

PHILIP F. D. RUBOVITS-SEITZ

Depth-
Psychological
Understanding

The

Methodologic

Grounding of

Clinical

Interpretations

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UNDERSTANDING

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Clinical Interpretations



Philip F. D. Rubovits-Seitz



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To the memories of two fathers,

Charles L. Seitz, M.D.

Frank E. Rubovits, M.D.

Contents

<i>Preface</i>	<i>ix</i>
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I. HISTORICAL BACKGROUND

1 Trouble at the Source: Freud's Methodologic Conflict	3
2 The Postpositivist Turn and the "Lingering Ghost of Positivism" in Interpretive Methodology	21

II. CONTEMPORARY APPROACHES

3 Methodologic Lag in Some Contemporary Models of Interpretive Inquiry	45
4 Some Language-Based Models of Interpretive Methodology: Evaluation of Linguistic, Discourse, and Narrative Models	67

III. NONCLINICAL COMPARISONS

5 Some Nonclinical Methods of Inferring Latent Contents: Comparisons with Clinical Inference and Interpretation	95
6 Evaluation of "Commonsense" (Intentional) Psychology as a Model of Interpretive Inquiry	147

IV. JUSTIFYING INTERPRETATIONS

7	The Probity of Clinical Interpretations in the Light of Grünbaum's Critiques	171
8	Justification of Interpretations: Evaluation of Individual Methods	211
9	Pluralistic, Posttherapeutic Justification of Interpretations: An Illustrative Case	249

V. SUMMARY AND CONCLUSIONS

10	The Methodology of Clinical Interpretation: Problems and Progress	285
	<i>Notes</i>	315
	<i>References</i>	343
	<i>Index</i>	431

Preface

This book deals with what Edward Glover (1952, p. 405) called “the Achilles heel” of psychoanalysis, that is, the uncertainties of inferring latent meanings and determinants in clinical data.¹ Glover chose that metaphor to emphasize the unsolved problems, limitations, and scientifically insecure status of our interpretive methods. We must distinguish in this connection between the methodology and the technique of interpretation. The former, the broader concept, includes both the construal of latent contents, on the one hand, and criteria of communicating depth-psychological information to patients, on the other, with only the latter comprising interpretive “technique.” This book focuses specifically on the construal and justification phases of the interpretive process.

Inferring latent contents is the “stock in trade” of psychoanalysts and dynamic psychotherapists, but the literature on this process is surprisingly meager and does not convey its central role in our clinical and investigative work. Because the construal of latent meanings and determinants is crucial to depth-psychological understanding, we must know as much as possible about the nature and functioning of interpretive methods—their modes of operation, capabilities, limitations, problems, and corrective measures. We have taken these methods largely for granted, however (Schimek, 1975, p. 863), and have assumed mistakenly that they are more reliable and accurate than they actually are.² This book explores the roots and results of neglecting these problems; evaluates clinical models of construing and justifying latent contents; and reviews both problems and progress in our interpretive methodology.

During the first half-century of psychoanalysis, Freud and his followers assumed that their methods of inferring latent meanings and determinants were scientifically sound. Freud (1905a)

claimed confidently and repeatedly that “it is easy to learn how to interpret dreams, to extract from the patient’s associations his unconscious thoughts and memories, and to practise similar explanatory arts: for these the patient will always provide the text” (p. 116).³ When analysts had difficulty agreeing on the interpretation of the same case material, Freud dismissed their disagreements with the ironic comment, “*Quot capita, tot sensa*”—“as many heads, so many opinions.”⁴ Even at the end of his career, Freud (1937b) continued to insist that psychoanalytic methods of confirming interpretations are “in every respect trustworthy” (p. 263).

The uncertainties of clinical inference and interpretation did not begin to surface in psychoanalysis until the 1950s, when Glover (1952, p. 405) and Thomas M. French (1955, pp. 502–503), working independently of each other and employing different methods of investigation, reported disturbing indications that clinical interpretation may not be as easy or reliable as Freud claimed. French was dismayed to find that individual analysts react differently to the same clinical data, and Glover expressed alarm at the variability of conclusions based upon interpretations.

The reports by Glover and French led a group of psychoanalysts in Chicago to undertake a systematic investigation of the consensus problem. Coordinated by the present writer (Seitz, 1966), the research team attempted to interpret various amounts and kinds of clinical data. We worked together for over three years, but were never able to achieve satisfactory consensus on the blind interpretation of the same case material. Since then, other investigators have documented the consensus problem;⁵ and some attempts have been made, though with questionable success, to surmount this limitation of our interpretive methods.⁶

It may be some comfort to clinicians that the consensus problem is not confined to clinical interpretations by psychoanalysts and dynamic psychotherapists, but applies equally to interpretive judgments in other clinical fields,⁷ and to the human and social sciences generally (Bernstein, 1988a, pp. 88–89). Since investigators in all of these disciplines have similar difficulties in agreeing on the interpretation of the same data (or texts), our problem does not appear to be that clinical data are unusually obscure; rather, interpretive methods in all fields of human study suffer from limitations of reliability. We should not be surprised, therefore, that questions of how best to infer latent contents and how

to formulate and justify interpretive hypotheses in depth-psychological disciplines remain unsolved.⁸

Clinical Interpretation as a Method of Inquiry

Interpretation is an increasingly important aspect of modern thought, part of the movement from metaphysics to epistemology—away from things as they are to how things are known (Coltrera, 1981, pp. 84–85). All branches of science, including the physical sciences, employ interpretive methods of some kind and to some extent (Holt, 1978a, pp. 50–51).⁹ In the human and social sciences, interpretive methods are the commonest approach to investigating human beings (Dreyfus and Rabinow, 1983, p. xxvii).¹⁰ Interpretation is thus a general method of inquiry, and clinical interpretation a specific variety of the more general approach.¹¹ Psychoanalysis and dynamic psychotherapy are relative newcomers to the art and science of interpretation, which began with Aristotle (Ricoeur, 1970, p. 20).

Clinicians tend to think of interpretation mainly in the context of therapeutic interventions—the communication of depth-psychological information to patients;¹² but the interpretive process is first and foremost a form of inquiry—an attempt to gain depth-psychological information and understanding. The philosopher Michael Polanyi (1966) points out that one gains an understanding of the mind through a process of inquiry, which involves picking out clues that have a bearing on something they seem to indicate. “And as in a scientific inquiry, many of the clues will remain unspecifiable and indeed be subliminal” (p. 31).

The therapist's job is thus primarily to learn, not to teach (Schwaber, 1990b, p. 239).¹³ During therapy sessions, for example, the clinician's mind is engaged in a continual process of interpretive inquiry, attempting to recognize and understand clues to unconscious meanings and determinants. Conveying depth-psychological information to patients is an important but only sporadic feature of the interpretive process.

Paradoxically, however, a vast literature has accumulated on problems of interpretive “technique,” that is, whether, what, when, and how to communicate depth-psychological information to patients, while relatively little attention has been paid to our

methods of seeking, construing, formulating, and justifying latent meanings and determinants. This volume, by contrast, focuses specifically on the latter problems—the methods clinicians employ in the process of interpretive inquiry.

With the increasing recognition of relational and interactive factors in the therapeutic process, some clinicians no longer consider interpretation and insight the principal determinants of therapeutic change.¹⁴ Interpretations are viewed as part of a larger interactive process (Greenberg, 1994, p. 12).¹⁵ Yet depth-psychological understanding seems more important than ever, for effective therapeutic interactions and accurate understanding of both patient and therapist appear to depend on and complement each other in the process of cure.¹⁶ Thomä and Kächele (1975, p. 88) note in this connection that scientists often underestimate the role of understanding because they tend to equate it inappropriately with philosophical speculation, whereas in fact understanding is one of the conditions of progress in every field.

The clinical process of interpretive inquiry attempts to identify unconscious determinants as well as meanings. Relations between meanings and determinants are not well understood, however, and often are difficult to distinguish (Shope, 1973, pp. 284, 290, 303).¹⁷ Historically, clinicians have assumed that in causal systems such as psychoanalysis and dynamic psychotherapy, the meanings that one construes often suggest the nature of their determinants (Edelson, 1988, p. 332).¹⁸ The philosopher Adolf Grünbaum (1984, pp. 55–56)¹⁹ maintains, however, that the interpretation of meaning relations in clinical data tells one nothing about underlying causal connections (for example, unconscious motives). This fundamental methodologic question has profound implications for the theory and practice of interpretive inquiry—an issue that is discussed at length in Chapter 7 of this volume.

Interpretive inquiry is an exceedingly complex and incompletely understood process which involves conscious, preconscious, and presumably also unconscious operations (compare Sandler, 1976, and Ogden, 1997, p. 587, n. 1, regarding unconscious “understanding work”), as well as numerous components and several overlapping stages, which include:

1. Various types of *prerequisite knowledge*, including “competences,” which are preliminary to but necessary for interpretive work—what Gombrich (1969, p. 71) calls the interpreter’s need for a “very well-stocked mind.”

