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PSYCHOANALYTIC CONTROVERSIES CONTEXTUALIZED

Psychoanalytic disagreements are famously heated, polarized, and prolonged. These controversies are often the reflection of a shared agreement by the participants to engage in debate at an abstract level far removed from the clinical context in which the disagreement first arose. As a specimen example of such disputes, a case report by Patrick Casement is examined, together with a series of polemical discussions it inspired concerning physical contact suddenly demanded by an analysand in session. Over two dozen authors were almost evenly divided on whether to agree with Casement's technical conclusions, but showed a disquieting indifference to the detailed information available in his report regarding how this clinical crisis developed. The substantive merits of the contending arguments are not at issue; rather, the point is to demonstrate the crucial need to refine a methodology of contextualization to clarify inferential assumptions in clinical discussions. Premature truth claims might then give way to a more rational comparison of the clinical sources of divergent opinions. The term *contextual horizon* is introduced to facilitate an understanding of how the psychoanalyst makes inferences from the patient's associations.

> *The very idea of empirical certainty is irresistibly comical.*
>
> —C. S. Peirce

> *There are the trivial truths and the great truths. The opposite of a trivial truth is plainly false. The opposite of a great truth is also true.*
>
> —Nils Bohr

The inference of latent meaning from the associations of the patient is the central task of analyst and patient on the path to therapeutic change. But for many reasons psychoanalysts have never developed a

consensually accepted canon of rules of evidence for deriving inferences from these associations. We know all too well that various analysts derive different meanings from the "same" clinical material. But it still is not sufficiently appreciated that this defect in our methodology has obscured the fact that many of our psychoanalytic controversies have been prolonged because the adversaries were really talking past each other.

Analysts differ a great deal in the way we organize the associations of the patient as we are "listening." The artificial description of the analyst listening with free-floating attention, innocent of memory or desire and refraining from imposing bias on the "data," is a fiction that seems to persist in spite of the correctives in our abundant literature on the irreducible subjectivity of the analyst. This helpful concern about our subjectivity has to date emphasized primarily the emotionally constituted subjectivity of the analyst and has neglected the very important problem of our confusion about how the analyst *contextualizes* the communications of the patient.

The inference of latent meanings from manifest data is a bedrock concept for all forms of psychoanalysis, and future historians of our field will no doubt investigate why we were content for so long to allow our inferential processes to remain so ambiguous. Whether within one theoretical model or comparing different models, we have learned the hard way that anything the patient says or does can potentially mean almost anything. One very important reason for this chaotic diversity of inferences is our failure to explicitly refine our methodology for defining the essential antecedent precursors of contextualization that will profoundly shape our inferences. Our various models map different explanations for the transformation from the raw data of the patient's associations into the inferred and imputed meanings. Our noisiest controversies have been at the higher levels of abstraction and too uncommonly at the lower, "experience-near" level. It is at this level that we have the best chance of reconstructing and comparing among ourselves the contextualizing criteria used to infer meaning during individual sessions. The neglect of this information has seriously impeded the development of a methodology for a coherent comparative psychoanalysis.

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What we reductively call "listening" of course subsumes a complex array of subprocesses. It is timely to recall that our considerable literature on the integration of theoretical models is rather silent about the role of the patient's associations in how the analyst listens. Certainly it is true that not all analysts in all models give equal importance to a close listening to the associations of the patient. But even within the so-called classical "mainstream" in the U.S., it is by no means true that all analysts listen to their patients with their inferential assumptions firmly rooted in the conviction that all the associations of the patient are of potential significance.

What I will have to say in this paper about contextualizing these associations is rooted in that conviction. This is a crucial point because our literature shows that we cannot assume that all analysts have ever privileged a *close* listening to the sequential unfolding of these associations as an organizing factor in the manner used to achieve contextualization. As analysts listen, they oscillate between mere "listening" and trying to understand what is heard as transformed or altered. In many but not all models it is assumed that the alteration is defensive. The assumption of some form of alteration is in evidence whenever the analyst offers an interpretation. To be clear, I do not wish to reduce the complex question of what is mutative in the psychoanalytic process to the single factor of interpretation. Instead I wish to raise questions about the inferential processes of the analyst who decides to explain something to the patient in the hope that this explanation will be helpful. In this discussion I emphasize only one of these processes: a complex, reciprocally enhancing, and dialectical interaction between trial contextualizations and contextualizing criteria.

By *contextualizing criteria* I mean any or all of those inferential assumptions employed by the analyst to infer meaning from the raw data. These criteria derive from diverse levels of abstraction. Putting red beads together must precede deciding if there are more red beads or more blue baubles. Only then can one decide if the red beads are worth more than the blue baubles. Contextualizing criteria universally filter the communications of the patient and privilege some of them and deemphasize others. The analyst has used a contextualizing criterion whenever he or she considers whether things said by the patient should be linked together, or whether certain things said by the patient should be linked to the theoretical ideas of the analyst. Contextualizing criteria are often applied preconsciously or unconsciously, but not always.

They represent a vastly diverse group of linking decisions made by the analyst that derive from many different frames of reference. This is another instance where the use of a noun, here *criterion,* instead of a verb tends to reify the manner in which we form contextualizing hunches. It would be far closer to what I intend here to speak of contextualizing processes and ideas, because *criterion* promises too much certainty, as though the analyst at work had a manual of reliable and repeatable contexts, or a canon of contexts. The advantage of retaining the term *contextualizing criteria* is its clear articulation with the existing literature. If a contextualizing criterion is a linking criterion, so is a correspondence criterion. Both terms connote the conceptual linkages used by the analyst to infer meaning from the associations of the patient by integration with the theoretical preferences of the analyst. But the term *contextualizing criteria* arises in the frame of reference of clinical data, whereas the correspondence criteria concept arises in the frame of reference of science. *Correspondence criteria* is a more modest term than *rules of evidence,* but suggests or implies a canon of evidence and promises a far greater degree of validity than does the term *contextualizing criteria.* I use the first term to connote views currently held by a given analytic group—this is never totally consensual—about the reliability of inferring that certain elements of theory correspond to actual conflicts in the patient (for a similar definition, see Garza-Guerrero 2000).

