Thus, this symptom is simultaneously a defense *against* and an expression *of* her fantasy that intercourse makes one sick. The symptom is based on both knowledge and ignorance; in order to decipher it, one must understand the scope and limits of the reality of Dora's interpersonal universe in relation to her fantasy life. The result is somewhat ironic. Whoever attempts to sketch the conditions of knowledge also maps the limits of his or her own self-ignorance. Whoever seeks the treasure of self-knowledge also discovers the sting of distortion and disguise.

Now let us turn from psychoanalytic discourse to philosophic discourse in order to give another brief example of the method of reconstruction.

Consider the following philosophic mini-theory: "Existence is perceived by the mind." On first reading this proposition may not seem false, but rather just plain absurd. So much the better for my purposes of illustrating how reconstruction discovers meaning amid apparent absurdity!

In order to discover the formula of distortion one must have enough respect for the integrity of our theory to ask: "What is the knowledge on which this (seemingly) absurd remark is based?" There is an implicit analogy concealed by the form of this remark. In explicit terms, one perceives colors with the eyes, smells with the nose, sounds with the ears, tastes with the tongue, etc. This much is given.

If we take the analogy one step further, then the formula of distortion emerges. The mind perceives particular patterns of sensations through the instrumentality of the sense organs; yet what is that instrument through which the mind perceives such universal attributes as existence and nonexistence, identity and difference, unity and multiplicity? The analogy leads us to the conclusion that the mind doesn't need any sense organ to perceive these attributes, and is its own organ in this respect. The formula of distortion is just this: "Existence is the same kind of attribute as those attributes accessible to us through the senses." This leads to the absurd attempt to perceive existence as one perceives the physical properties of objects. And in order to succeed we must posit "mind" as an organ analogous to our eyes, ears, nose, etc.

When we apply our formula of distortion to the original mini-theory, then the knowledge on which the theory is ultimately based becomes accessible. What the propostion "Existence is perceived by the mind" really attempts to teach us is just this: Knowledge isn't reducible to perception. Knowledge and perception are not the same. Perception is of individual attributes—particular objects of sensation—but knowledge also involves universal attributes—abstract properties like existence, identity, unity, etc.

Thus the reconstruction of an apparently meaningless formula discloses a surprising depth—the distinction between individual and universal. This is possible because the mini-theory summarized the conflicting requirements of an analogy between sense perception and abstract conception. The intersec-

tion of discourse about sensible particulars and conceptual universals led to a compromise that incorporated both knowledge and ignorance. (For a more detailed discussion of the mini-theory in its historical context see Plato, *Theaetetus*, pp. 184b-186e, from which the basic idea of the above paragraphs is drawn.)

The introduction of the two examples, one from a classical psychoanalytic text and the other from a classical philosophic text, suggests the following conclusion about the method of reconstruction. The method entailed is one of problem solving. At the beginning is a state of initial uncertainty. A recalcitrant symptom (Dora's vaginal catarrh) or an unintelligible maxim ("Existence is perceived by the mind") evokes a state of puzzlement. The question of meaning arises. Do these phenomena make sense? The search begins for a "formula of distortion." This is a principle for decoding the apparently incomprehensible elements of the problem. It is a criterion according to which nonsense can be transformed into a meaningful answer to the initial question.

A process of trial and error is the only way for deciding between alternative formulas. In the case of Dora's symptom alternative formulas might include the following. Instead of being an expression of a disguised wish to have intercourse with the father, Dora's symptom is intelligible as (1) hereditary veneral disease, (2) due to her masturbation, or (3) due to intercourse with an infected male. Freud actually does seem to suggest something like (1) and (2) at times. However, he later rejected (1) on the basis of advances in medical research. The second variation is dismissed in light of the fact that many girls masturbate without succumbing to a vaginal catarrh, much less hysteria. Thus (2) is clearly not a sufficient condition for the symptom in question. (This does not exclude the validity of some sort of principle of "somatic compliance" [1905, pp. 40-41], that a necessary condition of hysteria is the cooperation of the physical body with psychically determined meanings.) Finally, the third variant is eliminated in virtue of Dora's own testimony. In reporting her sexual experiences to Freud she recalled that Herr K. made sexual advances and tried to kiss her (1905, p. 28). She broke off the encounter due to a violent feeling of disgust. This goes against the interpretation of her symptom as due to intercourse with a man infected with V.D. We can indeed conceive of the possibility that she was deflowered and infected some time between age fourteen (when the first unsuccessful attempt at seduction occurred) and eighteen (when she came to Freud), and furthermore managed to keep it a secret from Freud. But none of the available textual testimony supports this reconstruction in terms of a real venereal infection from without.

