Erwin Kaiser (German Psychoanalytical Association): From Anxiety to Method in Psychoanalysis—and Retour (p. 65-80)

Introduction:

I first would like to thank warmly the EPF and especially this year's programme committee for kindly inviting me to present a paper on the topic of the nature of psychoanalytic knowledge. I am convinced that this topic is of utmost importance for all of us because it gives us a unique opportunity to reconsider and carefully reflect upon our professional identity and our self-understanding as psychoanalysts.

Freud's letter to Saul Rosenzweig

Let me begin with a quotation from a letter Freud wrote to Saul Rosenzweig in 1934. Rosenzweig had informed Freud of a recently completed experiment that verified psychoanalytic theory. Here is Freud's reply:

'I have examined your experimental studies of the verification of the psychoanalytic assertions with interest. I cannot put much value on these confirmations because the wealth of reliable observations (from the clinical situation) on which these assertions rest, make them independent of experimental verification. Still, it can do no harm' (Freud, 1934).

Freud's correspondence with the American psychologist Saul Rosenzweig clearly evidences Freud's preference for scientific observation over experimentation and that he was not exactly enthusiastic or in any way interested in Rosenzweig's psychoanalytic experimentation providing empirical evidence for psychoanalytic theories and psychoanalytic

knowledge from the perspective of academic psychology.

The fact that Freud remained completely unfazed shows, yet again, his sceptical attitude towards psychoanalytic experimentation and how much he trusted in his own psychoanalytic observations. Freud's unperturbed and unflappable attitude probably strikes us today as rather strange and a relic from a long gone and distant past. Or, could anybody seriously imagine a present-day psychoanalyst responding in a similar vein (as Freud in 1934) to a neurologist who purports to have found evidence for the validity of psychoanalytic knowledge?

The issue of the scientific status of psychoanalysis today becomes increasingly relevant for psychoanalysts as their organisations and periodicals more and more tend to privilege a scientific approach in psychoanalytic research. The reason for this shift is predominantly to be found in the 'pressing need', as Kernberg put it (2006), to demonstrate to the public—especially the public health administrations worldwide—that there is sufficient evidence for psychoanalysis representing an efficient psychotherapeutic method. Equally, the psychoanalytic discipline nowadays is called upon to prove its status as a 'science' in order to gain recognition and meet with the approval of the academic psychology and the medical sciences.

The rules of the game on 'How to become scientific?' is obviously an issue that is utterly taken for granted and not to be questioned or further scrutinized. Those rules often seem to set down once and for all in the canon of the 'unified science', i.e. '.in the philosophy of logical positivism, a doctrine holding that all sciences share the same language, laws and methods'1—that is, the laws and methods of physics, or at least what's generally deemed to be such laws and methods.

This seems to represent a universal trend which, to my mind, is rather detrimental to our own discipline: Now, this being said I would like to

state explicitly that I personally think that psychoanalysts all over the world belong to an endangered species that is doomed to become extinct before long—with the only exception being certain parts of South America perhaps. My argument is that today there is a collective anxiety in the psychoanalytic community, which fuels the conviction that the psychoanalytic method has to be changed. Thus, in the psychoanalytic community there is to be found an ever increasing consensus of opinion that the application of a unified scientific method would contribute immensely to regain the recognition and reputation psychoanalysis once had but now has lost in the course of the last few decades.

To my mind this widespread belief in the usefulness and applicability of such a unified scientific method has since caused a great deal of confusion. Unfortunately, this confusion is not restricted to some kind of esoteric methodological discussion about psychoanalysis but inevitably also has considerable implications for our theoretical psychoanalytic thinking as well as our psychoanalytic technique and clinical practice.