It would be helpful to articulate the relationship of the notion of contextualizing criteria to that of correspondence criteria. But such a clarification would plunge us into very complex issues concerning epistemology and the philosophy of science. That is because the term *correspondence* is linked to a series of polarized views of science predating Freud: the *Naturwissenschaften,* or sciences of nature, of which the model is physics, versus interpretive or ideographic sciences, the *Geisteswissenschaften,* or "cultural" or "human" sciences. Correspondence is deeply entrenched in the literature of philosophy (the correspondence theory of truth) and in psychoanalysis as a component of the rules or canon of evidence. There are also strong connotational linkages via the bridge of the correspondence theory of truth to a well-known series of polarized disagreements about the definitions of psychoanalysis and science. Merely to indicate what I have in mind I refer to explaining versus understanding, narrative truth versus historical truth, nomothetic versus ideographic sciences, and the relation of

inductive and deductive inference (see, e.g., Ahumada 1994a,b). These issues in turn underlie a very important attack in recent years on the very idea of an observing analyst as a logical fallacy, an attack stemming from the tendentious view that the idea is a relic of positivism (Wallerstein 1986).

For these reasons I will use the term *contextualizing criteria* in this paper and avoid the term *correspondence criteria* (in the sense of a canon of evidence or rules of evidence). Finally, I have observed in discussions with colleagues a misunderstanding about contextualizing: that its use conveys epistemological commitment to a philosophical or psychoanalytic camp. The argument goes this way. Contextualization is the goal of the analyst who uses the narrative-hermeneutic model, so you can't have it both ways: you espouse a view of psychoanalysis either as a science or as a branch of hermeneutics. I disagree that there is a contradiction here. I have written this paper on the premise that psychoanalysts are imperfect, subjectively biased observers, as are many other people in scientific enterprises, but that our observations are about actual affective urgencies that our patients have struggled with before ever meeting us.

Our literature is strangely silent about our confusion over how to compare alternative contextualizations. Given the daunting complexity of the antecedents and precursors of the inferences at which the analyst arrives, it is nonetheless quite possible to state clearly and exactly what the patient has said that is the basis for an inference. But it is a rarity to find such information in our literature, and in what follows I will illustrate the confusion and errors that arise from ignoring this problem. I am not suggesting that we catch Niagara Falls in a small bucket or that we can reduce the extraordinary complexity of the exquisitely complex intersubjective interaction of analyst and patient to simple linear accumulations of data. I am suggesting instead that this very complexity serves too often as an excuse to avoid clearly stating information that it *is* possible to report.

Trial contextualizations by the analyst are the counterpart of the trial identifications that are the core of analytic empathy. Just as the analyst must be able to establish a mobile, transient identification with the patient to know what the patient is feeling, so the analyst must form trial contextualizations of what the patient is communicating. For example, the analyst may privately ponder whether the first and second narrative in an hour are thematically related; if they seem manifestly

related, the question then arises as to how they might be latently linked or dynamically congruent. The analyst may establish a conjecture based on this contextualization that in turn is related to contextualizing criteria linking this dynamic connection to still others, only to hear a third, dissonant narrative requiring rejection of the first conjecture. In many sessions that rarely end up as papers in our literature, the analyst concludes the session with a confused feeling that not one of the several contextualizing conjectures that had at first seemed plausible were timely to share with the patient. In my experience the continuing absence of such confusion signals a smug analyst, but the extended and repetitive presence of sessions in which there are no evident contextual horizons is a danger signal of a disorganized analysis under strain.

One promising instrument for the clarification of our disagreements about clinical evidence is the development of *explicit* descriptions of how the analyst has contextualized the associations of the patient. This could some day lead to greater clarity regarding the contextualizing criteria that govern the entire range of decisions we make about the organization of associations and their supposed correspondence to the theory and technique of the analyst. We do have many contextualizing guidelines in our oral educational tradition. A major purpose of this paper is to call attention to the fact that our guidelines or criteria for contextualizing have too often remained tacit, implicit, and in the oral tradition instead of becoming a regularly explicit evidential obligation of psychoanalytic authors.

Arlow's description (1979) of how the analyst formulates an interpretation is a brilliant exception and is closely related to my idea of a contextual horizon:

> In general there are certain criteria that transform what would seem to be random associations or disconnected thoughts into supportable hypotheses that can be entertained with conviction and buttressed by fact. . . . Most important is the *context* in which the specific material appears. *Contiguity* usually suggests dynamic relevance. The *configuration* of the material, the form and sequence in which the associations appear. Other criteria are to be seen in the *repetition* and the *convergence* of certain themes within the organized body of associations. The repetition of similarities or opposites is always striking and suggestive. Material in context appearing in related sequence, multiple representations of the same theme, repetition in similarity, and a convergence of the data into one comprehensible hypothesis constitute the specific methodological approach in psychoanalysis used to validate insights

obtained in an immediate, intuitive fashion in the analytic interchange [p. 202; emphasis added].

An analogy to contextualization is the staining technique of the pathologist, whose chemical treatment of human tissue vividly exposes contrasting shades of red and blue patterns under the microscope, instead of the confusing shadows of colorless natural tissues. Depending on which stain is used, certain structures are more visible and others less so.[1]

The relation between technique and theory is famously confusing. As T. S. Eliot remarked about poetry, "we cannot say at what point 'technique' begins or where it ends."[2] When analysts enter into disputes about technique they often appeal to theoretical arguments at high levels of abstraction rather than going back to the original data to pose evidential challenges. But a number of clinical controversies appear very different when viewed in the perspective of careful descriptions of how inferences were inductively contextualized from the data instead of being deduced from theoretical abstractions or technical rules.[3] It has been said in this regard that it is easier to understand mankind than to understand a single man. Without the relevant contextualizing information, it is far easier to prolong polemics. A specimen of such a dispute will be examined below at greater length. I wish to be clear at the outset that my interest in this discussion concerns not the pros and cons of technique being debated, but the dispute itself as a model specimen through which to study the anatomy of polarized arguments in our field.

The paper I will use to illustrate these problems is "Some Pressures on the Analyst for Physical Contact during the Re-living of an Early Trauma," by Patrick J. Casement, originally published in 1982.[4] Its author subsequently published an expanded version of the paper in two books (Casement 1985, 1990; see also 1991, 2003) and a response to a number of discussants of this case presentation in its several versions (Casement 2000). At present count, over twenty-five authors have given

[1]Jorge Ahumada (1994b) drew my attention to this analogy first introduced by Cheshire and Thomä (1991, p. 429); see also Bachrach (2002, p. 46).

[2]Quoted by W. Pritchard from Eliot's *The Sacred Wood* in his review of *Collected Poems by Robert Lowell* (*New York Times Book Review,* June 29, 2003, p. 11).

