Commentary: Psychodynamics and Social Cognition—Notes on the Fusion of Psychoanalysis and Psychology

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ABSTRACT   Interest in linking psychoanalysis with scientific psychology waxes and wanes. In part, the difficulties have been caused by the preference of psychoanalysts for Freud's clinical theory (and its emphasis on narrative truth) as opposed to his metapsychology (with its requirement for historical truth). Even though contemporary scientific psychology evolved largely independently of psychoanalysis, the articles on object relations, transference, and defense published in this special issue show that the theory remains a source of inspiration, observations, and hypotheses.

Every once in a while someone tries to connect psychoanalysis to academic psychology. The first person to attempt this was, of course, Freud himself. In his *Project for a Scientific Psychology* (1895/1966a), Freud tried to produce what he described to Fliess (Letter 23, April 27, 1895; see Masson, 1985) as a "psychology for neurologists" (see also Pribram & Gill, 1976; Sulloway, 1979). Somewhat later, of course, there was Dollard and Miller's (1950) *Personality and Psychotherapy*, which translated the psychoanalytic theory of neurosis into the language of Hullian learning theory. More recently, both Erdelyi (1985) and Horowitz (1988) have attempted to interpret psychoanalytic prin-

ciples from the framework(s) of information-processing theory and other viewpoints within cognitive psychology and cognitive science.

Whatever the era, the attempt to link psychoanalysis with scientific psychology is always interesting, if for no other reason than that there are so many different forms of psychoanalysis and thus so many ways to make connections. Rapaport (1959, reprinted 1960) noted that there are at least five different levels of psychoanalytic theory: (*a*) the neuroscientific theory of *Project for a Scientific Psychology* (Freud, 1895/ 1966a); (*b*) the intrapsychic dynamics of *The Interpretation of Dreams* (Freud, 1900/1953a); (*c*) the ego psychology of *The Ego and the Id* (Freud, 1923/1961), especially as elaborated by Hartmann and by Rapaport himself; (*d*) the structural concepts detailed in *Inhibitions, Symptoms, and Anxiety* (Freud, 1926/1959); and (*e*) the psychosocial views of Horney, Kardiner, Sullivan, Erikson, and Hartmann. To Rapaport's list there is now at least one new level: (*f*) the psychoanalytic object relations theories that began with Fairbairn, Melanie Klein, and Winnicott, and continued with Kernberg, Kohut, and others (for a review, see Greenberg & Mitchell, 1983).

Just how rewarding these connections can be, at least in principle, is illustrated by selections from the early volumes of *Psychological Issues*—the house organ of ego psychology. At a time when academic psychology was still dominated by functional behaviorism, sensory psychophysics, and S-R theories of animal and verbal learning, this journal was publishing investigations of life-span development, memory for connected discourse, attributions of causality, individual differences in cognitive style, cognitive development in infants and children, preconscious processing, and mental imagery. It's enough to make a cognitive psychologist cry. Much like the monks of the Middle Ages, the ego psychologists held fast against the behaviorist onslaught, preserving what was most interesting in psychology until psychologists were ready to study the mind again.

Unfortunately, most of this work was ignored by mainstream academic psychologists. Part of the problem was that the concepts of psychoanalysis were mentalistic. Even if one could tolerate the mentalism, though, the theory just seemed to resist any kind of empirical test (Grünbaum, 1984). But part of the problem came from the other side, as well: Too many psychoanalysts seemed to feel that the theory, if not entirely self-evident, was shown to be true by the success of psychoanalytic treatment. When two parties treat each other with contempt, it is hard to find common ground.

Whatever Happened to Metapsychology?

