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Remarks on the first century of the International Psychoanalytic Association and a utopian vision of its future[1]

HELMUT THOMÄ

Introduction

HORST KÄCHELE*

Helmut Thomä died in August 2013, having slowly lost his personal capacities over a period of two years. His lifetime achievement consists in his arduous endeavor to further the scientific basis of psychoanalytic therapy.

Born in 1921 in Stuttgart, Helmut Thomä belonged to the generation of young men who were about to be drafted at the beginning of World War II. He chose to apply for service in the medical field and was drafted to study medicine in Berlin. At the end of the war, he served as an assistant doctor in Bavaria. At first, he gained unpaid experience in the realm of internal medicine, but subsequently he took up a trainee position at the psychiatric department of the University Clinic Freiburg in 1949. A short psychoanalytic experience with Dr. Schottländer in Stuttgart – at that time the only member of the International Psychoanalytic Association in West Germany – helped him to apply for a post at Alexander Mitscherlich's newly established Psychosomatic Hospital in Heidelberg. Back then, Thomä felt encouraged to experiment with a diversity of psychotherapeutic approaches. He also enjoyed his meetings with like-minded people and intense debates about philosophical and theological issues whose central focus was the issue of German collective guilt.

Joining Mitscherlich's team in Heidelberg certainly was the most formative environment for an aspiring young psychiatrist in the early 1950s in Germany. Mitscherlich's group was heavily involved in the upcoming debates between Viktor von Weizsäcker's holistic psychosomatic theory, and the psychoanalytic discourse that Mitscherlich had initiated. During this time, Thomä published a variety of clinical papers on diverse subjects until he finally published a monograph on anorexia nervosa (1961) that would become one of the first German psychoanalytic texts published in English (1967). His relation to the Anglo-American psychoanalytic world was established during an academic year at Yale Psychiatry Institute, which was then under the leadership of Theodor Lidz. A couple of years later, Thomä spent a year in London, where he used the opportunity to undertake a short but intensive analytic experience with Michael Balint. When Thomä returned to Heidelberg, he brought with him an interest in the details of therapeutic interaction.

In 1967, as chair of the newly founded department of psychotherapy at Ulm University, Thomä was in a position to initiate the tape-recording of his own analytic treatments as an instrument for process research. Supported by substantial long-term funding from the German Research Foundation, we were able to embark on the study of what happens in the analytic space. Thomä was an avid reader of the research literature and a generous supporter of Ulm's process research field. He liked to envision himself as a research-minded clinician that tended to critically identify problematic areas in clinical theorizing and practice. His many publications – mainly in his German journal *Psyche* and in the *International Journal of Psychoanalysis* – are marked by an enthusiastic engagement with the psychoanalytic cause

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and a pivotal critical attitude toward the weak points of theory and practice. His companion in this attitude was Merton Gill, with whom he shared the deep opinion that only by the study of recorded sessions could the problems of theorizing be solved.

After 15 years of close collaboration, Thomä and I jointly published the two-volume Ulm textbook on psychoanalytic therapy, first in German (1985/1988) and then English (1987/1992), which anticipated the American move to intersubjective and relational psychoanalytic thinking. The book's worldwide success – having now been translated in more than 10 languages – shows that the psychoanalytic community is heading toward greater openness and self-critical evaluation.

The paper in this journal, which Helmut Thomä finished in the spring of 2010, was his last effort to spell out his deep concerns for the future development of psychoanalysis. It was painful and even ironic to him that the reviewers of the *International Journal of Psychoanalysis* could not appreciate his deep concerns. Instead, they pointed to the paper's scholarly weaknesses. I very much appreciate that the *International Forum of Psychoanalysis* is publishing this outlook on the future of psychoanalysis and celebrating him as one of the outstanding figures of German post-war psychoanalysis.

More details on Thomä's scientific works can be found at www.horstkaechele.de.

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Author

Horst Kächele is the former chair of the department of psychosomatic medicine and psychotherapy at Ulm university and a training and supervising analyst of the German Psychoanalytic Association (DPV). He now teaches at the International Psychoanalytic University in Berlin. He has collaborated with Helmut Thomä for 40 years.

PART 1

Abstract

The International Psychoanalytic Association (IPA) was founded in 1910, the same year that Freud's paper on "wild" analysis appeared. Most important for understanding the history of the psychoanalytic movement is a coincidence: that in this same year too the famous Flexner Report was published, which led to the rise of American medicine when it ended privately organized medical training in the USA. This report applied the Humboldtian idea of the unity of teaching and research to medicine – supplemented by treatment – and forced universities to include medicine in their academic life.

And so in 1910 began the tragic history of psychoanalysis, which exists due to the "exclusion of psychoanalysis from universities" (Freud, 1919). Up to the present day, this exclusion has been responsible for most of the contemporary negative aspects of the psychoanalytic movement. It is the history of an unfortunate 100-year-old love affair that is discussed in detail in Part 1 of this article under four headings: "Wild" analysis and the foundation of the IPA; Psychoanalysis and the university; Clinical intersubjectivity; and From the historical "Controversial Discussions" to "True Controversies."

Key words: *countertransference, history of psychoanalysis, intersubjectivity, psychoanalytic education, research, transference*

"Wild" analysis and the foundation of the IPA

The apologetic justification for the formation of the IPA runs as follows: the IPA was meant to protect patients against the dangers of "wild" psychoanalysis, guarantee the integrity of the new doctrine, and defend it against hostile attacks from society and science. Here was the beginning of the psychoanalytic movement, and its cohesion was to be secured on the basis of the recognition of certain fundamental assumptions.

The relevance of Freud's account "On the history of the psycho-analytic movement" (1914) was reaffirmed by Makari's (2008) opus on the history of medicine (see also Wurmser, 2008). My rereading of Freud's text impressed me equally in terms of its design and of its style of argument; Freud appears as an average person in his description of his reaction to personal attacks. On another note, Freud used the forum of the second IPA congress in Nurnberg in 1910 to recommend the relocation of the centre of psychoanalysis from Vienna to Zurich, a decision that appears all the more impressive in light of the fact that Freud is the founding father of psychoanalysis. The act illustrates Freud's willingness to put the cause before his personal interests. Which great inventor or discoverer has the ability to entrust his creation to the care of others? Freud provides the following reasoning:

> The Zurich group thus became the nucleus of the small band who were fighting for the recognition of analysis. The only opportunity of learning the new art and working at it in practice lay there. Most of my followers and co-workers at the present time came to me by way of Zurich, even those who were geographically much nearer to Vienna than to Switzerland. (Freud, 1914, p. 27)

Partly as a result of the association experiments conducted at Burghölzli, "[the] first bridge linking up experimental psychology with psychoanalysis had been built" (Freud, 1914, p. 28). With regard to Jung, however, Freud would be grossly mistaken[1]:

> What I had in mind was to organize the psycho-analytic movement, to transfer its centre to Zurich and to give it a chief who would look after its future career. … he seemed ready to enter into a friendly relationship with me and for my sake to give up certain racial prejudices which he had previously permitted himself. (Freud, 1914, p. 42f.)

Freud seemed to have been of the belief that Viennese anti-Semitism, which had severely impeded the acceptance of psychoanalysis, would be marginalized under a non-Jewish Swiss president.

The Viennese psychoanalytic group objected to Freud's proposal, but the following compromise was achieved. In Freud's words:

> The object of the Association was declared to be 'to foster and further the science of psycho-analysis founded

[1]Jung advanced to the position of editor of the *Zentralblatt für Psychotherapie* following Ernst Kretschmer's decision to resign as the journal's publisher in response to the expulsion of Jewish colleagues. As early as 1933, Jung published the following anti-Semitic editorial: "In my opinion it has been a grave error in medical psychology up until now to apply Jewish categories – which are not even binding to all Jews – indiscriminately to Germanic and Slavic Christendom. Because of this the most precious secret of the Germanic peoples – their creative and intuitive depth of soul – has been explained as a morass of banal infantilisms, while my own warning voice has for decades been suspected of anti-Semitism. This suspicion emanated from Freud. He did not understand the Germanic psyche any more than did his Germanic followers. Has the formidable phenomena of National Socialism on which the whole world gazes with astonished eyes, taught them better? Where was that unparalleled tension and energy while as yet no National Socialism existent? Deep in Germanic psyche" (C.G. Jung, December 1933, quoted by Makari, 2008, p. 416).

> by Freud, both as pure psychology and in its application to medicine and the mental sciences; and to promote mutual support among its members in all endeavors to acquire and to spread psycho-analytic knowledge'. The scheme was strongly opposed only by the Vienna group. Adler, in great excitement, expressed the fear that 'censorship and restrictions on scientific freedom' were intended. Finally the Viennese gave in, after having secured that the seat of the Association should be not Zurich, but the place of residence of the President for the time being, who was to be elected for two years. (Freud, 1914, p.43)

The alienation between Freud and Jung increased. After the separation of the Zurich Group associated with the Psychiatric University Hospital Burghölzli, Binswanger remained the only Swiss member of the IPA.

Bleuler had published his revolutionary work *Dementia praecox or the group of schizophrenias* in 1911. His withdrawal from the IPA had especially unfortunate ramifications for the acceptance of psychoanalysis by the German-speaking psychiatric community. This most likely explains Freud's special attention to Bleuler's three position papers on psychoanalysis. He summarizes Bleuler's appraisal of depth psychology[2] as follows:

> Bleuler rallies his forces in the face of the attacks made on him for having introduced psycho-analysis into his book on schizophrenia, and makes what he himself calls a 'presumptuous claim'. 'But now I will make a presumptuous claim: I consider that up to the present the various schools of psychology have contributed extremely little towards explaining the nature of psychogenic symptoms and diseases, but that depth-psychology offers something towards a psychology which still awaits creation and which physicians are in need of in order to understand their patients and to cure them rationally; and I even believe that in my Schizophrenia I have taken a very short step towards that understanding. The first two assertions are certainly correct; the last may be an error.' Since by 'depth-psychology' he means nothing else but psycho-analysis, we may for the present be content with this acknowledgement. (Freud, 1914, p. 41)

Bleuler's doubts over the explanatory value of libido theory with regard to psychotic illnesses, which was current at the time, were entirely justified. And although he held Freud's discoveries in high regard, he was very skeptical of their speculative aspects. One can only regret in retrospect that he was subsequently lost to the movement. The crisis at the time was traumatizing:

> Jones found the Freudian movement in peril. At the next congress, it seemed quite possible that Freud and his followers would lose control of the I.P.A., the two-year-old organization they had founded to protect them. The Zentralblatt was in Stekel's hands, while the Jahrbuch was controlled by Jung. Jones along with Otto Rank and Sándor Ferenzci, began to brainstorm: What could the loyal Freudians do? (Makari, 2008, p. 282)

In this situation, Freud is said to have been delighted about the idea of a "secret council" of the best and most trustworthy. Jones did not emphasize secrecy:

> but Freud emphasized it: 'this committee had to be strictly secret in his existence and in his actions.' … The members were Ernest Jones, Sándor Ferenczi, Karl Abraham from Berlin, and two loyal Viennese, Otto Rank and Hans Sachs. Distrust among the paladins was apparent when Ferenczi welcomed Rank by pointedly asking: 'I suppose you will stay loyal?' (Makari, 2008, p. 283f.)