2. A set of *basic methodologic (core) concepts*, that is, general background assumptions (in contrast to specific clinical theories) that orient, guide, and inform interpretive inquiry (see, for example, Rapaport, 1944, pp. 182–220).

3. *Data-generating methods*, some applicable to the patient and others to the therapist. The goal of both is to produce as extensive, diverse, and relevant a data base as possible for interpretive inquiry.

4. *Data-gathering methods*, for observing and collecting as large a number and as wide a variety of clinical data as possible from both patient and therapist.

5. *Data-selection methods* and criteria, to reduce the voluminous clinical data to a workable but adequate sample of highly relevant information.

6. *Data-processing methods*, for cognitive transformation of selected clinical data and information into unique personal meanings and determinants that are specific to the individual patient.

7. *Construction* of tentative, alternative interpretive hypotheses.

8. Methods of *checking, revising, and rechecking* alternative constructions in order to determine the most plausible hypothesis.

9. Methods of *justifying* the most plausible interpretive hypothesis employing multiple criteria of evidence and truth.

10. *Reformulating* the hypothesis verbally so that it may be conveyed to and understood by the patient—the technique of interpretation.

11. *Progressive modification* of the interpretation in response to feedback from and “negotiation” (Goldberg, 1987, pp. 122–125) with the patient, as well as from further information as it accrues during the course of the therapeutic process.

12. *Reflection* on one’s interpretive understanding of individual patients, which occurs both during and outside of therapy sessions and may lead to the formulation of tentative “working orientations” and “grounded hypotheses” concerning the patient and the patient-therapist dyad (Tuckett, 1994b, pp. 1174–1175).²⁰

Organization of the Book

Progress in the methodology of interpretive inquiry has followed a distinctly dialectical course, marked by positivist/antipositivist, objectivist/subjectivist, intrapsychic/interpersonal, doctrinal/

contextual, instinctual drives/object relationships, conflict/deficit, insight/interaction, and other debates (compare, Grolnick, 1982, pp. 697–698). Even the origins of clinical interpretation involved methodologic conflict between Freud's positivist preference for purely observational methods versus his clinical need for a flexibly nonpositivist interpretive approach. Chapter 1 focuses on Freud's attempts to deal with that methodologic conflict; some of his attempts were prescient and productive, while others were not.

Chapter 2 reviews the paradigm shift in general science from a positivist to a postpositivist perspective, compares the two viewpoints, and stresses the compatibility of interpretive inquiry with a postpositivist approach (Nasser, 1994, p. 179). The legacy of Freud's positivism still pervades clinical thinking, however, and interferes in some ways with progress in the further investigation and development of our interpretive methodology.

Chapters 3 and 4 evaluate post-Freudian models of interpretive inquiry, focusing on problems and progress in contemporary approaches. Chapter 5 compares methods of clinical interpretive inquiry with interpretive approaches in nonclinical fields and notes particularly the potential relevance of some nonclinical approaches to the methodology of clinical interpretation. Chapter 6 evaluates a relatively new (nonclinical) model of interpretive inquiry, a depth-psychological extension of "commonsense" (intentional) psychology.

The next three chapters discuss problems and progress in justifying interpretations. There is a great deal more to know about this subject than I have included in these chapters; interested readers can consult the end-notes for suggestions about where to look for further information. Chapter 7 reviews the debate stimulated by Adolf Grünbaum's (1984, 1986, 1993) philosophical critiques of psychoanalysis. The discussion focuses specifically on aspects of the critiques and ensuing debate that bear on the probity of low-level clinical inferences and interpretations, in contrast to higher level theories. Chapter 8 evaluates various methods and models of justification and Chapter 9 presents a clinical case illustrating a pluralistic, posttherapeutic approach to interpretive justification.²¹ The final chapter summarizes and attempts to integrate the principal themes and conclusions of the book.

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PART I

HISTORICAL BACKGROUND

The work of analysis consists of two quite different portions. . . . the person who is being analysed has to be induced to remember something that has been experienced by him and repressed; the dynamic determinants of this process are so interesting that the other portion of the work, the task performed by the analyst, has been pushed into the background.

[Sigmund Freud, 1937b, p. 258]

It is strange indeed that so little emphasis has been laid on the absolutely primal role of methodology in psychoanalysis. . . . methodology is the very essence of psychoanalysis.

[Roland Dalbiez, 1941, p. x]

Even for the practical man of today, whose science is exposed to attacks and revisions which he is supposed to assimilate or reject, some methodological thinking is indispensable.

[David Rapaport, 1967, p. 174]

The formulation of interpretations is performed, as are all of our analytic acts, under the influence of hidden preconceptions. We partly know why we formulate them as we do, and partly we do not. To discover these preconceptions and bring them to consideration is always a useful and widening enterprise.

[Dennis Duncan, 1989, p. 694]

1

Trouble at the Source Freud's Methodologic Conflict

In one of his encyclopedia articles, Freud (1923a) noted that psychoanalysis began as an “art of *interpretation*,” that is, as an attempt to discover hidden meanings in patients’ associations (p. 239).¹ As a positivist, Freud would have preferred to use purely observational rather than interpretive methods (see, for example, Freud, 1914b, p. 77); for the scientific goal of positivism is the discovery of absolutely certain knowledge, obtained by rigorous observational methods applied to strictly empirical data. Freud (1937b) was intent on demonstrating that psychoanalysis is a natural science, and that its methods are “trustworthy in every respect” (p. 263). Despite his positivist ideals, however, Freud found it necessary to develop and use nonpositivist interpretive methods, because without such methods he could neither discover nor understand latent contents in his patients’ communications (see, for example, Steiner, 1995, p. 437).

The methodologic conflict between Freud’s positivist ideals and his clinical need for interpretive methods may have originated when he stopped using suggestion and relied completely on his patients’ free associations, for at that point he needed more than a strictly observational method.² To understand latent meanings and determinants in such voluminous and seemingly disconnected discourse, he needed a flexible and imaginative form of interpretive methodology. This chapter

examines Freud's ambivalence towards interpretive methods, and the ways he attempted to deal with the methodologic conflict. Some of his attempts were prescient and productive; others were not.

Productive Ways of Dealing With the Methodologic Conflict

First, a word about Freud's positivism: He was a positivist in the original meaning of the term—as defined by Auguste Comte (1790–1857), the French sociologist who founded the philosophical school of “Positivism” during the middle of the last century (Frank, 1957; Scharff, 1995). Comte's positivism advocated the application of scientific method not only in the physical sciences but also in human studies, employing the methods of mathematics, logic, observation, and controlled experimentation. Subsequent developments in positivism continued to emphasize the unity of the sciences and the importance of restricting science to the observable. The line of descent in the history of positivism led from Comte to members of the Vienna Circle, who developed “Logical Positivism” (or “Logical Empiricism”) during the 1920s (Scriven, 1969, p. 197). Thus “Logical Positivism” did not appear until the next-to-last decade of Freud's life, whereas the influence of Comte's positivism had pervaded his entire scientific training and career. Logical positivism insisted that no hidden entities or causes should be postulated (Lacey, 1986, p. 183), a principle that was antithetical to Freud's thinking and approach. Rather, he embraced Comte's version of positivism, namely, that to carry out observations our minds need some theory. Comte wrote that, if we did not attach our observations to certain principles, we would not be able to recognize them; the facts would remain unnoticed beneath our eyes (Frank, 1957, p. 15).

Some members of the Vienna Circle were sympathetic to Freud's work, however, because it “revealed connections between a great range of new and surprising observed facts” (Frank, 1957, pp. 308–309). Chertok (1968) illustrates Freud's innovative concepts with the following example: Realizing the dangers of erotic complications, investigators of the psychotherapeutic relationship prior to Freud had become very cautious about any affective

involvement with female patients. Breuer had become entangled in such a complication and had fled from the problem, giving up his research on hysteria:

Freud, placed in a similar predicament, confronted the situation. He found a method of defense, which consisted in the belief that the patient was establishing a "false connection," and that her emotional demands were not directed to him personally, but to some person belonging to the patient's more remote past. It is thus that his concern for his own protection led him to a most productive discovery—that of the transference [Chertok, 1968, p. 575].

Despite his lasting commitment to the original form of positivism, Freud's psychological investigations suggest that in some important respects he was able to surmount the methodologic strictures of positivism and to anticipate the development of postpositivist science. Izenberg (1976, p. 13) notes, for example, that it was Freud who broke with the conventional approach of nineteenth-century psychiatry. By postulating hidden intentions, emotions, and beliefs in neurotic behavior, he called into question the prevailing medical model of mental illness.

Additional examples of Freud's (1923a) flexibility and farsightedness in this regard include his leaving a great deal of discretion in clinical interpretation to the individual clinician's tact and skill, rather than applying strict rules to the practice of interpretation (p. 239); his (unacknowledged) breaking of certain technical rules to deal with the conflict between his patients' needs and his own wish to promote the scientific standing of psychoanalysis (Kris, 1994, p. 662); and his occasional, though wavering, acceptance of some limitations regarding the reliability of clinical observations and interpretations, as for example in his acknowledgment that one may sometimes make a wrong surmise, and is never in a position to discover the whole truth (Freud, 1910c, p. 226; see also 1925a, pp. 127–130).