Then, beginning in the 1970s, just when academic psychology began to get interested in the mind again, organized psychoanalysis seemed to give up any pretense of being scientific at all. The milestone here was George Klein's (1969, 1973) distinction between two versions of psychoanalytic theory, the metapsychology and the clinical theory. In Klein's view, Freudian metapsychology was reductionistic, too heavily loaded with quasi-physical concepts to be a pure psychology and too grounded in mechanistic conceptions of causality that seemed embarrassingly outmoded. Instead, Klein argued that psychoanalysis should focus on the clinical theory, with its repertoire of technical procedures such as free association and transference, its theories of infantile sexuality and the Oedipus complex, repression and other mechanisms of defense, and its interest in meaning and interpretation. In short order, psychoanalysis was transformed from a scientific discipline, tied (at least in principle) to the natural and social sciences, to a hermeneutic discipline, tied to the arts and humanities. The result was a shift in emphasis from historical truth to narrative truth (Spence, 1982, 1994): The goal of psychoanalysis was no longer to find out what caused a patient's problems in a manner consistent with scientific knowledge about how the mind works, but rather to make up a good story about the origins of those problems. It is therefore not an accident that contemporary psychoanalysis finds a warmer welcome in departments of comparative literature than in departments of psychology.

I hasten to note that the burden of this shift should not be placed entirely on Klein's shoulders, because recent historical studies have made it clear that Freud himself had a strong preference for narrative over historical truth (Macmillan, 1991; see also Crews, 1993, 1994). Two examples (both from Macmillan, 1991) will suffice. (a) Although Freud (1933/1964, p. 120), recounting the original infantile seduction theory of neurosis, wrote that "almost all of my women patients told me that they had been seduced by their father," examination of the actual cases referred to by Freud (1896/1966b, 1896/1966c, 1896/1966d) reveals not a single instance in which this was the case (for documentation see Schimek, 1987, whose conclusions are different from mine). In most cases, the seducer was either another child or an adult unrelated to the child; in any event, there is no evidence that any patient ever *told* Freud she had been seduced by *her father*. This is nothing more than an inference on Freud's part—an interpretation that was critical when it came time to abandon the infantile seduction theory for the

theory of infantile sexuality. (*b*) In his initial presentation of the theory of infantile sexuality, Freud (1905/1953b, p. 179ff) cited as evidence a study of "pleasure sucking" (e.g., sucking the thumb rather than breast or bottle) carried out by Lindner (1879–1880/1980) which, Freud asserted, showed that pleasure sucking was common among infants, and was in turn commonly associated with such behaviors as rubbing of the breasts and genitals, masturbation, orgasmlike motor behaviors, and post-sucking sleep. In fact, Lindner found that pleasure sucking was rare, and the behaviors noted by Freud even rarer still (Macmillan, 1980).

I want to argue that Klein was mistaken, and so were those, like Gill (1976), Holt (1976), and Schafer (1976), who followed his lead. The reason for this is that the natural science framework ostensibly embraced by Freud (if only as a public relations gesture) is not necessarily reductionistic. One can do perfectly respectable science at the psychological and sociocultural levels of analysis without incurring any obligation to reduce psychological and sociocultural concepts to biological and physical ones. While Klein was quite right to reject Freud's physiologizing, this was not the whole, or even an important part, of Freud's metapsychology. On the contrary, the core of Freud's (1915/1952) *metapsychology* is a pure psychology, with no more physiologizing (and, frankly, a whole lot less) than one would find in a standard textbook for Psychology 101. Moreover, adherence to the natural science framework ostensibly embraced by Freud might well have kept psychoanalytic theory closer to the empirical evidence and prevented it from wandering off into a hermeneutic wonderland.

