Narrative Truth and Theoretical Truth
Donald P. Spence

ABSTRACT

Although Freud was inclined to believe that every effective reconstruction contained a "kernel of truth," it is by no means clear how this kernel can be identified and separated from the set of equally likely fabrications which make up a good part of the patient's life story. If we have no sure way of identifying historical truth, we may be seriously handicapped in our attempt to frame theoretical laws. What may be effective in a particular clinical instance (narrative truth) may not automatically generalize to the larger domain of clinical theory.

The force of psychoanalytic argument has traditionally relied rather more on rhetorical persuasion than on appeal to the data. This tradition was heavily influenced by Freud who never felt it necessary to reveal all the evidence for a particular interpretation. Whatever the reasons for his reluctance, he later rationalized this tendency by stating that if the reader was not inclined to agree with his formulation, then additional data would scarcely change his mind (Freud, 1912p. 114).

Largely as a result of his initial decision, a custom has grown up which gives contributors to the literature the right to allude to the facts rather than state them in full. Because the evidential basis for any statement is only partly open to public inspection, the criteria for conviction are necessarily softer than they would be.

This paper is an outgrowth of the line of thought developed in a forthcoming book, Narrative Truth and Historical Truth (Spence, 1982). More extended presentation of such concepts as the nature of narrative truth, its place in the healing process, and how it compares with the historical truth of what "really happened" can be found in the book.
be in a field where all the evidence must be presented. Precisely because the evidential domain is partly closed, the skeptical reader of the psychoanalytic literature is in no position to challenge any conclusion he may come across. For the same reason, agreement among readers can never be complete or compelling; acceptance of an interpretation or theoretical conclusion always reduces to agreement on faith.

Whether or not this tradition must be observed in just this way, the fact that not all the evidence is ever available for public scrutiny must necessarily affect the standards of argument and debate within the field. Just as Freud was unwilling to accept a patient who would not report everything (because, as it happened, he was bound to keep certain professional secrets [see Freud, 1913, p. 136, n.]), so the field cannot function in an ideal manner so long as its reasoning rests on only partial data—and for exactly the same reasons. Just as the reluctant patient would be able to reclassify all his difficult moments as somehow secret and therefore exempt from analysis, so the reluctant author, faced with a piece of fuzzy reasoning or doubtful logic, is able to simply omit the embarrassing details from publication; given the tradition just cited, he does not even need to supply an explanation for the omission.

A further consequence of this tradition bears on the matter of generalizing from clinical particulars to the larger theory. If the full details of a given happening are never cited, then it becomes impossible to challenge the category formed from these details; if a given clinical happening, for example, is used as evidence for the return of repressed oedipal strivings, the assertion can only be taken on faith because the outside reader is never in possession of all the facts. As a result, clinical observations can be rather easily turned into far-reaching generalizations. The generalization, because of its greater visibility and rhetorical appeal, takes on an importance that goes far beyond the merits of its data base. In many cases, the generalization may be completely unwarranted.
To take a homely example from another field—suppose I give you a brief description of a cottage I just bought by saying that it has four walls and a roof. I choose not to provide further details. You hear my description, match it with a description of your own cottage, and conclude that yours is exactly like mine—even though yours is a modern beach house and mine is a madeover garage. In a similar way, incomplete clinical data lend themselves to premature labeling, unwarranted categorization, and give rise to theoretical concepts which are more metaphor than construct because of their largely nonexistent data base. As a result, conceptual agreement is much greater than the data justifies, and the theoretical state of the field seems more advanced than is actually the case.

If all the data for a given formulation are not available for independent examination, there is no possibility for the gradual refinement and clarification of theory. Rather than a particular formulation serving as a tentative and provisional arrangement of the data, subject to subsequent test and cross-validation, the formulation tends to become substituted for the data. Despite Freud's proviso that his theoretical concepts were meant to be taken as only temporary scaffolding subject to subsequent re-formulation (Freud, 1914, p. 77), the concepts have proved to be the most enduring parts of the clinical adventure; the relevant data are either incompletely reported or, even when made public, essentially inaccessible to anyone with only normative competence. With the exception of a relatively small number of recorded cases, there is no clinical archive; most utterances exist only in memory, and in a form that is more distorted than veridical.

As a result of this one-sided arrangement, the path from observation to theory can never be retraced. Not only does this fact place a heavy burden on the original discoverer; it also

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1 Elsewhere I have discussed the distinction between normative and privileged competence: "Normative competence belongs to all members of the psychoanalytic community… Privileged competence belongs to the analyst at a specific time and place in a particular analysis" (Spence, 1981, p. 114).
makes it difficult to try out other formulations because the data can never be
looked at twice. Given the lack of public findings, the theory has necessarily
grown in largely individual fashion, with separate investigators each
contributing his own set of concepts drawn from his private set of
observations (Freud, of course, leads the list). Assuming the usual level of
erroneous observation and faulty logic, there would seem, however, to have
been surprisingly few conceptual mistakes. How has this high standard of
performance been maintained?