With the help of the "secret council," "Freud fully controlled a winnowed but more theoretically homogenous Freudian movement. The process of internal purification that began in Nuremberg had run its course" (Makari, 2008, p. 291). A secret council was naturally not in accordance with the scientific ideals that Freud had advanced in his account of the history of the psychoanalytic movement (Freud, 1914, p. 58). It was, however, useful in that crisis situation. The secret council would break up at the beginning of the 1920s when Rank introduced his "birth trauma" hypothesis, incompatible with the academic sensitivities of the group.

I will now turn to a function of the IPA that nobody could have predicted, and that foiled the intent of the American Psychoanalytic Association to declare independence. The Paris congress of 1938 had originally been chosen as the occasion for moving forward with the split. Plans changed, however, in response to the annexation of Austria by Hitler's Germany. Lawrence Kubie called together the group of dissidents that did not wish to adhere to the training requirements of the IPA to solve a much more urgent problem: an Emergency Committee had to be created to save Jewish colleagues from the Nazis:

> in the end, the Emergency Commission secured over 47,000 dollars. Working with Jones, Kubie and the Americans gave financial support to some 68 individuals, provided affidavits for 82, and were in contact with

[2]My original assumption that Bleuler had diplomatic reasons for using the term "depth psychology" as opposed to "psychoanalysis" probably does not hold. Rather, the term "depth psychology" was coined by Bleuler and gained the attention of Freud (letter from Sigmund Freud to C.G. Jung, February 29, 1912; see also the corresponding footnote referring to Bleuler in McGuire, 1974, p. 489).

> another 136. By 1943, 149 exiled psychoanalysts and psychiatrists had been relocated somewhere in the United States. (Makari, 2008, p. 474)

When Schafer (1985) wrote about wild analysis 75 years after Freud's essay, pluralism and incompatibilities were growing within the IPA. Schafer investigated the systems of Melanie Klein, Heinz Kohut, and Merton Gill. He suggested replacing "wild" by "comparative" psychoanalysis as his method of investigation, and made the following pertinent comment:

> One must acknowledge the fundamental propositions one has accepted as true before beginning a critique of wildness. This acknowledgement is required because there can be no theory-free and method-free vantage point from which to assess in some absolute manner competing approaches and the often diverse phenomena to which they give rise or which they require to be emphasized. All too often in our field, debate has been conducted simply on the basis of unqualified assertions concerning "the facts" or the "correct method." This kind of debate or pseudo debate is incapable of resolving controversy. (Schafer, 1985, p. 277)

Contrasting "true propositions" and "unqualified assertions" gives rise to a series of questions, which confront contemporary psychoanalysis. We all face the problem that "comparative psychoanalysis" makes it necessary to have criteria that make comparisons meaningful (Thomä & Kächele, 2007). It is striking that the question of comparative psychoanalysis appears contemporaneous with investigations on the competencies of analysts (Tuckett, 2005).

Freud offered two definitions of psychoanalysis, an early and a late one: according to the early definition, one only needs to recognize the existence of transference and resistance as basic experiences no matter which conclusions are being drawn (Freud, 1914, p. 16). Freud later augmented the definition. As *Corner-stones of psycho-analytic theory* one has to recognize:

> unconscious mental processes, the recognition of the theory of resistance and repression, the appreciation of the importance of sexuality and of the Oedipus complex — these constitute the principal subject-matter of psycho-analysis and the foundations of its theory. No one who cannot accept them all should count himself a psycho-analyst. (Freud, 1923a, p. 247)

It takes several years to recognize the extent of the emotional meaning contained in the definitions of these cornerstones. Nothing makes this aspect of recognition clearer than the Hebrew word *Shibboleth*. Freud used it to characterize the analyst's sense of group affiliation (Freud, 1914, 1923b, 1933). According to the biblical story (Judges XII), the correct pronunciation of the word *Shibboleth* decided between life or death when a person wanted to leave a beleaguered town during a siege.

I am a German psychoanalyst of the first post-war generation. This has led me to place special emphasis here on the Ulm textbook (Thomä and Kächele, 1987, p. xvii) in relation to the difficulties of providing a constructive criticism of the history of psychoanalysis due to unconscious reasons associated with the Holocaust.

Psychoanalysis and the university

The ramifications resulting from the exclusion of psychoanalysis from the academic arena are still visible today. In his work *On the teaching of psychoanalysis in universities*, Freud articulates a politically motivated intervention to the potential appointment of Ferenczi as the first chair of psychoanalysis in Budapest (1919).[3] The following ambiguous position is presented in the first section:

> The inclusion of psycho-analysis in the University curriculum would no doubt be regarded with satisfaction by every psycho-analyst. At the same time it is clear that the psychoanalyst can dispense entirely with the University without any loss to himself. For what he needs in the matter of theory can be obtained from the literature of the subject and, going more deeply, at the scientific meetings of the psycho-analytic societies as well as by personal contact with their more experienced members. (Freud, 1919, p. 171)

The quote clearly points to the history of a disappointed love. It compounds the impression that Freud was proud of the independence of psychoanalytic training, without sacrificing the hope of a late repatriation:

> As regards practical experience, apart from what he gains from his own personal analysis, he can acquire it by carrying out treatments, provided that he can get supervision and guidance from recognized psycho-analysts. The fact that an organization of this kind exists is actually due to the exclusion of psycho-analysis from Universities. And it is therefore evident that these arrangements will continue to perform an effective function so long as this exclusion persists. (Freud, 1919, p. 171)

[3]The letter to Lajos Lévy had until recently only been accessible as a translation from Hungarian into English and from English back into German. However, the German original version was discovered by M. Schröter (2009) in the estate of Max Eitingon.

Despite its exclusion from universities, psychoanalysis remained without competition for many years. On the occasion of its 10-year anniversary, the Berlin Psychoanalytic Institute, started by the foundation of Max Eitingon in 1920, presented an impressive report (Radó, Fenichel, & Müller-Braunschweig, 1930) dedicated to the founder. The report, which has remained unknown to most German analysts, conveys a sense of the destruction after 1933. I shall only cite the contribution by Zilboorg, who left Russia as a Jew in 1919 and subsequently provided an exaggerated tribute to the research culture of the Berlin Institute from his perspective as an American analyst:

> Half a dozen full-professorships for psychoanalysis at one or the other of the traditional universities would mean less and bare less [than the one Berlin Institute], because any official recognition of such a completely *free* discipline as psychoanalysis is by the established sciences would turn psychoanalysis into dogmatism and simplification. (Zilboorg, 1930, p. 69; original in English, translated by current author, emphasis in the original).

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Clearly, Zilboorg experienced a special liberty at the Berlin Institute. This of course begs the question of how Zilboorg would judge the liberty of psychoanalytic institutes today.

Like all analysts that have chosen a university career, I belong to a minority. The majority of practicing analysts lack the resources to carry out ambitious research. The proverb of "making a virtue out of a necessity" continues to reflect, in my opinion, the ambiguous attitude of most towards university psychoanalysts. It comes as a surprise that even Eitingon, known to cultivate university links, feared dependencies. Gilman describes the reception of a position paper by Eitingon at the 9th psychoanalytic congress in Bad Homburg in 1925 in this way:

> The anxiety about intellectual synthesis was indeed a demand for psychoanalysis to avoid the strains of the Humboldtian university created not only to research and teach but also to test ideas across and within disciplines. Remain separate and remain pure, said Eitingon. His talk was heralded with a powerful round of applause and after a debate of over three hours the proposal to limit training to the institutes was accepted. (Gilman, 2009, p. 1103)

Indeed, to test ideas across and within a discipline is not only the task of the Humboldtian university – it characterizes science and research in general. Criticism of the style of psychoanalytic education institutes mostly comes from university teachers that are used to asking critical questions and testing hypotheses. Wallerstein most recently provided a fairly comprehensive overview of the previous attempts to solve the question of a "full-time" integration of psychoanalytic institutions in universities:

> Clearly, the desideratum for psychoanalytic education as for any other serious academic and professional enterprise is full-time immersion, full-time studenthood. (Wallerstein, 2009, p. 1114)

Wallerstein exposes these failed attempts forthrightly. He sees:

> obstacles in but two major domains, the one that I will call psychological, the attitudes, predilections and prejudices on both sides on this hoped for partnership, and the other, equally salient, the financial underwriting of such a program. (Wallerstein, 2009, p. 1114)

It is alarming that, already by the early 1990s, Wallerstein regarded it as futile to expect financial support for psychoanalytic research from university sources or foundations. He thus made a plea to organized psychoanalysis to raise funds amounting to several million dollars a year to finance a central research institute (Wallerstein, 1991). One should expect, in the age of globalization and regionalization, that the current conditions for establishing psychoanalytic institutions at universities vary from country to country, and they have most likely become even less favorable. The establishment of a "higher education institute for psychoanalytic training" at a university in Buenos Aires (Ferrari, 2009, p. 1139) is providing new exciting material for observation.