Instead we are left with the task of deciphering Dora's symptom in terms of intrapsychic fantasies and interpersonal relations. The meaning of her symptom is made more intelligible as the expression of a disguised wish to copulate

with her father and a simultaneous punishment for this wish. The proposed formula of distortion in operation here is Dora's belief, a belief of which she need not have been consciously aware, that sexual intercourse makes one sick. This notion further contributes to solving the problem of why she felt disguist when Herr K. embraced and tried to kiss her. A transformation of desire into the anticipated sickness occurred in terms of her expectations that the consequences of intercourse would be infection.

Of course, we must admit that the variation and selection process of trial and error does not lead to absolute knowledge. The point is to show how the method of reconstruction is an instance of problem solving. We must admit the possibility that if a particularly compelling alternative reconstruction is offered in the future, then a given question will have to be reopened, and a decision reached as to whether the present solution is false or a limiting case of a more general principle.

A reflection of this kind can also be made about the solution of the problem presented by our philosophic maxim, "Existence is perceived by the mind." Here the formula of distortion summarizes a lot of thought that isn't evident in the account as it's presented above. At first I thought the formula of distortion was "Knowledge and perception are not the same." But I realized that this statement was rather the result yielded by the application of a formula that I hadn't yet discovered. In other words, I discovered the answer to the meaning of the problem *before* I obtained the principle of translation according to which the answer is justified. This just means that I knew the solution before I could say how I arrived at it. This is often, though by no means always, the case in problem solving by the reconstructive approach.

Now let's take a moment to summarize the results of the last two sections before proceeding to the final ones. Various texts were cited in which Freud employs an analogical comparison of the method of the practicing analyst with the method of the archaeologist. This employment can be found in his early (1896, 1905) as well as his late writings (1930, 1937). Furthermore, the vocabulary of reconstruction, which is an intimate part of the archaeological analogy, is employed by Anna Freud (1936) in her discussion of defense mechanisms.

Meanwhile, the method of reconstruction is given independent, though related, application in the work of Collingwood. The focus of the relation is once again the archaeological analogy. Collingwood generalizes the method of reconstruction from archaeology to philosophy. He engages the task of overcoming the distortion inherent in false theoretical constructs, which nevertheless disguisedly express a dimension of correct knowledge. Disentangling knowledge from ignorance requires reconstructing the formula of distortion.

The juxtaposition of Freud and Collingwood suggests that the methods of philosophic and psychoanalytic thought are much more alike than the partic-

ular problems that each discipline treats. The methods of both are oriented toward discovering meaning amid apparent absurdity.

The introduction of two specific examples lends substance to this claim. The examples represent problems. Although the subject matters presented are incommensurable, the methods of treating them are comparable. In both cases, the methods of reconstruction involve problem solving. So the methods of philosophy and psychoanalysis chart a course of discovery to the limits of intelligibility. They suggest longitudinal lines on a globe, parallel on the equator but convergent at the poles.

In the final sections, the question is raised how the introduction of the method of reconstruction affects the debate about the epistemological status of psychoanalysis as a science. Here the practicing psychoanalyst must remind the philosopher that analysis is not only a theory of knowledge (epistemology) but also (and indeed primarily) an approach to personality change and a way of alleviating suffering. At the same time, philosophic reflection on the method of reconstruction can help bridge the gap between metapsychological theory and clinical practice, and guarantee the methodolgical integrity of the discipline. These and related topics now become the points of discussion.