[3]The interaction of deductive and inductive reasoning is far more complex than indicated by this simple statement, but the issue cannot be pursued here (see Hanly 2005). For another perspective on the preference for rules for technique, see Levine (2003).

[4]This case was originally reported at the 32nd International Psychoanalytical Congress, Helsinki, July 1981.

major attention to Casement's original report, and the publication of these discussions has become something of a cottage industry, rivaling other famous cases in our literature. Casement was initially praised for ultimately refusing physical contact with his patient (Fox 1984, 1988; Hoffer 1991; Roughton 1993; Meissner 1996, 1998; Katz 1998). In 2000 the case was the topic of an entire issue of *Psychoanalytic Inquiry* (vol. 20, no. 1), in which Casement was criticized for this same refusal by a number of authors (see, e.g., Breckenridge 2000; Shane, Shane, and Gales 2000). Whatever the diversity of views in these discussions, the one thing they all share is that they consider Casement's dilemma as predominantly a *technical* issue. The controversy, then, concerns the technical pros and cons of Casement's decision ultimately to not hold his patient's hand. The discussants are by no means in agreement that there should be a *rule* either way (see, e.g., Ruderman 2000), but they are essentially all in agreement in arguing on the basis of the clinical "needs" of the patient, without reference to how this particular patient developed this particular need at a specific time during her analysis.

Casement (1982) stated at the outset that he wished to consider an important *technical* controversy:

> Is physical contact with the patient, even of a token kind, always to be precluded without question under the classical rule of abstinence? Or are there some [extreme] occasions when this might be appropriate, even necessary. I shall present a clinical sequence during which the possibility of physical contact was approached as an open issue. There seemed to be a case for allowing a patient the possibility of holding my hand. The decision to reconsider this was arrived at from listening to the patient and from following closely *the available cues* from the countertransference. The clinical material *clearly* illustrates some of the issues involved in this decision [p. 279; emphasis added].

Note that the "availability" of cues is predicated on arbitrarily assumed and unexamined contextualizing criteria. Although I have my own opinions about this dispute, I wish to emphasize again that my purpose here is not to agree or disagree with Casement's views, or those of his discussants, on whether he should have held his patient's hand. I believe that he is a dedicated, honest analyst who was obviously very helpful to his challenging patient. I further believe that she would have been a very difficult patient for me or most other analysts to treat. Instead, I wish to note his remark that the decisions he made in this instance were based on his "listening to the patient and from following

closely the available cues from the countertransference." I will attempt to show that his inferential decisions were based on processes far more complex than listening. When we evaluate an interpretation, we simply cannot assume that "close" listening, either to the patient or to the countertransference (as advised by Casement and others), is a sufficient guarantee of optimal understanding. I am not referring to what has now become our familiar postmodern insistence on the *subjectivity* of the analyst. The notion of "close" listening begs the question, listening to *what?* Even the familiar distinction between listening *to* and listening *for* is inadequate here. This putative close listening assumes a generic analyst who will hear the same thing as any other analyst if only he or she would pay close enough attention. Of course, the irreducible subjectivists among us will hasten to say: that is what we have been trying to warn against. But I am not referring here to the subjectivity of the analyst, which supposedly disqualifies any analyst from being able to observe. I am referring instead to a neglected but widespread methodological problem regarding how the analyst listens and how we evaluate one another's clinical work. It is a problem that deserves to be distinguished from the familiar warnings about the subjectivity of the analyst.

What I have to say here is based on the epistemological assumption that there is an imperfectly knowable real world. This is the position known as critical realism. In this view the term *evidence* connotes the data used to support an inferential assumption; it is not a synonym for perfect and absolute truth. On this basis I assume that to speak of an observing analyst is not a contradiction in terms.[5] Since these issues cannot be pursued here, I will limit myself instead to an important and neglected problem in the evaluation of interpretations: the consequences of arbitrarily ignoring selected associations of the patient for reasons of any kind—epistemological difficulties, countertransference, irreducible subjectivity, theoretical deficiency. What I emphasize is that the reasons for omitting certain associations from a contextual horizon may be either deliberate or inadvertent. The familiar problem I wish to reconsider in this unfamiliar light is that even two analysts from the same theoretical school will often contextualize the same clinical material very differently, and will often debate the difference quite heatedly. What has been insufficiently appreciated is that we have

[5]The assertion on epistemological grounds that an analyst can never logically be viewed as an observer is a category error. The interested reader can find relevant discussions of this problem in Hanly (1990, 1992) and Ahumada (1994a,b).

no explicit agreement about the constituent elements of our contextual horizons, even among adherents of the same theoretical model.

By the term *contextual horizon* I mean a group of associations that are dynamically linked by the contextualizing criteria the analyst uses to capture the major dynamic urgency in a given session. The analyst employing Arlow's contextualizing criterion of repetition of a theme in the associations of the patient would arrive at a different contextual horizon than the analyst who at that moment privileged information about the relationship with the patient. We learned after 1492 that horizons can be illusory. An advantage to the term *contextual horizon* is its metaphoric insistence that what lies beyond the horizon is not *yet* visible.

It is often said that our ability to evaluate the usefulness (not a synonym for validity or truth) of an interpretation depends on the proper understanding of the associations of the patient immediately after the interpretation (Wisdom 1967). For instance, when a patient suddenly recalls a dream or a dynamically relevant memory after an interpretation, it is part of our oral tradition to assume that the interpretation is "correct." But it is not uncommonly the case that bad interpretations can be followed immediately by very important new disclosures or new memories, or even by the report of a hitherto unreported dream. An additional and less appreciated source of evidence for the evaluation of an interpretation is provided by the associations immediately *in advance of* the interpretation. These associations often provide a contextual and dynamic insight into how the analyst formulated the interpretation and how the patient perceived it. We feel greater assurance about the validity of an interpretation when there is dynamic congruence between the associations immediately before and those immediately after the interpretation.

CASEMENT'S CLINICAL MATERIAL

> The patient, whom I shall call Mrs B, is in her 30s. She had been in analysis about 2½ years. A son had been born during the second year of the analysis (and at this point was about 10 months old). When she was 11 months old Mrs B had been severely scalded, having pulled boiling water on to her while her mother was out of the room. She could have died from the burns. When she was 17 months old she had to be operated on to release growing skin from the dead scar tissue. The operation was done under a local anesthetic. During this the mother had fainted. It is relevant to the childhood history that the father was largely absent during the first five years. Soon after the summer holiday of the analyst Mrs B presented the following dream.