For the record, here is Freud's metapsychology, summarized by Rapaport (1959, 1960; see also Rapaport & Gill, 1959) in terms of 10 points of view:

Empirical: "The Subject Matter of Psychoanalysis is Behavior." (Rapaport, 1960, p. 39, uppercase in original)

Gestalt: "Behavior is Integrated and Indivisible: The Concepts Constructed for Its Explanation Pertain to Different Components of Behavior and Not to Different Behaviors. (p. 40)

Organismic: "No Behavior Stands in Isolation: All Behavior Is That of the Integral and Indivisible Personality." (p. 42)

Genetic:	"All Behavior Is Part of a Genetic Series, and through Its Antecedents, Part of the Temporal Sequences Which Brought About the Present Form of the Personality." (p. 43)
Topographic:	"The Crucial Determinants of Behaviors Are Unconscious." (p. 46)
Dynamic:	"The Ultimate Determiners of All Behavior are the Drives." (p. 47)
Economic:	"All Behavior Disposes of and Is Regulated by Psychological Energy." (p. 50)
Structural:	"All Behavior Has Structural Determinants." (p. 52)
Adaptive:	"All Behavior Is Determined by Reality." (p. 57)
Psychosocial:	"All Behavior Is Socially Determined." (p. 62)

Stated in these terms, the theory is obviously pure psychology.

In referring to behavior in his statement of the empirical point of view, Rapaport meant to include thoughts, feelings, and motives as well as actions as the subject matter for psychology. Who among us can disagree with that? The Gestalt point of view brings overdetermination into play, which makes for difficult theory testing, but Rapaport sees this as an asset rather than a liability for psychoanalysis. His explication is worth quoting at length:

> Academic psychologies did not develop such a concept [as overdetermination], probably because their methods of investigation tend to exclude rather than to reveal multiple determination. But they did not escape the problem itself: every behavior phenomenon has perceptual, learning (memorial), conceptual (cognitive), motor, etc., components; and the rival psychological theories (perceptual theory of cognition, learning theory of perception, motor theory of thought, etc.) show both the presence of the problem and the confusion resulting from a failure to face it squarely. (1960, p. 41)

Overdetermination or not, at a time when psychologists happily study both bottom-up and top-down influences on perception and both the cognitive and biological underpinnings of emotion, nobody should have any problem with a proposition like this.

In his statement of the organismic point of view, Rapaport simply

argued that psychoanalysis is intended to be a complete psychological theory, not a special explanation of some arcane symptoms and phenomena. Because psychoanalytic explanations of symptom formation must be consistent with other laws of mental life and vice versa, the implication is clearly that psychoanalytic theory and practice can never be divorced from scientific psychology as a whole.

Many of the other points of view should also be regarded as non-controversial—for example, the idea, stated in the genetic point of view, that psychologists should be interested in developmental issues, both phylogenetic and ontogenetic, nature and nurture. The dynamic point of view could just as easily have been stated by Clark Hull. The adaptive point of view, which holds that behavior is not wholly determined by drives, lays the foundation for ego psychology and a connection to the scientific study of perception, memory, thought, and language—just as the psychosocial point of view creates a connection with social psychology. With respect to the economic point of view, Freud clearly had a hydraulic metaphor in mind, but it only takes a little effort to begin thinking about automatic and effortful processes, limited-capacity theories of attention, and the like. Similarly, in the structural point of view, Freud clearly had id, ego, and superego in mind, but the point of view itself is also consistent with any modular theory of mental life—an assumption which is quite popular today. The topographic point of view seems to refer to Freud's earlier system of *Cs.*, *Pcs.*, *Ucs.*, but the idea that conscious experience, thought, and action are determined by processes of which we are unaware is common currency in contemporary cognitive psychology (Barsalou, 1992).

The point of this exposition is not to engage in glib hand waving, implying that Freud intuited everything that modern scientific psychology has labored hard to understand. The point is that at its most general level of explanation, psychoanalytic theory doesn't look much different from other psychological theories of mind and behavior. Of course, at this level the theory is also untestable. But it is not untestable in principle: Nestled under the metapsychological propositions is a hierarchy of general, specific, and empirical propositions which are increasingly amenable to testing by means of conventional scientific procedures (Hilgard, 1952, 1968). So, for example, we can unpack the dynamic point of view into the general proposition that the important motives for behavior are sexual and aggressive in nature; at the specific level, that children harbor erotic feelings toward the parent of the opposite sex and hostile feelings toward the parent of the same sex; and at the

empirical level, that young boys regard their fathers as rivals for their mothers' affections. It is at these levels that psychoanalytic theory has been tested in clinical, observational, and experimental studies; and it is at these levels that, as we have known at least since the review by Sears (1947), psychoanalytic theory has fared badly. It should be understood, though, that the failure of the theory at the general, specific, and empirical levels doesn't mean that the metapsychology has to be rejected as well.