## THE PRACTICE OF INTERPRETATION

One of the main points of initiation of the debate about psychoanalysis as a science was the exchange between Heinz Hartmann and Ernest Nagel (Hartmann, 1959; Nagel, 1959). Taking off from Nagel's philosophic remodeling of the kinetic theory of gases, Hartmann compared psychoanalytic metapsychology to just such an axiomatic system of propositions. The basis of the comparison was the fact that neither molecular interactions nor processes in the unconscious are directly accessible to perception. Nevertheless we have indirect confirmation of the existence and efficacy of these phenomena in such effects as temperature, pressure, and change in volume, and dreams, symptoms, and slips (respectively). Nagel countered by attempting to point out inconsistencies in metapsychology. He claimed that the "correspondence rules" or "operational definitions" for relating theoretical to observational terms were loosely formulated, and that the economic point of view was just a metaphor (Nagel, 1959, p. 40).

This debate was reopened by Ricoeur's presentation of *Freud and Philosophy* (1970). There his strategy consisted in applying the terms of a general question about the epistemological status of the humanistic disciplines (or *Geisteswissenschaften*) to the specific field of psychoanalysis. He advanced the thesis that psychoanalysis is *not* an observational science (1970, p. 358). This may seem enigmatic at first. However, it does not imply that the analyst

is prohibited from using his eyes. Rather, it means that the analyst's use of his eyes (and ears and other senses) is different than the use any scientist makes of his perceptual organs in an experimental setting. Ricoeur's thesis should be taken to imply an opposition to modeling therapy on the research laboratory. The comparison of psychoanalysis with the physical sciences should not be taken too far. Ricoeur then attempts to turn his thesis, which is allegedly a criticism in the hands of the opponents of analysis, into a counterattack against the logical positivist interpretation of science.

Let's take a step back and put matters in perspective. The philosopher Wilhelm Dilthey (1833-1911) distiguished the natural sciences and the humanistic disciplines according to the principles of knowledge on which they were founded. The former employs mechanical explanations of the causal interconnections between phenomena, while the latter aims at understanding human purposes and intentions in an intersubjective context. Dilthey proposed to complement the mathematical foundation of the natural sciences with an historical foundation for the humanistic disciplines. The understanding of language, art, and cultural expressions of human subjectivity happens through the interpretation of these phenomena in their historical milieu. (For an account of Dilthey's work see Palmer, 1969, pp. 98-123.)

Even before Dilthey, Immanuel Kant (1724-1804) attempted to demonstrate that mechanical explanations must be complemented by an account of purposefulness in order to intelligibly encompass the rule-governed organization of living beings. Kant's thinking, however, was more cautious than Dilthey's. He insisted that purposefulness was a function of our finite human understanding and, in fact, a limitation to it. Indeed purposefulness was a form of the subject's reflection, not a determination of the object (Kant, 1790, pp. 233-234, par. 79).

Ricoeur draws explicitly on this tradition but is otherwise original in applying its terms to psychoanalysis. A positive thesis is implied in saying that analysis is not an observational science. The alternative is that analysis is a science of interpretation. It employs the method of reconstruction for solving questions of meaning. From this perspective, analysis is as much (if not more) like archaeology and history than like physics or biology. One might easily overlook this if one remained at the level of a theoretical system of deductively linked propositions. Instead one must look at the method that's practiced.

Relying on Dilthey's distinction between causal explanation and understanding purpose, Ricoeur states his case:

> Analytic experience bears a much greater resemblance to historical understanding than to natural explanation. Take for example the requirement put forward by epistemology of submitting a standardized set of clinical data to the check of a number of independent investigators. This re-

> quirement presupposes that a "case" is something other than a history, that it is a sequence of facts capable of being observed by many observers. [1970, p. 374]

Clinical practice is oriented toward problem solving in a genetic context. It is not a form of experimental research. In any case, analysis is clearly not an observational science in the sense that the molecular theory of gases is one. The latter has the restriction and the privilege of defining its object of inquiry in a setting where independent investigators can repeat, check, and publically control the reporting of data. In comparing analysis with archaeology, Freud gives us warrent for saying that the study of the individual life history is an historic discipline in a broad sense.

