Introduction

This is a book about the future that we hope will arouse the curiosity of clinicians and point a direction for researchers. It marks the surprisingly rapid evolution of psychodynamic psychotherapy research from an applied toward a basic science, and, as its title implies, describes strategies to follow rather than results to live by. It was not always thus. A quarter of a century ago the editors of two volumes of psychotherapy research reports summarized the state of the field then:

Although there has been a great accumulation of clinical observations and experimental findings, the field has made relatively little progress. There has been little creative building on the work of others (Parloff and Rubinstein 1962).

Psychological research generally has tended to be insufficiently additive. Research people often find it hard to keep informed of related work done on the same site and elsewhere, and therefore do not build upon each other's foundation (Luborsky and Strupp 1962).

Twenty-five years later the state has significantly changed, as we hope will be adequately illustrated (though of course not proven) by the research strategies reported here. But for many of the intervening years psychodynamic psychotherapy research has served as handmaiden to other masters, employed both in attempts to demonstrate its efficacy over untreated controls, and to show its superiority over other treatment techniques including "placebos" (see Smith, Glass and Miller 1980). The first task is widely believed to have finally been settled in favor of the good effects of any psychotherapy, but the supposed superiority of psychodynamic therapies over others has not been proven.

Given this state of affairs a significant minority of psychodynamic researchers (including most of those represented in this volume) have subtly changed their agendas and commitments. They have come to regard their true goal as not simply to ask, "Does psychotherapy do any good?" but rather to ask fundamental questions about the nature of psychopathology, how psychotherapy addresses the pathology, and what the effects of the treatment interventions are on the pathology. Such questions are part of the strategies of a basic science in its own right, one with its own agendas, its own coherent problems, and its own methods for solving them. Its overall strategy is aimed at three things: (1) the detailed

description of an individual patient's maladaptation, (2) the identification of the nature of interventions that succeed in changing those maladaptations, and (3) the development of mini-theories to explain the relationship between (1) and (2). In the process these researchers have rediscovered some very old basic categories, namely wishes and beliefs, and they have begun to study them divorced from the polemics and politics of past applied necessity.

Most remarkable of all, given the diverse backgrounds of the researchers, during the past decade there has been a demonstrable, and, we believe, quite unplanned convergence of both strategies and tactics. Within the more limited domain of the psychodynamic tradition (read loosely, Freudian) the convergence of strategies has been ever more apparent, as is the manifest concentration on repetitive maladaptive structures where each of these words is becoming increasingly well defined. Moreover, as should become clear to a careful reader there is surprising overlap in the early findings.

We hope that one unanticipated but happy result of this convergence and shift toward a basic science strategy will be that psychoanalysts and psychodynamic therapists more generally might accommodate to this fledgling science and to both the new research insights and the mini-theories designed to explain them. Ultimately (say in another quarter century) we might hope that research findings will share equal billing with published clinical reports. Researchers are not longer mainly trying to demonstrate that psychotherapy products are safe and effective. Rather, they have begun to investigate psychic structures under conflict in their own right – an entirely different and more fundamental goal.

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All of the contributors to this book were among those invited to participate in July 1985 in the 8th Workshop on Empirical Research in Psychoanalysis prior to the International Psychoanalytic Congress in Hamburg. The workshop was organized by the Department of Psychotherapy (Prof. Dr. H. Thomä, Chair), the collaborative research program # 129, "Psychotherapeutic Processes" (Prof. Dr. H. Kächele, Director) of the University of Ulm and the Psychoanalytic Institute of Ulm, and was generously supported by the Breuninger Foundation of Stuttgart. Originally our plan was to produce a simple Proceedings of the Workshop. But because of other commitments and the wishes of a number of participants to expand on or revise their contributions we decided on a simple collaborative volume. And curiously, our final classification of the chapters into two Parts, (I) Towards the Objective Analysis of Psychodynamic Structures and (II) The Evolution of Single Case Study Methods, also classified the contributors into Americans and West Germans! In fact, these categories seem to reflect quite different, though complementary, research strategies in the two countries, as will become evident to the reader.