> She had been trying to feed a despairing child. The child was standing and was about 10 months old. It wasn't clear whether the child was a boy or a girl. Mrs B wondered about the age of the child. Her son was soon to be 10 months old. He was now able to stand. She too would have been standing at 10 months. (That would have been before the accident.) Why was the child in her dream so despairing, she asked. Her son is a lively child and she assumed that she too had been a normal happy child until the accident. This prompted me to recall how Mrs B had clung to an idealized view of her pre-accident childhood. [That would be prior to age eleven months.] I thought she was now daring to question this. I therefore commented that maybe she was beginning to wonder about the time before the accident. Perhaps not everything had been quite so happy as she had always needed to assume. *She immediately held up her hand to signal me to stop* [emphasis added].
>
> During the following silence I wondered why there was this present anxiety. Was it the patient's need still not to look at anything from before the accident unless it was seen as perfect? Was the accident itself being used as a screen memory? I thought this probable [Casement 1982, p. 279].

Note carefully what happened next, when in spite of the patient's urgent warning that Casement should stop, he persisted in repeating his interpretation.

> After a while I said that she seemed to be afraid of finding any element of bad experience during the time before the accident, [i.e., before age 11 months], as if she still felt that the good that had been there before must be kept entirely separate from the bad that had followed. She listened in silence, making no perceptible response during the rest of the session.
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> The next day Mrs B came to her session with a look of terror on her face. For this session, and the five sessions following, she could not lie on the couch. She explained that when I had gone on talking, after she had signaled me to stop, the couch had 'become' the operating table with me as the surgeon, who had gone on operating regardless, after her mother had fainted. She now couldn't lie down 'because the experience will go on'. Nothing could stop it then, she felt sure.
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> In one of these sitting-up sessions [i.e., immediately after the crisis session] Mrs B showed me a photograph of her holiday house, built into the side of a mountain with high retaining walls. She stressed how essential these walls are to hold the house from falling. She was afraid of falling forever. She felt this had happened to her after her mother had fainted. (Here I should mention that Mrs B had previously recalled thinking that her mother had died, when she had fallen out of her sight during the operation, and how she had felt that she was left alone with

no one to protect her from the surgeon who seemed to be about to kill her with his knife.) Now, in this session, Mrs B told me a detail of that experience which she had never mentioned before. At the start of the operation her mother had been holding her hands in hers, and Mrs B remembered her terror upon finding her mother's hands slipping out of hers as she fainted and disappeared. She now thought she had been trying to re-find her mother's hands ever since, and she began to stress the importance of physical contact for her. She said she couldn't lie down on the couch again unless she knew she could, if necessary, hold my hand in order to get through the re-living of the operation experience. Would I allow this or would I refuse? If I refused she wasn't sure that she could continue with her analysis.

My initial response was to acknowledge to her that she needed me to be 'in touch' with the intensity of her anxiety. However, she insisted that she had to know whether or not I would actually allow her to hold her hand. I felt under increased pressure due to this being near the end of a Friday session, and I was beginning to fear that the patient might indeed leave the analysis. My next comment was defensively equivocal. I said that some analysts would not contemplate allowing this, but I realized that she might need to have the possibility of holding my hand if it seemed to be the only way for her to get through this experience. She showed some relief upon my saying this.

On the Sunday I received a hand-delivered letter in which the patient said she had had another dream of the despairing child, but this time there were signs of hope. The child was crawling towards a motionless figure with the excited expectation of reaching this figure. On the Monday, although she was somewhat reassured by her dream, Mrs B remained sitting on the couch. She saw the central figure as me representing her missing mother. She also stressed that she hadn't wanted me to have to wait to know about the dream. I interpreted her fear that I might not have been able to wait to be reassured, and she agreed. She had been afraid that I might have collapsed over the week-end, under the weight of the Friday session, if I had been left until Monday without knowing that she was beginning to feel more hopeful.

As this session continued, what emerged was a clear impression that Mrs B was seeing the possibility of holding my hand as a 'shortcut' to feeling safer. She wanted me to be the motionless figure, controlled by her and not allowed to move, towards whom she could crawl with the excited expectation that she would eventually be allowed to touch me. Mrs B then reported an image, which was a continuation in the session of the written dream. She saw the dream-child reaching the central figure, but as she touched this it had crumbled and collapsed. With this cue as my lead I told her that I had thought very carefully about this, and I had come to the conclusion that this tentative offer of my hand might have appeared to provide a way of getting through the

> experience she was so terrified of, but I now realized that it would instead become a side-stepping of that experience as it had been rather than a living through it. I knew that if I seemed to be inviting an avoidance of this central aspect of the original experience I would be failing her as her analyst. I therefore did not think that I should leave the possibility of holding my hand still open to her. Mrs B looked stunned. She asked me if I realized what I had just done. I had taken my hand away from her just as her mother had, and she immediately assumed that this must be because I too couldn't bear to remain in touch with what she was going through. Nothing I said could alter her assumption that I was afraid to let her touch me. The following day the patient's response to what I had said was devastating. Still sitting on the couch she told me that her left arm (the one nearest to me) was 'steaming'. I had burned her. She couldn't accept any interpretation from me. Only a real physical response from me could do anything about it. She wanted to stop her analysis to get away from what was happening to her in her sessions. She could never trust me again [pp. 279–281].[6]

During the next two weeks a harrowing and stormy period of battle continued during which the patient threatened suicide and Casement sought consultation from a respected senior colleague. Despite the consultation, he continued to suffer painful doubts about what course to pursue with his patient regarding holding her hand.

> I reflected upon my dilemma. If I did not give in to her demands I might lose the patient, or she might really go psychotic and need to be hospitalized. If I did give in to her I would be colluding with her delusional perception of me, and the avoided elements of the trauma could become encapsulated as too terrible ever to confront. I felt placed in an impossible position. However, once I came to recognize the projective identification process operating here I began to surface from this feeling of complete helplessness. This enabled me eventually to interpret from my countertransference feelings. We could now see that if I had agreed to hold her physically it would have been a way of shutting off what she was experiencing, not only for her but also for me, as if I really couldn't bear to remain with her through this. She immediately recognized the implications of what I was saying and replied, 'Yes. You would have become a collapsed analyst. I could not realize it at the time but I can now see that you would then have become

[6]Holder (2000) viewed the withdrawal of the analyst's offer of physical contact to be the more important issue. Mitchell (1991) felt that both the offer and its withdrawal were helpful. Schlessinger and Appelbaum (2000) agree with Mitchell but caution against rigid rules. B. Pizer (2000) rejected Casement's decisions because they were rooted in an outmoded one-person authoritarian model.