Clinical intersubjectivity

Freud observed resistance and transmission as a therapist. These observations led to the triadic definition of the psychoanalytic methodology (Freud, 1923a, p. 253), which preceded the description of the substantive cornerstones. Findings were always linked to individuals – doctors, other academics, or talented lay-people. The subject, the person of the analyst, received a negative designation stemming from the reduction to countertransference.

Freud attempted to eliminate the personal influence of the therapist as much as possible in order to ensure objectivity and create a "social null-situation" (de Swaan, 1980). The possibility that therapy would destroy the scientific method of psychoanalysis became Freud's constant worry. In the Anglo-American literature, this topic is discussed under the title of *The nagging persistence of Wilhelm Fliess' Achensee question* (Meehl, 1994). So as to prevent

any "contamination" from therapeutic influence, psychoanalytic epistemology, originally merely recommended by Freud as a strict and impersonal system of rules, evolved into the "pure gold" of psychoanalysis. To quote from the original:

> It is very probable, too, that the large-scale application of our therapy will compel us to alloy the pure gold of analysis freely with the copper of direct suggestion; and hypnotic influence, too, might find a place in it again, as it has in the treatment of war neuroses. But, whatever form this psychotherapy for the people may take, whatever the elements out of which it is compounded, its most effective and most important ingredients will assuredly remain those borrowed from strict and untendentious psycho-analysis. (Freud, 1919, 167f.)

In agreement with Sandler and Dreher (1996), I consider "aimless analyzing" as a "self-deception" and am therefore relieved to find a quotation from Freud about the need for a patient-oriented flexibility:

> We are also now coming to the opinion that analytic technique must be modified in certain ways according to the nature of the disease and the dominant instinctual trends in the patient. (Freud, 1910, p. 145)

Of course, one cannot base one's evaluation of Freud's work and the history of psychoanalysis on one or the other quotation. Freud himself was both modest and proud, and exposed himself to an unusual degree of self-criticism. I reread Freud's brief treatise "On transience." Most touching is the sentence emphasized by Freud about mourning an experienced loss:

> Mourning, as we know, however painful it may be, comes to a spontaneous end. When it has renounced everything that has been lost, then it has consumed itself, and our libido is once more free (in so far as we are still young and active) to replace the lost objects by fresh ones equally or still more precious. (Freud, 1916, p. 307)

Applying Freud's own thoughts on transience to his discoveries, I arrive at the following conclusion: Freud underestimated the systematic problems that were part and parcel of the introduction of a person-oriented methodology. His explanations are linked to the "Causality of fate" described by the German philosopher Habermas (1971). If one accepts that (unconscious) processes imply reasons as causes, the "causality of destiny," which Habermas took from Hegel, is a central issue within psychoanalysis. These motivational forces must be investigated in single case studies. In addition, the explanatory models that are found can only be applied to similar cases.

Presuming that all analysts think causally and search for explanations to understand their patients, the dividing line is not between an understanding of psychoanalysis as either hermeneutic art or empirical science. Instead, there are differences in the attitudes towards causality: in practice, only statements of probability and inductive and statistical explanations are possible; deductive explanations from "covering laws" are impossible (Cavell, 1988, 1998; Chassan, 1960; Strenger, 1991; von Mises, 1951; von Wright, 1971). As the psychoanalyst Fonagy (2003) stated, "facing the logical weaknesses of our position we have tended to raise the status of clinical theories to laws" (p. 19). The psychoanalytic methodology has contributed to overcoming the dichotomy between social and natural sciences. It thus seems all the more surprising that polemic discussions remain on the classification of psychoanalysis as a science nestled between the two poles of "understanding" and "explaining" (Luyten, Blatt, & Corveleyn, 2008).

For decades, many psychoanalysts were highly reluctant to criticize Freud in any major way – a position that I shared for many years with the London analyst Klauber. In contrast, the "Neo-Freudians", for instance those at Columbia University, had already initiated their critical points of view in the late 1940s. Seen from the vantage point of today, a critique of Freud's therapeutic and scientific model is necessary for a renewal of psychoanalysis. Klauber (1981) believed that psychoanalysts had not been fully able to accept Freud's death. The unconscious processes associated with this led on the one hand to a restriction of our own thinking, and on the other hand to the inability to perceive how transient all scientific, philosophical, and religious ideas are, Freud's theories among them. Klauber's interpretation provides an explanation for the fact that rigidity and revolt run parallel in the history of psychoanalysis, and that the question of the psychoanalyst's identity has been the focus of interest for quite some time (see Thomä, 2009b).

The *Identity of the psychoanalyst* (Joseph and Widlöcher, 1983) became the topic of an IPA symposium in 1976. This in itself shows that analysts felt they could no longer rely on their identification with Freud. The world of psychoanalysis had changed. New ideas no longer led to splitting off and expulsion. Kohut, for instance, discovered the "tragic man" instead of Freud's "guilty man." More importantly, he was already giving empathy a special place in the psychoanalytic method at the beginning of his deviation from ego psychology (Kohut, 1968). Through his discovery of self-object transference configurations, he introduced the therapeutic recognition of patients' fundamental needs (Kohut, 1984). So one of the reasons why psychoanalysis

undergoes changes is that original contributions by psychoanalysts themselves have demonstrated the transient nature of some of Freud's ideas.

One more example of the relevance of science history is the conversion of Gill from the Saul of metapsychology (Rapaport & Gill, 1959) to the Paul of tape-recording. The widening scope of psychoanalysis led him to distinguish intrinsic and extrinsic factors in defining differences (Kächele, 2010; Migone, 2000;). Gill's criticism (1982) of the theory of the transference as pure distortion brought the intersubjectivity of the psychoanalytic methodology and all corresponding research problems to the fore. The perceptions of patients during the transference are, to say the least, "plausible." The early observation by Freud that transference affiliates with the real characteristics of the person or the relationship to the doctor contains a central meaning for Gill. Freud describes the interactional emergence with the help of paranoid perceptions:

> they do not project it into the blue, so to speak, where there is nothing of the sort already ... and they, too, take up minute indications with which these other, unknown, people present them, and use them in their delusions of reference. (Freud 1922, p. 226)

The one-sided consideration of intrapsychic conflicts marginalizes the interactional, interpersonal understanding of therapy. Laplanche (1987/1989) famously in his "General theory of seduction" and in his book *La Revolution copernicienne inachevé* (1992) draws theoretical conclusions out of the bipersonal understanding of transference.

The differentiated description of transference inevitably results in discrimination, and thus in evaluations that are dependent on different positions. The link to real conditions does not obviate distortions. Patients who search the environment with a looking glass and are able to hear the grass grow are guilty of such incredible exaggerations that one is simply obliged to speak of distortions. Gill could thus draw on Freud's original understanding of the triggering of transference through real perceptions. Freud's term "transference" was directed at the past, and all that was created new remained open. Equally, this understanding of transference as distortion, while reducing the psychoanalytic encounter to the transference in its "non-objectionable"[4] part, robs the analyst and the patient of the common ground on which they can walk and talk. Freud pleaded the case for an asymmetry, which he did not adhere to in his own practice and which is neither therapeutically informed nor scientifically appropriate.

The polarization between the "copper" of direct suggestion and the "pure gold" of psychoanalysis is also a misleading metaphor. Loewald "is one of those rare figures in psychoanalysis who have managed to be intellectual revolutionaries with none of the trappings that usually accompany a revolution" (Cooper, 1988, p. 15; see also Chodorow, 2008). He spoke, unnoticed by many analysts, as if in an aside of the "new object" (Loewald, 1960). In fact, he rehabilitated the analyst with all the contingent consequences as an excluded subject. The "intersubjective turn" (Altmeyer and Thomä, 2006), which seems to only get its full appreciation in our own time, also changes the degree of asymmetry in the unique psychoanalytic encounter, without abrogating it. The intersubjective composition of the encounter enables an even more intense analysis of the transference than does Freud's monadic and wrong therapy model. The experimentum crucis of a therapeutically proven intersubjectivity can be illuminated with an understanding of self-disclosure (Thomä, 2007). This is a misnomer because it is not personal confessions but exclusively impartations to the patient that are part of the patient's "*Funktionskreis*" (experiential circle) and that thus complement interpretation.[5] These confessions form a part of the professional self. The analysis requires a step-by-step assessment of the qualitative changes in the patient's experience and thinking . Especially important, of course, is investigating the reasons for a standstill or even for negative therapeutic reactions. This is the context of the so-called "inseparable bond":

> In psycho-analysis there has existed from the very first an inseparable bond between cure and research. Knowledge brought therapeutic success. It was impossible to treat a patient without learning something new; it was impossible to gain fresh insight without perceiving its beneficent results. Our analytic procedure is the only one in which this precious conjunction is assured. It is only by carrying on our

[4]Freud introduced the "unanstößige Übertragung", translated by Strachey as "non-objectionable transference," as the parental basis of the psychoanalytic encounter. Obviously, Freud was fully aware that a common ground is necessary between patient and analyst from which to look upon and to explore the transference. Not to analyze a patient's "nonobjectionable transference" is, according to Segal (2006), to support him living as a liar. This brings the psychoanalytic method into equivalence with the metaphor of Münchhausen, who drew himself out of the morass by his own hair.

[5]The German word "*Funktionskreis*" refers to Jakob von Uexküll's biological description of the specific world of animals. Viktor von Weizsäckers "*Gestaltkreis*" has similarities with it. In their transference, patients expect reactions from their analyst and test him as described by Weiss and Sampson in their control mastery theory (1986). The "blank screen analyst" does not exist. Self-disclosures are necessary for patients to become aware of their *Funktionskreis*.

analytic pastoral work that we can deepen our dawning comprehension of the human mind. This prospect of scientific gain has been the proudest and happiest feature of analytic work. Are we to sacrifice it for the sake of any considerations of a practical sort? (Freud, 1927, p. 256)

Freud had not argued for this unity of therapy and research in 1912. By 1927, his recommendations had been weaned – alas, neither in the interest of the therapeutic function nor in the sense of a scientific description of the psychoanalytic processes. In Freud's comparison, it is the "evenly hovering attention," as an opening for new experiences, that is contrasted with a biased disposition. The "evenly suspended attention" in turn naturally only floats until it comes to rest somewhere, a metaphor we chose in the Ulm textbook (see Thomä and Kächele, 1987, pp. 236ff.). The supposedly context-free "just analyzing" is replaced by the analyst's interventions, which are strongly determined by the theory to which the analyst adheres. The "self-deception" (Sandler & Dreher, 1996, pp. 1–2) has negative consequences in terms of the analyst's deliberation: it exonerates him from the obligation to make tentative assumptions about the relationship between his interpretations and their aims.