Noting that science does not consist of strictly proved theses, Freud (1915–16, p. 51) also recognized the necessity to employ circumstantial evidence; most scientific propositions have only a variable degree of probability. To Freud, acceptance of such approximations and continuing one's investigative work in the absence of final confirmation indicates a scientific attitude.

During the 1920s, Freud (1923a) acknowledged still further uncertainties regarding his observational and interpretive methods:

Psycho-analysis is not, like philosophies, a system starting out from a few sharply defined basic concepts, seeking to grasp the whole universe with the help of these and, once it is completed, having no room for fresh discoveries or better understanding. On the contrary, it keeps close to the facts in its field of study, seeks to solve the immediate problems of observation, gropes its way forward by the help of experience, is always incomplete and always ready to correct or modify its theories [p. 253].

Freud still claimed, however, that “with impartiality and practice” it was usually possible to obtain trustworthy results, that is, results confirmed by their repetition in similar cases (p. 239). The latter statement is still tinged with hopes of certainty; but, by use of the qualifying term usually, Freud hinted, at least, that “trustworthy” and “confirmed” knowledge is not always possible with psychoanalytic interpretive methods. In the same paragraph Freud stated that his original interpretive method was still being used, “though with a sense of greater assurance and with a better understanding of its limitations” (p. 239). Mentioning improvement of his interpretive method and also understanding of its limitations in the same sentence illustrates the transitional stage that Freud appears to have reached during the 1920s between a positivist and a postpositivist perspective regarding psychoanalysis and its interpretive methodology.

The same mixture of positivist and postpositivist viewpoints is evident in the last writings published during Freud’s lifetime. “Analysis Terminable and Interminable” (Freud, 1937a) emphasizes the limitations of psychoanalysis, whereas “Constructions in Analysis” (Freud, 1937b) alternates between positivist assertions regarding the confirmation of constructions, which Freud insisted were completely trustworthy, and scientifically more tentative statements such as, “We do not pretend that an individual construction is anything more than a conjecture which awaits examination, confirmation, or rejection” (p. 265). Thus the transition between positivist and postpositivist viewpoints that characterized Freud’s thinking about psychoanalysis and its interpretive methodology during the 1920s appears to have remained largely unchanged during the final decade of his life.

In addition to the appropriate and useful ways that Freud dealt with the posited methodologic conflict, another set of his reactions appears to have been counterproductive. Clinical necessity forced him to employ interpretive methods, but in compliance with his positivist ideals:

1. He attempted to minimize the role and importance of interpretations in his methodology.

2. He never described his interpretive methods clearly or systematically.

3. He attempted to make his interpretations appear objectively empirical, certain, and reliable, as his positivist ideals demanded.

4. He attempted to shift the task of interpretation to the patient and focused on the fallibility of ancient interpretive methods rather than studying the limitations of his own approach.

5. He ignored the investigations of interpretation by a renowned contemporary hermeneutic scholar, Wilhelm Dilthey, and did not acknowledge his intellectual debt to his teacher, Franz Brentano.

6. He became hypersensitive and overreacted if his empiricism and objectivity were questioned.

7. He devalued methodologists and depreciated their standards of objectivity as excessive.

Minimizing the Role of Interpretations

Perhaps the simplest way for Freud to minimize the role and importance of interpretation in his methodology was to avoid the subject—to say as little as possible about it in his writings. To illustrate, the General Subject Index of the *Standard Edition* (Richards, 1974, pp. 309–310) includes only eleven references to interpretation, most of which deal with the issue of interpreting sensory stimuli. None of the references deals with interpretive methodology. One reference concerns the “ease” of interpreting patients’ secret wishes (Freud, 1913a, p. 140); and another refers to the “complete certainty” of interpretations (Freud, 1913b, p. 208).

Another example of Freud’s saying very little about interpretation occurs in one of his most important case histories, that of the

Wolf Man (Freud, 1918). In that entire case report Freud mentions only a single, minor interpretation that he communicated to the patient (pp. 89–90). Namnum (1978) asks in this connection: “In his comments on technique, including all the later papers, why did Freud not use the term *interpretation*, which now seems to us so apt, so familiar—indeed, so indispensable?” (pp. 318–319). Namnum notes further that even when Freud did use the term “interpretation” (as in Freud, 1937b, p. 261), “he did so only to criticize that general usage and to propose instead *constructions*, a term with no transitive form” (Namnum, 1978, pp. 318–319). Freud’s (1940a) posthumously published “Outline” also does not mention the term interpretation in its discussion of technique, but refers to the “information” we give the patient in the form of a “translation” of his or her associations (p. 174). In his essay on lay analysis, Freud (1926a) refers to interpretation through his imaginary interlocutor, who expostulates, “‘Interpret!’ A nasty word! I dislike the sound of it; it robs me of all certainty. If everything depends on my interpretation who can guarantee that I interpret right?” (p. 219).

Leavy (1980, pp. 23–24) observes that, except for *The Interpretation of Dreams* (Freud, 1900), Freud was consistently uninformative regarding his method of interpretation. Even his papers on technique avoid the question of how clinicians arrive at their interpretations (Coltrera, 1981, p. 86; Rangell, 1987, pp. 10–11). *The Interpretation of Dreams* itself has surprisingly little to say about how to identify, construe, formulate, and justify latent meanings and determinants. Its 750 pages deal almost entirely with Freud’s theory of dream formation, rather than with his methods of interpreting dreams. One comparatively brief chapter focuses on interpretive methods, which Freud illustrates with his “Specimen Dream” of Irma’s injection (pp. 96–121); another small section suggests a number of theory-driven interpretive heuristics based upon postulated mechanisms of representation in dream formation (pp. 310–338).

Eissler (1985) suggests that Freud probably fabricated the Specimen Dream in order to demonstrate his thesis that dreams can be interpreted—which may explain the asseverative tone and content of the chapter, in contrast to a more substantive and detailed description of his interpretive methods. Eissler’s suggestion also raises the question whether Chapter 2 in *The Interpretation of Dreams* can be considered a true picture of Freud’s interpretive approach.

Another example of Freud's taciturnity with respect to interpretive methods is the brief section in one of his encyclopedia articles entitled, "Psychoanalysis as an Interpretive Art" (Freud, 1923a, pp. 238–241). The latter section is a highly condensed overview of clinical interpretation and says very little about the actual methods, strategies, and heuristics that guide interpretive work.

Still another example of Freud's minimizing the role of interpretation in his methodology is his proposal of *lapsus linguae* as the model of psychoanalytic investigation. He suggested that one ask the speaker why he had made the slip and what could the speaker say about it? If the person who made the slip then offered an explanation by saying the first thing that occurred to him, "this small active step and its successful outcome are already a psychoanalysis and are a model for every psychoanalytic investigation" (Freud, 1916–17, pp. 47–48). Note, however, that this model of psychoanalysis does not include interpretation. Freud gives the impression that a person's first association to a slip is transparent, that it provides the unconscious meaning of a slip without interpretation, which overlooks the central role of interpretation in his methodology.³

Other writers have noted that Freud sometimes dealt with clinical problems by avoiding or minimizing them. Greenberg (1986, pp. 240–241) observes, for example, that Freud minimized the problem of therapeutic outcome by seldom mentioning it, focusing instead on theoretical issues. Grünbaum (1993, pp. 185–186) points out that Freud was disinclined to attribute therapeutic failure to incorrect interpretations; he considered therapeutic failure compatible with correctness of interpretations and blamed poor therapeutic results on patients' resistances (see, for example, Freud, 1920b, p. 164).

Wax (1995, p. 551) suspects, however, that at some level Freud must have realized the interpretive nature of his clinical work;⁴ but the paradox is that, despite his emphasis on constructing the patient's life story, he did not appear to recognize or attempt to deal with the methodologic problems involved in understanding the meanings of their various communications.

By minimizing the role and uncertainties of interpretive methods in clinical practice, Freud set a problematic precedent for psychoanalysis and dynamic psychotherapy which continues to the present day. To illustrate: (1) Although many clinicians would agree that the construal of latent meanings and determi-

nants is an important part of therapeutic work, some may not appreciate just how central and pervasive its importance is; for aside from what a patient actually says and does during therapy sessions, everything else is inferred (Meehl, 1973, p. 117). (2) Like Freud, relatively few clinicians acknowledge in published or presented reports the uncertainty of their interpretations. (3) Descriptions of how clinicians arrive at specific interpretations are rare. (4) Attempts to justify interpretations are virtually nonexistent. (5) Some recent empirical investigations suggest that the average level of interpretive accuracy by clinicians is low (Crits-Christoph, Cooper, and Luborsky, 1988).