> the same as my mother who fainted. I am so glad you didn't let that happen [p. 282].[7]

Thanks to Casement's meticulous honesty, we are allowed to know that the ostensible crisis of technique in which his patient seemed suddenly to demand that he hold her hand as a reenactment of her childhood trauma can be viewed alternatively as a consequence of his insisting on repeating his confrontations about her having idealized her infancy, even after she implored him to stop doing that.

It is well known, however, that no matter how conscientious the analyst, important information is inadvertently omitted from case reports (see Spence 1982). To this seeming inevitability may be added the propensity of commentators to ignore even material that is included. In this instance, two notable examples are evident. The first is this. In 1990, eight years after his original 1982 paper, Casement participated in a panel discussion about the work of Winnicott (reported in Blum and Ross 1993), at which time he added a vitally important piece of information to the history of this patient:

> Mrs B was the youngest in her family. . . . She had suffered severe burns from scalding water at eleven months, shortly after beginning to walk. *Barrier-nursed after this, she could not be held by her mother.*[8] At seventeen months, she had had surgery under local anesthesia to release her scar tissue. It was a dream, with a memory of this trauma that brought Mrs B to analysis 30 years later. In the dream her mother had "fainted out of sight" after holding her hands, and she herself was about to be killed by a man with a knife. She "couldn't face it alone" anymore, she said, and was afraid of committing suicide once her own children left home [Casement, quoted in Blum and Ross 1993, p. 225; emphasis added].

This tragic and total physical separation and isolation of the infant patient from any human contact evidently continued for a lengthy period of time. It would certainly have been traumatic to an even greater degree than the scalding accident and the surgery. It is noteworthy that neither Casement nor his numerous discussants after 1993, when this information was published, noted its relevance to their heated debates about

[7]Aron (1992) agreed that this was the sucessful turning point in her analysis.

[8]". . . barrier nursing her, that is, not holding her or touching her except with sterilized gloves and then only for the most minimal and essential feeding and cleaning of the child. Whatever she did, the mother must not pick up her baby—however much the baby cried to be picked up—for if the mother did pick up her baby, it might lead to her dying from infection." (Casement 2000, p. 178).

Mrs. B's panicky insistence on physical contact. Since the 1993 panel report, eighteen additional commentaries[9] have been published on Casement's patient and his views on technique. Only two commentators, DeMattos (2003) and S. Pizer (2004), even mention the barrier nursing; the former does so only in passing, while the latter discusses it in a very different frame of reference.

But my main purpose here is to illustrate the consequences of a second omission—a failure all around to consider Casement's insistence on repeating his interpretation in spite of the patient's vigorous protests as contextually relevant to the crisis that immediately ensued. We must keep in mind that Casement and all his discussants have asked us to join them in the assumption that the issue of technique, of whether or not to hold this patient's hand, could be usefully decided on an abstract level separate from the interaction between the analyst and his patient immediately before the crisis.

It might be argued that, after all, the authors in the spedcial issue of *Psychoanalytic Inquiry* on his topic were invited specifically to address the issue of technique regarding the pros and cons of physical contact with the analyst. But it is precisely the assumption that this isolation of technique is logical and the willingness to consider this question as an abstraction disconnected from the context of the origin of this clinical crisis that I call into question.

We might ask which issues of technique, if any, should be discussed categorically, in terms of rules, rather than individually, in terms of dynamic context. Some issues can be. Certainly gross boundary violations are always wrong. But this illustrates the tension between the specific and the general, as well as the contrast between inductive and deductive reasoning, which is outside the scope of this discussion. But we can at least say that we should be wary of the substitution of rules for understanding when deciding about the incredible complexities of a clinical impasse. An alternative view of these events is to consider this interaction between analyst and patient as an unrecognized enactment of a sadomasochistic transference-countertransference struggle centering on the issue of control of the analysis.

[9]Bass (2004); Beebe, Lachmann, and Jaffe (1977); Boyer (1997); Breckenridge (2000); Cooper and Levit (1998); DeMattos (2003); Feiner (1998); Fosshage (2000); Holder (2000); Katz (1998); Levine (2003); McLaughlin (2000); Meissner (1998); B. Pizer (2000); S. Pizer (2000); Ruderman (2000); Schlessinger and Appelbaum (2000); Shanc, Shane, and Gales (2000).

The point is not that I wish to "prove" that such an alternative view is the "truth." Rather, I wish to show that this view assumes a very different contextual horizon. Viewed in this different context, the question whether to hold her hand takes on a very different significance and leads to alternative strategies. It seems to me it would have been possible to address these matters in a number of ways, and that the salient question was not what the analyst should have physically done, but why and how the dilemma arose. Experience teaches us that it is difficult to reason one's way out of a dilemma created unreasonably. Many authors have applauded Casement's integrity; perhaps equally many have criticized his insensitivity. I am suggesting that we view his remarkable insistence on repeating his views over Mrs. B's explicit and dramatic objections as a "day residue" for the impasse that was then enacted. I believe that this suggestion, that the events before and after the hand-holding crisis are an enactment, accounts better for more of the available facts, but that, of course, is my own bias. What is very clear, however, is that my view arises from contextualizing criteria different from those assumed by most of the published discussants.[10] At issue here is not the *truth* of this alternative contextualization, but its *plausibility.* To be sure, once the enactment rose to the crisis level that it did, it seems perfectly plausible to me that what Casement did was quite helpful to her. But to focus on technique isolated from context can sometimes be a sign of a strained moment in an insufficiently understood transference-countertransference impasse.

The polarized discussions of Casement's patient illustrate another fallacy in our literature. Can we assume a generic analyst who might really have altered his perceptions and behavior with this patient by following a simple technical rule? Technique in the background is very different from technique as a foreground issue. To put it another way, technique is sometimes what an analyst does when he or she doesn't understand what is going on and has become embroiled in an enactment with the patient in which transference fantasies threaten to degenerate into reality (Tarachow 1963).