I think that by choosing this year's theme for the conference in Copenhagen the EPF and its programme committee not the least aimed at instigating a more profound discussion on the various relevant issues. The theme actually refers to the title of a book written by Georges Devereux that saw its first publication in 1967. This book represents a fundamental critique of the behavioural sciences and their unrealistic and illusory convictions concerning scientific objectivity and the quantitative research tradition in general. Not unlike Freud—as for instance in his letter to Saul Rosenzweig—Devereux (1967) with quite some determination and selfassuredness put forward the thesis that the actual psychological 'facts' can only be discovered by closely looking inside, i.e. by analysing the countertransference of the researcher or investigator and are not to be found outside, i.e. with the respective object of research.

The firm belief in a 'unified science' within the psychoanalytic community: Robert Wallerstein

One of the most prominent representatives of the unified scientific method is Robert Wallerstein. Wallerstein takes the diametrically opposed point of view from Georges Devereux: In a paper published in the International Journal of Psychoanalysis in 2009, Wallerstein states that 'psychoanalysis, as presently constituted and presently functioning … operates (still imperfectly to be sure) as a science, simultaneously developing general laws about how the human mind works, as well as pursuing the individualized working of these laws in the endowments and life experiences of the patients it treats' (Wallerstein, 2009, p. 111). Or, put differently: 'a (behavioural) science, one committed both to the elaboration of general laws of the mind and to their specifically differing particularization in the diverse individuals under study' (p. 113).

I take the view that this programme of the unified scientific methodology, as it is supported, for instance, by Wallerstein and others has been falsified by the scientific development of psychology. Furthermore, I am convinced that the knowledge gained by this very method—no matter how differentiated this knowledge otherwise might be—is completely irrelevant for clinical psychoanalytic practice. The reason for this is that this methodology relies to a large degree upon inadequate or even false ideas as far as the object of research is concerned. Epistemologically speaking, we are today in the position to make use of different basic assumptions that are much more appropriate for psychoanalytic knowledge and much more pertinent to the actual work of the psychoanalyst.

In the course of my discussion I will further elaborate my argument and will also draw some necessary conclusions.

Critique of the idea of a unified science 1: Fragmentation

All advocates of a unified scientific research methodology have one particular feature in common: all of them hold the firm conviction that it is only the scientific programme favoured by them that is truly valid and in the end will prove to be the most successful one. Though this may sound somewhat exaggerated, I, at times, certainly get the impression that the unified scientific research method is assigned the properties of a 'phantastic object' that David Tuckett (Tuckett & Taffler, 2008) speaks of, for instance, when referring to the world of the financial markets. You may probably remember that Tuckett refers to the Dutch tulip bulbs as one of the most significant manifestations of such 'phantastic objects' that during the period of the so-called tulip mania in the 17th century constituted the first speculative bubble in the history of mainstream economics.

But still, contrary to the high expectations of those scientific researchers the actual results of this kind of research are almost always quite disappointing: after more than one hundred years of scientific psychology in none of its departments has one come across a thing that would only remotely deserve the name of a 'general law', but what we have got instead is only a huge accumulation of isolated, limited and fragmented mini-laws.

What Koch (1981) called 'cognitive pathology' and for which he invented the term 'syndrome of non-meaningful thinking', is responsible for this kind of fragmentation in the area of psychological research (p. 264). By this he meant to describe 'a highly developed form of cognitive limitation, a reduction of uncertainty and insecurity by means of denial that must needs lead to some version of false security caused by the tacit undoing of anything that seems too problematic, complex or subtle' (p. 264). This is based on the presumption that the application of a particular research method is automatically going to guarantee and result in an

increase of knowledge. But on this condition the objects of research actually lose a great deal of their reality, thus being turned into faceless caricatures. According to Koch, methodological fetishism always displays compulsive and magic features (p. 259). Those familiar with Devereux's monograph Anxiety and Method in the Behavioral Sciences (1967)—which of course was one of the main inspirations for the topic of this year's EPF conference—will, no doubt, be reminded of his line of argumentation.