Never Describing His Interpretive Methods Clearly or Systematically

Another way that Freud minimized the role and importance of interpretation in his methodology was by not presenting a systematic description of his interpretive approach (compare Kerr, 1994, p. 8, 62–63, 65). His avowed reasons for the omission, mentioned in the Dora case (Freud, 1905a, pp. 11–12), were that a description of interpretive methods would lead to “hopeless confusion,” and would distract from his theoretical discussions.

What did Freud mean by that statement? Did he mean that interpretive methods are so complex and poorly understood that they cannot be explicated? Or that interpreters would not be able to agree on methods of construing latent contents? Or did he use “hopeless confusion” and “distraction from theoretical discussions” as rationalizations, to keep from discussing a subject that was problematic for other reasons, for example, its incompatibility with his positivist ideals for psychoanalysis (see, for example, Hertz, 1983)? A possible answer to these questions is suggested later.

In lieu of a clear exposition of his interpretive methods by Freud, Grinstein (1983) has attempted to systematize Freud’s “rules” and suggestions regarding dream interpretation. Combing the Freudian corpus for such references, Grinstein found a variety of comments on interpretation scattered throughout Freud’s writings. Close reading of Grinstein’s text reveals, however, that there really are no hard and fast “rules” of interpretive methodology, rules implying invariant procedures; and there actually is no

organized, articulated system of interpretive methods in Freud's approach (see also Hartmann, 1951; Blomfield, 1982, p. 289).

The largely ad hoc nature of interpretation, which varies with each interpreter (Seitz, 1966) and with each change of clinical context (Caston, 1993a; Caston and Martin, 1993) appears to preclude systematization of interpretive methodology (see, for example, Mahoney and Singh, 1979, p. 439). Interpretation is guided by an extensive, loosely knit collection of heuristics rather than by rules (see, for example, Lloyd, 1989, p. 213). The uncertainty of such methods and the unpredictability of their results may well have been the basis of Freud's ambivalence towards interpretation. The "hopeless confusion" of which Freud wrote in justifying the omission of his interpretive methods may have alluded, therefore, to the relatively uncertain nature of interpretive methods and conclusions. To maintain his positivist image and convictions, Freud may have decided not to open that can of worms.

Attempt to Make His Interpretations Appear Empirically Objective and Certain

In keeping with his positivist ideals, Freud (1916–17) attempted to make his interpretations appear as empirically objective, scientific, and certain as possible. He emphasized the role of causation by using terms like "motive," "goal," "intention," and "purpose" in his explanations of symptoms (pp. 61, 107). Shope (1973, p. 276) and Grünbaum (1984, pp. 66–67; 1989, p. 479) also stress Freud's emphasis on "causal continuity" in his concept of meaning. At the same time, however, Freud (1916–17) also referred to the meaning of symptoms in a semantic or communicative sense (p. 36). Wallace (1985) notes in this connection that Freud, "was too good a clinician not to recognize that, however much he longed to theorize in terms of biological tensions, excitations, and drives, in the consulting room he dealt with mental experience and human communication" (p. 161). Wallace concludes that Freud attempted to resolve the tension between his positivism and clinical realities by employing a teleological concept of psychic causation while paying lip service to mechanistic concepts.

Izenberg (1976, pp. 17–19) contrasts Freud's definition of interpretation (for example, 1900, p. 96) to the concepts of *Verstehen*

proposed by Dilthey, Simmel, Weber, and Jaspers. *Verstehen* views meaning as immanent in the object to be interpreted, and attempts to draw out the meaning in terms of manifest contextual significance. Freud, on the other hand, replaced the material to be interpreted with another element, which itself was meaningful but causally related to the original material.

Schimek (1975, pp. 845–846) suggests that Freud attempted to reduce his interpretations of meanings mediated by symbolic and linguistic processes to causal connections between “factual” events and the “natural” forces associated with them. By equating interpretations with causal explanations based upon the same types of causal inferences which are fundamental to the natural sciences, Freud presented his methodology as basically observational, supplemented only by necessary causal inferences, and thus an empirical foundation for psychoanalysis. Terwee (1987, p. 362) concludes similarly that Freud attempted to justify his interpretations by appealing to causal laws.

Another strategy for making his interpretations appear objectively empirical was to assert repeatedly his unwavering commitment to empirical observation. Freud wrote (1914b), for example, that speculative ideas are not the foundation on which science rests, that foundation being observation alone (p. 77). Freud's assertions of this kind appear to have convinced some psychoanalysts of his resolute adherence to empirical observation. For example, Gedo et al. (1964, p. 747) and also Meissner (1971, p. 305) present idealized descriptions of Freud's empiricism; but Freud did not always distinguish clearly between observations and inferences (Holt, 1992, pp. 378–379). In one of his writings Freud (1914b) referred to psychoanalysis as, “a science erected on empirical interpretation” (p. 77). Terwee (1987, p. 362) concludes that the level of observation and evidence in Freud's writings is actually the level of clinical interpretation.

Freud was at pains to assure readers that he based his conclusions on extensive clinical observations rather than on interpretations derived from his own theories.⁵ The following quotations from various periods of his career illustrate the tone and content of Freud's assertions regarding his empiricism. Only the final example mentions interpretive activity, which he refers to as “drawing undeniable conclusions”:

I can only assure the reader that I approached the study of the phenomena revealed by observation of the psychoneuroses

without being pledged to any particular psychological system [Freud, 1905a, pp. 112–113].

I cannot guarantee the completeness of my results, but I can answer for the care taken in arriving at them [Freud, 1908, p. 210].

I learnt to restrain speculative tendencies and to follow the forgotten advice of my master, Charcot: to look at the same things again and again until they themselves begin to speak [Freud, 1914a, p. 22].

Psychoanalysis is founded securely upon the observation of the facts of mental life [Freud, 1926b, p. 266].

[M]y working hypotheses invariably came about as a direct result of a great number of impressions based on experience [cited by E. Freud, 1960, p. 396].

[T]he associations give us far more than we need for formulating the latent dream-thoughts—namely, all the explanations, transitions, and connections which the patient's intellect is bound to produce in the course of his approach to the dream thoughts. On the other hand, an association often comes to a stop precisely before the genuine dream thought: it has only come near to it and has only had contact with it through allusions. At that point we intervene on our own; we fill in the hints, draw undeniable conclusions, and give explicit utterance to what the patient has only touched on in his associations [Freud, 1933, p. 12].

Statements of this kind by Freud can be questioned from several standpoints. Is it plausible, for example, that he was theoretically uncommitted to his own psychological system? And is his disavowal of speculation tenable? The construal phase of the interpretive process requires imaginative speculation in search of possible alternative hypotheses. Freud's use of terms like "founded," "invariably," "great number," "facts," "observation," and "direct result" express positivist ideals of empiricism, objectivity, and certainty; but they also suggest a rhetorical intention to persuade. In addition to its exaggerated empirical claims ("the associations give us far more than we need for formulating the latent dream-thoughts"), the final quotation also illustrates Freud's attempt to present clinical interpretation as an essentially

observational process which is transparent, objective, and certain—a relatively uncomplicated procedure of filling in hints, “drawing undeniable conclusions,” and thus making the patient’s allusions explicit.

Freud’s frequent and insistent protests regarding his empiricism, his disavowal of allegiance to any theoretical system (including his own), and the relative absence of references to interpretive inferences and conjectures in association with his observations, support the conclusion that he downplayed the role of interpretive methods in psychoanalysis and attempted to put as empirical a face as possible on his methodology.

Freud (1916–1917) was deeply concerned about the charge that interpretations produced their effects through suggestion. In addition to his extensive previous experience with suggestive methods and patient compliance, another important reason for his concern was that “there is a risk that influencing our patient may make the objective certainty of our findings doubtful” (p. 452). Thus, to satisfy positivist criteria of completely certain as well as objective knowledge, Freud attempted to make his interpretations appear as veridical as possible. For example, while discussing the role of inference in psychoanalytic work, he stated unequivocally that psychoanalytic technique “holds its ground against any criticism” (Freud, 1940a, p. 159).

Leaving aside the dogmatic quality and overcompensatory implications of the preceding assertion, Freud’s statement suggests that he did not fully appreciate the complexities, uncertainties, and unsolved problems of interpretive methods. For example, unlike strictly natural science approaches, interpretive methodologies do not derive from or depend upon scientific laws, nomic universals, or even a formal theoretical structure. Rather, the only solid referent to interpretive statements is their empirical bearing (McIntosh, 1979, pp. 408–409). The grounding of interpretations is never entirely empirical, however, but relies also on a network of additional, interdependent interpretations which undergo continuous, progressive modification. The same holds true of inferences generally; that is, one induction corrects another induction—we make most inferences not in isolation, but within a network of inductions (Reichenbach, 1951, pp. 240–248). Thus to understand a particular meaning one must turn to wider contexts that precede and follow the data in question (Peterfreund, 1971, pp. 239–241); and since the adjacent contexts must themselves be interpreted, the grounding of individual

interpretations is not tied to any single set of observables (Gergen, 1988, p. 35).