We have here a paradigmatic and disquieting example of some two dozen analysts divided into two opposing camps, ostensibly disagreeing about technique in the abstract but in fact sharing a tacit

[10]Although McLaughlin (2000) and S. Pizer (2004) note Casement's insistence on his interpretation in the face of the patient's vigorous hand signal for him to stop, they draw conclusions different from mine.

assumption that relevant data from a specific analysis may be omitted in our evaluation of generalized technical strategies. Unlike the Oxford don who had strong opinions lightly held, we analysts seem often to have light opinions strongly held. Heated controversies and polemics all too frequently characterize our disagreements. In the specimen case I have chosen, these opposing camps assume they are discussing the same case, when in fact they are discussing how to use selected aspects of selectively reported material to support their own theoretical preferences.

This blithe disinterest in the events immediately preceding the crisis with this patient is to be found in almost all the published discussions of this case, even though the discussants represent a wide variety of theoretical orientations. They seem paradoxically in agreement in their disregard of important clinical evidence while disputing theoretical claims at remote levels of abstraction (see Waelder 1962).

EIGHT CONTEXTUAL ORGANIZERS

But there are additional complexities to consider in the discussion of Casement's paper. At least eight other evidential issues that deserve to be viewed as contextual organizers are neglected to varying degree by most of the discussants. If they are even mentioned, these topics are not viewed as contextualizing organizers for interpretations very different from those of Casement. I propose that we define a contextual organizer as a bridging tool. It is a dynamic theme that links theory, context, and technique. It defines the boundaries of a contextual horizon. As I have suggested, at least eight of these can be identified in Casement's material.

1. *The analyst had just returned from his summer vacation*. This entire crisis developed immediately after he responded to her dream about a despairing child. The analyst ignores the transferential implications of his recent absence and feels no need to ask for associations to the dream. Instead her dream report prompted him to repeat his earlier interpretations about her infancy. Analysts of a different persuasion might regard this intervention as a defensive avoidance of the transference by a retreat into the childhood history. So an alternative path of interpretation opens up with this different contextualizing criterion. Also, we are told in one version of the case report (Casement 1982) that at about this time and shortly before the crisis the analyst had quickly agreed to Mrs. B's proposal to reduce the frequency of her sessions from

four to three times weekly. We do not know from the available data whether the analyst felt that his vacation and the patient's wish to cut back were related, or if this was discussed.[11]

2. *Analyst and patient have obviously established a prior agreement that they can rationally and logically discuss the accuracy of her memory of her feelings in the first year of life.* Here one might question the transference implications for the patient of such extravagant claims for certainty by her analyst about matters that fly in the face of his claims. In fact, not long ago Casement's paper was cited as evidence for the registration of infantile memories:

> numerous case histories of child and adult treatments *document* the continuing effects of events in the first year. . . . For example, Casement describes an adult treatment case in which the patient was preoccupied by having been severely burned at 11 months. This traumatic event was a major organizing theme in the adult analysis. These sources of *evidence* provide a basis from which to infer that organization, or structure, accrues across the first year [Beebe, Lachmann, and Jaffe 1997, p. 150; emphasis added].[12]

Experienced analysts are familiar with uncanny examples of stunning reenactments of infantile traumatic experiences. But the issue here is that Casement's discussants did not entertain even the possibility that these protracted discussions of infantile memories might be false.

3. *The patient's father was for the most part not present in her life until she was five years old.* (Casement, personal communication).The consequences of a one-parent oedipal phase of development have been usefully discussed long ago by Neubauer (1960). Wouldn't her father's

[11]McLaughlin (2000) is the only one to note this important fact. He is also one of the very few who even notes the repetitious insistence of the analyst that the patient give up her idealized view of her infancy just before the crisis occurred. I have previously commented on several of these discussions about Casement's patient in an abbreviated form (Boesky 1998). McLaughlin is also the only contributor to *Psychoanalytic Inquiry*'s special issue who noted the important differences in the details provided in Casement's reports on the case. McLaughlin pointed out that the crisis occurred after Casement's quick acceptance of the patient's request to cut back on sessions. He also noted Casement's insistence on his own theoretical views over the patient's urgent pleas that he stop. Nonetheless, McLaughlin does not relate these matters to the disagreements about technique between himself and some of the other contributors. He prefers to discuss these issues in terms of criticizing too great a reliance on arbitrary technical rules.

[12]For a similar citation of Casement as an authority for the reliance on infantile memory, see Boyer (1997), Cooper and Levit (1998), and Feiner (1998). For a contrasting view of infantile memories see Tuckett and P. Tyson, quoted in Blum and Ross 1993, p. 229).

absence and return have profoundly shaped her attachments to her mother and her sexual development as a child and her perceptions of men when she became an adult? Would it not have contributed to her sadomasochistic absorption in issues of controlling and being controlled? The patient's father is as absent in all of this literature as he was in her life for her first five years. We simply lack any information whatever about her relationship with him after the age of five, and the discussants do not seem to consider this a problem. If this information about her father was omitted for purposes of protecting her privacy, one wonders why he was mentioned at all.

4. *The patient had become a mother just ten months before she had the dream that ushered in her crisis*. This baby was now just about the same age as the patient at the time she had suffered her terrible scalding accident. What is it that revived her identification with a despairing child at just this time in the analysis?[13]

5. *The patient very clearly stated in her report of her dream that the gender of the baby was ambiguous*. She reported this dream to the analyst immediately before he began to insist on his interpretation and immediately before the crisis ensued. What she had told him was the following: "She had been trying to feed a despairing child. The child was standing and was about 10 months old. *It wasn't clear whether the child was a boy or a girl*" (emphasis added).

We have no consensus among analysts about the extent to which we would wish to have associations to important elements in the manifest dream before venturing an interpretation to the patient, but this question does not arise in the Casement literature. Just as the enactment of his repeating his interpretation over her vigorous objection could be ignored, it was also generally accepted by all of the discussants that this manifest element of the dream did not require contextual articulation with the ensuing crisis. Note also that the dream in the note that was hand-delivered to the home of the analyst on a Sunday was a continuation of this dream about the despairing baby of ambiguous gender. This omission may reflect the fact that an increasing number of analysts today do not feel the need to have information about most of the important manifest elements in a dream. My experience has been that it is useful to at least be curious about anything the patient has deemed important enough to communicate—which is to say that not all of the associations

[13]Fosshage (2000) raises this question but views the crisis quite differently on grounds of relational epistemology.

can be contextualized, but all of them are laden with potential meaning if we only could discover it. We should be able to say not only what we have left out, as in the unpacking prescribed by Spence (1982), but also *why* we have left it out. The "why" reflects the consequences of contextualizing decisions by the analyst.