In the spirit of our postulated paradigm change we emphasize the commitment to systematic empirical research by including, as Chapter 2, a complete transcript of a Specimen Hour which is a source of common data (to different degrees) for six of the Chapters (3-7 and 11).

In the same spirit we have given pride of place in Chapter 1 to Strupp, Schacht and Henry's principle of PTO Congruence – a principle whose time has come, not by design, but as an implicit consequence of the paradigm shift. This principle is that the description and representation, theoretically and operationally, of a patient's conflicts, of the patient's treatment, and of the assessment of the outcome, must be congruent, which is to say, must be represented in comparable, if not identical terms. Strupp, Schacht and Henry put it this way:

The principle of the P-T-O Congruence proposes that the intelligibility of psychotherapy research is a function of the similarity, isomorphism, or congruence among how we conceptualize and measure the clinical problem (P), the process of therapeutic (T) change, and the clinical outcome (O) (p. 7).

Their claim is that the requirement for PTO congruence differs sharply from the procedures of a traditional psychotherapy treatment study in which, for example, the problem is evaluated by a set of diagnostic criteria (e.g. DSM-III, SCL-90, Beck Depression Inventory, etc), the treatment is evaluated by the "therapists' adherence to specified protocols" of a particular school of psychotherapy, and the outcome is assessed by a composite index of the repeated initial measures, an index which often has perverse measurement properties. By contrast, when there is PTO congruence, the representation of the psychopathology explicitly focuses on what the candidates for change are and thus explicitly points to what has to change if change is to occur; then the treatment measures can be explicitly examined for their specific effects on these candidates; finally, the outcome is measured in terms of explicit changes in the original psychopathology. Thus, this is tantamount to a theory of change independent of any particular brand of psychotherapy because the problem, its treatment, and the outcome are all intrinsically defined by the assessment of the pathology.

Although Strupp et al. explicitly formulated this principle, its true origins go back to Luborsky's (1977) first representation of a patient's problem as the Core Conflictual Relationship Theme (CCRT). This theme consists of three categories of events found in the patient's "relationship episodes" told to the therapist during treatment: a Wish (W), a Response from an Other (RO), and a Response from the Self (RS); the goal was to represent one or more of the most repetitive and important CCRT's for each patient. In Chapter 6 Luborsky and Crits-Christoph illustrate this method with two CCRT's that they found in the Specimen Hour. Then in Chapter 8 Crits-Christoph and Luborsky demonstrate the natural extension of this approach with the development of a measure to assess the degree to which the therapist's interventions accurately address the CCRT and by their assessment of the patient's degree of self-understanding of his own CCRT wishes and responses. And obviously the next step (already underway) is the assessment of the fate of the CCRT itself. Thus in just over a decade one particular mini-theory of pathology has led to a complete instantiation of the principle of PTO Congruence.

This implicit strategy is well represented in Part I of this volume. It includes Strupp et al.'s Dynamic Focus (Ch 1), Bucci's Frames (Ch 3), Dahl's Frames (Ch 4), Hoffman and Gill's

Patient's Experience of the Relationship with the Therapist (Ch 5), Silberschatz et al.'s Plan Compatibility (Ch 9), and Teller's Frames (Ch 11). Each of these approaches depicts the central roles of repetition, structure, and knowledge re-presentation, including the careful, systematic description of a patient's manifest wishes and beliefs. For example, it is clear that the logic of Strupp et al.'s Dynamic Focus is exactly comparable to Luborsky's although the former use four predetermined categories, having differentiated RO's into Expected and Actual RO's.