The grounding of individual interpretations is thus neither scientific law, nomic universal, formal theoretical structure, nor even purely observed fact, but is largely a shifting, ever-unfolding context of interpreted events (see, for example, Gaudin, 1987, p. xxxiii). Hence the cogent observation by Tuckett (1994a) that, "Interpretations rest on interpretations, rest on interpretations, rest on interpretations, etc." (p. 869). As a result, every possible aspect of interpretation involves an element of uncertainty, an epistemologic gap which must be considered a defining feature of interpretation (Hirsch, 1967, p. 322). And because of these methodologic limitations, depth psychologists must be content to allow latent meanings and determinants to accrue gradually, rather than feeling pressured to make immediate interpretive use of current clinical data (compare Ogden, 1997, p. 570).

Throughout his writings Freud said almost nothing about the methodologic problems and limitations of interpreting clinical data. He minimized the difficulties and fallibility of interpretation, insisting that free association provides a "plentiful store of ideas" that put the therapist on the right track of unconscious processes; and that clinical data contain such "plain and numerous hints" that the therapist is able to guess what is repressed (Freud, 1924, pp. 195–196). He believed and stated that "it is not difficult for a skilled analyst to read the patient's secret wishes plainly between the lines of his complaints and the story of his illness" (Freud, 1913a, p. 140).⁶ Thus, to make his interpretive methods appear reliable and his constructions certain, Freud sometimes lapsed into scholasticism. As Shope (1987) observes, "even psychoanalysts sometimes express distress over the extreme confidence that Freud frequently expressed in his interpretations" (p. 209; see also Rubinstein, 1997, p. 563).

Shifting the Task of Interpretation to the Patient, and the Fallibility of Interpretations to Ancient Methods

Freud (1900, p. 98) noted that ancient (Greco-Roman) dream interpretation also employed the principle of association, but in an incorrect way, that is, deriving meanings from what the dream elements brought to the mind of the interpreter. Freud used this

historical information tendentiously. Rather than recognizing and acknowledging the problems and limitations of his own interpretive approach, he attributed interpretive fallibility to the ancient interpretive method: "An insuperable source of arbitrariness and uncertainty arises from the fact that the dream-element may recall various things to the interpreter's mind and may recall something different to different interpreters" (p. 98, fn. 1). In describing his own approach, Freud claimed that his procedure eliminated the unreliability of the ancient method. He wrote that his technique

differs in one essential respect from the ancient method: it imposes the task of interpretation upon the dreamer himself. It is not concerned with what occurs to the *interpreter* in connection with a particular element of the dream, but with what occurs to the *dreamer* [p. 98, fn. 1].⁷

Freud neglected to mention, however, that the patient's associations, by themselves, do not provide the latent contents of a dream; that the therapist responds to the patient's associations with his or her own associations (for example, thoughts, fantasies, memories, feelings, and images); and that the interpretation which results derives from both the patient's and therapist's associations, and from a highly complex process of data processing by the clinician. Freud's attempt to shift the task of interpretation to the patient is misleading, therefore, and does not exempt the clinician from active participation in the construal of latent mental contents.

Ignoring Interpretive Methods and Concepts In Other Disciplines

Interpretive methods became indispensable to Freud's clinical approach, but he lacked expertise in the theory and methodology of interpretation. A few years earlier, when he needed to learn more about the method of hypnosis, he had travelled to Paris to study with Charcot; but when he needed to learn more about interpretive methodology, he did not seek or use some highly relevant knowledge of interpretation that was available to him. He remained self-taught in the theory, methodology, and practice of clinical interpretation.

Freud could have learned a great deal from the work of Wilhelm Dilthey, a German cultural historian and leading contemporary hermeneuticist, who lived from 1833 to 1911. Hermeneutics had expanded rapidly in a number of fields during the latter half of the 19th century, and Dilthey (1958, 1961, 1976) was its principal spokesperson.⁸ Dilthey saw in hermeneutics the possibility of a methodologic foundation for the human and social sciences—disciplines which, like psychoanalysis but unlike the natural sciences, attempt to interpret the manifold expressions of the inner life of human beings.

Dilthey and Freud faced similar intellectual tasks, namely, synthesizing disparate epistemologic and methodologic perspectives. Izenberg (1976, p. 13) points out that, independently and almost simultaneously, Dilthey and Freud recognized the need for a genetic and historical approach to psychology. Dilthey's view that study of the individual required a hermeneutic method found a parallel in Freud's use of clinical interpretation (see also Sulloway, 1983, p. xvi).

Dilthey's aim was to develop objectively valid methods of interpretation for the human and social sciences, which conceivably might have interested Freud during the years prior to 1900 when he was engaged in writing his *magnum opus* on the interpretation of dreams; for Freud labored under both inner and outer pressures to make his own interpretations as objectively scientific as possible (Havens, 1973, p. 287).

Another parallel between the two men was Freud's use of a part-whole concept similar to that of hermeneutic methodology, for example, his insistence that the meaning of any fragment depends upon the meaning of the whole (Freud, 1911b, p. 93; 1916–17, pp. 108–109). Both Freudian and hermeneutic approaches to interpretation require the ability to perceive the multiplicities of “part” meanings in the material while simultaneously recognizing the central theme (or “whole” meaning) that runs like an undercurrent through all of the data.

Parallels such as the foregoing led Habermas (1971, p. 214) to conclude that Freud based his interpretation of dreams on the hermeneutic model of philological research;⁹ but in a cogent study of Freud and interpretation, Kermode (1985, p. 7) concludes that Freud ignored Dilthey. I concur with Kermode's conclusion: that is, ignoring Dilthey's important work on the methodology of interpretation may have represented yet another way that Freud avoided the subject of interpretation, and by

doing so minimized its role and importance in his methodology.

Freud also ignored a seminal contribution by his teacher, Franz Brentano (1874), whose work pointed to a nonpositivist view of psychology. A number of writers¹⁰ have called attention to suggestive parallels between Freud's theory of mind and the theory of intentionality developed by Brentano (see Chapter 6 for a fuller discussion of this issue).

Becoming Hypersensitive to Criticism of His Methodology

A dramatic incident in Freud's relationship with Fliess suggests that Freud may have become hypersensitive and prone to overreact if his empiricism and objectivity were questioned. During their final "Congress" at Achensee, Fliess raised the question whether Freud might be reading his own ideas into his patients' associations. Freud found the question so objectionable that it contributed to his withdrawal from Fliess (Meehl, 1983; see also Mautner, 1994). From the standpoint of the present thesis, Fliess' question may have disturbed Freud's positivist scientific conscience. Freud must have realized at some level that his clinical "observations" were not as pure, uncontaminated by presuppositions, or as certain as his positivism demanded.¹¹ Interpreting the incident in clinical terms, Freud may have dealt with the inner disturbance produced by Fliess's question by projecting his own overly strict positivist conscience to Fliess, and then withdrawing from Fliess as a demanding and hypercritical superego-figure. Rubinstein (1997, p. 558) cites Sulloway (1979, p. 99) and Jones (1953, p. 255) regarding a similar "completely unreasonable reaction" by Freud to criticism from Breuer.

Devaluing Methodologists

Ekstein (1959, p. 226; see also 1978) reports a possibly related incident in which Freud harshly denounced methodologists as people who spend all of their time cleaning their eyeglasses without ever looking through them. In that instance, Freud may have projected his own demanding positivist conscience to methodologists,

whom he then devalued critically for excessive and unproductive standards of objectivity. Again, it may have been Freud's own positivist-empirical ideals that were excessive and in some respects unproductive: for example, they appear to have interfered with full acceptance, investigation, and more complete development of his interpretive methods, compelling him to seek, and at times to overstate, a degree of methodologic reliability and factual certainty which are unattainable with interpretive methods.

Summary

This chapter argues that Freud set a problematic precedent for psychoanalysis and dynamic psychotherapy by neglecting problems of interpretive methodology. Freud appears to have struggled with a methodologic conflict between his positivist ideals, on the one hand, and his clinical need for flexible, nonpositivist interpretive methods, on the other. Freud's positivist preferences for pure observation, objectivity, and certainty contributed to his ambivalence towards interpretive methods.

In some respects Freud overcame the excessive strictures of positivism and anticipated the development of postpositivist science, for example, by developing imaginative and productive methods of construing latent meanings and determinants. Some of the ways that he attempted to cope with the methodologic conflict were counterproductive, however. For example, in compliance with his positivist ideals Freud's writings attempted to minimize the role and importance of interpretations in his methodology; he never described his interpretive methods clearly or systematically; he attempted to make his interpretations appear objectively empirical, certain, and reliable; he attempted to shift the task of interpretation to the patient, and emphasized the fallibility of ancient interpretive methods rather than studying the problems and limitations of his own approach; he ignored what might have been useful methods and concepts of interpretation in other disciplines; he became hypersensitive and overreacted if his empiricism and objectivity were questioned; and he devalued methodologists, depreciating their standards of objectivity as excessive.