6. *It was to be eleven years after his original 1982 paper that Casement published the vitally important information that the patient was barrier-nursed after the scalding for an unknown length of time.* In a personal communication, Casement has informed me that to protect his patient's privacy he could not offer further explanatory details about this barrier nursing and why it was necessary, beyond what he published in 1993. But for my purposes the relevant issue is that Casement did provide at least this much information about the barrier nursing in his 1990 panel presentation and that only two of the papers published about his patient *after* this panel was reported in 1993 so much as mention this obviously crucial fact. It is puzzling, then, that without exception none of these later discussants has commented about the traumatic barrier nursing as a factor in this technical debate. Had there been no scalding accident at all, we would expect that the rigidly enforced requirement that a human infant be deprived of any holding, hugging, or nurturing physical contact with her mother would be a devastating psychic trauma.

7. Earlier I noted that in his most recent discussion of his paper, eighteen years after its original appearance, Casement (2000) reported another critically important piece of information:

> it had been a feature throughout those first years of this analysis that Mrs B would frequently seek to control me, a control that I usually allowed her. Latterly, however, we had been negotiating the beginning of some separation from that near total control of me as analyst. *Occasionally, as when I did not accept her signal for me to stop*, I had stood my ground in the course of some interpretive work. Gradually, through such moments as these, Mrs B had begun to allow me a more separate existence and a mind of my own, that was not held totally within her control. I believe that we had to find a way for this gradual differentiation between herself and me, as not merged and as not totally controlled by her, in preparation for what was to come later [p. 171; emphasis added].

So when he insisted on repeating his interpretation to the patient about her infancy before the accident not being as happy as she insisted,

he was consciously attempting to reduce his prior willingness to allow her to control his behavior with her in the analysis. He seems to acknowledge here for the first time that his insistence on confronting her with the possible error of her idealized memory of her infancy, even after she held up her hand and told him to stop, was a factor in the ensuing crisis about holding her hand. That is left implicit. But even more important is the fact that throughout this entire period of crisis, while patient and analyst agonized over whether or not he should hold her hand, he never once told the patient that his insistence on the correctness of his interpretation was a *change* in his behavior at all, nor did he explain to her why he had decided just now to change his behavior with her. Of course, it is just such an omission of the role of the observing analyst in evoking the very behavior that is ostensibly being only observed (an exclusively one-person model) that is often correctly faulted by relational theorists, as well as by advocates of enactment theory in the conflict model. And only with the benefit of this last piece of information is it possible to interpret this power struggle as an enactment by patient and analyst of an unconscious sadomasochistic fantasy.

Three discussions of Casement's patient have appeared since he published this last new piece of information in 2000, and once again the new information seems to have been disregarded (DeMatos 2003; Levine 2003; Bass 2004). Only DeMattos notes it as an issue, but she comments no further on its implications. In fairness to Casement's scrupulous honesty, he would gladly have provided this new information about his important change in technique had he thought it relevant. *But that assumption of irrelevance is exactly the problem for all of us*. Indeed, the ambiguities concerning such assumed "irrelevance" is one of the central causes of the famously vexing dissociation between theory and practice in psychoanalysis (Smith 2003). In this sense the experience of comparing the divergent discussions of Casement's work is reminiscent of the film *Rashomon*. For example, one of Casement's discussants correctly stated with approval and agreement that Casement "decries the analyst who imposes his ideas upon a patient" but saw no contradiction between that theoretical position and Casement's actual behavior with his patient: imposing his ideas is exactly what Casement did when he ignored his patient's signal to stop repeating his interpretation (Symington 1992, p. 168).

The single most sharply critical discussant of Casement's work is Albert Mason (1987), a Bionian-Kleinian who had the following to say:

> While I feel a certain sympathy with Casement and what he is saying in his book, and with the hard work and sincerity which are clearly evident there, I find his depiction of many of the problems oversimplified and somewhat naïve. It seems to me that he seeks to contain therapists' anxieties in a reassuring way rather than helping them to become therapists with a full understanding of the psychodynamics underlying the problems involved. . . . This is a book I would recommend to students, residents, and perhaps to *beginning analytic candidates*, but I think most practicing analysts would find it rather elementary [pp. 714–715; emphasis added].[14]

Our confusion about how to view psychoanalysis as a science is illustrated by the enthusiastic endorsement for Casement's work by one of our most prominent advocates of psychoanalysis as a science. Peter Fonagy (1985) described Casement's work as imaginative and strongly commended Casement for taking "enormous care to illustrate clinically all the ideas he has mentioned just as in a good text of mathematics the author would carefully derive all formulae from first principles" (p. 507). Fonagy also commends Casement for being nondirective, which is puzzling when we recall Casement's insistence on trying to make Mrs. B admit he was right by repeating his interpretation. In fact Fonagy states that Casement "gives students a head start in untangling the confusion of terms surrounding unconscious communication" (p. 507).

8. Casement provides no information about the sexual conflicts of his patient, an omission that continues from his initial report through successive discussions up to his recent personal communication to me (2004). This is a telling omission which by and large his discussants do not question. This vignette is open to alternative conjectures that would include attention to the patient's unconscious sexual conflicts.

TERMINOLOGICAL SUMMARY

I have introduced and used a group of loosely related and overlapping terms here that I will attempt to summarize in ascending levels of abstraction.

Contextual horizon: a group of associations that are dynamically linked by the contextualizing criteria used by the analyst to capture the

[14]Bass (1993) also felt that Casement simplified matters too much but in a later review (2004) was generally favorable. His major criticism was Casement's failure to acknowledge commonalities between his views and those of the American relationalists.

major dynamic urgency in a given session. I have introduced this term to facilitate our comparing our inferential processes with each other more coherently. A contextual horizon is the least abstract of the terms I have used and refers often to material in a single session, although the notion I have in mind may also refer to contextual continuities over varying lengths of time. It is important that there are alternative horizons available to any one analyst or to any group of analysts who are reviewing the clinical material of another analyst. The idea of trial contextualization that I used above refers to the important fact that while in any one session a number of contextual horizons will be plausible, the analyst will often believe that one of these is optimal and best captures the affective urgencies of that session. The question addressed in this paper is "optimal" for whom and what exactly makes it optimal.