On the other hand Dahl's and Teller's and Bucci's Frames differ significantly in that for them the clinical events are specific for each patient and are simply and directly classified into the everyday commonsense categories (anticipating Edelson's (1985) later advice) that make up the Frame structures. They do not use a small set of predetermined categories that are assumed to be regularly arranged in a fixed sequential structure (e.g. W, RO, RS). Instead they permit each individual structure to be determined by the repetition of particular sequences of each patient's specific events as they emerge in the narratives of the free association itself. Nonetheless it is worth noting that thus far all the events that make up these Frame structures are easily categorized as special cases of wishes and beliefs, i.e., emotions and defenses. And clearly, interventions can be judged for the degree to which they address the Frame events and structures and outcomes can be judged by the fate of the Frames themselves.

Of course it seems likely that clinicians who are used to thinking in terms of unconscious libidinal and aggressive drives and hidden defenses will not only resist the strategy of using manifest content to represent pathology, but will believe it to be deeply implausible. Such persons might well find Hoffman and Gill's focus on different categories of transference manifestations a more fitting way to represent a patient's pathology. Nonetheless it is important to note that they too have consistently and firmly rooted their scoring in the manifest data of transcripts. One does not have to agree with all of their judgments about the Specimen Hour (e.g. the basis for the analyst's intervention in paragraph 51 and/or the overall rating of 3.0 which they assign to the analyst's conduct of the hour) to applaud their efforts to systematize the description of transference phenomena. Certainly the description o the problem in terms of the transference can lead to a systematic assessment of the degree to which the analyst's interventions accurately address transference manifestations. And presumably the outcome could be evaluated by changes in the transference manifestations.

On the other hand clinicians and researchers who are hungry for new and different psychoanalytic theories might be attracted by Weiss's (1986) hypotheses of "Higher Mental Functioning" and "Unconscious Pathogenic Beliefs." Weiss has postulated that the dynamic function of the therapist's neutrality lies in its effect on the patient's ability to express warded-off and unacceptable wishes and beliefs. Thus in treatment a patient repeatedly tests a therapist to find out what it is safe to express and the degree to which the therapist's response both contradicts the patient's pathogenic beliefs and fits the patient's "unconscious plan" to change. In Chapter 9 Silberschatz, Curtis, Fretter and Kelly report on their efforts to empirically compare predictions from Weiss's theory with those derived from standard

analytic theory, using both psychoanalytic and short-term psychotherapy data. Ultimately Weiss's theory requires the formulation of a patient's "unconscious plan," but Silberschatz et al. do not deal here with the complex issues involved in formulating this plan; Caston (1986) has written about the procedure and the reliabilities of formulating this plan.

To the degree to which Koenigsberg et al.'s development of a method for describing therapist's techniques with Borderline patients is based on an implicit or explicit theory of the pathology of these patients, then their approach also implies at least PT congruence. Teller's concluding chapter of Part I will be considered along with the final chapter of Part II.

In sum, there has been an impressive convergence among a group of American dynamic psychotherapy researchers in the past decade. These approaches have all been characterized by: (1) Mini, local, modest theories about the essential characteristics of typical (meaning, recurrent) maladaptive structures of behaviors that are reliably definable and describable. Among these are CCRT's, DF's, FRAMES, and PERT'S.

For the first time, we have converging research definitions of psychopathology that have been translated into relatively narrow, formalized, structural descriptions. And the power of these descriptions is that they in turn can serve as guides for clinicians to likely specific therapeutic targets. In other words, the research categories, derived from many different sources, may ultimately, we hope, offer practical clinical foci, instead of the traditional situation in which clinical problems have defined research foci.

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Beginning Part II, Kächele, Thomä, Ruberg and Grünzig state that "the tape-recording of psychoanalytic sessions should by now be standard procedure for those who are prepared to undertake serious empirical research on the psychoanalytic process" (p. 179). But of course they are not blind to the fact that "the number of those who expose themselves to this procedure is still small, nearly as small as the number of those willing to engage in the careful scrutinizing of what they do when practicing psychoanalysis" (p. 179). Later they directly confront one of the central issues involved:

When a psychoanalyst asks a patient for informed consent to make audio recordings of the analysis he explicitly or implicitly informs the patient that the boundaries of the privacy of the consulting room are extended to involve other people, known or unknown to the patient. . . . In contrast, patients undergoing supervised analyses are rarely if ever informed of the fact and the implicit extension of the boundaries of privacy that it entails. It is curious that analysts who might be very uncomfortable with the first extension, which the patient knows about, are likely to be quite comfortable with the other extension of which the patient is typically ignorant. (p. 192)

This contrast also captures a major difference of the Ulm Psychoanalytic Institute form most other Psychoanalytic Institutes, American and European, namely its extraordinary

commitment to empirical research. Its members, as part of the Department of Psychotherapy of the University of Ulm, have participated in establishing a large and significant database of recorded psychoanalyses and psychotherapies, successful and unsuccessful, as well as initial interviews, etc. As Mergenthaler and Kächele document in Chapter 13, many years ago they anticipated this need and the result now is the Ulm Textbank, far and away the largest accumulation of transcribed sessions. These are all stored in a protected computer system together with sophisticated classification and retrieval programs which make them a prized resource for interested and qualified researchers throughout Europe and America. There is little question but that the Textbank is now the true prototype for others to emulate. And its usefulness to English speaking researchers will increase as transcripts in English continue to be added.

Just as a significant portion of Part I has been organized around the Specimen Hour, five chapters of Part II deal with Case B from the Ulm Textbank: Kächele et al. (Ch 12), Grünzig (Ch 14), Neudert and Hohage (Ch 15) Hohage and Kübler (Ch 16), and Leuzinger-Bohleber and Kächele (Ch 19). These innovative approaches are nowhere better illustrated than by Grünzig's use of time-series analyses to study problems of sampling in Case B. His results speak for themselves. On the one hand he shows that several computer content-analysis measures applied to four quite different samples of the psychoanalysis all gave essentially similar results when they "spanned the whole treatment." On the other hand samples of the first and second halves of the analysis gave quite different results, as one would expect if significant changes in the patient had indeed occurred.

Few psychotherapy researchers haves used sophisticated time-series-analysis techniques. This is a shame because, as Grünzig's analyses show, despite using what might appear to the uninitiated as vastly oversimplified measures, when applied to testing carefully chosen clinical hypotheses, these methods have demonstrated their ability to empirically support one hypothesis over rivals. In principle these techniques are even powerful enough to allow one to answer questions about the causal nature of interactions between patient and therapist. As Grünzig puts it, "Who mainly influences or follows whom? Does the patient adapt to the therapist or vice versa or neither?" These are questions we all want to answer and we owe it to ourselves and to our field to master and use these methods.

Hohage and Kübler, in developing their measure of Emotional Insight, also anticipated Edelson's (1985) advice to use commonsense categories derived from manifest contents. Indeed common sense supports their claim that "raters do not have to be clinically trained because the judgments are based on the language characteristics and not on clinical inferences" (p. 244). Using sessions from Case B they applied five scales which, taken together, are meant to capture the concept of Insight. It is worth noting that their categories of Emotional Insight and Cognitive Access strongly remind one of Bucci's Referential Activity (Ch 3), which indexes the degree to which the verbal system accesses and represents nonverbal (read emotional in this context) contents. They support their claim of superiority fo Emotional Insight over previous scales such as Meaningfulness, Productivity, or Experiencing by showing details of the interactions between the patient and analyst in hours

1, 2, and 7, including the interactions which correspond to plots of the course of four of their scales. Clearly this is another promising tool for assessing the effects of analyst/therapist interventions.

Researchers whose results are based on transcripts of audio recordings of therapy sessions regularly expect and receive pointed questions from analysts and therapists who are skeptical of their ignoring nonverbal and other information in the treatment setting. Such clinicians may take real comfort from Krause and Lütolf's research on facial expressions during treatment. What their meticulously planned and executed study shows is that a patient's facial expressions may indeed reveal both his central conflicts and his defensive expressions, such as masking anger and disgust by smiling. Moreover, their finding that the therapist smiled at the patient almost exclusively when the patient was not masking anger, raises the question of the curative function of such interaction. This is surely a salutary corrective to the omissions of most of the other contributors to this book and it speaks to the importance of encouraging further similar research.

