such "certainty" can provide only a spurious feeling of confidence about analytic work for the analyst.

What other sources can provide the analyst with a necessary sense of confidence about analytic work? Leichsenring, recently reviewing the literature, concludes: several controlled quasi-experimental effectiveness studies showed that psychoanalytic therapies fulfill the criteria (A) [A treatment has proved to be superior to a control condition – placebo or no treatment] or (B) [To be as effective as an already established treatment]. These studies included control groups for which comparability with the psychoanalytic treatment groups was ensured by measures of matching, stratifying or statistical control

of initial differences. In all these studies, psychoanalytic therapy was significantly superior to the respective control condition, including shorter forms of psychodynamic therapy (Leichsenring, 2007).

At a more personal level, the analyst may have found that his/her own training analysis produced therapeutically helpful changes. In addition, the analyst probably had succeeded in being helpful to prior patients. Therefore, this empirical and experiential evidence, taken together, makes it plausible and realistic to believe that if the analyst is concerned about and cares about the patient, and is genuinely trying to be helpful (as Thomä was with Amalia X) the analyst can be reasonably confident that he/she will succeed in being helpful to many of his/her patients.

Psychoanalytic treatment, however, is a difficult enterprise under the best of circumstances, in part because the personality of the analyst is so intrinsically involved in the process. The context within which the treatment is conducted is likely to influence its course. Whether it is the personal context of analyst or patient, or the societal context. Germany, where Amalia X was treated, provides an unusually supportive context for analytic treatment. It is no accident that the intensive, long-term, multi-disciplinary, expensive studies of Amalia X were possible in Germany. In other countries, the societal context is less supportive. Probably the greatest contrast is with the United States where psychoanalysis has been steadily declining in status and prestige. The United States goverment provides no reimbursment, directly or indirectly, for psychoanalytic treatment and private insurance provides little reimbursement. The number of patients in psychoanalytic treatment has been slowly but continously decreasing. We estimate that currently the 3500 members of the APsaA have in psychoanalytic treatment at four or more sessions per week a total of 6000 patients in a nation of 300 million people. In contrast, in Germany there are approximately 200,000 patients in psychodynamic psychotherapy of 1 session per week and about 40,000 patients in psychoanalytic therapies of 2-3-4 sessions per week in a nation of 80 million people. It should come as no surprise to psychoanalysts that conducting analytic treatment in a context or atmosphere of criticism and depreciation, whether at a societal or personal level, is apt to intensify defensiveness both of analyst and patient. In the case of the patient, the parents or the spouse may oppose analytic treatment and ridicule it.

It is our impression that conducting analytic treatment in a context or atmosphere lacking support and including active hostility and criticism, increases the risk that both analyst and patient will become defensive. For the analyst, this increased defensiveness is likely to include conviction about knowing excactly how psychoanalytic treatment should be conducted (Schachter, 2005b). Such „knowingness“ will most probably be deleterious to the treatment. It is imperative that we remain open to innovate ideas and approches to analytic practice.

European outcome studies on psychoanalytic therapies (Richardson et al., 2004) point in a direction „that many of the traditional ideas concerning psychoanalytic psychotherapy will need to be revised“ (Fonagy, 2004). The German studies (Huber & Klug, 2003, 2007; Leuzinger-Bohleber et al., 2003a; Leichsenring et al., 2005; Grande et al., 2003, 2006) show that little differences in symptomatic improvements between low and high dose of treatment can be ascertained, but that the gains in structural changes are the field where the battle will be won or lost (Jakobsen et al., 2007).

Conclusion

Presentation of our studies and their results may be of differing relevance for clinicians. “Bridging the gap” between practice and research has long been called for (Talley et al. 1994). The controversy should not center around “clinical conviction or empirical evidence?” (Dahlbender and Kächele, 1999); instead the crucial demanding task consists in reconciling empirical knowledge and clinical experience (Soldz and McCullough, 1999). We want to encourage other psychoanalysts to make their private work accessible to the scientific public. We also strongly recommend educating young scientists in acquiring sufficient clinical experience as has been recommended by Kernberg (1986), Thomä (1993) and Thomä and Kächele (1999). At the same time**,** the training of experienced clinicians in quantitative and qualitative research methods is necessary (Teller and Dahl, 1995). The success of the Research Training Program initiated by the Research Committee of the International Psychoanalytic Association demonstrated the feasibility and acceptance by younger and more senior analysts. These meeting have turned out to provide “creative alliances” that enhance the interfaces between various forms of psychoanalysis and a multitude of research approaches (Hauser, 2004). We need psychoanalysts as clinicians and researchers who bring with

them the strength to make steady and cumulative progress. We need institutions that make such scientific teams possible. To produce a cadre of researchers sufficient in numbers to address empirically the scope of unresolved analytic principles, it may be necessary fundamentally to transform psychoanalytic education (Teller and Dahl, 1993). All teaching of candidates should be done jointly by researchers as well as clinicians, and candidates should be expected to become knowledgeable about analytic research as well as knowledgeable about analytic practice. Admittedly, this would constitute a drastic transformation in psychoanalytic education. We believe that the world-wide scope of stress on psychoanalysis and the trajectory of decline in status and prestige constitute a drastic situation, and drastic situations require drastic changes. The broad implementation of such scientific activities will decisively enrich psychoanalysis, and foster its growth and development Some years ago the editors of an important handbook for clinical practice on "Psychodynamic Treatment Research" promised to their readers that this volume would inform about the manner in which Freud´s treatment concepts have been ingeniously operationalized and validated:

> The translation of rich, multifaceted clinical phenomena into definable variables amenable to precise and reliably measurement constitutes a critical milestone in the scientific evaluation of our field. (Luborsky et al., 1993, p. XV)

This kind of work has been the shibboleth of our own efforts. In this handbook Wallerstein (1993) reminds the readers of our position on testing psychoanalytic propositions:

> Thomä and Kächele (1975) note that, in addition, extraclinical testing carries its own severe limitations. They state: "If the psychoanalytic method is not employed, and the process takes place outside of the treatment situation, only those parts of the theory can be tested that do not need a special interpersonal relationship as a basis of experience, and whose statements are not immediately related to clinical practice". (p. 63).

This statement obviously endorses the view that psychoanalytic practice must be "the crucial place where the proof of its explanatory theories is to be rendered - we would not know where else they could be fully tested" (quote Thomä and Kächele by Wallerstein, 1993, p.102). It really is a matter of ecological validity.

We hope that we have been able to at least partially having fulfilled this claim.