Object Relations, Transference, and Defense

Like cognitive psychology, psychoanalysis is interested in how people form mental representations of objects and events, store them in memory, retrieve them as needed, and manipulate them during reasoning and problem solving. Like social psychology, psychoanalysis assumes that humans are social creatures and that behavior cannot be understood without reference to the sociocultural context in which it occurs. The collection of articles included in this special issue offers new ways to link psychoanalysis with mainstream psychology, and in particular with theories and research in social cognition, around the theme of emotion and mental representation. The general idea behind these articles is that the mental representation of significant interpersonal relationships and other emotion-laden material has major implications for personality and adjustment.

This agenda clearly emerges in the Herculean case study presented by Horowitz and his colleagues. These investigators begin with the concept of the person schema, as an abstract, integrated representation of past interpersonal relationships; they then seek to discover how schemas act to organize current social interactions, as well as how they themselves change in response to shifts in relationships. In other words, they are studying problems of psychoanalytic object relations theory with the tools of the social-cognition laboratory. In a very promising blend of the clinical and the experimental, they recruit as experimental subject an individual who is suffering a pathological grief reaction, and they perform their studies in a therapeutic context.

This focus on a single case study has all the problems of representativeness and generalizability that beset such reports. However, in marked contrast to most case literature, their analysis is highly quantitative in nature, and the results quite interesting. From transcripts of the therapy sessions, the investigators identified a number of salient

and recurring topics of discourse and then coded whether the patient engaged in disclosure or expression. Based on both textual and video-tape records, they then coded the patient's behavior with respect to the therapist, modulation of emotion, and nonverbal expressions of defensiveness; they also collected measures of autonomic activity. The result was the identification of two patterns of emotional reactivity, one characterized by conflict and ambivalence, the other by denial and resistance. In addition to its intrinsic interest value, the case displays a technology that might be useful in social-psychological research outside as well as inside the therapeutic context.

Transference

One vehicle for linking psychoanalysis to social psychology is the concept of transference, which can be thought of as the generalization of early childhood relationships onto later, adult ones. The social-cognitive translation of transference is quite simple: Early relationships generate mental schemas, prototypes, and scripts that can be shown to affect one's adult social relationships. In studies of psychotherapy sessions, Luborsky, Crits-Christoph, and their colleagues (e.g., Luborsky & Crits-Christoph, 1990) have employed the Core Conflictual Relationship Theme (CCRT) method to code people's social relationships (as described in relationship episodes extracted from transcripts of therapy sessions) in terms of the patient's wishes, responses from the other, and responses to the self. The general finding of these studies is that patients' wishes with respect to one person in their lives are much the same as their wishes with respect to other people; the responses of these people, and the patients' responses to them in turn, are also fairly consistent from one target to another.

One problem with the CCRT method, at least as applied in these studies, is that the same judge codes material from all the patient's relationships. Obviously, this procedure allows great opportunity for the codings to be contaminated by the rater's bias. In the final analysis, then, it is unclear where the consistency lies—in the patient's object relations or in the rater's head. This is an old problem in the psychology of personality (Block, 1971; Shweder, 1982), and it is indeed a serious one. In their contribution to this special issue, Crits-Christoph and his colleagues try to solve rater bias by means of a new coding procedure, known as the Quantitative Assessment of Interpersonal Themes (QUAINT). One of the salient differences between the CCRT and

QUAINT methods is that in the former, all relationship episodes are coded by the same judge, while in the latter, different episodes are coded by different judges, each blind to the ratings of the others.