For those especially interested in Koch's genuine perspective you can find an 'update' of Koch's ideas in a publication of Howard Orlinsky (2006). Orlinsky is one of the most prominent contemporary researchers in the field of psychotherapy. In a communication to the Society for Psychotherapy Research he made the following statement: 'I must start by confessing that I don't really read psychotherapy research when I can help it. Why? The language is dull, the story lines are repetitive, the characters lack depth, and the authors generally have no sense of humour. It is not amusing, or at least not intentionally so.'

As a matter of fact, Orlinsky suggests that the implicit paradigmatic consensus of psychotherapy research merely represents an entrapment in a constricted and unrealistic model. What Orlinsky above all criticizes is that in practice the standard research model or dominant research paradigm implicitly defines psychotherapeutic treatment as a unidirectional process. And therefore he comes to the conclusion: 'The target of treatment is not actually the patient as an individual but rather a specifically diagnosed disorder'. Likewise the agent of treatment studied is not actually the therapist as an individual but rather a specific set of manualized treatment skills. Orlinsky criticizes the basic assumption of this model according to which the components of reality are self-contained units that can be brought into relation with other such units. Sigmund Koch

most certainly would go along with that critique of Orlinsky!

Critique of the idea of a unified science 2: practical irrelevance

But this feature—the desolate, fragmented state of theory in unified science psychology and psychotherapy research—is of minor relevance compared with another deficit of this approach: its complete lack of relevance for clinical practice. This feature has been a constant issue of debate in psychology and has been addressed in various ways by researchers and clinicians alike. This complete lack of relevance for clinical practice has since become some kind of running gag of unified science psychology and psychotherapy research.

For most of you who probably agree with Orlinsky's point of view and like him rarely do read papers on psychotherapy research here is an example from an article by Strupp who is one of the more prominent representatives in his field. In his article from 1989, entitled: 'Psychotherapy. Can the practitioner learn from the researcher?' (Strupp, 1989), Strupp makes the attempt explicitly to refute the practitioner's arguments against the applicability and validity of unified science psychotherapy research. Now, let's see what comes out of it: 'My collaborators and I have called particular attention to the damaging consequences of communications that are experienced by patients as pejorative, and we believe that the development of the therapist's skills in dealing with the patients' negative transference should receive particular attention in training programmes' (p.717). Drawing on extensive quantitative research experience Strupp then continues by listing a number of examples that are supposed to illustrate his main thesis. Let me cite only a few of the examples Strupp enumerates in his article, since they are no more than a mere description of psychotherapeutic interactions that surely any practising psychotherapist is more than familiar with. Strupp says that the empathic attitude of the

therapist is very important, and that he has to refrain from giving any advice; psychotherapeutic jargon is to be avoided; equally it is important to pay heightened attention to the ‘here and now’; the therapist should refrain from acting out his negative counter-transference feelings etc. What then follows is a long list of recommen-dations on how to deal with negative countertransference reactions gained from numerous quantitative psychotherapeutic research studies: (a) depreciatory and confusing statements on the part of the therapist cause negative therapeutic results; (b) even if the therapist only occasionally (re-)acts as described under (a) this can have a detrimental and devastating effect; (c) even very experienced psychotherapists do, from time to time, revert to such negative reactions and (d) therefore any psychotherapist should always be open to re-education; since (e) there is always the possibility that a certain category of patients is liable to provoke the therapist unwillingly to lapse back into those negative forms of interaction; (f) negative reactions on the part of the therapist can not completely be avoided but should be minimized by means of attentive self-monitoring’ (Strupp, 1989, p.722).

However, the really relevant question, that is: ‘What helps the therapist to become aware of his negative countertransference manifestations and to deal with them in a responsible and non-acting-out way?’ respectively: ‘How to minimize acting-out?’ is not answered by Strupp in his article. He therefore unintentionally confirms what he initially set out to refute in his paper: ‘Psychotherapy research has often been criticized by practitioners who believe that it has little to offer that can be used in professional practice’ (Strupp, 1989, p. 717).