Freud never fully relinquished his positivist ideals, but erected the above-mentioned compromises in an effort to accomodate both

his clinical need for interpretation and his positivist preference for “objective truth.” He attempted to make psychoanalysis both a clinical art and a positivist science. His positivist aspirations live on in the scientific lore that underlies psychoanalysis, and contribute to the continuing neglect of problems in our interpretive methodology.

2

The Postpositivist Turn and the “Lingering Ghost of Positivism” in Interpretive Methodology

The positivist model defined knowledge narrowly as empirical, objective, and certain. Positivists evaluated human studies by the same standards, that is, data and methods that produced certain knowledge, or “objective truth” (von Wright, 1971, p. 121). Nagel (1959, pp. 49–50) insisted, for example, that unless data were obtained under carefully standardized conditions, even an extensive collection of data (as in psychoanalysis or dynamic psychotherapy) is an unreliable basis for inference.

By the middle of this century, however, positivism had come under increasing attack from several directions; and by the 1960s, most scientists and philosophers recognized that the logical-empirical paradigm could not provide a secure foundation for absolutely certain knowledge (see, for example, Bonjour, 1978; Bauer, 1992). With the downfall of positivism, a radical shift or “postpositivist turn” has occurred in both the philosophy and the conduct of science (Polkinghorne, 1983, pp. 1–57). Human and social scientists have reexamined their methodologies, leading to broader understanding of science and novel approaches to inquiry (Stent, 1975; Nasser, 1994). The previously vaunted unity of science is no longer widely accepted; science has become increasingly diverse and pluralistic, each domain developing its own terminology, methods, and concepts (Colby and Stoller, 1988, p. 12).

Unlike positivism, postpositivist science questions the notion of certain truth, challenges foundationalism, and holds that

human beings, including scientists, have no direct access to truth (Rescher, 1982, pp. 218–219; Beahrs, 1986). Direct, nonpresumptive knowledge is not possible; indirect, inferential, presumptive knowing is the best that we can do (Overman, 1988, p. 335). Lewis Thomas (1978) comments in this connection that the most significant contribution of twentieth century science to human intelligence is recognition of the depth and scope of our ignorance. We are finally getting glimpses of how vast the questions are, and how far they are from being answered. “Because of this, these are hard times for the human mind” (p. 1462; see also Ziman, 1978, p. 148).

Postpositivist science, however, is not a unified school of thought having distinct scientific propositions of its own. It does not seek to replace the positivist paradigm with a single, specific, postpositivist approach. It is primarily an attitude or perspective regarding science and inquiry, namely, that there is no one correct method to follow (Bauer, 1992). Science is not a system of following methodologic rules that lead to acceptable results. Science is a search for better understanding by employing whatever methods are relevant to the problems being studied. One of the investigator's tasks, therefore, is to find or devise methods and strategies that can answer the questions being asked (Polkinghorne, 1983, pp. 3–4; Hull, 1988, p. 516).

Percival Bridgman expressed this attitude bluntly: The scientific method is simply doing one's damndest with one's mind, no holds barred.¹ Medawar (1969) quips that when scientists are asked about the scientific method, they become “solemn and shifty-eyed” (cited by Colby and Stoller, 1988, p. 19). Feyerabend (1975, p. 28) contends that there is only one methodologic rule in science that applies under all circumstances: “anything goes” (but some limitations of that view are discussed later).

The postpositivist perspective has resulted in a growing trend toward less formalized, more pragmatic, eclectic, qualitative, and problem-oriented approaches to scientific inquiry. In this “new paradigm” which emphasizes qualitative methods, data are collected and analyzed in a more flexible, discovery-oriented, open-ended manner.² The qualitative approach does not exclude traditional hypothesis testing, however, but goes beyond it in a number of ways (Stiles, 1994, pp. 158–164; 1993):

1. Results are expressed mainly in words rather than numbers, for example, in dialogues and narratives which may not be coded, rated, or reduced to numbers of any kind.

2. Empathy is used as an interpretive method, emphasizing the meanings, purposes, and significance of what people say and do.

3. Results are interpreted and reported contextually; and since each context is unique, no two events are exactly alike.

4. Recognition is given to the polydimensionality of experience, and to the variations associated with each dimension.

5. Recognition is given also to nonlinear causality, including systems which involve feedback and may behave unpredictably. Such systems can be completely deterministic but impossible to predict more than a few steps ahead because of sensitivity to initial conditions.

The foregoing characteristics make the epistemology of qualitative approaches more pluralistic and tentative than that of traditional hypothesis-testing methodologies. Rather than a single, overarching epistemology, postpositivist science accepts multiple systems of inquiry, each of which contributes progressive approximations to more "truthlike" knowledge. Human and social scientists in particular feel freer to experiment with diverse epistemologic frameworks that are foundationless, relativistic, and pluralistic (MacKay, 1989, p. 2; Wimsatt, 1994, p. 494). Everything is revisable, including common sense (Churchland, cited in Callebaut, 1993, p. 16).

Contrary to positivism, methods in postpositivist science are determined and shaped by the system of inquiry in which they are employed. A method is not an isolated activity operating independently of a system of inquiry. Investigators, including interpreters, often require novel methods designed specifically for a particular problem being studied. An example is the work of Osgood, Suci, and Tannenbaum (1957) on the measurement of meaning. The method they developed, the "Semantic Differential," has no standard concepts or scales, the latter being adapted to whatever specific problem is studied. In other words, the nature of the problem defines the method, rather than the other way around. Single methodological perspectives with clearly defined procedures are understandably tempting to investigators, but they are illusory because they ignore the anomalous aspects of human experience (Polkinghorne, 1983, p. 9).

The relativism (or perspectivism) of postpositivist science recognizes that all of our knowledge is conditional, that is, formulated within particular conceptual systems; but the context-bound

character of knowledge does not necessitate a complete relativism. Between the extremes of absolute certainty and absolute uncertainty, alternative knowledge claims within the same conceptual system can be assessed by comparison with each other in relation to available data; thus some interpretive hypotheses (within the same conceptual system) can be accepted and others rejected. Diverse conceptual frameworks within a particular discipline, which generate different interpretations or explanations of the same phenomena, also do not pose a crisis epistemologically or methodologically; for choice of a specific reference frame does not imply that the approach and its results are valid, but only that it is capable of proposing plausible hypotheses to interpret or explain the phenomena (Miller, 1987, p. 501).

On the other hand, the fact that all knowledge is formulated within particular conceptual systems does produce serious problems of communication between colleagues identified with different schools of thought. We tend to translate other theories into our own conceptual framework in an attempt to understand them, but it is important to understand a hypothesis in the context of its own conceptual framework. If we interpret it according to our own way of thinking, we change its meaning, distort it, and interpret only a caricature (Diesing, 1985a, pp. 82–84). Spence (1982a, p. 256) emphasizes the need to adopt a colleague's "metaphor" in order to understand his or her interpretive reasoning and conclusions. And Schwaber (1983a, b, 1986) employs a similar transformation of mental set in relation to patients, that is, adopting the patient's perspective in order to understand his or her psychic reality.

The search for knowledge in the human and social sciences (including psychoanalysis and dynamic psychotherapy) continues to employ two very dissimilar methodologic approaches: one which is research-oriented, positivistic, quantitative, objective, and cumulative; and another which is clinically-oriented and predominantly humanistic, subjective, qualitative, interpretive, pluralistic, and noncumulative (Overman, 1988, pp. vii–xix). Modell (1978a,b) suggests that the two perspectives are related by a principle such as Bohr's complementarity. Some writers (for example, Talley et al., 1994) stress the importance of uniting empirical research and clinical practice, but so far the two camps remain separate and have relatively little influence on each other.

For more detailed descriptions and discussions of positivism, see Achinstein and Barker's (1969) *The Legacy of Logical Positivism*—

especially Michael Scriven's chapter, "Logical Positivism and the Behavioral Sciences" (pp. 195–210) and Achinstein's chapter, "Approaches to the Philosophy of Science" (pp. 259–291). Polkinghorne's (1983) *Methodology for the Human Sciences* is an accessible and informative treatise on the postpositivist perspective in contemporary science. Nasser (1994) discusses the significance of the postpositivist turn for psychoanalysis and dynamic psychotherapy. Leary (1994) and also Seidman (1994) have written useful reviews of the related, but not identical, "postmodern" perspective. Barratt (1993) examines the relation of postmodern views to psychoanalysis.