(For reasons of his own Hartmann rejects the archaeological comparison. One suggestion is that this is intended as a criticism of Wilhelm Reich, who saw analysis exclusively as a peeling of historic layers of the personality. The issue is complicated by Hartmann's explicit acknowledgment of the value of the "reconstructive approach," independent of the arachaeological analogy. See Hartmann, 1959, pp. 8-9.)

In addition, Freud notes that neurotics are usually unable to give an ordered history of their life in its relation to their illness (1905, p. 16). This happens due to actual fear and shame of revealing certain events as well as genuine amnesia, which serves the purposes of the unconscious.

We must remember that a case history is precisely that, a *history*. It's a unique sequence of events. This sequence is not capable of being repeated or of being repeatedly observed by many investigators. The events are accessible only through the interpretive reconstruction of memories, dreams, associations, and related reflections. Without reconstruction there may be a calender of events. The events hang together thanks to the method of the chronicler, descriptively labeled "scissors-and-paste" history by Collingwood (1946, p. 278).

In a sense, the spontaneous reports, the free associations of the analysand are like the historian's "sources" or, as they used to be called, "authorities." The critical historian questions, cross-examines, and revises his sources. If he is not only critical, but also scientific, then he realizes that the false statements in his sources are as important as the true ones. Men's blind spots reveal the dimensions and scope of their limited situations. For the historian, the reports provided by the men and women engaged in living their lives' time (the "authorities") are not yet evidence. A selection process must first occur through the posing of questions that the historical thinker wants to answer. Random information is constituted as evidence in juxtaposition with questions that are answered through it. Evidence is what answers questions.

Similarly, in psychoanalysis random free associations are not evidence. Neither is observable behavior or symptoms evidence. In this connection, it's

significant that the same overt behavior, the same symptom, can have many different meanings. Psychoanalytic questions seek to reconstruct meaning, not describe behavior. (The description of overt behavior is not a specifically analytic task, though it may be a necessary condition of one.) What then is evidence in the analytic field where fantasies are as important as—and indeed more important than—objectives states of affairs? A tentative closure to this question is won by taking a clue from the historian. A select part of the mass of analytic discourse and behavior is constituted as evidence through its answering questions about the patient's (implicit and explicit) intentions, purposes, projects, and desires. Once again, evidence is what answers questions.

This thesis leaves the field open for the possible introduction of uniquely psychoanalytic modes of transmitting information. Of particular relevance here is the concept of empathy. The analyst perceives the affects of the analysand through empathy, which, as I understand it, is a special use of the sense organs involving both distance *from* and participation *in* the emotional life of the other person. A detailed consideration of the literature on this topic can further the task of defining the specificity of psychoanalytic evidence. (See, e.g., Kohut, 1959, 1971; Modell, 1973.)

The questions and answers of the psychoanalytic setting unfold in a situation that is intermediate between the naive naturalness of everyday life and the manipulative artificiality of the laboratory. Clinical therapy is a practice governed by rules of its own. On the one hand, these rules are more rigorous than those of everyday life. One must try and tell everything, no matter how trivial or embarrassing. At the same time, clinical practice is more flexible than scientific experimentation. Human beings have to be respected as ends in themselves and not mere means to attaining metatheoretical knowledge. In short, the therapeutic situation is a dialogical one, in which human existence, not properties of an object, is under scrutiny. With simple devotion to the phenomena, the analyst turns his "evenly hovering attention" toward the analysand's "free associations" as they emerge (Freud, 1912, pp. 111-112). This is a way of initiating understanding. A life story, a case history, unfolds between these two poles of communication. Questions are posed and answers sought to solve problems that are intimately personal, yet no less universal for all that.