At this point allow me to add an ironic note: Even if such research findings are completely irrelevant and of no real use for clinical practice we nevertheless can’t help but realize and admit that we are in the midst of a process of development where actual methods of research exert a

considerable impact upon the actual psychotherapeutic treatment and not vice versa. In order to render the clinical data comparable psychotherapy research today promotes and fosters a tendency to view the patient in a reductionist and de-indiviualised way. As a corollary of this tendency, modern psychotherapists are encouraged increasingly to adhere to a specific set of formalized treatment skills or technique that no longer considers the patient as an individual. I must admit that I was quite shocked when recently a colleague of mine—who is by the way a wellknown psychoanalyst—claimed that principally it should be possible even to formallize psychoanalytic treatment.

Thought experiment: Perfect nomothetic law

I would now like to invite you to a thought experiment.
Suppose Wallerstein had been successful in discovering every single psychological law that is relevant: what implications would this have for the interaction in the consulting room? The analysand would say something while the analyst with all the relevant psychological laws available to him would be in a position to interpret, explain and predict correctly anything the patients says or does. I simply mention here the phenomenon of a butterfly wing flutter in China causing a storm in Europe. That’s a good metaphor that beautifully captures the sheer impossibility of identifying any possible causes within a highly complex system.
Even though in the context of the unified scientific approach theoretical knowledge is used as a means or a tool to make predictions based upon this knowledge in order to then make practical use of it: thus the basic assumption of this approach is the logical equivalence of explanation and prognosis.
Let us now return to our thought experiment. There Wallerstein would apply his knowledge in the consulting room just as Newton would apply

his knowledge of the law of falling bodies in some experiment of physics. Newton would know exactly how high to hang the apple to guarantee that the apple reaches the ground at a certain pre-calculated velocity of speed. Accordingly Wallerstein would know exactly what to say to his patient in order to achieve the desired effect.

Whatever this hypothetical scenario of Wallerstein's interaction with his patient might be called—it could, in any case, no longer be designated psychoanalysis.

Wallerstein would rather act like some neuro-linguistic programming therapist. NLP is a form of 'psychotherapy' that is based on simple assumptions like this. Since psychotherapy research has found out that patients feel better when adopting the same bodily posture as their therapist, the NLP-therapist consequently imitates the bodily posture of his/her patient in order to convey good feelings to the patient.

The theories of analytic philosophy: Donald Davidson

I propose that these problems—the fragmented state of theory and the irrelevance of research for clinical practice—hint at a more fundamental construing error in the unified science approach and its epistemology. For further clarification I am now going to turn to analytic philosophy.

The epistemology of the unified science approach of psychology has its roots in the thinking of Descartes. In his 'Meditations', Descartes asserts that there isn't anything about which it is impossible to doubt. As a consequence of this the entirety of our intellectual perceptions is subject to doubt and may even be caused by a 'Genius Malignus'. In these conditions, external reality might ultimately be a mere figment of the mind. Now, the Cartesian doubt and its after effects have exerted a strong influence upon epistemology even in the 20th century. And this doubt has also deeply informed the research methods of unified science psychology

to this day: If a cause-effect link has been established that—with the likelihood of less than 5 per cent—cannot be attributed to chance, the hypothesis is considered scientifically proven.

Modern analytic philosophy is based and relies on the notion that logic and science represent paradigms for true theories. Thus, Wittgenstein's attempt at formulating a comprehensive logical system of sentences in his famous Tractatus Logico Philosophicus can be considered the climax of this development of analytical philosophy. But still, at the end of his Tractatus Wittgenstein has to admit: (6.52): 'We feel that even when all possible scientific questions have been answered, the problems of life remain completely untouched'. He later gave up the 'picture theory' of language the Tractatus Logico Philosophicus relied on in favour of an analytic theory of ordinary language use. This perspective of the later Wittgenstein can be considered to form the basis of modern analytic philosophy of language.

The revolution brought about by the linguistic turn in analytic philosophy consisted in a change of the Cartesian question. From now on the crucial question was no longer: 'How can the subject know the object beyond any doubt?'—but it was replaced by the question: 'What does a speaker mean if he makes the assertion that a sentence is true?' In a later statement Davidson compared the paradigmatic situation of the analytic philosophy of language with a tripod, that is: two speaking subjects and external reality—by taking away one of the legs the whole thing is going to collapse.