Freud emphasizes the impossibility of gleaning new insights without realizing "beneficent results" (Freud, 1927, p. 256), that is, changes in the patient. Whether these actually occur is, by today's perspective, needy of verification. It is not self-evident that an inevitable link should exist between "method" and "result." Clinical research in psychoanalysis assumes observations in the context of micro-steps and macro-results, which must be presented in interaction reports in order to convince third parties. As early as 70 years ago, Susan Isaacs, a student of Klein, observed:

The question of the criteria by which we test the validity of our convictions in analytical work is one of great practical importance in the day to day carrying on of our work ... It enters into the discussion of controversial issues between analysts ... Lastly it is of central importance in the statement of our theory for the non-analytic public, who have the right to challenge our premises and conclusions and to be shewn our methods of testing and verification. (Isaacs, 1939, p. 148)

I consider Isaacs' early publication to be a methodological role model for "True Controversies." The Psychoanalytic Electronic Publishing (PEP) Archive lists only 25 publications in 70 years that quote Isaacs' work. Remarkably, none of the authors of these 25 publications is a Kleinian. Isaacs's early death is not the main reason why this contribution of a critically thinking, academically schooled psychoanalyst has been "overlooked" for so long. Isaacs' lecture from 1943 about "unconscious *ph*antasies" (Isaacs, 1948) during the "Controversial Discussions" probably contributed to Isaacs' fundamental contribution to methodology having been "overlooked." "Isaacs introduced the spelling of *unconscious phantasy* to indicate the specific meaning that fantasy has in the Kleinian literature" (Hinshelwood, 2007, p. 1484). This idea seems to enable a direct access to the imaginative representatives of the libidinal and aggressive drives. Isaacs' earlier publication, however, demands evidence of unconscious phantasies. Providing such evidence placed requirements on research that could not be provided for in the first century of the IPA in "free-standing institutes" for the training of practicing analysts.[6] This scientific position focuses on the single case research as detailed in the book *From psychoanalytical narrative to empirical single case research* (Kächele, Schachter, & Thomä, 2009), written in Ulm.

From the historical "Controversial Discussions" to "True Controversies"

During the "Controversial Discussions" in London, Strachey, in a letter to Glover, called the

> attitudes on both sides purely religious the very antithesis of science. They are also (on both sides) infused by, I believe, a desire to dominate the situation & in particular the future – which is why both sides lay so much stress on the training of candidates. … But in any case it ought naturally [to be] the aim of a training analysis to put the trainee into a position to arrive at his own decisions upon moot points – not to stuff him with your own private dogmas. (quoted in Grosskurth, 1985, p. 257)

The current deep crisis, incomparable to any previous situation, is caused by a continuation and worldwide extension of "'controversial discussions" on the one hand *and* the scientifically based argumentation described by Bernardi (2002) and Eizirik (2006) as "true controversies" on the other. As with the historical "Controversial Discussions," the great questions of our time also relate to who and which school most purely represents the psychoanalytic truth.

[6] I referenced the methodological principles of Susan Isaacs (Thomä, 1967; Thomä & Houben, 1967) in my first clinical-empirical psychoanalytic research project, which Alexander Mitscherlich attempted to make into the common ground for the state-financed institutions under his auspices: the psychosomatic department of the University of Heidelberg and the Sigmund Freud Institute in Frankfurt am Main. From 1967 onwards, this project gave rise to the Ulm psychoanalytic process and outcome research on the basis of audiotaped psychoanalyses that originated particularly from my practice (see Dahl, Kächele, & Thomä, 1988; Thomä & Kächele, 1992; Kächele et al., 2009).

The "True controversies" have a much humbler goal: they limit themselves to attempting to make therapeutically related relationships empirically probable. Two publications are especially suited to illustrating the fault line that runs between these controversies and the theory and practice of psychoanalysis as a whole. Hanly, in his work "Pragmatism, tradition, and truth in psychoanalysis," takes a stance on the epistemological questions that the topic of truth poses for psychoanalytic practice. Hanly elucidates the relationship between the pragmatic and the correspondence theory of truth with the help of the so-called tally argument:

> Freud (1916, p. 452) said that the patient's 'conflicts will only be successfully solved and his resistances overcome if the anticipatory ideas he is given tally with what is real in him'. In this sentence, in a discussion of the problem of how suggestion may contaminate psychoanalytic observations with the analyst's influence, Freud implicitly states his conjoint use of correspondence and pragmatic criteria of truth. The correspondence criterion is implied by the idea that interpretation must tally with what is real in the patient. For example, the interpretation, 'you are seeking to reexperience the pleasure of being comforted by your mother at the breast in your need to take in only the sound of my voice, disregarding what I say,' can be effective if and only if it tallies with (corresponds with) the patient's currently operative transference wish. (Given resistance, this interpretation too will, at first, be experienced by the patients in terms of its sound and not its sense.) The pragmatic criterion of the effectiveness of the interpretation is linked to correspondence insofar as correspondence is the condition for effectiveness, while effectiveness is a test of correspondence. (Hanly, 2006, p. 280)

Segal (2006) renews the Freudian Shibboleth in the same issue of *Imago*. The article entitled "Reflections on truth, tradition, and the psychoanalytic tradition of truth" assumes that, with the help of the Freud–Klein–Bion therapy model of the neutral analyst, truth about the genesis of psychic suffering can be discovered. Her further assumption is that the correct interpretation is of necessary or sufficient therapeutic effectiveness. The only exception placed on this thesis relates to Bion's mysticism with regard to the cognition of O:

> When I speak of the Freud/Klein/Bion model, I must add that I do not include in that Bion's later work on Transformations in O and becoming O which have to do with a kind of immediate union with what Bion refers to as the 'thing-in-itself.' It seems to me very mystical – a sudden illumination coming from some unnamable O. It sounds too much like a transcendental *T*ruth … (Segal, 2006, p. 290)

Segal believes that she can justify her position with the argument that non-neutral analysts take over the parents' role and thus leave their professional role. Indeed, the analysis of transference dominates all in the Kleinian school, although it is only one component of the intersubjective relationship. Segal writes:

> In further developments, the Middle Group, which changed its name to the Independents, also established a new model of the mind, deriving from Ferenczi and developed by Balint, Winnicott, and, later in the United States, by Kohut. The fundamental difference between this model and those of Freud, Klein, and their followers lay not in the fact that it took into account new clinical evidence, but rather in the kinds of uses that it made of clinical evidence. A new concern emerged that focused on various notions of cure and change that did not rest on attaining truth and that considered the personal influences of the analyst – e.g., his support, advice, and comfort – to be integral to the analytic process. Here the changes in technique were of a kind that made them essentially non-analytic. They went against the psychoanalytic effort to bring about change through the search for truth. For when the analyst actively takes upon himself the parental role, he invites the patient to live in a lie. This in turn promotes concrete functioning rather than a symbolization and psychic growth. (Segal, 2006, p. 288f.)

Schafer's (1994) constructive criticism about the contemporary London Kleinians has retained its validity, while remaining disregarded for 15 years:

> The current London Kleinians present according to Schafer their material, including their countertransference, as if they were in the position of the independent objectifiying observer. Yet, Schafer argues that particularly under the aspect of object relationship one would expect the opposite. It does not surprise that Schafer criticizes the lack of a) presentation of life stories b) the explaining construction of Pathogenese as well as c) the comprehensive and complete therapy reports. (Thomä, 1999, p. 836, translated by current author)

The majority of contemporary psychoanalysts have replaced Freud's monadic model with an intersubjective model of therapy. In this vein, Segal (1990) too has accepted what had been a highly controversial thesis on the "corrective emotional experience" as a therapeutically potent factor. We have to live as much as ever with extreme opinions, and are thus made aware of a progress-arresting aspect of tradition. This makes critical and consequent scientific thinking difficult or blocks the same among the believing members of schools.

Therapeutically desirable changes arise out of the new experiences that are won with the analysis of transference and countertransference. That is why the criterion of pragmatic truth is first among all truth theories (Renik, 2004a, 2004b; Spillius, 2004). Not by chance did Kächele (1994) take as the title of a paper the biblical wisdom: *Ye shall know them by their fruits* (Matthew 7:16). The way there is not aimless. The diagnosis of therapeutic changes justifies and substantiates long and high-frequency psychoanalyses (Sandell, Blomberg, Lazar, Carlsson, Broberg, & Schubert, 2000). Failures must be investigated with double intensity; the contribution of the analyst to negative reactions poses a special challenge to the community (Caspar and Kächele, 2008).

The rule system of the "neutral" analyst in the high-frequency setting, which Freud conceived, was never optimal, at least from today's perspective. The sophisticated, empirical, comparative psychoanalysis is perhaps of a newer make, but clinical experience yields an unequivocal result: the professional self of the analyst, only abstractly separated from the person, is conjoined with his therapeutic impact. The contingent problems of process and outcome research are immense. The extent to which Freud's discovery of transference, with all due care, provides inferences about the genesis of psychic suffering becomes more than ever a scientific problem. It is necessary to differentiate theories about the genesis from theories about therapeutic processes and outcomes.

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PART 2

Abstract

The 10-year report, in 1930, of the famous Berlin Psychoanalytic Institute (privately founded by Max Eitingon's family fortune) called it a "small university." However, it was destroyed by the Nazis. According to Balint (1948), the original Eitingon model of "therapy for the broad masses, research and training," clearly delineated by Freud, became completely lost in what is known today as the contemporary "Eitingon model of training." In this tripartite model, the training analysis takes center stage. At the beginning of Part 2 of this paper an impressive, incomplete list of 24 critical papers on psychoanalytic training indicates the number of, to this day, failed attempts to change this very one-sided training system. Insofar as the deep contemporary crisis of our discipline is mainly home-made. Orthodoxy and dogmatism are the consequence of an education that does not center upon Freud's scientific paradigm in its modern form. Paula Heimann's and Herbert Rosenfeld's "liberation from orthodoxy" is described in detail. Finally, I suggest that an International Psychoanalytic Consortium should be founded as soon as possible, of which all psychoanalytic and psychodynamic associations should become members. In order to secure the future of psychoanalysis, it is essential to finance a few training-cum-research institutes, where psychoanalysts will be educated by full-time teachers.