Here is the point at which psychoanalysis goes beyond history. Even if analysis can be classed with history for epistemological purposes, still the practice of analysis uses history as a means to an end of its own. In undertaking the compilation of a life history, in uncovering an archaeology of the subject, the therapist is engaged in an enterprise oriented toward alleviating human misery. The patient must not only remember the past, but he must also transcend it. In the words of Shakespeare, the patient and therapist must "pluck from the memory a rooted sorrow. . . . "

Now some may object that, without really taking Nagel's criticisms seriously, we've plunged into an alternative way of representing psychoanalysis in comparison with history, not physics. This objection is warranted, and we must retrace our steps to eliminate this shortcoming. The reason for postponing this consideration was not to repress Nagel's views, but rather to prepare the way for an intelligible reconstruction of them.

According to Nagel, theoretical knowledge consists of a system of propositions, deductively linked by valid logical inferences. Scientific method involves specifying observable events or processes as the meaning of otherwise unobservable theoretic terms through "correspondence rules" or "operational definitions" (Nagel, 1959, p. 40). Thus scientific method is a technique of relating theoretical terms to observational terms.

However, this way of representing the scientific method is questionable. The view of scientific method that emerges here is too narrow. It is, in fact, not a scientific method, but the method used by a particular philosophic view of science to analyze theoretical constructs. The trap into which we are led is one of conceiving of scientific rationality in the form of a system of deductively linked propositions. The result is preoccupation with the distinction between theoretical terms and corresponding observational ones. We forget that the correspondence is arrived at in terms of practical techniques and operations. That is to say, theoretical terms are applied to observations in the context of practice.

For the practicing scientist, theory and observation are more intimately related than the discussion of Nagel and the language of operational definitions would have us believe. Experimental evidence is theory laden. Random observations have no place in science.

The history of philosophy offers us a lesson here. Kant understood that theory and observation are separable only for a transcendental idealist, not an empirical realist. He gave a valuable clue for representing the relation between theory and observation in his maxim: "Thoughts without content are empty, intuitions without concepts are blind" (1787, p. 93). Following this formula we can say: Theory without content is empty, observations without theory are blind. Similarly, Wittgenstein went in search of an example of pure observation, unalloyed by any theoretic or conceptual determination. He didn't find pure seeing. Instead he found "seeing as . . . " (1945, p. 200). He always found the "echo of thought in sight" (1945, p. 212). Even the most primitive perception is already structured by thought *as* this or that.

Scientists do not make accidental observations, but constrain phenomena to answer questions of their own formulation. Once again, evidence is what answers the investigator's question. When Galileo, for example, let balls roll down an inclined plane, he had already mathematically anticipated the curve they would describe. The parabola is the shape yielded by the spatial inter-

pretation of the algebraic function for acceleration under the influence of gravity, $F(x) = \frac{1}{2}at^2$. He was cross-examining his witness, in this case nature herself, to test out his mathematically based conjecture.

It is instructive to consider that Galileo was giving a physical interpretation to a mathematical formula of the form, $F(x) = x^2$. Similarly, the molecular theory of gases consists in an interpretation of Newton's second law of motion (net force = rate of change of momentum) in the closed system of a volume, $V$, within a limited temperature range. The result is a function relating the pressure exerted by the molecules' mass and velocity to the temperature in terms of the kinetic energy of the system.

These examples should upset our complacency somewhat. They imply the breakdown of the neat dichotomy between the natural and humanistic sciences from an unexpected direction. The natural sciences are more humanistic than we thought. The interpretation of symbols is a source of meaning in both!

The point is that there are indeed important parallels between the methods of psychoanalysis and physics, even in the most theoretical aspects. But these parallels emerge at the level of the practice of interpretation, not in terms of the artifical distinction between theory and observation. (Two qualifications are needed. One, no one wants to claim that dream symbols have the atemporal invariability of the symbols of the language of mathematics. Dream symbols are inevitably culture bound. Two, the use of mathematical models in physics is not free historic change. Mathematics provides absolute certainty only as long as it is disconnected from the contingencies of empirical situations. Physics remains an empirical science subject to the vicissitudes of future variation.)