Table 2-1 presents a schematic comparison of positivist and postpositivist perspectives:

Table 2-1

Comparison of Positivist and Postpositivist Perspectives

Positivism	Postpositivism
Method-oriented	Problem-oriented
Foundationalist	Antifoundationalist
Monistic	Pluralistic
Absolutist	Relativist
Rule-governed	Flexible
Doctrinal, Theory-driven	Data-driven, Context-sensitive, Heuristic, Pattern-seeking
Apodictic knowledge (Neopositivism: Probabilistic)	Assertorial knowledge, Probabilistic
Verification: Correspondence theory, Prediction	Justification: Coherence, correspondence, pragmatic; Multiple methods

Interpretive Methodology and the “Lingering Ghost of Positivism”

Clinical interpretive methods originated during the positivist era, but much of their development has occurred during the postpositivist period. Kermode (1985, pp. 3–4) notes that positivist assumptions began to be questioned during Freud’s lifetime. New concepts arose about what constitutes valid interpretation, and natural science approaches began to seem less appropriate for the human sciences. But Freud took little notice of these changes.³

Many clinicians since Freud also appear to have taken little notice of these changes. The fact that, like Freud, most psychoanalysts and dynamic psychotherapists in North America are physicians may tend to perpetuate a positivist perspective; for physicians are trained primarily in biological science. As the evolutionary geneticist R. C. Lewontin (1994, p. 479) observes, most biologists are really positivists in their attitudes towards research, evidence, proof, and in the form of their scientific reports. The methodologic preferences and even the case reports of many medically-trained psychoanalysts retain the positivist stamp of their basic training in biology. Leavy (1980, pp. 4–11) also emphasizes the role of the medical setting and of the clinical-historical method in the origins and traditions of clinical interpretive inquiry.

What Spence (1984a) calls the “lingering ghost of positivism” (see also Agassi, 1981) still pervades our thinking, and some of the same positivist-based blind spots that Freud was unable to surmount limit the vision of some contemporary clinicians (compare Reichenbach, 1951, p. 143; Margolis, 1993). Examples of persisting positivist attitudes include the widespread tendency to minimize the difficulties and fallibility of interpretive inquiry, which is a major obstacle to progress in depth-psychological science; the persistent tendency of some clinicians to overvalue initial constructions, which are only conjectures for the most part and thus the most uncertain step in the interpretive process; unsubstantiated claims that continue to be made about uniquely revealing varieties of clinical data and methods of construing unconscious meanings and determinants; and the continued use by clinicians of doctrinal interpretation based upon specific clinical theories (see, for example, Wyss, 1966, pp. 526–527; Eisold, 1994).

Positivist-sounding statements such as the following continue to appear in the clinical literature:

Psychoanalysis rests on a large and expanding base of verified observations and these are connected in an orderly fashion with certain theoretical constructs. The observations are collected by means of unbiased inquiry; the theoretical generalizations become increasingly certain and the predictions deriving from them increasingly probable, concomitant with the volume of verified observations [Blight, 1981, p. 152].

Herbert Schlesinger (1994) writes that, "despite the manifold possibilities for understanding and intervention, generally only one idea occurs to the analyst as fitting the situation of the moment" (p. 32). Schlesinger also refers to "processes that lead us at a certain juncture to propose to our patient the one (usually right) thing that has come to mind" (p. 36).

Brenner (1968) reports a striking example of anachronous positivist thinking by clinicians regarding the reliability of interpretations. A group of colleagues at an international congress insisted that "if a psychoanalyst has been well analyzed, he will never be mistaken in his interpretations. His every inference concerning the unconscious meaning of his patient's associations will necessarily be correct" (p. 691).⁴ With equally misplaced confidence, Goldberg (1988) claims that "reckless interpretations are no more tolerated in psychoanalysis than in physics" (p. 14). Rubinstein (1983/1997), by contrast, emphasized and illustrated "the arbitrariness, because of lack of adequate confirmation, of a great many clinical interpretations" (p. 572). Reichenbach (1951, p. 143) observed wryly that intellectual recognition of our limitations in achieving truth does not give us the strength to resist the deep-seated emotional appeal of seeking certainty.

The "ghost of positivism" sometimes appears during debates, as in a panel discussion of the American Psychoanalytic Association on "Interpretation: Toward a Contemporary Understanding of the Term" (Panel, 1983, p. 244). Brenner emphasized the traditional perspective of psychoanalysis, that is, interpretations are directed at the specific etiology of the patient's psychopathology. Schafer's model of clinical interpretation, by contrast, is that of a narrative transaction, which rests upon a pluralistic (postpositivist) perspective and stresses the inevitably inexact and incomplete nature of interpretations. None of the panel members could accept the pluralistic and relativistic aspects of Schafer's interpretive model.

Brenner attacked Schafer's pluralism as a mixture of empiricism and solipsism which he insisted has nothing to do with psychoanalysis. Other panelists also rejected the epistemologic

assumptions of pluralism, and one member stressed the importance of “accurate” reconstructions. Schafer responded that reconstructions by different clinicians result in different accounts shaped by interpreters’ preferred paradigms. What Schafer objects to most in the traditional psychoanalytic view of clinical interpretation is the narrow, positivist-like explanation of what is involved in interpretive work. Brenner’s description, Schafer argued, is not sufficiently complex and represents only a rudimentary, oversimplified conception of what interpretation is.⁵

The previously described panel discussion illustrates one of the reasons that some psychoanalysts and dynamic psychotherapists are reluctant to give up a positivist perspective, namely, their concern that the ostensibly “firm ground” of genetic and biological “facts” might be lost (compare foundationalism). The panel members reacted to Schafer’s emphasis on narrative constructions as a threat to these vaunted, traditional “foundations.” It is important to realize, however, that there are very few, if any, really “hard” facts in depth-psychological therapies (“Interpretations rest on interpretations, rest on interpretations, etc.”). At the same time, the solution to this methodologic problem is not to go completely narrative and forget all about facts. There are various ways to get at (or near) depth-psychological “facts”; and although our methods are inexact, they are the best we have and are not totally indefensible.

Psychoanalysis and dynamic psychotherapy have been slow to accept the epistemologic and methodologic shift from a positivist to a postpositivist paradigm (compare Margolis, 1993). This epistemologic lag is paradoxical when one recalls that Freud’s work was a significant factor in the development of structuralism (Edelson, 1972, p. 250); in the growth of the social sciences (Gardner, 1981, p. 7); in paving the way for pragmatism (Rorty, 1986); and in contributing to the concept of relativism (Holt, 1989, pp. 356–357; Bernstein, 1988a, pp. 88–89). In these ways, the postpositivist legacy of Freud’s work is more evident in other disciplines than in psychoanalysis itself.

Combining Positivist and Postpositivist Perspectives

Instructive as it is to contrast positivist and postpositivist models of science, the distinction between the two paradigms is not

absolute. A postpositivist approach does not require the elimination of all positivist elements. The pluralism of postpositivist science encompasses multiple epistemologic and methodologic approaches, including some positivist-empirical methods and concepts. Some contemporary philosophers of science and other scholars point out that despite the demise of positivism empiricism retains a place in science as a theory of evidence, though not of truth.⁶ Thus modern science emphasizes the search for empirical knowledge not because it rests on a foundation, but because it is a self-correcting process (Williams, 1977, p. 180).⁷

Correspondingly, contemporary neopositivist approaches adopt a probabilistic rather than an apodictic viewpoint. There is no need, therefore, to discard positivism altogether. Meehl (1991, pp. xxv–xxvii; see also Caws, 1994) writes that he still finds the positivist tradition helpful in certain ways; and the philosopher of science Dudley Shapere (1984c) points out that

the contributions of the positivistic tradition deserve greater appreciation than we might give it if we considered only the deficiencies of so many of its contentions. For, by looking at science carefully in certain ways, it also raised a great many problems regarding the interpretation of science; its answers to those problems, while perhaps far from being all that one would have wished, still provided considerable illumination and insight [p. 376].

One of the advantages of a broader, more encompassing perspective is illustrated by the clinical controversy regarding objectivity. In an essay entitled "Farewell to the Objective Analyst," Arnold Goldberg (1994) maintains that therapists cannot stand outside of the therapeutic relationship and make neutral observations because: (1) they bring presuppositions to the clinical situation; (2) they participate and therefore change as a result of the experience; and (3) they are susceptible to countertransference reactions. Consequently, "our interpretations are never able to be limited to one fixed reading and so the exchange between patient and analyst is always open and continuous" (p. 21).

Goldberg's view is a restatement of arguments by Kuhn (1962), Feyerabend (1975), and other philosophers regarding the theory- and context-bound nature of observations in science. One of the problems with Goldberg's view, however, is its one-sidedness and categorical nature (note for example his use of the terms "never" and "always"). Some philosophers of science and

scholars in other fields take a different, more moderate position on the issue of objectivity;⁸ and Wallerstein (1988) reports similarly moderate views by clinicians and clinical investigators.

Drawing on the writings of Kordig (1971a, b, c, 1973), Meehl (1983/1991, p. 322) argues that the theory-ladenness of observational statements and associated meaning-variance apply mainly to "cosmological theories" that deal with everything there is. Psychology is different; hence we should not assume uncritically that philosophical arguments about the Copernican hypothesis or relativity theory or quantum mechanics apply equally to depth-psychological therapies and clinical interpretations. Such arguments may apply to astrophysics or to quark theory, Meehl concludes, but not to animal conditioning, or classical psychometrics, or even to psychoanalysis.