Part of the answer lies in what we have identified as the tradition of
incomplete data. If the grounds for a general proposition are incomplete, then
there is no way for a critic to claim that a particular set of findings did not
support the concept because the original discoverer can argue that the missing
pieces of this set, had they been seen, would suffice to complete the puzzle.
On the other hand, it is still possible to support the concept if and when the
available findings happen to fit into that particular pattern. Thus the missing
data allow occasional support of theory but make falsification impossible.
Given this state of affairs, no proposition will ever be disconfirmed. (It
should be pointed out, of course, that the apparent confirmation is also suspect
because it does not happen in every instance; but since we never have access
to the full range of findings, we can never be sure of just what proportion of
cases fail to fit.)

A second part of the answer is based on the difference between the context
of discovery and the context of justification (see Reichenbach, 1951). A
given theoretical proposition may, in the context of a given clinical encounter
(the context of discovery), provide necessary and sufficient reasons for a
given piece of behavior; because it allows all pieces of the encounter to be
fitted into a continuous narrative, it gives a sense of satisfaction and closure
and the feeling that now at last a new understanding has taken place. Both
patient and analyst may be convinced that an explanation has been found for
an anomalous piece of behavior. This kind of "ah-ha" experience seems to be
central
to the process of therapeutic change; narrative fit permits certain kinds of formulations to "become true" (see Viderman, 1979) and thereby to become an accepted part of the patient's contents of consciousness.

But even though an explanation may be perfectly adequate within the therapeutic hour, it does not instantly qualify for the status of a general law. To reach that level, it must meet other kinds of tests, usually carried out in what is known as the context of justification. To become a part of our theoretical system, the proposition must not only suit the individual case, where it was first discovered, but meet the requirements of all other cases which share the same characteristics as the target case. When we move from the single case to the larger sample, we need to see all the data—but as already noted, this wish is defeated by the system.

We now begin to recognize the need for two standards of reporting. When the clinician is reporting on a new piece of clinical insight, he has no need to convince his readers; his mission has already been accomplished, so to speak, with the conviction of his patient. He is speaking within a particular context of discovery. Any further reporting is probably after the fact; whether or not his readers agree, the clinical discovery has already taken place. But when he is attempting to justify his concept, the new readers must be convinced, and conviction, as we have seen, can only take place when we have access to all the data. This kind of argument requires a different kind of presentation.

Freud never made the distinction between the two kinds of reporting. When describing a new piece of clinical insight, he found it sufficient to present only what he thought was sufficient, and he was rather scornful of the need to provide all the evidence; as in telling a joke, if it does not seem funny the first time, telling it in more detail will usually not help. And his aim in his case histories was more to share than to convince; the clinical work had already been accomplished, and he only felt
that others might be interested. For the purpose of initial reports, the tradition of incomplete data is perfectly adequate. But when he turned to a more general formulation, Freud needed to present a more complete analysis in order to bring about conviction. He never saw the need to provide a more elaborate presentation, and the failure to distinguish the two kinds of reporting has remained with us to the present.

How, exactly, do we go about finding confirmation? In the context of discovery, we seem to depend heavily on narrative fit and on the extent to which a given formulation or interpretation lends coherence and continuity to a particular set of happenings. Narrative coherence ranks as one of the four essential criteria used by Ricoeur (1977) to validate psychoanalytic facts: to "explain here is to reorganize facts into a meaningful whole which constitutes a single and continuous history..." (p. 861). Narrative fit has been described by Sherwood (1969) as a critical feature of the clinical explanation. But while narrative fit can be highly persuasive in the immediate clinical situation, its compelling quality does not necessarily indicate the presence of more general truths. And it should be borne in mind that narratives are notoriously accommodating because they are almost infinitely flexible and because they usually depend on a rather simple chronological syntax ("and then ... and then ... and then" [see Atkinson, 1978 p. 129]); as a result, they can almost never be used to exclude particular happenings. Thus a narrative is not well suited to disconfirm a particular hypothesis; I can hardly say, except in a very small set of cases, that Event A could not have happened before Event B.

As a result of its flexibility, a narrative cannot be used to make a definitive test between two formulations. It therefore follows that we can hardly ever use narrative fit as a test of historical truth (and, as we will see later, of theoretical truth). True and false happenings (in the sense of historical truth) can both be smoothly fitted into a patient's developing story, and the goodness of fit cannot be used to distinguish the first from the second. A
good narrative explanation may be useful because it brings together disparate pieces of a patient's life in an appealing way, but what might be called its narrative truth may depend more on its aesthetic properties (which stem from matters of timing and phrasing) than on its historical validity. This state of affairs has an ironical consequence. If we were able to know, by some kind of magic, which pieces of the narrative were historically false and could revise our narrative to leave them out, we might seriously jeopardize its aesthetic appeal, just as a painting might be seriously flawed by turning it into a strict representation of reality.

In what way does narrative truth differ from historical truth? Consider a specific interpretation about how the patient might conceivably have felt at a certain time in his life. Proof that he actually had these feelings is probably out of reach because feelings and attitudes leave few traces—thus the historical truth is always in doubt. But to the extent that the interpretation explains many subsequent aspects of the patient's behavior and to the extent that it completes the unfinished clinical picture in just the right way, it acquires its own truth value and no further checking is necessary. To the extent that an explanation is persuasive and compelling, it acquires features of what might be called narrative truth.