This digression into the philosophy of science was necessitated by the objective of making sense out of the comparison between the molecular theory of gases and psychoanalytic metapsychology. This comparison was initiated by Hartmann and developed by Nagel. Unfortunately, both men are bewitched by a preconceived notion of scientific method. Their formula of distortion (see pp. 515-516) represents scientific method as mapping unobservable terms (e.g., molecules or unconscious process) onto observable ones (e.g., pressure or dreams). The reconstruction of the sense of their dialogue consists in showing that interpretation has a positive role to play in the method of the physical sciences, where there is a practical inseparability of theory and observation.

In a way, this discussion has the consequence of deepening our perplexity about the relation between psychoanalytic metapsychology and the case histories of clinical therapy. Here the gap between theory and practice seems greater than in the natural sciences. Some psychoanalysts resist theorizing, are quite antipathetic toward metapsychology, and even ignore it, but they are still skilled at helping patients to overcome their problems and regain their personal integrity and well-being.

One paper that contains an engaging proposal on how to deal with this gap is George Klein's "Two Theories or One" (1973). In effect, Klein argues that the gap would be eliminated if one pole of the opposition was discarded. Klein suggests that the economic and dynamic points of view of the metapsychology are really a veiled extension of the physiologizing effort of Freud's 1895 "Project." Klein argues that, although Freud abandoned this particular form of the neurological program, he never gave up the idea. But this effort to find a neurological model is actually an obstacle to the development of the psychoanalytic enterprise. According to Klein, psychoanalysis is most intimately related, not to biology, but to the work of the dramatist and novelist (and historian), for it focuses on the intentional structure of human encounter, conflict, and understanding (cf. Klein, 1973, pp. 115, 126, and Ricoeur, 1970, p. 375). There are really two psychoanalytic theories presently. One is oriented toward "unlocking meaning" (1973, pp. 109, 113). The other aims at a general psychological theory in which behavior is explained causally, ultimately in terms of physiological mechanisms (1973, pp. 106-107). Klein proposes that this latter theory be abandoned and replaced with further efforts toward developing "experiential" and "functional" concepts (1973, p. 110) in the clinical setting.

Klein's presentation is loaded with valuable material. He finds great sympathy with the reconstructive themes of this paper in locating the essence of the psychoanalytic method in "unlocking meaning." Klein's representation of the "two theories" of analysis definitely intersects with Ricoeur's distinction between energetics and hermeneutics. There is a further intersection of Ricoeur and Klein in that both turn to the model of the historian in contrasting the clinician with the experimenter (through Klein actually suggests several alternatives).

In addition, Klein's interest in research designs using data germaine to the psychoanalytic setting recalls C. G. Jung's word associations tests (1907). Certainly the time has come to update the criteria of responsible knowledge that are available to the investigations of the research analyst (see Klein, 1973, p. 128).

Unfortunately, the force of Klein's proposal is undercut somewhat by the fact that at least two of his three statements about Freud's assumed philosophy of science are open to serious question.

Klein asserts that Freud's philosophy of science assumed that "concepts of purposefulness and meaning are unacceptable as terms of scientific explanation" (Klein, 1973, p. 104). However, in discussing slips, as we will see, Freud talks in terms of double intentions. In fact, Freud argues vehemently against those who want to reduce these slips to mere organic aberrations. Such a reduction serves the interests of resistance, though it is not always an example of resistance to cite organic compliance with psychic acts. Freud says that slips

are "psychic acts," arising "from mutual interference between two intentions" (1915, p. 60). Furthermore, he believes that this thesis *explains* slips in a way that no appeal to organic influences can. He says that his explanation of errors as psychic acts has

> won for psychology phenomena which were not reckoned earlier as belonging to it.
>
> Let us pause for a moment over the assertion that parapraxes are "psychic acts". Does this imply more than we have already said—that they have a sense? I think not. . . . The question will then be whether the particular mental phenomenon has arisen immediately from somatic, organic and material influences—in which case its investigation will not be a part of psychology—or whether it is derived in the first instance from other mental processes, somewhere behind which the series of organic influences begins. It is this latter situation that we have in view when we describe a phenomenon as a mental process, and for that reason it is more expedient to clothe our assertion in the form: "the phenomenon has sense". By "sense" we understand "meaning", "intention", "purpose" and "position in a continuous psychical context". [1915, pp. 60-61]