The study revealed levels of intraperson consistency in relationship themes that, while sometimes achieving conventional levels of statistical significance, are quite low. Even the most pervasive wish isn't all that pervasive. Apparently, a lot of the transference found with the CCRT exists in the rater's perception of the patient's relationships and not in the relationships themselves. This is an important finding, especially in view of the centrality of the transference concept in psychoanalytic theory and therapy; transference is one of Freud's most interesting hypotheses about social cognition and behavior; and, based on this evidence, apparently it is wrong.

Even if the pervasiveness correlations had come out as expected, such a result would only be the beginning of a test of transference. Obviously, nothing in these results supports the most radical aspect of Freud's hypothesis: that the child's early erotic relationships are displaced onto adult relationships (including the relationship with the therapist). After all, no one has any idea what these patients thought *when they were children*. Perhaps their relationships with their parents changed from childhood to adulthood; perhaps these adult parental relationships do color relationships with others; but perhaps relationships with others color one's interactions with one's parents as well. The transference hypothesis demands more than a correlation; it demands some evidence about the direction of causality as well.

In this respect, the experimental study of Andersen and Baum in this issue at least shows that one's significant interpersonal relationships can affect the person's more mundane ones. In their study, subjects were asked to form an impression of a new person who was described in such a way as to resemble someone with whom the subject already had a significant positive or negative relationship; controls formed impressions of a target who resembled someone else's significant other. The authors found that subjects' evaluations of the targets were influenced by the degree to which the target resembled their own significant other. Moreover, when asked to remember attributes of the target presented earlier, they tended to falsely recognize attributes of the significant other whom the target resembled.

This study does not address the role of archaic relationships, however, because the significant others were not necessarily parents or parental figures. However, the experimental methodology employed does show

that representations of important people in one's life do indeed color
new social relationships. This is a great advance, but in the final analysis
one wants to know whether it is anything more than a case of stimulus
generalization. Is the whole apparatus of psychoanalytic theory needed
to predict this result? Or will this simple precept do—that the best pre-
dictor of behavior in a new situation is the person's behavior in similar
situations in the past?

The mechanisms of defense

Certainly the psychoanalytic concept that has received the most atten-
tion from experimental psychologists has been that of defense, and
particularly repression (Singer, 1990). Here we have a fairly concrete
psychoanalytic hypothesis: that people suppress awareness of conflic-
tual, anxiety-evoking ideas, impulses, and the like. Like transference,
repression lies at the core of psychoanalytic thinking. As Westen notes
in his contribution to this issue, the notion of conscious and unconscious
self-regulation of affect cuts a swath through quite a bit of clinical and
experimental psychology. The idea that emotions are not merely pas-
sive responses to stimulation, but rather are mental states that people
actively seek or avoid by means of thought and action, has a great deal
of appeal. On the other hand, as with transference, one has to wonder
whether psychoanalytic theory has any unique contribution to make:
After all, as Westen notes, Carver and Scheier (1981) came to the same
conclusion from a cybernetic control-theory perspective, without any
explicit influence of psychoanalytic thinking.

Credit for reviving contemporary work on repression goes, in large
part, to Weinberger and his colleagues. Weinberger's procedures for
distinguishing between repressors and the truly nonanxious are a great
improvement over the earlier Repression-Sensitization Scale of Byrne
(1961) and have been widely influential. In their contribution to this
special issue, Weinberger and Davidson asked subjects to say nega-
tive things about themselves under emotionally expressive or inhibi-
tive conditions. There were three measures of emotional arousal, each
corresponding to one of Lang's (1968) three components of emotion:
behavioral (actual responses to the task), subjective (reports of nega-
tive affect during the task), and physiological (heart-rate variability).
Compared to highly anxious impression managers, repressors were
more defensive in their overt behavior and showed similar patterns of
cardiovascular activity; however, they also reported feeling less negative

emotion and more positive emotion during the task. So, once again, repressors showed a discrepancy between subjective and behavioral or physiological indices of distress.