At the same time, those who are skeptical of the value of using measures of manifest contents and scaling etc. should carefully read Neudert and Hohage's study on different types and sources of suffering during psychoanalysis. Indeed their interest in this question derived from the current theoretical controversies over the function of the analyst's abstinence in psychoanalysis (cf. Silberschatz et al., this volume). And their ingenious method of analysis permitted them to discover the answer in one block of hours in which the patient manifestly complained about the analyst more than at any other time in the treatment This was demonstrably the result of the patient having learned from the analyst's abstinence (in this instance, refraining from being defensive) that it was safe to fully experience her fears of being observed and her fears of physical closeness to men, thus supporting Weiss's (1986) Control-Mastery hypothesis over the prediction of standard psychoanalytic doctrine.

Since Meyer is clearly addressing a fundamental (and nearly totally unresearched) question about the role of a clinician's "Minimodels" in the conduct of therapy it is important to distinguish his concept from the "mini-theories" described earlier in this introduction. Indeed the differences are apparent in Meyer's labels such as: "sadomasochism," "castration anxiety," "anal assault," and "inhibition of exhibitionistic wishes." Such terms, common in clinical papers and discussions, are conspicuously rare in the research literature. Why so? We believe that a substantial part of the answer lies in our Coda at the end of his chapter in which we note analysts' pervasive tendency to seek confirmatory evidence for their prior theories and the related tendency to use theoretical categories as initial classificatory schemes instead of following Edelson's (aforecited) advice to use commonsense categories first. Our emphasis on the fundamental role of starting at the surface, with manifest content, and theorizing at later stages, as illustrated elsewhere throughout this volume, is our basic response to Meyer and the large number of clinicians who will surely be sympathetic to his approach. Nonetheless, it must also be said that studies about "What Makes Psychoanalysts Tick?" are conspicuously rare and fraught with difficult methodological problems.

Finally we are left with the last chapters of Parts I and II, which have to do with Artificial Intelligence (AI). What on earth are they doing here? What can AI possibly have to do with psychoanalytic and psychotherapy research? And what chutzpa enables Teller to propose A as a basic science for psychoanalysis? In fact she goes so far as to suggest that if the disciplines that make up current Cognitive Science had been available to Freud he might we have created an AI model of the mind instead of the seventh chapter of The Interpretation of Dreams. Those who are curious for answers will have to review her arguments and decide for themselves whether her proposal for a Pattern-Directed Inference System (PDIS) is too much pie in the sky. Its conception is simple: it should do what analysts and researchers have demonstrated they can do, namely detect the many different repetitive maladaptive patterns of behavior that are illustrated in this book. Whether a computer program will be able to do the same is simply unknown at this time.

Leuzinger-Bohleber and Kächele have taken a different approach. They started with a psychoanalytic model of the mind that Clippinger (1977) incorporated in ERMA, a computer program that actually produced a convincing example of text from a patient in conflict. One o ERMA's central postulates is that the mind is composed of a number of independent, but interacting modules such as Freud (roughly, ego) and Calvin (superego). Then, turning matters on their head, they asked human judges to examine psychoanalytic transcripts of one "unsuccessful," two "moderately successful," and two "very successful" patients, to play computer, so to speak, and see if changes in the functions of the modules correspond to what one might predict clinically. The fact that the judges' ratings (and computer content analyses) discriminated the three outcome categories is itself warrant for pursuing further such approaches. But it is well to keep in mind that, although an AI model was used in this essentially clinical research, more direct contributions of AI methods and technology to psychoanalytic process research still await us.

Finally, let us say once more that our intent has been to outline strategies, not to proclaim conclusive findings. Nonetheless we believe that strategies such as these will indeed make i possible for some reviewers a quarter of a century from now to look back on what we have done as crude and elementary compared with what they will have achieved by then.
Not all participants in the Workshop chose to contribute chapters to this volume.

Hartvig Dahl