Here Freud doesn't even pay lip service—as he occasionally does elsewhere—to the idea that an organic explanation will eventually be found. He is clearly interested in establishing the autonomy of psychology vis a vis physiology. He sharply distinguishes the field of psychological investigation from the region of organic influences, which are shoved away "somewhere behind" the field of mental phenomena. The autonomy of psychoanalytic psychology is secured in orienting itself toward phenomena that have meaning or sense.

Klein's second assertion about Freud's philosophy of science asserts that Freud assumed that an acceptable explanation had to be purged of teleological implication (Klein, 1973, p. 104). The status of this assertion turns on what Klein means by "teleology." If teleology just means purposefulness, then this second statement is a positive form of the first one. And it's corrected along with the first one, too. However, if Klein is alluding to Freud's strictly antivitalist theory of life, then it may be allowed to stand. A complete discussion of the issues would entail an adequate decision about the theory of life implicit in *Beyond the Pleasure Principle*, and this cannot be engaged in at this late point.

Klein's third assertion maintains that Freud assumed that purposive regularities would eventually be described through the use of purely physiological models (1973, p. 104). But this is open to serious question even at the point of the origins of psychoanalysis. Very early on Freud recognized that hysterical symptoms are rooted, at least in part, in the hysteric's ideas. He says: "Hysteria behaves as though anatomy did not exist as though it had no knowledge

of it" (1893, p. 169). A physiological explanation is clearly impossible where there is no "anatomical lesion." There is certainly some involvement of the organism. But the leison is "dynamical" and "entirely independent of the anatomy of the nervous system" (1893, p. 169).

If this were not enough to show the limits of physiological investigation, there is the further fact that hysterical paralysis corresponds to the popular conceptions of anatomy: "It takes the organs in the ordinary, popular sense of the names they bear" (1893, p. 169). Freud never denies that "somatic compliance" is a necessary, though admittedly insufficient, condition of neurosis. But he also never forgets that what the body or soma complies *with* has the character of the popular psyche's conception of anatomy, not the scientist's specialized theory. The point is that physiology is inadequate to explain hysteria. The psychic contributes something to the intelligibility of the phenomena in question, and analysis claims its own proper field as the unmasking of this often hidden contribution.

Admittedly, Klein's abstract of Freud's alleged philosophy of science is only a limited part of his paper. It is the weakest part, but perhaps not the most important part. In the end, his conception of psychoanalysis is not that far from the present structure of the discipline. Once Klein has abandoned the dynamic and economic (i.e., the more metphorical) aspects of metapsychology, then he finds that he must stretch his notion of clinical theory to accommodate some clinically relevant concepts that were previously stationed there. Instead of two theories, Klein gives us one theory with two kinds of concepts.

According to Klein, clinical concepts are divided into experiential and functional concepts, intra- and extra-phenomenological ones (1973, p. 110). It's a generalization, but an accurate one, to say that this division represents a distinction between concepts that are experience-near and those that are experience-distant (respectively). Concepts such as projection, introjection, repression, and the ego's other defense mechanisms operate as autonomous processes independently of the subject's immediate experiential awareness. But these functions are a "part of his reality" (1973, p. 111). Presumably they are a part of the subject's reality, for they serve to transform and bind anxiety (an experiential phenomenon) in a way that furthers adaptation. These mechanisms are classified by Klein as "functional concepts" (1973, p. 110).

The problem is that Freud often engages in a use of language involving both dynamic forces in conflict and meaningful purposes, both mechanisms and intentionalities (Mischel, 1974 has documented this extensively). Freud talks about forces and intentions in the same breath. For example, he does this where the phenomena of psychology are described as ". . . signs of an interplay of forces in the mind, as a manifestation of purposeful intentions working concurrently or in mutual opposition" (1915, p. 67). No matter how often one tries to suppress the language of dynamic forces in conflict, it re-

emerges. Even Klein admits this. After successfully rooting out any mention of physiological mechanisms (at the level of psychoanalytic explanation), he still needs to reintroduce "the dialectic of directed forces to which a person is subject" (1973, p. 115). Psychoanalysis cannot do without its dynamic metaphors, whether one draws them from the context of physiology as Freud or from the context of drama and history like Klein.