I think that for somebody not primarily concerned with analytic philosophy, it is probably rather difficult to appreciate the importance of this linguistic turn. (Incidentally, linguistic philosophy so far has had a relatively poor reception on the European continent compared to that in the Anglo-Saxon language area.) Once again: There has been a significant

change of the paradigmatic situation since Cartesian doubt, which was preoccupied with the question: ‘Can one truly perceive or know an object beyond any doubt?’ Now the question may be put as follows: ‘How can two subjects make themselves understood to one another on anything outside of them?’ As a corollary of that, Cartesian doubt has become the exception and is no longer the rule: Of course one can be wrong about almost anything—yet on the whole, it’s our established firm beliefs that ultimately put us in a position to realize that we were mistaken about one or the other of our beliefs. And as a rule we then draw the logical conclusion from this and will correct our errors.

The most prominent representative of this branch of philosophy, which has its key point of reference in the later Wittgenstein, was the American philosopher Donald Davidson. For lack of space let me just briefly sketch out a few of Davidson’s ideas relevant to the present discussion:2

In the course of their investigations Davidson and other analytic philosophers also became interested in the explanations of actions. They hold that every action can be explained by reference to a belief and a desire of the person acting. Here is a simple example of this template: Example:

(a) ‘X believed that drinking water satisfies his thirst.’ (Belief)
(b) ‘X was thirsty.’ (Desire)
(c) ‘X drank a glass of water.’ (Action of X)
‘(a) and (b) > (c)’ (Explanation) (Davidson, 1963)

Another part of Davidson’s investigations relevant for the present discussion is that explanations of actions by reference to belief and desire are genuine causal explanations and that they are epistemologically equivalent to explanations by causes in the natural sciences. One of the

corollaries of these ideas is Davidson's statement that there are no strict psychological laws because every explanation of an action depends on the perspective of the person acting. (As the above example dem-onstrates, the explanation of X drinking a glass of water because he wishes to satisfy his thirst would be incorrect if X actually wanted to kill himself and believed that the water was poisoned.) As the perspective of the person acting is not something objectively given, something definitely fixed, accordingly there can be no 'objective' explanation like in physics (Davidson, 1974, p. 70).3

You probably may find this bit of a schematic description of explanations of actions rather amusing. Even though it bears some relevance for the nature of psychoanalytic knowledge because it helps to understand how interpretations are structured (see the above example). Richard Wollheim explored the analogy of these explanations of actions based on linguistic analysis and the explanations derived from psychoanalytic concepts and theories. Wollheim states that for psychoanalytic explanations of actions it is characteristic that they represent an extension and completion of everyday psychological knowledge as these psychoanalytic explanations (a) unconscious motives are added; (b) another explanatory mode is applied by taking into account the associative logic of displacements of motives; (c) specific motives like 'castration anxiety', 'oedipal wishes', 'repetition compulsion', 'wishfulfilment', 'omnipotent thinking' are introduced; and finally (d) 'actions become contextualised', i.e. wishes and/or defence mechanisms are conceived of as being integral elements of a complex structure ontogenetically corresponding to the respective bodily zones: oral, anal, phallic and genital (Wollheim, 1993).

Let me give you a brief example in order to demonstrate the difference in perspective of this conception of theory and the one of the unified science approach. Or, put differently: What is at issue here is the