Key words: *countertransference, history of psychoanalysis, intersubjectivity, psychoanalytic education, research, transference*

Psychoanalytic education

> who ever is concerned with the problem of psychoanalytic training is inevitably concerned with the future of psychoanalysis. (Heimann, 1968, p. 528)

Shortcomings of the psychoanalytic training models had been known for a long time, at least since Balint's critical papers (1948, 1954). After the destruction of the Berlin Psychoanalytic Institute by the Nazis, Freud's and Eitingon's concept degenerated into a mere tripartite training model, without a systematic research orientation and without free clinic treatments. Already by 1948, Balint was complaining about this impoverishment:

> The original idea: psychotherapy for the broad masses …, became completely lost in the years of the development. It is a justified charge against us analysts that we are so little concerned about it, and only a fair consequence that the therapy of the masses is passing more and more into other hands and will eventually be solved – rightly or wrongly – without us. The same is true about the second original aim of the institute, about research. The results in this direction are so poor that they are hardly worth mentioning. Perhaps the only exception to this sad record is the Chicago Institute. (p. 168)

Balint's early allusions to the deficiencies and one-sidedness of training have since been supplemented by a large array of publications (Auchincloss and Michels, 2003; Berman, 2004; Bruzzone, Casaula, Jiménez, & Jordan, 1985; Cremerius, 1989; Ermann, 1993; François-Poncet, 2009; Kächele & Thomä, 2000; Kappelle, 1996; Kernberg, 1986, 1992, 1996, 2000, 2001, 2006, 2007, 2008; Lothane, 2007; Morris, 1992; Target, 2001, 2002; Thomä, 1993, Thomä & Kächele, 1999; Wallerstein, 2007, 2009a) – with minimal consequences.

Criticism includes the lack of research and the associated dogmatization. An example of this is the situation in the USA, where, in comparison with other psychoanalytic associations, the American Psychoanalytic Association has increasingly attempted evaluations. The "least expected findings" of a survey conducted by Morris (1992) included the following:

> In none of the 28 institutes of the American Psychoanalytic Association is it the practice to have training analysts or even junior faculty ever present in continuous case conferences, though faculty may present brief vignettes of clinical material in their other courses. Rather it is always the practice that candidates present recent or current material in such conferences, and no institute reported having the goal of following a single case from start to finish. Thus, the only completed analysis that a candidate experiences longitudinally is his or her own. (p. 1200)

Morris regrets that:

> Loewald's 1956 encouragement for experienced faculty to present their case material to students not become a reality, but also current candidates have less and less opportunity to participate in and benefit from continuous case discussion or supervision of a case through termination. (p.1209)

Unfortunately, it is now evident that the apprenticeship model of psychoanalytic training has for decades been lacking a critical aspect: the masters'

exemplary demonstration of a psychoanalytic treatment from start to finish.

The deficiencies typical of most institutes cannot be made up for by certain improvements in the "core curriculum" on which Morris seems to base his optimistic judgment that "psychoanalytic education is alive and well" (p. 1207). Of course it is alive since it exists, but is it well? If a healthy life means progress and change, and not stagnation or even regression, psychoanalytic education is not at all well. In fact, there has been a conspicuous decrease in the average number of candidates from 60 to 24 per institute between 1960 and 1990, although the number of institutes of the American has doubled during this time span (from 14 to 28). The total number of candidates has not grown in proportion: there were 888 candidates (all MDs) in 1960, and 1051 (with 17–20% nonmedical) candidates in 1990. Still, as we shall see later, the era of decay started after these dates.

It is also striking that the tripartite psychoanalytic curriculum – personal analysis, seminars, and supervision – is far removed from the triad of research, training, and treatment that Freud favored in the classic academic tradition. More than 50 years ago, Knight (1953), in his paper on "The present status of organized psychoanalysis in the United States," complained about the consequences of certain American training regulations. He says bluntly: "our regulations may have the effect of drying up the supply of research psychoanalysts" (p. 215). This evaluation is still valid and true for almost all institutes of the IPA.

In fact, Morris's survey confirms once more that the tripartite psychoanalytic curriculum does not include research. The narcissistic misunderstanding of Freud's inseparable bond thesis turned every practicing psychoanalyst into a "researcher" – the search for truth became "research", if only on paper. By equating practice and research, the IPA could not satisfy its claim to be the wholly responsible heir of Freud. On the contrary: this position ultimately served the control of all that distinguishes psychoanalysis. The "pure gold" was said to have been found in Freud's *junktim* thesis – another attempt at trying to make a virtue out of necessity.

It thus comes as no surprise that the agendas of the IPA congresses are, due to their richness in generating hypotheses, always stimulating, but in terms of the context of justification they are a disappointment. Research questions would not be discussed until the late 1970s. The first meeting of a research-oriented group at an IPA congress did not take place until 1981 in Helsinki. The announcement of the first international Ulm Workshop on Psychoanalytic Process Research (see Dahl, Kächele, & Thomä, 1988) prior to the 24th IPA congress in Hamburg in 1985 had originally been included in the official program by the local organization committee. It was then subsequently struck from the program based on a directive by Limentani, who was the President at the time. The program had to be reprinted.

Since Wallerstein, the IPA has made great efforts to overcome the large deficits in the research of the "*Mutterboden*" (translated by Strachey as "home ground") of analytical therapy. In 1990, Sandler initiated a research conference, which organized – among other meetings – a Research Training Program in order to teach formal qualitative and quantitative research methods for psychoanalysts. At the same time, the style of the "Controversial Discussions" continued: academically rooted psychoanalysts were now blamed for doing only "quantitative, positivistic, empirical" research. Formal research to sort out these debates was either not desired or not available. This illustrates the long-term effects of the nonacademic history of the psychoanalytic movement:

> Mimicking Freud, but profoundly misunderstanding him, most psychoanalytic training programs have failed to provide an adequate education in the basics of research—how to do it, how to read and evaluate a research paper. This has contributed enormously to the loss of intellectual and scientific status of psychoanalysis today. (Cooper, 2008, p.)

Incidentally, these research deficits also apply to all other psychoanalytic institutes outside the IPA. They also affect the professional competencies of practicing analysts. The point is not to make all psychoanalysts researchers. Instead, the point is to make clinically active analysts familiar with research problems, as called for by Cooper.

The fall of the Iron Curtain fundamentally altered the world stage and opened up new opportunities for psychoanalysis, most notably in the countries of the former Soviet Union and in China. In the context of globalization, the IPA has, for the first time in its history, shown flexibility in terms of psychoanalytic training, as seen with the East European Institute for Psychoanalysis. Worrisome, however, is the "process of babelisation," described most recently by Jiménez (2009). The opening and increasing flexibility of the IPA has not changed the framework of the old ideologically driven controversies. Rather, they have intensified, most likely out of the fear of an imminent demise of the "true psychoanalysis." Interestingly enough, the most immediate subject matter is the setting, especially the frequency of sessions.

The extreme decline of interest in a psychoanalytic education among the younger generation in most countries of the West is not being offset by the

intellectual curiosity of young people in other parts of the world. In the hundred-year history of the IPA, an asynchronous global development has taken place that brings with it particular problems. And although the "Zeitgeist" is also against psychoanalysis, our problems are fundamentally home-made. It is by all means imperative to consider our shortcomings. The pertinacious criticism over many years of the psychoanalytic training models has had basically no consequences. Levy's summarizing statement is fitting to Germany as well as to other countries:

> Our current model in North America is largely uninspiring, uninviting, prohibitively expensive, frequently infantilizing and rife with conflict of interest among participants; not surprisingly, it fails to attract the students we want. (Levy, 2004, p. 8)

Levy himself recently wrote a fascinating paper about university-linked attempts to offer a diversity of courses, which seem to be attractive not only for full psychoanalytic training (Levy, 2010).

The gravity of the situation is underscored by numbers from the USA and Germany: since the publication by Morris 15 years ago, the number of applicants in the USA has dramatically declined, as shown in Table 1.

There are 53 institutes in the German organization, the DGPPT13 of which belong to the DPV. The rest are so-called "free institutes," whose training (with the exception of the DPG institutes that offer a high-frequency training analysis and supervision) cannot lead to membership of the IPA. The number of applicants to the DPV annually diminished from 194 to 16 within 20 years, from 1989 to 2009 (Table 2).

The rapid decline of psychoanalysis in Germany has coincided with a historical event, namely the change in the country's public health system. 1999 witnessed the creation of a third healing profession next to "physicians" and "quacks": the psychological psychotherapist. Since then, the number of practicing behavioral therapists has multiplied and the number of applications for a psychoanalytic education has dramatically decreased.

Another factor worth mentioning is that the number of rejected applicants has dramatically declined in the last 10 years. It is unlikely that so many more doctors or psychologists were, for whatever reason, deemed unfit to train between 1980 and 2000. It is rather more likely that the decline has unveiled the questionability of the whole application regulations (Kapelle, 1996), which has been known to all concerned for decades. I know few rejected applicants that are in retrospect happy about their rejection. Few have become friends of psychoanalysis; many have become its enemies.

Especially lamentable is that, in Germany, the influence of psychoanalysis within medicine as a whole has diminished, and that there are only a few doctors among the applicants. Beginning in 1967, many chairs at universities for psychosomatic medicine and psychotherapy were awarded to psychoanalysts. In the past 10 years, applicants with psychodynamic and neuroscience qualifications have had a better chance. In the departments of clinical psychology in German universities, psychoanalysis is almost no longer represented. The founding of the first private psychoanalytic university in Berlin (in 2009) can be viewed as a reaction to this development

Table 1 Number of US applicants

Year	APSA – total new candidates
2000	122
2001	56
2002	101
2003	73
2004	92
2005	119
2006	79
2007	67
2008	104
2009	65

Information provided from the office of the APSA, February 2, 2010.