The hermeneutic scholar E. D. Hirsch, Jr. (1976) rejects the pure phenomenological form of perspectivism which considers all interpretations or perspectives equally valid, and which denies the possibility of mediating among them.⁹ Maquet (1964), a philosopher, concludes that perspectivistic knowledge is not inherently nonobjective; it is partial. It can become nonobjective if it is mistakenly considered global rather than partial.¹⁰

From considerations such as these it would appear that, as with everything we study, the objectivity of scientific (including clinical) observations and interpretations is not absent but is variable. At times, and under certain conditions, our observations and interpretations may be relatively objective; at other times and in other circumstances, they may be grossly unobjective. We need not throw up our hands, however, and say, "Farewell to objectivity." We can strive to extend and improve it; for clinical interpretive inquiry is a "subjective study of subjectivity struggling to become the objective study of subjectivity" (Stoller, 1979, p. 193). Some clinicians misuse the concept of relativism tendentiously for obscurantist and antiempirical purposes; but, as Meehl (1970/1973) observes, it would be unfortunate if efforts to objectify psychoanalytic inference were abandoned or watered down by a comfortable reliance on generalizations such as, "There is no such thing as a pure observational datum, utterly uninfluenced by one's frame of reference" (p. 116).¹¹

The philosopher Shapere (1984a, pp. xvii–xxi) suggests that both the insights and limitations of positivist and postpositivist science are complementary. By taking into account the presuppositions that influence observations and theories, but without

adopting a complete relativism, science has developed greater openness to new ideas and at the same time has preserved some degree of objectivity.

Drawing on case studies in the history of science, Shapere concludes that the debate between positivists and their critics represents the continuation of a more general controversy between "absolutist" and "relativist" views that have permeated philosophy since the time of Plato. "The real point of both skepticism and relativism lies in their exposure of shortcomings in our understanding of the nature of knowledge and of the knowledge-seeking process" (pp. xx-xxi). Since both absolutist and completely relativist arguments have been found to be inadequate, the task is to develop a theory of knowledge-seeking that avoids absolutism, on the one hand, but does not collapse into complete relativism, on the other.

The more science learns, the more it is able to learn. The richness of interpretation provided by background information makes it possible for science to develop new ideas. Science is thus a process of building on the best beliefs it has available. Viewed in this way, science can avoid both absolutism and complete relativism (p. xxv; see also Putnam, 1983c, esp. p. 198). The concept of science building on the best beliefs it has available is relevant also to the methodology of clinical interpretation; for inasmuch as the reliability (consistency) of interpretive methods is problematic, and interpretive conclusions are inherently fallible, depth-psychological understanding of patients is based necessarily upon the best constructions available to the clinician, that is, the "most plausible" interpretive hypotheses among various alternative constructions at a given time. This point is discussed further later, in connection with the justification of interpretations (Part IV).

The concepts and procedures involved in science do not come with guarantees, of course. Every aspect of scientific belief involves reasons for doubt; but the mere possibility of doubt is not by itself reason to doubt any particular proposition, or to avoid using best beliefs in seeking further knowledge. Even the doctrine that all knowledge of nature comes from observation of nature has exceptions. Thus the contemporary derivative of classical empiricism is "contingent empiricism." Similarly, we now recognize that things in nature have "relevance-relations" to each other, and that understanding and successful dealing with nature necessitates the use and systematization of such relations

(Shapere, 1984a, pp. xxvi–xxx). The importance of relations and patterns in clinical interpretation is discussed further in Chapter 5.

Eagle (1980b, pp. 371–373) notes that methodologic ambiguities such as the foregoing are reflected in the dual nature of psychoanalytic theory, for example, Freud's preoccupation with both meaning and mechanics, clinical theory and metapsychology. Rubinstein (1976) observed that our discourse about unconscious mental events "straddles two worlds—our everyday human world and the world of natural science" (p. 256; see also Modell, 1978a, b; Strenger, 1986, 1991).

The philosopher of science Gerald Radnitzky (1973, pp. 257–261) draws on concepts of Apel (1965) and on the model of knowledge development in the psychoanalytic situation to argue for an integration of human and natural science perspectives. The two approaches mediate each other so that knowledge develops by a continuous "tacking" between the two perspectives. For example, the human and social sciences, including psychoanalysis and dynamic psychotherapy, need information obtained by naturalistic methods, both as data and as methods of checking; and the natural sciences need information about human psychology obtained by interpretive and other methods of the human and social sciences, in order to deal with psychological and sociological factors that influence scientific investigators and their work (see, for example, studies by the sociologist of science, Karin Knorr-Cetina, 1981, 1984, 1993, p. 180). With respect to the integration of human and natural science perspectives, it is worth noting that one of the earliest and most extensive such integrations was the methodology developed by Freud.

Growing Status of Interpretation as a Method of Scientific Inquiry

The ascendancy of postpositivist science has given rise to some belated recognition of and respect for interpretive methodologies—an increased acceptance, even appreciation, of interpretive methods by scientists in many fields, including the philosophy of science. It is now widely accepted that all fields employ interpretation of some kind and to some extent (Holt, 1978a, pp. 50–51; Bernstein, 1988a, pp. 88–89). Ahumada (1994) writes that "from Baconian *interpretatio naturae* onwards interpretation has been

part and parcel of empirical science, be it 'exact' or 'observational'" (pp. 697–698). The philosopher Stephen Toulmin (1982, pp. 116–117) asserts that use of an interpretive approach does not diminish the quality of an investigation or relegate its results to the inferior status of subjective opinion, for such a choice remains open to all of the objective possibilities which may be used as rational support for interpretations.

Recognition of the importance and ubiquity of interpretation is attested also by growth of the constructivist viewpoint, that is, that stimuli, such as a patient's productions, acquire meaning through their assimilation into a person's, for example a therapist's, existing knowledge system. The hermeneuticist E. D. Hirsch, Jr. (1983) emphasizes that interpretation is the central activity of cognition; perceived meanings are always interpreted constructs. "We always perceive something other than the language through which we know something. This constructive process is interpretation" (p. 322).¹²

A constructivist viewpoint has begun to appear even in Kohutian self psychology. Contrary to Kohut's singular emphasis on the method of empathy (1984, p. 32), which some critics consider an inappropriately monistic and method-oriented way of attempting to understand the therapeutic dialogue (for example, Hoffman, 1991; Cooper, 1993), some of Kohut's followers (Wolf, 1986, 1989; Goldberg, 1989) now acknowledge the therapist's constructive activities in shaping meanings.¹³

Several recent treatises attempt to establish the central importance of interpretation also for philosophy. Marcia Cavell's (1993) *The Psychoanalytic Mind: From Freud to Philosophy* stresses the importance of interpretation to a theory of mind. *The Need for Interpretation: Contemporary Conceptions of the Philosopher's Task* (Mitchell and Rosen, 1983) attempts to reorient philosophy to interpretation as its central project. An increasing number of younger philosophers believe that philosophy is an interpretive enterprise. *The Interpretive Turn* (Hiley, Bohman, and Shusterman, 1991) argues that the earlier epistemologic and linguistic turns in philosophy have been superseded by an "interpretive turn," characterized by predominant interest in interpretive activities and disciplines. Two authors in the latter collection, Thomas Kent (1991) and Charles Taylor (1991), stress the Russian language philosopher M. M. Bakhtin's (1986) concept that human beings are "constituted in conversation," which places dialogue and its interpretation at the very center of human life.

Interpretation is recognized increasingly, therefore, as a universal feature of all human activities. Dreyfus (1991) refers to human beings in this respect as “interpretation all the way down” (cited by Bohman et al., 1991, p. 7; see also Campbell, 1988, pp. 335–388). Bohman, Hiley, and Shusterman (1991, p. 11) conclude that no experience, meaning, or evidence is independent of or more basic than interpretation.

Although interpretation is a form of practice rather than of theory, Bohman et al. point out that theory always depends upon a background of established interpretive practices which both initiate the theory and continue to influence it. Psychoanalysis illustrates that relationship: for example, Freud acknowledged that psychoanalysis began as an “art of interpretation” (1923a, p. 239). Bohman et al. (1991, p. 11) emphasize that the interpretive turn in philosophy is a practical turn, one that stresses the central role of practice. They offer four reasons why interpretation always involves practical applications: (1) There is always an intentional aspect of interpretation; it is always applied to some intentional object. (2) Interpretation employs a particular perspective which shapes what it interprets. (3) Interpretation is context bound. (4) As purposive agents, we have motives and needs of our own that influence what and how we choose to interpret.

Toward an Epistemology of Interpretive Inquiry

The central argument of this chapter is the relevance of a post-positivist perspective to interpretive inquiry—a pluralistic perspective that accommodates selected methods and concepts of disparate viewpoints, including some (mainly empirical) aspects of positivism. A pluralistic approach, however, does not require unification “into a homogeneous discourse, but rather the recognition of where, when, and how each *mode* of discourse is applicable and appropriate” (Lamm, 1993, p. 33). As Oakeshott (1962) puts it, different universes of discourse “enjoy an oblique relationship which neither requires nor forecasts their being assimilated to one another” (p. 199).

The question arises whether a particular philosophical school, or combination of viewpoints, exists within the postpositivist per-