Strictly speaking, Freud doesn't recognize, or at least doesn't adhere to, the classic distinction between mechanical, causal explanation and understanding purposeful intentionality (see p. 523). In mixing the language of forces and meanings, Freud also mixes the corresponding explanations and understandings. For Freud, it is an explanation to discover (through interpretation) that an apparently absurd slip, dream, or symptom is understandable, i.e., has a meaning. Interpretation is one way of showing that certain phenomena, apparently unintelligibly subject to chance, do not escape the deterministic network (1901, p. 239). Interpretation is a form of explanation which seeks understanding amid absurdity. (Klein is basically in agreement with this view, though he sometimes gives the impression that he's revising Freud's view rather than describing it [1973, p. 116].)

The introduction of the method of interpretation into the debate about the epistemological status of psychoanalysis as a science has three significant consequences. (1) Psychoanalytic knowledge shows itself to be more like that available to the historian than that accessible through theories in physics. The approach to knowledge in analysis and history is reconstructive. (2) Psychoanalysis and the physical sciences do have a comparable area of intersection. But this comparison is not available through the distinction between theory and observation. One must rather look to the level of the interpretation of symbols as a source of meaning in both. (3) In psychoanalysis the distinction between explanation and understanding breaks down. Giving an interpretation, in which what was nonsense becomes understandable, is a form of explanation.

## CONCLUSION

We still have to say something about the relation between metapsychological theory and the case histories of clinical practice. Can we find an alternative way of bridging the gap between theory and practice short of eliminating metapsychology itself? The key insight here consists in realizing that, although there may be many psychoanalytic theories, there is still only one psychoanalytic method.

On the one hand, we have metapsychological theory with its various perspectives, which can always discover adequate causes to determine every example of behavior. And even if some examples escape the deterministic net-

work, the legitimate task of metapsychology is the reduction of behavior to causal mechanisms. On the other hand, clinical practice deals with the purposes, conflicts, and goals of human behavior in its intersubjective and dialogical aspects. Here the task is to reveal the meanings of symptoms, to understand their relation to the patient's interpersonal environment, and to restore the patient's well-being. People are not causal mechanisms. However mechanistic the theoretical consideration of symptom formation may aspire to be, the practice of undoing symptoms still relies on discourse about human intentions and purposes.

The solution to the problem of how to bridge the gap between theory and practice consists in realizing that the link is a methodological one. The method of interpretive reconstruction traverses the distance between mechanical explanation and human understanding. It seeks understanding amid seeming nonsense as a form of explanation. The reenactment and mastery of the breakdown of this distinction is the solution to the problem.

The method of interpretive reconstruction is an invariable function that links the variable metatheories to clinical practice. Here the theories are rather like those mathematical formulas that acquire physical meaning in being interpreted in a spatiotemporal context (see p. 527). The application of a metapsychological model requires its reconstruction on the basis of the materials already available in the case history to which it is being applied. This reconstruction is a transitional function mediating the application of theoretical structures to practical processes of therapy. It's therefore a kind of reasoning involving both theory and practice.

Only one more point. In a sense, all theories are alike, whether in philosophy or psychoanalysis: they come too late to change the world, they can only reflect it. But the goal of therapy is precisely that, to change the world by changing the people who dwell in it. The practice of therapy aims at answering the desperate needs of personal disintegration and confusion with the antidote of self-knowledge. Macbeth's desperate question is transformed into an assignment; the task is to

> minister to a mind diseased,
> Pluck from the memory a rooted sorrow,
> Raze out the written trouble of the brain,
> and with some sweet oblivious antidote
> Cleanse the stuff'd bosom of the perilous stuff
> Which weighs upon the heart.

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