Table 2 Overview of applicants to the DPV

Year	Number	
1989	194	97 accepted
1990	122	56 accepted
1991	139	72 accepted
1992	100	49 accepted
1993	88	51 accepted
1994	85	49 accepted
1995	68	44 accepted
1996	100	68 accepted
1997	62	35 accepted
1998	67	44 accepted
1999	41	27 accepted
2000	41	33 accepted
2001	41	33 accepted
2002	33	26 accepted
2003	37	28 accepted
2004	20	17 accepted
2005	38	34 accepted
2006	33	21 accepted
2007	34	29 accepted
2008	27	27 accepted
2009	16	13 accepted

Information provided by the Secretary of the DPV Training Committee, January 20, 2010.

The central position of the training analysis

Training analysis was the focus of an already mentioned closed conference on the topic of "The identity of the psychoanalyst," which took place in 1976 in England (Joseph and Widlöcher, 1983). Anna Freud intervened with the following remark:

> The heart of the matter is that the problem doesn't really seem to have changed much in the last forty-five years! But in listening to you here, I also got the impression that my colleagues who first advocated the introduction of training analysis… — if they had known of all the dangers, of the positive and negative transferences, and splits, and hates, etc. would probably never have advocated it! They would have said, 'Let them be as they are!' (A. Freud, 1983, p. 259)

As a participant, I was under the impression that Anna Freud had probably gone too far. I summarized her subsequent addition as follows:

> This negative assessment was balanced by a positive supplement to the effect that too little mention had been made in the symposium of the identificatory learning process, transmitted via the training analysis, which, she maintained, inspires love for psychoanalysis. The examples she gave show impressively how enthusiasm for psychoanalysis can be passed on through identification and not by indoctrination. (Thomä, 1993, p. 6)

Most of today's problems remain incomprehensible unless one is familiar with the developments that occurred between 1910 and 1939. Freud's (1910) discovery of countertransference and his observation that every psychoanalyst can only go as far as "his own complexes and inner resistances permit" (p. 145) must be placed in the same historical context. This is why Freud stipulated that the analyst should begin his work with a self-analysis:

> An analysis such as this of someone who is practically healthy will, as may be imagined, remain incomplete. Anyone who can appreciate the high value of the self-knowledge and increase in self-control thus acquired will, when it is over, continue the analytic examination of his personality in the form of a self-analysis, and be content to realize that, within himself as well as in the external world, he must always expect to find something new. But anyone who has scorned to take the precaution of being analyzed himself will not merely be punished by being incapable of learning more than a certain amount from his patients, he will risk a more serious danger and one which may become a danger to others. (Freud 1912, p. 117)

High expectations, indeed the highest, were placed on the self-analysis and later the training analysis. Without it, Freud warned:

> he will easily fall into the temptation of projecting outwards some of the peculiarities of his own personality, which he has dimly perceived, into the field of science, as a theory having universal validity; he will bring the psycho-analytic method into discredit, and lead the inexperienced astray. (Freud 1912, p. 117)

In order to reduce the subjective aspect concerning pathological factors with the aim of achieving an ideally fictive normal ego on the side of the analyst, the training analysis was placed at the core of the education. Ferenzci even called the training analysis, according to Balint (1952, p. 283), "the second fundamental rule of psychoanalysis." Ferenczi had the "definite" impression that, since the introduction of the obligatory training analysis, the differences in analytic techniques were beginning to disappear. Balint, in his independent mind, commented:

> It is a pathetic and sobering experience to realize that although this idealized, utopian description gives a fairly true picture of any of the present cliques of the psychoanalytic movement, it is utterly untrue if applied to the whole. Ferenczi foresaw correctly the results of *one* "supertherapy", but he had not even thought of the possibility that the real development would lead to the co-existence of several "supertherapies" competing with one another and leading to a repetition of the Confusion of Tongues. (Balint, 1952, p. 284)

Given these prophetic words, it is amazing that "*the training analysis* is [still] considered to be the most important component of the tripartite model of psychoanalytic training" (Lasky, 2005, p. 15) From the beginning, complicated embroilments with profound ramifications arose as a result of the training analyst having a say in the candidate's professional qualifications. Even though the reporting system was abolished almost everywhere, candidates still find themselves in the role of patients, and analysts in the role of therapists:

> Target noted an interesting finding from her own studies and discussion groups in Europe: The most common slip made in discussions among training analysts about candidates in classes or in supervision with them was to refer to them as 'patients.' (Seidel, 2006, p. 249)

The fact that the evaluation of professional competencies is dependent on the changes that the candidate undergoes in the course of the training analysis constitutes the main problem of training analysis. In the French system, application for training is only possible after an extended analytical process. The commission is not capable of outlining the criteria of competence or the changes that are desirable for individual cases. Thus, only a radical

AQ15 separation has a future: the own analysis must be in its entirety a purely personal matter.

I am convinced that only the candidates themselves should be entitled to decide about their "personal" analysis, an unsuitable word chosen to avoid "therapeutic" or "didactic." Over the years, I gave up all compromises and arrived at a radical position. My first proposal was based on the assumption that institutes have the right to require a didactic training analysis for professional reasons (Thomä, 1993). On an abstract level, in a thought experiment, I divided the personal analysis into a didactic and a therapeutic component. If one takes as the aim of the didactic aspect that a candidate learns something about the effects of unconscious processes on his feelings and thinking, I am still convinced that about 300 sessions are a reasonable amount of self-experience. As clinicians, Kächele and I agreed with Kernberg when he wrote that "candidates with narcissistic character pathology, for example, may require more than two or three years of personal analysis to overcome their narcissistic defences" (see Kächele and Thomä, 2000, p. 114f.); as teachers, however, we strongly rejected the notion that a training committee is either competent to diagnose a candidate's pathology or indeed justified in doing so (Kächele and Thomä, 2000).

The therapeutic aspects of the personal analysis, its frequency and duration, should in my opinion stand outside administrative regulations. Misunderstandings could be expected as my abstract division is only an artificial act. Therefore I gave up an untenable compromise: that as an exemplary experience, the training analysis can only prosper in complete freedom from external factors, that is, in privacy. Only a rigorous solution can secure the therapeutic quality and prevent candidates from being pathologized. The evaluation of candidates' professional quality should no longer be tied to judgments about their personality and its modification in therapy. It is insufficient to abolish the unethical reporting system that clearly violates the analyst's duty to maintain confidentiality. Discretion is absolutely necessary to protect the therapeutic space that training analyses have not so far had (Thomä, 2004).

The Zimmer (2003) report on the Tenth Congress of Training Analysts includes Amati-Mehler's reasoning for rejecting the reform proposals on psychoanalytic education, in contrast to Auchincloss and Michels' suggestions:

> An adequate training analysis, she asserted, should explore the candidate's *psychotic levels*, so as to develop the capacity of candidates to work clinically with the *countertransferences that are central to clinical work with very ill patients*. Such exploration might not be necessary in a non-training analysis. Psychoanalytic education should be upgraded so as to enable candidates to analyse patients with more serious psychopathology, rather than relegating them to other forms of treatment. The training analyst function must also be upgraded; and training analysts must have superior clinical expertise and be educated to increase their awareness of difficulties peculiar to the training analysis.
>
> She disagreed with the interpretation of Drs Michels and Auchincloss of Thomä and Kächele's proposal; it does not, she asserted, merely seek to place the tripartite system into a larger context, but to replace the triad of seminar, supervision and personal analysis with one of teaching, treatment and research, *eliminating the personal analysis except for a very brief experience*. She added that the empirical research at the centre of this model is not what most psychoanalysts think of as psychoanalytic research. (Zimmer 2003, p.148, emphasis added)

Clearly, my proposal has been interpreted as wanting to reduce the training analysis to a "very brief experience." The correct view, however, is that I do not see 300 sessions as being "very brief." I myself regret that my own training analysis with M. Balint only lasted 230 sessions. Further sessions would undoubtedly have enriched my life. On the other hand, I am fairly certain that my psychoanalytic competence would have not seen a substantial improvement as the result of a greater number of hours. Amati-Mehler, on the other hand, seems to make out a particular professional competence derived from the length and content of the training analysis. Her objections are "unqualified assertions" in Schafer's (1985) sense about controversies.

Practicing psychoanalysis is not identical to doing clinical research. Wallerstein (2009b) expressed his caveat by referring to the difference between "searching for truth" and doing "research." To turn searching into clinical research has to combine process research with outcome research by defining and investigating qualitative and quantitative criteria. Case reports that focus on the description of transference processes and countertransference processes are often far away from the possibility of evaluating the analytic process (see Kächele, Schachter, & Thomä 2009, pp. 99 ff.). What is more, there is a widespread lack of digesting the results of the general field of psychotherapy research that has been quite rich in providing tools for evaluation (Lambert, 2004; Luborsky & Spence, 1978).

Liberation from orthodoxy

Orthodoxies often grow in groups under the influence of a charismatic leader. The liberation of orthodoxy is thus necessarily linked to people, with drawn-out wars of liberation as a result. The

experience is traumatizing. I choose two examples here: Paula Heimann (1899–1982) and Herbert Rosenfeld (1910–1986). Rosenfeld only changed his psychoanalytic position in his autumn years, approximately around 1978, and subsequently had to endure having become an outsider in his own group (Steiner, 2009).

Heimann[1] was a creative psychoanalyst, the analysand and close co-worker of Melanie Klein and regarded as her crown princess. Her lecture "On counter-transference" at the now historic Zurich Congress 1949 (Heimann, 1950) made a prominent contribution, along with that of Racker, Little, Searles et al., in changing the emphasis of the psychoanalytic technique. The title of her second to last publication "On the necessity for the analyst to be natural with his patient" (Heimann, 1978) provides an insight into the human aspects and temperament that may have allowed her to contribute to the intersubjective turning point of the Freudian paradigm.

King's introductory memoir to the collected papers of Paula Heimann (King, 1989) revealed her Zurich lecture to have been presented with the help of Ernest Jones and unbeknown to Melanie Klein. It is remarkable that Melanie Klein, as the founder of the school, remained alone in adhering to the traditionally defined term of countertransference. The point of no return for Paula Heimann seems to have been Klein's thesis on inborn envy and its derivation from the death drive.