pluralism of psychoanalytic theories. From the perspective of the unified science research though, there is only one possible question: Which theory is the right one? To try to answer this question, this would require a standard research model involving particular treatment settings or treatment conditions and the comparative study of a number of interpretations based on basic assumptions from different theoretical backgrounds in order then to evaluate their respective truth content. I suppose you all more or less know and have a feeling that such a venture would ultimately turn out to be unsuccessful and in the end lead nowhere. From the perspective of both, Davidson and Wollheim, pluralism means that there are different schemata of 'explanations of actions'. According to this perspective the 'solution' is achieved in that two different speaking subjects—or, in our case, at the EPF conference, analysts of different psychoanalytic orientations—write down in detail and then present their clinical work and along with this discuss their theoretical basic assumptions in order then to try to understand and analyse together in every single case the perspective of the other colleagues. For us psychoanalysts there is, of course, nothing unusual about the fact that each psychoanalyst brings along his own psychoanalytic theory, his own individuality, his own personal history and his own patient and finally his own psychoanalytic process: all these data together will be decisive if an interpretation is ultimately 'right' or 'wrong'. After all: psychoanalysis is about communication and not some sort of scientific experimentation or experimental verification!

Ethnological example: The Working Parties of the EPF

It is beyond the scope of this paper to present here a clinical example from my own psychoanalytic practice in order to exemplify the issue in question. Instead I would like to carry out some field-research and discuss

the topic with the help of a few observations I made while participating in some of the EPF Working Parties on Comparative Clinical Methods (see Hewison, 2009).

Since the EPF Conference in Sorrento I have been a regular participant in various of these Working Parties. It is because of these Working Parties organized by the EPF that for me the EPF conferences proved to be the most rewarding and interesting conferences I ever attended. The setting of the Working Party (WPCCM) (as originally initiated by David Tuckett) gave me the unique opportunity to explore in depth and great detail a sequence of analytic sessions together with other colleagues from a wide range of psychoanalytic orientations and different cultural backgrounds.

Observation (1): The participants of the Working Party (as well as the entire atmosphere within the group) changed noticeably as soon as they were asked to classify - according to predefined categories—the various interventions having been made by their colleagues in the course of the group process. Whereas up to this point the group members seemed very committed and focussed on the task—in retrospect I would say: we communicated with each other by way of explanations of actions—they now regressed and almost behaved like immature, inattentive school children lacking in concentration, making jokes, chatting together and yearning for the imminent break.

Observation (2): One of the female moderators proved a competent, empathic and psychologically and analytically minded leader of the group during the clinical part of the workshop; but no sooner had the ‘classification-part’ begun she regressed and turned into some kind of authoritarian teacher. She put considerable pressure upon the group members, constantly reminding them of their task to classify and categorize the interventions made although the resistance of the group

against this task was considerable. Yet, to my surprise, the members of the group were not at all capable of talking about their resistance. Obviously, a significant splitting had occurred: During the work with the clinical material of the sessions presented the group members were able to make use of their various countertransferences creatively; but as soon as the 'classification part' was initiated by the moderator the participants invariably resorted to splitting, in other words, from now on their countertransference was experienced as a source of disturbance and not a source of inspiration—see Devereux!

Observation (3): When directly addressed to their psychoanalytic colleague who presented the case material the comments and interventions of the participants during the clinical part of the workshop would always be respectful, empathic and cautious about judging. Then, however, with the onset of the classification part the participants' attitude suddenly underwent a noticeable change towards being less considerate and less careful, almost as if the colleague wasn't present any longer, had become non-existent, so to speak, and the interventions were no longer expressions of personal experiences and feelings. Through the change of method—introduction of classification part!—at the same time a distance was established, which gave rise to the denial of the existential aspect characteristic of any psychoanalytic session.

Or, put more generally: a rater who in an experimental laboratory is listening to a psychoanalytic session would hear something quite different from what the analyst or his/her analysand are hearing, who both are involved in the analytic process personally and existentially.

Observation (4): The working method of the Working Parties is based on a one-way communication. Although the workshop participants continuously meet over a period of years they are not informed of the discussion meetings of the moderators nor are they in any way involved

in those discussions of the organizers of the Working Parties. When once during one of the coffee-breaks I was talking to a colleague of mine who also took part in the workshop, we both came up with the idea that the classification-categories might be conceived of as representing a onedimensional picture of a multi-dimensional reality, and I then jokingly proposed to my colleague perhaps to commit this idea to the taperecorder later on. As a matter of fact, I think that this bit of regression on my part may best be understood as an identification with the splitting inherent in the setting of the Working Parties, a splitting that is also codified in the unified science research approach according to which: the distillation of true knowledge depends upon the strict separation between, on the one hand, the test leader or scientific investigator and, on the other hand, the test person or study participant.