The contrast between the two kinds of truth is highlighted even further when we look at the nature of autobiography. Gusdorf (1980) tells us that in autobiography the truth of facts is subordinate to the truth of the man, for it is first of all the man who is in question… The significance of autobiography should therefore be sought beyond truth or falsity, as these are conceived by simple common sense [an apparent reference to historical truth]. It is unquestionably a document about a life, and the historian has a perfect right to check out its testimony and verify its accuracy. But it is also a work of art… The literary, artistic function is thus of greater importance than the historic and objective
function in spite of the claim made by positivist criticism both previously and today (p. 43).

At first glance, the contrast between narrative and historical truth may seem equivalent to the contrast between psychic and external reality. In fact, however, the terms have quite different meanings and should not be confused. External reality, first of all, is not the same as historical truth. We are coming to be skeptical of Freud's far-reaching positivism and are discovering that in many ways external "reality" is very much a construction of the perceiver and thus contains large portions of narrative truth. My account of yesterday's football game is undoubtedly a mixture of what really happened and what I wanted to happen, a mixture of the objective physics of the game and its subjective meaning. Conversely, psychic reality may often be securely grounded in historical fact, and one of our tasks as analysts is to increase the historical truth of the patient's inner world in whatever way we can. At the same time, however, we are also interested in enlarging its narrative base and providing more complete access to fantasy when this seems appropriate. Thus, psychic reality may, at any one time, contain elements of both historical and narrative truth, and its power to persuade is apparently independent of which truth is represented. In similar fashion, external reality is usually composed of both fact and fiction, and one consequence of analysis is to make the patient philosophically more sophisticated and less of a naïve realist.

If narrative truth differs from historical truth, how does it compare with theory? We can say that a narrative account derives its appeal from the particular; theory, from the general. Among the elements of the narrative account, some may be true in the sense of having actually happened (historical truth), and others may be true in a narrative sense, but false from the standpoint of history. In a number of places in his writing, Freud admitted that both true and false fragments could be contained within a particular reconstruction, but he claimed (somewhat optimistically it would seem) that this was no problem because
it was the "kernel of truth" that made all the difference (Freud, 1937p. 268).
But it is by no means clear how the "kernel of truth" can be identified and
separated from the set of equally likely fabrications which make up the
patient's life story, and if we have no sure way of identifying historical truth,
we are seriously handicapped in our attempt to frame theoretical laws. This is
not to say that narrative truth is an unimportant aspect of the true or to deny
that it contributes significantly to therapeutic success; it is only to point out
that narrative truth is not always equivalent to "the whole truth" and to make
clear that we cannot use therapeutic success to validate our general theory
(see Hartmann [1964] and Eagle [1980a] for similar cautions). This very
success may be an important reason for taking a proposition seriously in the
first place, but to rest a general theory on the results of one patient (or even
one hour) is to seriously restrict our data base and to ignore the importance of
individual differences. Once again, context of discovery should not be
confused with context of justification.

But this would seem to be an unfair accusation; confirmation of theoretical
constructs always seems to rely on more than one patient, and we can find
countless examples in our literature of new clinical findings which seem to
support established theory. The trouble lies in the nature of the confirmation,
and this problem brings us back to our tradition of incomplete data. As we
have seen earlier in this paper, when all the facts are not presented, loose
confirmation can be rather easily achieved. Precisely for this reason, the fit
between theory and findings can never be more than preliminary when not all
the facts are available, and particularly good fits are special causes for alarm
because of the possibility that the good fit is achieved by judicious selection
and arrangement of the findings.

It would seem as if the apparent validation of theory by clinical data,
because it is necessarily based on incomplete evidence, must necessarily
depend on "soft" pattern-matches. If all the facts were known, many of these
matches would not stand up
to scrutiny. But because the large store of clinical utterances disappear as soon as they are uttered, there is no going back; thus the validity of a pattern-match can never be tested, and with the authority granted by printer's ink, it comes to take on a status which it probably does not deserve. Soft pattern-matches turn into hard confirmations, without our ever being quite aware that the change is taking place.

Another reason for the general belief in confirmation stems from a second aspect of narrative truth. We have seen that narrative truth can be distinguished from historical truth in the analytic hour and that the former may contribute significantly to the effectiveness of an interpretation. But there is a second kind of narrative truth which lies at the heart of the way each of us thinks about the general theory. We are always constructing a conceptual narrative from a combination of clinical experiences and theoretical exposure, fitting together into a coherent whole our sense of psychoanalytic theory and psychoanalytic process. We bring to this task criteria which are relevant to the problem of narrative fit. As a result, we are more than usually tolerant of soft pattern-matches and incomplete evidence because we aim to construct a coherent theoretical "story," not an ironclad account of the "facts."