The fact that impression managers didn't show the same pattern as repressors tends to contradict the suggestion (e.g., Holmes, 1990) that repressors actually feel distress but simply do not communicate it to others. This conclusion must remain tentative, however, because Weinberger and Davidson's experiment lacks some crucial control groups: First, comparison groups of nondefensive, nonimpression-managing, low- and high-anxious subjects are needed, in order to confirm (as has been found in other studies) that repressors behave more like anxious than nonanxious people. A group of low-anxious impression managers also is needed for direct comparison to the defensive low-anxious repressors. For the moment, however, repressors do indeed seem to be engaged in affect regulation rather than impression management. Put another way, while impression managers may inhibit emotional expression, repressors appear to inhibit emotional experience.

The idea of repression also lurks behind Wegner and Zanakos's study of chronic thought suppression. Conscious thought suppression is not necessarily the same as unconscious repression, but as Erdelyi and Goldberg (1979) argued, the mechanisms underlying the two processes may very well be similar. In a provocative series of studies (reviewed by Wegner, 1992, 1993), Wegner and his colleagues have found that the intention to suppress an unwanted thought produces a paradoxical preoccupation with the thought. Two mechanisms seem to be involved in thought suppression: self-distraction and monitoring the success of the suppression. Unfortunately, monitoring increases the very contents to be suppressed. Accordingly, the chronic suppression of unpleasant thoughts and feelings may well produce a kind of obsession with them, which in turn leads to elevated levels of unpleasant moods such as anxiety and depression.

In order to test this hypothesis, Wegner and Zanakos constructed a questionnaire measure of thought suppression, the White Bear Suppression Inventory (WBSI), which correlates significantly with measures of obsessions (but, interestingly, not compulsions), depression, and anxiety. As the investigators themselves note, some of the common variance is an unavoidable consequence of content overlap—after all, most chronic thought suppression must be an attempt to control precisely those unpleasant thoughts, memories, and images that are symptomatic of anxiety and depression. Moreover, the direction of cau-

sality isn't clear. Still, at least in principle, the WBSI is a more direct measure of repressive tendencies than the other available instruments. Byrne's (1961) Repression-Sensitization Scale is really a measure of anxiety or neuroticism, while Weinberger's procedures infer repression or denial from high levels of social desirability, rather than measuring this attribute directly. Thus, the WBSI is a promising instrument, and I hope to see researchers using it in future work.

If Wegner and Zanakos are right, one consequence of thought suppression are anxiety disorders; another may be physical illness. In previous studies, Pennebaker and his colleagues (e.g., Pennebaker & Beall, 1986) have found that self-disclosure has positive consequences for physical health. In preliminary research testing a promising new psychophysiological technique, Hughes, Uhlmann, and Pennebaker (this issue) found that expressing and inhibiting thoughts have different physiological consequences. Much work remains to be done—for example, expression and inhibition, as well as various forms of inhibition, may differ in terms of the demands they place on cognitive resources, as well as in the direction of mental activity (affirmation vs. negation). Still, the finding of momentary physiological correlates of affirmation and negation, if confirmed, will provide an important link between disclosure and defense on the one hand, and health and illness on the other. This is an important avenue for future psychosomatic research.

What Use Is Psychoanalytic Metapsychology Now?

Psychoanalysts shouldn't have given up on metapsychology because, as Rapaport (1959, 1960) understood clearly, it provides both the scientific undergirding for clinical practice and the only means for connecting psychoanalytic theory to the mainstream of scientific psychology. On the other hand, it can be argued that if scientific psychologists had taken more of an interest in psychoanalytic metapsychology, perhaps ignoring the details of the more specific propositions, they would have been spared some of the excesses of the dark days of behaviorism and stimulated to ask some interesting questions about the mind. The New Look in perception is a clear example of the benefits of contact with psychoanalysis (Bruner, 1992; Bruner & Klein, 1960).