Contextual organizer: an element in the patient's associations that the analyst deems to have inferential priority for widely diverse reasons on many different levels of abstraction. Examples are the first association of the hour, or the fact that the hour under discussion occurs immediately before or after the analyst's vacation. This information is a contextual organizer insofar as the analyst links what follows in the session with this organizing idea in mind as he or she devises trial contextualizations. The contextual organizer is a dynamic theme as well as a provisional hypothesis. The analyst consciously or preconsciously privileges this theme as he or she listens and forms trial contextualizations. I use the term *contextual organizer* as a less abstract, closer to the data indicator than is the next term, contextualizing criteria. The list of eight contextual organizers I have proposed for the discussion of the Casement paper involve very different levels of abstraction, and this diversity of levels is a hallmark of the term.

Contextualizing criteria: This term is more abstract than contextual horizon or contextual organizer. Contextualizing criteria are the vast panoply of theoretical beliefs the analyst uses to prioritize the dynamic importance of the specific associations of the actual moment in the treatment process. The analyst uses his abstract contextualizing criteria to select contextual organizers in order to accomplish the filtering so essential to psychoanalytic inferential processes. To make such an inference is a form of psychoanalytic triage: in the heat of the fray, what do we emphasize and select, and what do we deemphasize and neglect? These criteria are far more vague and abstract, and it is our willingness not to require information about the contextual organizers and horizons

that perpetuates this vagueness. It is also precisely this confusion about contextualizing criteria that perpetuates our famous disconnect between theory and technique.

Rules of evidence: To speak of such "rules" is a utopian yearning at this time in our history. I use the phrase to indicate the hope that our scientific methodology for evaluating clinical evidence will always be evolving. I agree with those who feel that there are many definitions of "science" (Grossman 1995), and I use the word to argue against those who believe that clinical evidence is not relevant to psychoanalysis because of the daunting complexity of the analytic process or because the analyst should be epistemologically disqualified as an observer. "Rules of evidence" at this time appears to be a wildly exaggerated phrase, but we psychoanalysts are able to make reliable predictions in certain instances. For example, we can predict that an adult obsessional male patient with distressing obsessive-compulsive symptoms will reveal in the course of psychoanalytic treatment that he struggles with the derivatives of unconscious passive anal sexual conflicts and fantasies (Jacob Arlow, personal communication). This empirical finding has been repeated hundreds of times by successive generations of psychoanalysts. We can repeatedly demonstrate that the sequential patterning of the associations of the patient is meaningful rather than random. We can also often plausibly support and refute individual interpretations (Wisdom 1967; Hanly 1992).

These proposed terms to describe our inferential and contextualizing processes do not fit together easily or clearly, but perhaps they will serve as the basis for wider discussion and increasing clarification of the methodology of our inferential proecesses.

CONCLUSION

A neglected aspect of the problem of evaluating clinical evidence is that we have never defined the optimal relation between the kinds of data we have available and the hypotheses we wish to test. Consider three kinds of question. Was an interpretation the best available explanation for the data available in a given session? Is self-disclosure useful or not? Was an analysis successful? Roughly speaking, we are referring to the differences between process, abstract principles of technique, and outcome research. We badly need to clarify which kind of evidence is most suitable for the diverse levels of abstraction that characterize

psychoanalytic theories. Testing the bricks of a house is very different from inspecting the completed home. It is often said that a single session tells us little about the vicissitudes of the progression of the treatment. That is true, but much can be learned from single sessions about how the analyst has conducted the treatment; indeed, we cannot place much confidence in outcome studies that do not include the data from at least a few sessions to illustrate how the analyst has inferred meanings from the patient's associations.

Who is "correct" in this controversy about Casement and technique is not my point here. What is important is to ask why all of the participants felt they knew enough to agree or disagree with Casement's views. Our debates can lead only to a sterile polarization of views if we do not compare the contextual horizons of the two sides. I am not saying that the choice of different contextual horizons was the "cause" of this disagreement about technique; I am saying that lack of clarity about which contextual horizons were utilized allows and facilitates our confusion. Only when we make the contextual organization explicit does it become possible to see what we are disagreeing about at a level closer to the original data. That is no small achievement. We can then agree or disagree more coherently and to greater effect.

This confusion about evidence highlights the consequences of our increasing tendency to neglect the associations of the patient; indeed, the erosion of interest in their latent meanings parallels the widespread derogation of clinical evidence. It is probably no coincidence that this downplaying of the patient's associations is linked also to the waning interest among many analysts in the sexual and aggressive conflicts of our patients.

The discussions cited here were geared to an abstract question of technique: Is it good or bad to have physical contact with the patient? But how we can discuss such a question in absolute terms, as either proscribed for all patients or indicated for all patients? The examples cited here support the view that our evaluation of evidence would be enhanced if we could clarify and identify more precisely what the reporting analyst has taken as evidence. If we were more clear about how specific clinical evidence has been contextualized, the deeper complexities of our disagreements would also become more clear. Our literature now commonly calls for more details about the personal feelings of the analyst during a clinical interaction, but it is rare to hear a request that the analyst say why certain of the patient's associations are

included in a given contextualization and why others are omitted. Nor would tape recordings solve this problem. But these ambiguities involving contextualizing criteria have been obscured by debates about the more superficial problems of presenting case reports.

In consequence, the claims of any number of outcome studies are an inverted pyramid of descriptive changes resting on the highly questionable point of poorly understood underlying data. Today psychoanalytic communities are so far from consensus on how to adjudicate questions like the one raised by Casement that the very possibility of meaningful communication between groups becomes doubtful. But this grim prospect, I suggest, is due less to the theoretical differences between groups than to the methodological *agreement* among so many of us to shrug our shoulders when it comes to evaluating clinical evidence. Pluralism of perspectives and theories is not only healthy—it is vital to the future of psychoanalysis. But the smug consolation of relativistic smoothing (we all get good results, don't we?) of the differences in our theoretical models is long overdue for scrutiny. Refining the role of contextualization in our evaluation of clinical evidence will enhance our efforts toward a more rational comparative psychoanalysis. Every contextualization is also a condensation of the associations of the patient. If we truly wish to reap the benefits of a pluralistic psychoanalysis, we are well advised to refine our understanding of what information about the patient has been used to support the conclusions reported. More important, that will help us clarify what associations of the patient have been left out. It may even help us reverse the steady erosion of interest in our literature about the role of the patient's associations in psychoanalytic treatment.

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