A couple of themes run throughout Heimann's work. Most immediate in this respect is Heimann's slow farewell to the death drive hypothesis, a topic that fascinated her immensely. She writes in the postscript to "Dynamics of transference interpretations":

> Since my student days I have been an enthusiastic supporter of the Freudian theory of life and death instincts, considered as the ultimate source of all instinctual processes. I still think that with this theory Freud presented an awe-inspiring concept – and in what fine language! – that has given us a glimpse of the relations between the various elements that make up the universe and which reconciles the contrasts between its various phenomena: attraction and repulsion, the expansion and contraction of the universe, animate and inanimate matter. However, I gradually came to mistrust this very enthusiasm and to realize (as I have said elsewhere (Heimann 1968) that my attitude was 'oceanic' rather than scientific, I have come to distinguish between the clinically verifiable elements of Freud's theory and the cosmic speculation that it involves. (Heimann, 1969, p. 252f.)

Heimann's own farewell dragged on until the memorable Viennese Congress in 1971. At this congress, Anna Freud delivered the final lecture about aggression (Freud, 1971). Her lecture underscores much more clearly than Heimann's the extent to which conscious and to a larger extent unconscious conflicts of loyalty dominated the discourse. Anna Freud thus managed to prove in rational lucidity that aggression lacks any of the characteristics of a drive, while, at the same time being a loyal daughter to our founding father, she continued to hold on to the belief of a death drive. The perspicacious defendant of Freud, Kurt Eissler, seemingly provided the appropriate argument: he alone had discovered the speculative biologist R. Ehrenberg. Ehrenberg was the only educated natural scientist who held on to the belief of the death drive. Remaining unmentioned is Eissler's argumentation along the lines of existential philosophical speculations about death. In short, Ehrenberg as well as Heidegger speaks of a death that is light years away from Freud's speculations on the death drive.

Back to practice and to Paula Heimann's Zurich lecture on countertransference. At the time, countertransference and the Kleinian theory and practice had nothing to do with projective identification. The founder of the school alone did not draw such a connection. Discussions with especially interested younger analysts about her understanding of countertransference published by Spillius, Roth, and Rusbridger (2007) from the Melanie Klein Archives show that she was against deriving countertransference from projective identification. In other words, she remained true to the classical understanding of countertransference. The back-projection of countertransference onto projective identification only began with the publication by Money-Kyrle (1956). Bion's "containment" has become an all-encompassing slogan here.

This digression was necessary in order to place a rarely quoted but most important sentence from Heimann's Zurich lecture into its historical context. It runs as follows:

> From the point of view I am stressing, the analyst's counter-transference is not only part and parcel of the analytic relationship, but it is the patient's *creation*, it is a part of the patient's personality. (Heimann, 1950, p. 83)

[1]There are also personal reasons why I give Paula Heimann greater attention. She took me and my wife, Dr. Brigitte Thomä, into her collegial circle of friends during our stay in London in 1962. We frequently met her afterwards at her seminars in Frankfurt am Mein and Heidelberg. Her first visit to Frankfurt probably occurred in November 1959, where she was a guiding figure for what would later be known as the Sigmund Freud Institute. Heimann became a regular visitor after 1961. Despite our good relationship, we respectfully avoided discussing personal aspects of her separation from Melanie Klein. Theoretical clarifications have always been more relevant to me than subjective confusions and the conflicts from which they arise.

Heimann would continuously point to the dangers of her innovative thesis and believed, speaking as a student of Melanie Klein, that these could be banished through a thorough engagement with the two positions. She argued:

> The approach to the counter-transference which I have presented is not without danger. It does not represent a screen for the analyst's shortcomings. When the analyst in his own analysis has worked through his infantile conflicts and anxieties (paranoid and depressive), so that he can easily establish contact with his own unconscious, he will not impute to his patient what belongs to himself. (Heimann, 1950, p. 83)

This reference to "working through" in one's own analysis would later be amended or even replaced with the decisive reference to the importance of the effect of interpretations in the therapeutic process. Although Heimann on several instances addressed projective identification, the theme of the "creation" of the patient remained unmentioned. After 1950, she made numerous critical comments on "misunderstandings." She was stimulated to further clarify her position in discussions that took place in Heidelberg and Frankfurt within the framework of the studies on the interpretive process that I initiated. These led to her publication on the analyst's cognitive process (Heimann, 1977). She finally so distanced herself from the thesis that countertransference is the patient's "creation" that she expressed amazement at having ever made such an assertion.[2]

A key to understanding what Heimann may have meant by the "creation" of the patient may be found in the "Third Ear" of Theodor Reik, with whom Heimann did her first training analysis. Much more pertinent to me, however, is her emphasis of the "relationship" as an all-embracing prerequisite for all restricted psychoanalytic concepts. It therefore comes as no surprise that Gabbard (1995) called countertransference the "common ground" of modern psychoanalysis. While I am generally of the opinion that transference and countertransference are to the highest degree specific to dyads and that the "common ground" must be something broad, the intersubjective turning point is undoubtedly the mark of modern psychoanalysis. Credit for this turning point is in no small part due to the work of Paula Heimann.

Herbert Rosenfeld recognized only late in his career negative therapeutic reactions as the result of the analyst's antitherapeutic behavior. There are similarities between the liberation of Paula Heimann and Herbert Rosenfeld. Both distanced themselves from the idea of tracing negative therapeutic reactions to "inborn" envy and, in the final instance, to the death drive. Rosenfeld describes negative therapeutic reactions as a consequence of excessive transference interpretations that are based on the assumption of an inborn envy as follows:

> interpretations of envy should not be repeated so often. The emphasis should be on helping the patient to bear the pain, discomfort, and shame which envy causes because it inhibits the capacity to love. Severely frustrating situations inevitably stimulate envy. The main problem that arises in analysis is that sometimes the patient feels humiliated because the analysts understands the patient so much better than the patient himself. This problem has to be faced by helping patients to understand that their progress in analysis depends on a joint effort on the part of patient and analyst and particularly on good timing and sensitive interpreting on the part of the latter. An over-emphasis on the interpretation of envy or the over-valuing of the analyst's contribution as compared to that of the patient is a frequent cause of impasse. (Rosenfeld, 1987, p. 266f.)

One criterion has to be fulfilled in order to accept inborn envy. This criterion only makes the whole phenomenology of envy out of the difference between having and not-having even viable. It is worthy of special recognition that the basic conditions of every envy reaction can be explained with a quote from the book by Roth and Lemma entitled *Envy and gratitude revisited*:

> Experiences of envy and experience of gratitude depend on an awareness of separateness – an awareness of the otherness of the other. It is hard to formulate a concept of envy which could take place within a relationship of absolute fusion between self and object; so long as what is Good is Me, what it experiences as good need not be envied since it belongs to me. Envy can only arise at the moment – however brief and however fleeting – when the individual becomes aware that what is Good is not Me. Similarly, gratitude can only be experienced in relation to another person – a not-Me. Klein believed that momentary awareness of the separateness of the object begins from birth. She believed that infants have an inborn awareness of a separate, bountiful object . . . which is met at the first feeding experience by the reality of the breast. (Roth, 2008, p. 6)

All orthodoxies share the characteristic that belonging to a certain school brings with it a diplomatic language. One does not dare to draw consequences, although they might be logically necessary. In this case, the question is whether or not the belief in inborn envy is justified. It is not. But a clear answer to that question is typically avoided.

[2] In a private conversation with B. and H. Thomä on August 3, 1980.

In order to grasp the full scale of Rosenfeld's change of positions, it should be noted that he stayed true to the traditional treatment rules for a long time. An example of this is his criticism of Klauber in a postscript in 1972. Both had revisited Strachey's historical work about the mutative interpretation in the same issue of the *International Journal of Psychoanalysis* (Klauber, 1972; Rosenfeld, 1972). Klauber emphasized the importance of the encounter. Rosenfeld in turn recommended, diplomatically encoded, some more analysis in order to be able to give up such nonanalytical ideas.

Rosenfeld was a source of innovation within the Kleinian school. Upon closer reading, one can see that he did not derive destructivity from the death drive. Destructivity instead serviced, according to Rosenfeld, a highly pathological narcissism. Despite all the differences, similarities arise here to Kohut's and Kernberg's conception of narcissistic rage:

> There is another point that has become clearer to me during recent years. This refers to the existence of the death instinct. I have always felt that there are aggressive forces which are fighting against the forces of life, a factor that became clear to me when I discovered the importance of destructive narcissism. (Rosenfeld, 1987, p. 267f.)

Most important are Rosenfeld's self-critical changes with regard to his late emphasis on the therapeutic relationship. Under the heading "The analyst's flexibility," he argues:

> I think it is essential that the analyst is aware that the analytic situation and transference situation are both affected not only by the patient's past experiences but also by the analyst's views, behavior, and countertransference. My understanding of the analysis of negative transferences and aggression has altered significantly. (Rosenfeld 1987, p. 270)

Rosenfeld was on the way to an intersubjective model of the therapeutic situation. That terms such as countertransference would suffer from a loss of relevance escaped him.

In closing, I would like to briefly address the effects of the liberation of an individual on the respective school. Schafer (2009) registered with surprise and almost with regret the effects of Rosenfeld's late "conversion":

> What cast a shadow over the latter part of his career now casts an intriguing shadow over this volume, rightly dedicated as it is to honoring Rosenfeld for his earlier and still valued contribution. (Schafer, 2009, p. 991)

From my point of view, Schafer in his review not only uses diplomatic language but also takes the side of the Kleinian school and therefore does not do justice to Rosenfeld's well-founded original contribution. I am pretty sure that the vast majority of contemporary analysts joins forces with the "converted" Rosenfeld's late points of view, but – strictly speaking – much research is necessary to validate clinical wisdom.

Unity lost - and regained?

Dissidence in the past was often linked only to individual people. This was especially true after the early foundation of independent organizations through Adler and Jung. Not until 1962, at a congress in Amsterdam, did prominent members of four psychoanalytic societies create the International Federation of Psychoanalytic Societies (IFPS)[3] as a kind of declaration of independence from the IPA. The founding societies were the Deutsche Psychoanalytische Gesellschaft, Sociedad Psicoanalitica Mexicana A.C., Wiener Arbeitskreis für Tiefenpsychologie, and William Alanson White Psychoanalytic Society.