But what are we, as scientific psychologists, to make of psychoanalysis now? In discussing the rocky relationship between psychoanalysis and psychology, I used the analogy of medieval monks preserving

classical knowledge during the Dark Ages. This is a role that psychoanalysis, particularly psychoanalytic ego psychology, tried to play, and it should be honored for its effort. However, the historical fact is that psychology regained its mind (if you will) largely independently of the preservational efforts of psychoanalysis (Baars, 1986; Gardner, 1985). When the cognitive revolution occurred, it was largely stimulated by virtue of advances outside the field, such as those in linguistic theory and computer science; inside the field, a primary role was played by the Gestalt psychologists (who so influenced Rapaport); and, somewhat paradoxically, another primary instigation came from discoveries about the peculiarities of conditioned responses in animals.

The result is that, although contemporary psychology has regained a broad theoretical perspective that somewhat resembles psychoanalytic metapsychology, its metapsychology is functionally autonomous from Freud's, and the specific commitments it makes are very different from what he had in mind. To take only one example: While contemporary cognitive psychology has rediscovered the psychological unconscious, the nature of unconscious mental life is much different from what Freud envisioned (Kihlstrom, 1987, in press). Interestingly, the articles in this special issue also make this same point. When scientific psychologists study pathological grief, they approach it in the here-and-now, without mentioning archaic involvements. When they study transference, they talk about generalization across significant others, or from significant others to new acquaintances, without saying anything about infantile erotic attachments. When they study defense, they discuss the regulation, both conscious and unconscious, of unpleasant affect in general, not merely the conflicts and anxieties stemming from primitive sexual and aggressive motives.

In the final analysis, the road to contemporary psychology largely bypassed psychoanalysis, and was little influenced by either the metapsychology or the clinical theory. Still, as the articles in this special issue show, new generations of psychologists can still find inspiration, observations, and hypotheses in the psychoanalytic literature. And perhaps that is as it should be.

REFERENCES

Baars, B. J. (1986). *The cognitive revolution in psychology.* New York: Guilford.
Barsalou, L. W. (1992). *Cognitive psychology: An overview for cognitive scientists.* Hillsdale, NJ: Lawrence Erlbaum.

Block, J. (1971). *Lives through time*. Berkeley: Bancroft.

Bruner, J. (1992). Another look at New Look 1. *American Psychologist*, **47**, 780–783.

Bruner, J., & Klein, G. S. (1960). The functions of perceiving: New Look retrospect. In B. Kaplan & S. Wapner (Eds.), *Perspectives in psychological theory* (pp. 61–77). New York: International Universities Press.

Byrne, D. (1961). The Repression-Sensitization Scale: Rationale, reliability, and validity. *Journal of Personality*, **29**, 334–349.

Carver, C. S., & Scheier, M. F. (1981). *Attention and self-regulation: A control-theory approach to human behavior*. New York: Springer.

Crews, F. J. (1993). The unknown Freud. *New York Review of Books*, **21**(19), 55–66.

Crews, F. J. (1994). Reply, in "The unknown Freud: An exchange." *New York Review of Books*, **22**(3), 34–43.

Dollard, J., & Miller, N. E. (1950). *Personality and psychotherapy: An analysis in terms of learning, thinking, and culture*. New York: McGraw-Hill.

Erdelyi, M. H. (1985). *Psychoanalysis: Freud's cognitive psychology*. San Francisco: Freeman.

Erdelyi, M. H., & Goldberg, B. (1979). Let's not sweep repression under the rug: Toward a cognitive psychology of repression. In J. F. Kihlstrom & F. J. Evans (Eds.), *Functional disorders of memory* (pp. 355–402). Hillsdale, NJ: Lawrence Erlbaum.