Conclusion: Three different kinds of knowledge

Now, let me finally draw a conclusion from the present discussion: I propose to differentiate clearly between three kinds of knowledge and the respective methods appropriate to achieve these three different aspects of knowledge in the psychoanalytic realm:

(1) Law-governed knowledge, which Wallerstein seems to believe in, a knowledge that is based on general laws as in physics.

(2) Average efficiency knowledge about the dynamics of groups, which I will discuss immediately.

(3) And finally the kind of knowledge that helps the psychoanalyst to arrive at an interpretation and to formulate it.

(1) I think, it has become clear by now that I am anything but a supporter of Wallerstein's general laws: the kind of knowledge generated by methods of unified science research and which I am going to call lawgoverned knowledge. Empirically speaking this kind of knowledge is of no

practical relevance whatsoever. Moreover, there are epistemological reasons that suggest that these general laws cannot exist at all. Any psychoanalyst trying to apply this kind of knowledge would no longer deserve to be designated a psychoanalyst but rather some kind of cognitive behavioural psychotherapist.

One of the characteristics of this law-governed knowledge is that it cannot but be an average knowledge: In order to avoid falling prey to the Cartesian 'Genius Malignus', unified science research relies on a multitude of observations (or investigations) based on the unshakeable belief that this 'bad spirit' may invalidate or falsify one or the other single observation but that it would not be within its power to invalidate (or falsify) a vast number of such observations. In the end the evaluation of the observations boils down to a proportion ratio whereby the results of the single observations are compared to the total number of observations made. This accounts for the fact that such general laws always only represent some hypothetical average, be it the average psychotherapist, the average patient or the average disorder; unfortunately—or, perhaps fortunately enough for us psychoanalysts—in our daily practice we as clinicians never come across such a thing as an average patient. What we meet with instead in our consulting room is an individual patient in the actual 'here and now' of the analytic situation who demands a particular interpretation that has a specific meaning for this particular patient only.

Our Danish colleagues invited the participants of this year's EPF conference to take part in a lecture on Kierkegaard and Freud focusing on their respective concepts of anxiety. It is an interesting and rewarding task to examine the affinity between Kierkegaard's and Freud's thinking on this topic. For the purpose of my present discussion I would like to put special emphasis upon another affinity between Kierkegaard's main concern and that of the psychoanalyst in the consulting room. In sharp opposition to the

systematic philosopher Hegel, who was the main representative of externalisation and the universal, Kierkegaard, on the other hand, emphasized the importance of the particular and the individual. In view of this perspective one might say that any interpretation that manages to reach the patient and make him or her feel understood and appreciated as a particular individual, is a good interpretation. In yesterday's presentation of his way of working with transference/countertransference dynamics, Giuseppe Scariati gave us a brilliant and vivid illustration of what is at stake in the psychoanalytic encounter with one particular patient when so generously sharing with us his own intimate ideas and very personal analytic working style.

Since law-governed knowledge can only describe regularities—whereas what is at stake in a particular case, for instance, in the analytic situation, is always the encounter of two different single individuals (or, as Devereux might put it: transference meets countertransference)—the relevance of law-governed knowledge for clinical practice is further diminished.

(2) There is another form of knowledge, which I am going to term efficiency knowledge. This kind of knowledge generated by methods of unified science research may in fact be relevant in terms of evaluation of the efficiency of psychoanalysis as a kind of psychotherapy: There are instances when groups of people are not primarily interested in the investigation of a particular patient in a particular session with a particular analyst but rather are concerned with questions about groups of patients, numbers and percentages: 'How many patients out of one hundred are successfully treated with treatment method X in comparison to treatment method Y?' Thus, society in general, governments and especially insurance companies must be concerned with getting answers to such

questions and are, indeed, only to a very limited degree concerned with the individual fate behind these numbers. One possibility to demonstrate to the public the efficiency of psychoanalysis is therefore to prove on scientific grounds that the financial investments in the enterprise that psychoanalysis can offer are worth the effort and are justified.