This attempt is good enough to provide us with a usable narrative which is always being reinforced by our ongoing clinical experience. Matches between clinical happenings and our developing narrative provide us with the sense that we are privy to a certain part of the truth; what we never stop to examine, of course, is the fact that the matches are often selective and that our theory never accounts for all of the data. We are also inclined to remember the positive and ignore the negative matches because we are searching for coherence and continuity; as a result, we tend to minimize anything which interrupts the "flow" of the narrative. Because we are searching for narrative truth, we are always attempting to write the best possible "story" from all available data; we are not attempting to keep careful track of matches and mismatches.

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It could be argued that the very reasons that allow us to construct a usable narrative prevent us from generating a valid theory. As the history of science makes clear, theory-building is discontinuous, episodic, and often marked by critical re-evaluations; at certain points along the way, old paradigms must be dismantled and replaced by substantially different world views. To build good theory, we must be prepared to give up a familiar formulation and return to the data of observation until a new explanation is discovered. But such a step not only requires access to the original observations (many of which are permanently out of reach), but also a readiness to throw away large parts of the enabling narrative which help us to make sense out of our day-by-day clinical encounters. Even though it may not make sense of everything, this narrative is still better than nothing and, as a result, tends to remain in place despite any number of disconfirming observations. Even though wrong in parts, it is also true in parts; enough confirmation is provided by our daily encounters with our patients to convince us that we are on the right track. If we assume, in addition, that the correct theory is only waiting in the wings, then we may find it even less necessary to carry out a critical review of all the evidence—assuming we could somehow make it reappear.

II

In our general satisfaction with our private narrative as a predictor of behavior, supported by confirmations from our daily clinical encounters, we have tended to lose sight of the fact that everyone—you, I, and the man on the street—operates in a roughly comparable fashion. Some theories are better than others, but in every case the theory user always believes that his system is true enough and gives a sufficiently good account of the facts to justify continued use. This state of affairs comes about because of what might be called the ambiguity of everyday life, a lesson well taught by Freud. Since any piece of behavior can be looked at from several vantage points, we can produce any number of pattern-matches simply by choosing the
piece which best fits our particular theory. To complicate matters further, this ambiguity is composed of two different types: the multiple meanings of events as they actually exist in the real world (a dream, for example, can be seen as both a source of information and a protector of sleep; a symptom as both an inconvenience and an appeal for sympathy); and second, the multiple meanings which we choose to project onto the world as we see it, meanings which are often guided by unconscious and preconscious fantasies (see Arlow, 1969). It is the second sort of ambiguity that is used to confirm the private theory of the paranoid and the mystic; the picture would be much simpler if these projected meanings could be simply set aside. But the difficulty seems to be that there is no easy way to discriminate the first set from the second—and therefore, all the more reason to be wary of private theory and subjective confirmation.

The problem is further complicated by language and the way it functions. In many situations, particularly when reality is unambiguous, it works as a simple pointer. If I say, "Look at the aardvark," I have both given you an order and, assuming you had never seen an aardvark before, a lesson in identification. But when reality becomes more ambiguous and we move from simple objects to complex relationships, then language can open the way to a great deal of mischief. Suppose I describe the case of a student who just failed an examination and say wisely, "It looks like castration anxiety." Language is pretending to be a pointer, as before, and you may hear me telling you something veridical about the student. In fact, however, I am not making an observation at all—or at least, not an observation that can be checked by a random set of other observers. On the contrary, I am invoking a piece of theory without saying so; I am making it appear as if the theory were proven rather than problematic; and by using the word castration in such an offhand fashion, I am giving you the feeling that everyone believes that castration fears have rather profound and long-lasting effects on behavior, and that this belief has been supported once again. In short,
I am mischievously using language to make it look as if we have found a confirmation of a general law—when, in fact, the notion of castration anxiety is at best a rather weak hypothesis of rather restricted scope.

How much of this mischief comes from my own projective system and how much is the sentence a valid description of the student's behavior? The answer depends on your own set of beliefs, your own theory of the world. If you are a Freudian, you will tend to hear the statement as a simple observation, a statement similar to my saying, "There is an aardvark." If you are not, you may hear it as a whimsical comment, as a metaphor, as an outrageous and unfounded accusation, or as a mixture of all these and more. If you are a Freudian, you will tend to hear my statement as an example of what Quine would call an observation sentence, defined as a sentence which does full justice to its stimulus meaning (in Quine's [1960] language, observation sentences "wear their meaning on their sleeves"). Observation sentences are the datum sentences of science; they can be described as "the bottom edge of language, where it touches experience: where speech is conditioned to stimulation" (Quine and Ullian, 1978, p. 28). Observation sentences produce agreement in all observers exposed to the same situation.

But while all observers would probably agree when I say, "There is an aardvark," a much smaller sample would agree with my statement, "It looks like castration anxiety." The nonbelievers might have been exposed to the same stimulation—the failing student—but choose to describe him in other ways. If the disagreement is large enough, we would have to call my statement something other than an observation sentence; as a result of this relabeling, it would not be used in the formation of a larger theory.