The German DPG psychoanalyst Schwidder had organized a preparatory convention in Düsseldorf a year before and invited prominent members of the IPA. It is remarkable that many IPA psychoanalysts were willing to go to that. In order to avoid the impression that a competing society would be founded, the slogan "liberal psychoanalysis" was adopted. Nevertheless, Scheunert and Mitscherlich, as DPV psychoanalysts, managed to find support from Hoffer and the IPA. The IPA informed analysts who had already confirmed their participation that this would be regarded as an act of disloyality. Due to this pressure, they withdrew their consent to participate at short notice (Lockot, 2010).

Fortunately, the dawn of the second century of psychoanalysis seems to be marking a new era, and the active participation of IPA members in the scientific program of the IFPS congress in Athens may signal a new perspective. However, only when members of the IFPS are invited to talk at an IPA congress will a situation of bilateral recognition have been achieved.

At the age of 89, I am still in love with psychoanalysis and deeply worried about its future. I share the concern voiced by Charles Hanly (2009) in his presidential message in the IPA's electronic newsletter. In my opinion, the future depends on changes in highly essential areas. An ideal reunion can only

[3]The IFPS today includes 26 psychoanalytic societies in 15 countries and at present has a total membership of 2650. The topic of the 2010 congress – "The Intrapsychic and the Intersubjective in Contemporary Psychoanalysis" – and the text of the invitation by the organizing committee clearly demonstrate the comprehensive perspective on contemporary psychoanalysis.

emerge if the IPA opens its doors for analysts who were not trained in IPA-recognized institutes, and of course if many analysts trained in free-standing institutes want to apply for IPA membership. Unfortunately, the divisions, splits, exclusions, and independent developments within the psychoanalytic movement cannot be overcome. Therefore my vision will remain utopian. At least, it is very unlikely that the whole membership of a non-IPA organization would join the IPA, or even be invited to consider such a reunion. Individual applicants will wonder about the attractiveness of the IPA. Finally, one should not underestimate the after-effects caused by the exclusion of the founders of a group or school. These can have a transgenerational influence to the extent that Margolis as outgoing president of the American Psychoanalytic Association described:

> To ask organizations with decades of successful experience in psychoanalytic education—whose founders may have been humiliated and rejected by our organization—to initiate merger discussions with our Board and risk being turned down is understandably unacceptable to them. (Margolis, 2001, p. 23f.)

On the other hand, it cannot be expected that the IPA should accept everybody who regards himself as an analyst and is a member of a non-IPA institution without any examinations. Still, I would like to play a thought experiment here: why not open the IPA to everybody that works as a psychoanalyst and finds an IPA member as a "bailsman"? Probably for the IPA, an applicant's recommendation by an IPA member will not suffice. My proposal amounts to a so-called referral test. In his attempts to educate competent psychoanalysts comprehensively, Tuckett (2005) asserts that so far the only valid and somehow reliable criterion of evaluating a psychoanalyst's competence has been a very subjective one, called the "referral test" – "To whom would you refer a patient?" In my opinion, passing such a referral test is a good enough entrance examination for IPA membership. I would even argue that the positive evaluation of an applicant's competence by one IPA member is at least as valid and reliable as the traditional membership paper.

It is unlikely though that the IPA will change its policies, or that many applicants should be expected. I thus arrive at a different solution. Such a solution needs to be realistic and allow for a reform that would secure the future of psychoanalysis. Solutions that can be realized must immediately start from a "common ground" that unites all psychoanalysts. We are all connected through our founding father. Further, I assume that my description of the current crisis is shared by the majority.

My review of the development of psychoanalysis shows two factors that have been especially restrictive. The first is the exclusion, to a large extent, from universities and, in this vein, training at free-standing institutes instead of full-time training. Night school education in free-standing institutes by practicing psychoanalysts corresponds to the training of doctors prior to the implementation of the recommendations of the Flexner Report in 1910, which placed the responsibility for the study of medicine into the hands of the universities.[4] 1910 is of course incidentally the year that the IPA was founded. There is no need for a special new Flexner Report for psychoanalysis, as requested by the psychiatrist, neurobiologist and Nobel Laureate Kandel (1999, p. 521), an admirer of Freud's work, who has blamed the outdated training system for the crisis in psychoanalysis. The decisive question here is whether in the long run a research orientated, full-time education by full-time teachers is possible at a few centers in the world.

It has been greatly satisfying to me to be able to give a positive answer to this question. The USA has been successful in bringing all its psychoanalytic institutes together under the roof of a Psychoanalytic Consortium, in the interest of greater common tasks. Margolis (2001), as former president of the American Psychoanalytic Association, commented on this development following the lawsuit (Simons, 2003; AQ17 Wallerstein, 2003; Welch & Stockhamer, 2003). He spoke of a "decade of change" and announced, under the subtitle "Alliances and unification," a new era in the relation between American and other psychoanalytic institutions in the USA. The realization of an International Psychoanalytic Consortium, similar to the American example, would leave all actors and psychoanalytic institutes their own freedoms.[5] An International Psychoanalytic Consortium

[4]Until Abraham Flexner's (1910) report, training in medicine was organized by registered doctors in privately owned institutes outside universities. Afterwards, medicine was instead accommodated in universities, which enabled the rise of medicine in the USA. The medicocentrism of American psychoanalysis goes back to Brill's attempts to facilitate the recognition of psychoanalysis in post-Flexnerian America: "Brill's desire was to encompass that claim in the new, post-Flexner world that distinguished between 'quacks' and 'physicians' based on certification by the new medical school" (Gilman 2006, p. 385; see also Richards, 2006).

[5]Indeed, that Consortium seems to have a similar function in the USA as the Deutsche Gesellschaft für Psychoanalyse, Psychotherapie, Psychosomatik und Tiefenpsychologie in my own country. This umbrella organization was founded in 1949 as the Deutsche Gesellschaft für Psychotherapie und Tiefenpsychologie (DGPT). Back then, the terms "psychoanalysis" and "depth psychology" were used synonymously. Membership was only denied to members of the so-called Reichsinstitute, who had been active Nazis. The revival of psychoanalysis in West Germany would not have been possible without this commonality in professional politics. The DGPT then amended its name by adding "psychosomatic" (1975) and "psychoanalysis" (1985).

unifying all psychoanalytic institutes would secure the future of psychoanalysis in the sense of a help for self-help. It is not reasonable to expect governments to finance, through universities or other state institutions, research and educational facilities for the full-time training of analysts. As so often before, psychoanalysts now have to fight for their cause through material support.

Before addressing the realization of the International Psychoanalytic Consortium, I would like to touch briefly on the dissenting opinion that it will likely face. It will be argued that university psychoanalysts in the USA had their chance as the chairmen of psychiatric departments. Similarly, German psychoanalysts were for a generation the chairmen of psychosomatic medicine and psychotherapy departments. In fact, in both countries not even half of all psychoanalysis was academically institutionalized. Free-standing institutes provided teaching until graduation.

To my knowledge, there has been only one institution that came close to an independent psychoanalytic university institute within a psychiatric department: the Menninger Foundation in Topeka, which comprised a psychiatric teaching hospital, a psychoanalytic training institute, and some other psychosocial facilities. The professional community knows about the worldwide importance of that unique birthplace of creative psychoanalysts, although its exchange with other academic fields was limited. The names Margaret Brenman, Rudolf Ekstein, Glen Gabbard, Merton Gill, Peter Hartocollis, Robert Holt, Philip Holzman, Otto and Paulina Kernberg, George Klein, Robert Knight, Lester Luborsky, Martin Mayman, Karl Menninger, Bill Pious, Ishak Ramzy, David Rapaport, Norman Reider, Arlene and Arnold Richards, Benjamin Rubinstein, Roy Schafer, Herbert Schlesinger, Howard Shevrin, Donald Spence, Ernst and Gertrud Ticho, Judy and Robert Wallerstein, Alan Wheelis, and more, are all associated with psychoanalytic publications that had a lasting impact on psychoanalysis.

To my knowledge, there is no truly autonomous psychoanalytic institute in the world that offers research-orientated full-time education through a full-time staff with part-time clinical professors. With the help of the membership fees of the psychoanalytic and psychodynamic societies united in an International Psychoanalytic Consortium, a sustained financing of such institutions could be realized. It is evident that such institutions should be linked to already existing, small psychoanalytic facilities at universities. The realization of this idea naturally depends on the existing large psychoanalytic societies sacrificing their claim to setting their own education standards. I regard the training analysis as a private affair with a practicing analyst outside institutions.[6] The marginal administrative tasks of the International Psychoanalytic Consortium could be assumed by one of the large societies. In addition, a World Congress should be held every four years as a "psychoanalytic Olympiad."

In conclusion, Sigmund Freud, using the psychoanalytic method, discovered unconscious processes in the development and therapy of mental illnesses. Here is its *Mutterboden*. The connection between the psychoanalytic method and the person of the therapist brought about an anthropological paradigm, which has led to a solution for the dichotomy between "understanding" and "explaining." Thus has emerged the only systematic psychopathology that has put the conflict at its core. Modern clinical psychoanalytic research on its "home ground" is an exacting combination of process and outcome research on as many single cases as possible. It depends on the tape-recording of psychoanalytic treatments. Therefore, in at least one of the projected institutes there should be a special thesaurus[7] that is accessible to all researchers. The resources of practicing analysts and those of the IPA-funded projects do not suffice for this. The projected psychoanalytic education and research institutes could provide our field with the appropriate scientific inquiries, with in turn a substantial positive influence on education and practice. In the long run, universities will not be able to shirk their responsibility to give psychoanalysts an academic home.

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[6]Rubinstein's experiences with Karl Menninger as a training analyst provide a warning example (Holt, 1997, p. 4).

[7]Since the retirement of Kächele and Mergenthaler in 2010, the Ulm Textbank (see Mergenthaler & Kächele, 2009) has no longer functioned as a provider of tape-recorded psychoanalytic dialogues and transcripts. Only the case of Amalia is still accessible, at erhard.mergenthaler@uni-ulm.de.

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