However, those questions of efficiency are not as easily answered as it may seem at first. For instance: what are the criteria for measuring the efficiency of psychoanalysis? And what's the point of reference for measuring this efficiency? And so on.

And therefore it's not a matter of psychoanalysts engaging in competition with other forms of psychotherapy in terms of efficiency but rather of having the courage to defend their own specific psychoanalytic perspective. Let me give you an example: Studies have shown that in the course of a psychotherapeutic treatment the general well-being of patients is steadily increasing. Researchers then came to the conclusion that from a certain number of sessions onwards no further improvement can be expected. What is, of course, nonsensical about this whole debate and its scientific research methods is the idea that we might be able to measure the well-being of human beings (for instance in numbers or percentages) and therefore—in order to take up the cudgels for psychoanalysis—it would be equally pointless to demonstrate that in the course of a psychoanalytic treatment 150% of well-being might be achieved.

Besides, there remains the fact that efficiency arguments as such can only be of very limited efficiency: If we psychoanalysts partake in the efficiency-contest we act as if believing that the main interests of society were motivated exclusively by rational considerations and weren't actually governed by intense and irrational affects. Although meanwhile many psychoanalytic terms have entered everyday language, psychoanalysis has, to this day, still not established a very good reputation in society. But

above all it is the medical profession that is still very reluctant to accept psychoanalysis—and in that respect nothing has changed since Freud—perhaps this situation has even worsened. One therefore might conclude: although efficiency knowledge may be of some political relevance today, it is actually only of very limited use for psychoanalysis. André Breetschen, for example, who lives in a country where psychoanalysis does not depend upon and is not part of the public health care system, may probably have a very different view on this efficiency debate than most of my German colleagues who constantly have to struggle to secure the funding of psychoanalysis through the public health care system and insurance companies.

(3) And finally, there is the kind of knowledge psychoanalysts make use of in the consulting room and that I am going to term: psychoanalytic knowledge. The paradigmatic example of this kind of knowledge is Freud's discovery of the Oedipus complex. Freud did not 'invent' the Oedipus complex but rather he discovered it. His discovery brought to light that the conflictual constellation of the Oedipus complex is a universal point of reference—and does not merely represent a singular event in the plot of Sophocles' tragic play. Thus the Oedipus complex is the paradigm for a certain kind of knowledge that cannot be validated nor de-validated or falsified by any series of test results or experimental designs. The appropriate method to validate and keep alive such knowledge is: to analyse. According to Wollheim—and I totally agree with him—a similar status must be assigned to all psychoanalytic concepts and theories as to the Oedipus complex: Klein's discovery of the depressive/paranoid-schizoid position; Winnicott's transitional object etc. All such elements of psychoanalytic knowledge represent complex patterns of belief-desirestructures in analytic philosophy language, interactions of the partstructures

of the personality in ego-psychology- or metapsychologylanguage, or unconscious phantasies in Kleinian language. They are not and cannot be verified or falsified—they make sense only when interpreting a single and specific situation (cf. Wollheim, 1993).

The nature of this kind of knowledge is still best captured by Freud's statement about the inseparable bond between cure and research:

'In psychoanalysis 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' (Freud, 1927).

I conclude: the three variants of knowledge discussed by me above are all categorically different, and for that reason cannot and should not be replaced by one another. To my mind, mixing up these three variants of knowledge and the methods appropriate to achieve them would harm our conception and our ideas of what psychoanalysis essentially is about, how it works and how it should be further developed.

(Translated from German into English by M. A. Luitgard Feiks and Juergen Muck, Nuertingen am Neckar)