Now here is where our particular troubles begin. The bulk of putative observation sentences in our literature can never be checked because the utterances giving rise to these sentences have disappeared. As a result, their status can never be challenged,
and the way is open to using them in a theory where they probably do not belong. Because of our language habits, statements which are cast in the form of observation sentences tend to be taken as valid observations; in the absence of primary data, this tendency becomes all-powerful and, as noted earlier, random comments may be elevated to the status of theory; with repetition, they are advanced to the level of eternal truths. A given proposition may be completely unfounded and yet every time it is invoked, even in the absence of any kind of confirming evidence, it carries the impression that the proposition has once more been confirmed. In the example just given, to invoke the concept of castration anxiety is to make the claim that it has once again been supported. Frequency of use breeds familiarity, and familiarity breeds persuasion. Just as a general law is strengthened with each new piece of supporting data, so a tentative proposition such as castration anxiety can be mistakenly supported simply by being invoked. Each usage may be completely without foundation, but the power of language is such that when I use words in a pointing fashion, you assume that I am being truthful and telling you something veridical about our shared world. You assume, in other words, that I am making a valid observation.

Here, then, is another way by which our theory becomes wrongly confirmed. Because it depends on incomplete data, it lends itself to pseudo-observation sentences which carry the conviction of confirmation. Because the data tend to disappear as soon as they are conceptualized, the statements about the data tend to take their place; the statements become the core of our literature and, because they are publicly accessible, they tend to function as evidence. What began as a tentative observation may be prematurely cast as a theoretical statement (see Esman, 1979) and becomes invested with authority and finality; given a certain frequency of appearance, it becomes established as tested theory. As the literature takes the place of the primary data, disputes are referred to specific papers when they should be
referred to the original evidence, and concepts remain current so long as they are published and tend to drop into disuse when they are no longer cited. With attention shifted to the literature, we no longer notice the absence of primary data, the small number of recorded cases, or the gap between what can be understood from a recorded case and what was available to the treating analyst when he was treating that particular patient.

III

In one of his papers to a more general audience, Freud (1933) stressed his interest in building a general theory.

I have told you that psycho-analysis began as a method of treatment; but I did not want to commend it to your interest as a method of treatment but on account of the truths it contains, on account of the information it gives us about what concerns human beings most of all—their own nature—and on account of the connections it discloses between the most different of their activities (p. 156).

Freud always wanted to go beyond interpretation and use the clinical material to establish general laws. He gained his insights from the individual case but hoped to avoid circularity by confirming them with other cases. His successors have tried to carry on this tradition by testing what might be called the working theory against the new circumstances of each ongoing case.

We speak and write as if this tradition was flourishing—and yet, as I have tried to make clear, on only a very few occasions is a general proposition ever confronted by the underlying evidence. The data are conspicuous mainly by their absence, and what confirmations we have are almost always incomplete and essentially private, inaccessible to the outside critic. We have no tradition which requires an assertion to be backed by all the evidence; on the contrary, as we have seen, Freud emphasized the right of privileged withholding, and we have followed in his footsteps. In similar fashion, we have no tradition of what might be called the loyal opposition, no forum for well-intentioned
disbelievers who are not satisfied with a given set of conclusions. It may be
that the lack of legitimate opposition is closely tied to the tradition of
incomplete data, because so long as there is a possibility that the data are
confirming but simply being withheld, the critic tends to suppress his
criticism. But as in other arenas, the absence of dissent does not always
indicate agreement.

I have tried to argue that narrative truth must be distinguished from
theoretical truth. An interpretation may be useful because it brings together
disparate pieces of a patient's life, because it reduces many pieces of
behavior to one underlying cause, and because it finds a "narrative home for
an anomalous happening" (Spence, 1982)—all of these satisfactions depend
on the narrative and aesthetic truth of the interpretation, but they have no
necessary bearing on its place in the larger body of theory. Narrative truth is
useful—indeed, essential—for generating our understanding of the particular
case we are treating but significantly incomplete for generating general laws.
We tend to lose sight of this conclusion because we keep having experiences
which seem to prove the opposite. Every day, some part of our clinical
experience seems to support one or more pieces of our general theory, and in
the face of these accumulating confirmations, we continue to think that the
clinical happening does, after all, provide the basis for sound theory. But as
we have seen, the confirmations tend to be piecemeal and can never be
checked by others; and because we are working toward building up a private
narrative of the theory, we tend to emphasize coherence and continuity rather
than mismatch and surprise. (If there were surprises, they might never be
remembered; and if they were remembered, where would they be published?)

I have argued that each of us is constantly developing his own version of
the received theory—our subjective narrative. Because it is based on private
data and never made public, it can never be compared with the working
theory of another analyst. Nevertheless, we all believe that our own private
theory is essentially the same as that of our colleagues; and furthermore, that
they all correspond almost exactly to the developing general theory which will some day appear. But this assumption needs to be challenged. The working hypotheses of any particular analyst may have no necessary connection with the general theory or with any other private theory; so long as they do not violate the narrative truth of the analyst's experience, they can range over a broad landscape of ideas, some true, others not so true; and so long as the large majority of clinical happenings are never recorded, there is little likelihood that his private hypotheses will ever be tested against another set of data.

Public theory is just as much protected—because of the tradition of incomplete data. Not only does a pseudo-observation statement derive authority by simply being stated; the more often it is used, the better its credentials seem to become because it takes on the character of a general law. As I have argued above, each particular use of a proposition may be based on only soft data, but the accumulation of many soft instances builds up an air of infallibility and general truth.

Most in need of examination is our general theory. It may be necessary to recognize the unpleasant fact that Freud was being somewhat wishful in his assumption that his theory contained a body of general truths, just as he was somewhat frivolous in his approach to confirmation. Discussing the problem of corroborating an interpretation, Freud (1923) writes that

what makes him [the analyst] certain in the end is precisely the complication of the problem before him, which is like the solution of a jig-saw puzzle… If one succeeds in arranging the confused heap of fragments, each of which bears upon it an unintelligible piece of drawing, so that the picture acquires a meaning, so that there is no gap anywhere in the design and so that the whole fits into the frame—if all these conditions are fulfilled, then one knows that one has solved the puzzle and that there is no alternative solution (p. 116).

But because of the elasticity of narrative fit, there are any number of good arrangements; and as we have noted, what suits the
context of discovery may not fit the context of justification. Freud was too ready to generalize from the single case—too excited, perhaps, by the rapture of discovery to realize that one swallow does not always make a summer.

We may have to reconcile ourselves to the possibility that a general theory may be a long time coming—because of the particular nature of our clinical operation which is based on narrative truth. As I have argued elsewhere, "If the analyst functions more as a pattern-maker than a pattern-finder, then we may be faced with a glaring absence of general rules. What rules there are, moreover, may pertain mainly to the more trivial aspects of our clinical material. If the impact of a particular interpretation is contingent … on the specific texture of time and place, the rules for it being true are just as much out of reach as the rules for any other kind of artistic masterpiece" (Spence, 1982).

Each time a particular interpretation is followed by more insight or a new set of associations, the analyst is apt to draw the mistaken conclusion that its historical truth is also being confirmed; from this, it is but a small step to conclude that something of theoretical importance has been established. But implicit in the notion that narrative truth can be distinguished from historical truth is the conclusion that narrative truth can be based on nonhistorical premises (see Viderman [1979] and his conception of an interpretation as a creative act). To the extent that interpretations work for nonfactual reasons (in the usual sense of the word), it is obviously a mistake to use clinical data as the main ingredient in a general theory. Some parts may indeed be relevant, but we are faced with the problem of never knowing, in any particular instance, which pieces of data have general relevance and which are specific to the hour and patient and analyst in question. We can imagine situations in which the persuasive power of an interpretation stemmed largely from the particular shape of the transference: the analyst, vested with all of his transference power, was more important than what he said. It would clearly be a mistake to use the content as the basis
for some general theory of behavior. At other times, the content might be more relevant to the effect and could be considered as the "kernel of truth" that produced the effect. It seems clear that we have no reliable way of distinguishing the first case from the second, and the same could be said of Freud. By failing to draw the distinction, he tended to raise all significant clinical happenings to the level of theoretical propositions. By failing to distinguish narrative from historical truth, he tended to confuse the specific effects of the treatment situation with the generality of the clinical happening.

IV

What, then, are the prospects for a general theory of "what concerns human beings most of all"? I have talked about the tradition of incomplete evidence and how this has stood in the way of critical thinking and vital theory. Is there a prospect for change? The answer, it would seem, must depend on how we learn to move beyond narrative truth, or more broadly, how we move from description to explanation (see Hartmann, 1964).

Because we are working with complex patterns of form and content, it often becomes difficult to identify the relevant data. More than the wording of an interpretation needs to be known, but how much more? We would like to know something about the state of the transference, about the history of that particular interpretation, about the additional meanings suggested by the wording but not made explicit—but in every case, we need to know how much ground to search, and we have no precise guidelines. Each investigator tends to use his own standards, and as a result, what is data for one researcher may be skeletal evidence for another.

A second reason to be dubious about whether we can gain access to all the evidence bears on the problem of privileged context. The visible text of the session under study is always being heard—by patient and analyst—against the background of what each of them is thinking (see Spence, 1981). Once again,
we are confronted with a complex mixture. Does conviction come from the wording of a specific interpretation or from the meshing of its lexical content with the patient's hopes and fears? In many cases, a particular interpretation gains extra force from the private meanings read into it by the patient, meanings which are invisible to the outside observer (we are back to the distinction between normative and privileged competence). The reason for giving a particular interpretation may often depend less on the demands of the analytic "conversation" than on the inner experience of the analyst who, because of some series of subjective feelings, decides that now is the time to speak; once again, this inner experience is inaccessible to the outside observer. Yet it is clearly part of the data, because without knowledge of this inner context, we can never "hear" the analytic conversation with the correct "phrasing" and "accent."

The problem of privileged context carries with it a formidable challenge. The more subjective the experience, the more difficult it is to reduce it to a set of general laws—and we might argue that any such attempt could lead only to sterile theory. We not only have the problem of how to translate a highly individual set of meanings into some kind of general formulation, but we must also find an answer to the question of how much context to include. To interpret a given session, do we need to know the detailed experience of the analyst during the preceding twenty-four hours? Forty-eight hours? Three months? Six months? The questions are endless because we have no systematic way of approaching the problem.

Thus it would appear that we will always be hampered by the tradition of incomplete data. Even with the best of intentions, there seems to be no systematic method for defining the proper size of the search space and gathering a proper context for each visible utterance. And note the irony—even with an awareness of what is missing and the knowledge that all the evidence must be searched, we are still helpless in formulating the next step. Perhaps Freud took his privileged stance for a reason!
As we discover that narrative truth lies at the heart of insight and therapeutic change, we are also learning that historical truth is less accessible than we are taught to believe. Toward the end of his life, Freud seemed to accept this verdict and argued for construction (a partly creative endeavor) over reconstruction (an attempt to resurrect a piece of the past); the former concerns itself with narrative truth whereas the latter is an attempt to find historical truth. In recent papers (see Shapiro [1981] in particular), we have again heard increasing doubts about the possibility of ever uncovering what "really" happened during the early years of a patient's life, and a recent series of studies by Loftus (1979) has shown how easily memories can be influenced by leading questions, making us necessarily skeptical about the effect of the treatment process on the patient's associations. From every side, there seems reason to doubt the historical truth of our evidence, and we are learning to treat even fairly "hard" memories with a certain skepticism. As a result, we have tended to see more and more significance in the narrative truth of a formulation and in the central position of narrative appeal in the treatment process.

But narrative truth brings with it its own set of problems. It is first and foremost highly relative; each story is different, and what makes a particular formulation persuasive and compelling is precisely the fact that it is carefully tailored to the patient's life. (If all our interpretations were the same, word for word, they would quickly lose their power to persuade.) But if each piece of narrative truth is relative, how can we build it into a general theory? Each piece of narrative explanation tends to be somewhat ad hoc, drawing its power to persuade from an accumulation of individual details which we can never hope to replicate in any other situation. Given the explanation and given its clinical effect, how do we go about expressing it in some more general manner so that I can compare my narrative with yours and ultimately develop some general laws of what

2 For an example of earlier doubting, see Kris (1956).
makes a good narrative? The study of literature has traditionally been concerned with what might be called the theory of the good story; we know certain things about form and content, about some of the transformations necessary to turn the chaotic details of reality into the smooth form of a persuasive story. We can also learn from the hermeneutic tradition something about how to go about finding the ingredients for our narrative—how to sift through the ambiguous material of dreams, memories, and fantasies to find the particular meanings we need. But neither story-telling nor hermeneutic discovery will teach us much about the science of man, and here is where the primary drawback of the narrative tradition comes home. If the bulk of historical "findings" are in some way biased in the telling, we have inevitably restricted the scope of our theory. We may have to recognize that the past is essentially out of reach and that our data are relevant mainly to understanding the here-and-now of a particular hour. We are in a privileged position to report on the here-and-now, but in only a weak position to report on the past. The difference may be described with the use of Russell's (1912) famous distinction between knowledge by acquaintance and knowledge by description. We are privileged observers of the data within the session and in a position to observe how a particular interpretation, let us say, is heard by the patient and how the hearing (or mishearing) can be understood in light of the transference neurosis. Experiences of this kind lead to knowledge by acquaintance. But reports of the past are a kind of hearsay with the referent permanently out of reach, and although we may make a genetic interpretation, it is always based more on knowledge by description (theoretical knowledge). An explanation rooted in the transference and reinforced by the countless details of the immediate context is always more persuasive than an explanation based on a hypothetical construction of the past.

But more than persuasion is at stake; there is also a certain respect for the evidence. By prematurely reducing a clinical
observation to some earlier, hypothetical event (a primal scene, for example), we may have convinced ourselves that we have explained the observation, but once labeled in this manner, it is never looked at again. Multiply this tendency by thousands of hours across thousands of analysts and we can see the waste of data. All the time we were settling for doubtful explanation, rooted in the past, we were being exposed to thousands of clinical events which contained significant amounts of unmined clinical information about the present. But because we had less interest in what was in front of our eyes, the significant data often slipped through our fingers as we continued to confirm doubtful propositions about the past.

We can put the matter more precisely by looking again at the conflict between public and private theory. Each analyst is continuously engaged in updating his private narrative, adding the latest examples of good and bad interpretations to his accumulating store of clinical wisdom. He is always trying to assimilate the evidence to his working hypotheses. But it would be a mistake for the field in general to take this approach; on the contrary, it must put the emphasis on accommodation and find ways in which the received theory must be changed to conform to the evidence.

To this end, it might be useful to separate journal contributions into two categories: the clinical discoveries (narrative truth) and the more abstract formulations (theoretical truth). Papers in the first category might serve as the archive from which general conclusions could be drawn; formulations drawn from these cases would appear in the second category. Papers in the first category would emphasize the clinical context of a particular happening; this would include (in much more detail than is currently found) a detailed exposition of the context of the case along with an account of the privileged context of the treating analyst. If enough detail were provided, in the appropriate context, it would be possible for an outside reader (someone with only normative competence) to understand and integrate
the information and be in a position to compare it with other accounts of similar events. The clinical evidence would then become publicly accessible, and theoretical discussions could be referred to one or more specific specimens, making it possible to generate public discussion and, even more important, to be able to retrace at will the path from data to construct. A new concept (e.g., "splitting") could be defined by reference to a particular sample of clinical accounts, and its evidential basis would be available for all to consider. For the first time, analytic data would become public property, publicly discussed.

If clinical detail is appropriately presented against the right kind of context, it should be possible to bring about conviction based on narrative truth alone. For almost the first time, an outside reader would be able to sense the excitement of a bona fide clinical discovery—rather than be told that such things exist, to be taken on faith, he would experience the conviction as it occurred. What has been knowledge by description would thus turn into knowledge by acquaintance. While it may be necessary to develop a mixture of literary and dramatic expertise to properly present a clinical happening in a way that can truly convince the reader, if this goal can be achieved, we will be able to capture a critical feature of narrative truth and build this into our theory.

The contents of these vignettes would be analyzed and discussed in the second category—the theoretical papers. Here the aim is to convince as well as discover; the specific reason of the particular instance would give way to the formulation of general laws (see Eagle, 1980b). Argument about formulation could thus be kept separate from argument about particulars, and a given piece of the clinical archive could be discussed by any number of authors (in contrast to the current practice of supplying new examples for each new discussion). Theoretical papers might also be judged on more general grounds; ad hoc arguments would give way to more general kinds of reasoning; and the detail of the case could be kept separate from its larger
meaning. Unresolved disagreements might lead to a call for new clinical instances (rather than the formation of new institutes); new additions to the archive would generate new discussions and further clarification of theoretical issues. (One is reminded of the case system in law and the way in which new cases lead to new decisions.)

To close with a specific example of how such a scheme might work, consider a recent paper by Langs (1981) on modes of cure in psychoanalysis. He proposes the idea that only interpretations based on the here-and-now context of the hour are curative in any significant degree, and that other kinds of interpretations, based on manifest content or genetic formulations, may actually reinforce the resistance. Only interpretations which take account of the interaction between patient and analyst "can offer the patient adaptive structural change and insightful symptom resolution" (Langs, 1981, p. 212).

Strong claims, one would think—where is the evidence? Unfortunately, it is largely invisible, and once again, the claims must be taken largely on faith. Whether or not the argument is true, there is no way in which it can be fairly evaluated. Think how much more convincing the argument would have been—and how much more lasting its consequences—if supporting data were available. Under those conditions, it would move from an interesting speculation to a matter of public record—from a matter of narrative truth to (if confirmed) a piece of theoretical knowledge. Very likely the original formulation would undergo several stages of modification; significant conditions might be uncovered which would increase or decrease the general effect; certain exceptions to the general rule might be discovered as it was challenged by new pieces of clinical evidence. But the end result would produce more general conviction than is possible in the absence of evidence; by embedding the rule in a context of particulars, we would move from knowledge by description (which comes cheap) to knowledge by acquaintance (which is significantly more lasting).

REFERENCES

ARLOW, J. A. 1969 Unconscious fantasy and disturbances of conscious experience Psychoanal. Q. 38:1-27


EAGLE, M. 1980a Psychoanalytic interpretation: veridicality and therapeutic effectiveness Nous 14 405-425


ESMAN, A. 1979 On evidence and inference, or the Babel of tongues Psychoanal. Q. 48:628-630

FREUD, S. 1912 Recommendations to physicians practising psycho-analysis S.E. 12

FREUD, S. 1913 On beginning the treatment (further recommendations on the technique of psycho-analysis I) S.E. 12

FREUD, S. 1914 On narcissism: an introduction S.E. 14

FREUD, S. 1923 Remarks on the theory and practice of dream-interpretation S.E. 19

FREUD, S. 1933 New introductory lectures on psycho-analysis S.E. 22

FREUD, S. 1937 Constructions in analysis S.E. 23


KRIS, E. 1956 The recovery of childhood memories in psychoanalysis Psychoanal. Study Child 11:54-88
LANGS, R. 1981 Modes of 'cure' in psychoanalysis and psychoanalytic
psychotherapy Int. J. Psychoanal. 62:199-214
Univ. Press.
QUINE, W. V. & ULLIAN, J. S. 1978 The Web of Belief New York: Random
House.
of California Press.
RICOEUR, P. 1977 The question of proof in Freud's psychoanalytic writings
J. Am. Psychoanal. Assoc. 25:835-871
SHAPIRO, T. 1981 On the quest for the origin of conflict Psychoanal. Q.
50:1-21
SHERWOOD, M. 1969 The Logic of Explanation in Psychoanalysis New
SPENCE, D. P. 1981 Psychoanalytic competence Int. J. Psychoanal. 62:113-
124

- 67 -
VIDERMAN, S. 1979 The analytic space: meaning and problems
Psychoanal. Q. 48:257-291 [→]
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