Freud, S. (1953a). The interpretation of dreams. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vols. 4–5, pp. 1–621). London: Hogarth Press. (Original work published 1900)

Freud, S. (1953b). Three essays on the theory of sexuality. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 7, pp. 130–243). London: Hogarth Press. (Original work published 1905)

Freud, S. (1957). Papers on metapsychology. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 14, pp. 159–260). London: Hogarth Press. (Original work published 1915)

Freud, S. (1959). Inhibitions, symptoms, and anxiety. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 20, pp. 75–175). London: Hogarth Press. (Original work published 1926)

Freud, S. (1961). The ego and the id. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 19, pp. 12–59). London: Hogarth Press. (Original work published 1923)

Freud, S. (1964). New introductory lectures on psycho-analysis. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 22, pp. 5–185). London: Hogarth Press. (Original work published 1933)

Freud, S. (1966a). Project for a scientific psychology. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 1, pp. 283–398). London: Hogarth Press. (Original work published 1895)

Freud, S. (1966b). Heredity and the aetiology of the neuroses. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 3, pp. 143–156). London: Hogarth Press. (Original work published 1896)

Freud, S. (1966c). Further remarks on the neuro-psychoses of defence. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 3, pp. 162–185). London: Hogarth Press. (Original work published 1896)

Freud, S. (1966d). The aetiology of hysteria. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 3, pp. 191–221). London: Hogarth Press. (Original work published 1896)

Gardner, H. (1985). *The mind's new science: A history of the cognitive revolution.* New York: Basic Books.

Gill, M. M. (1976). Metapsychology is not psychology. *Psychological Issues,* **9**(4) (Monograph 36), 71–105.

Greenberg, J. R., & Mitchell, S. A. (1983). *Object relations in psychoanalytic theory.* Cambridge, MA: Harvard University Press.

Grünbaum, A. (1984). *The foundations of psychoanalysis: A philosophical critique.* New Haven: Yale University Press.

Hilgard, E. R. (1952). Experimental approaches to psychoanalysis. In E. Pumpian-Mindlin (Ed.), *Psychoanalysis as science* (pp. 3–45). Stanford: Stanford University Press.

Hilgard, E. R. (1968). Psychoanalysis: Experimental studies. In *International encyclopedia of the social sciences* (Vol. 13, pp. 13–45). New York: Macmillan.

Holmes, D. S. (1990). The evidence for repression: An examination of sixty years of research. In J. L. Singer (Ed.), *Repression and dissociation: Implications for personality theory, psychopathology, and health* (pp. 85–102). Chicago: University of Chicago Press.

Holt, R. R. (1976). Drive or wish? A reconsideration of the psychoanalytic theory of motivation. *Psychological Issues,* **9**(4) (Monograph 36), 158–197.

Horowitz, M. J. (Ed.). (1988). *Psychodynamics and cognition.* Chicago: University of Chicago Press.

Kihlstrom, J. F. (1987). The cognitive unconscious. *Science,* **237**, 1445–1452.

Kihlstrom, J. F. (in press). The rediscovery of the unconscious. In J. Singer & H. Morowitz (Ed.), *The mind, the brain, and complex adaptive systems.* Reading, MA: Addison-Wesley.

Klein, G. S. (1969). Freud's two theories of sexuality. In L. Breger (Ed.), *Clinical-cognitive psychology: Models and integrations* (pp. 136–181). Englewood Cliffs, NJ: Prentice-Hall.

Klein, G. S. (1973). Two theories or one? *Bulletin of the Menninger Clinic,* **37**, 99–132.

Lang, P. J. (1968). Fear reduction and fear behavior: Problems in treating a construct. In J. M. Shlein (Ed.), *Research in psychotherapy* (Vol. 3, pp. 90–102). Washington, DC: American Psychological Association.

Lindner, S. (1980). The sucking of the fingers, lips, etc. by children (pleasure-sucking). *Storia e Critica della Psicologia,* **1**, 117–143. (Original work published 1879–1880)

Luborsky, L., & Crits-Christoph, P. (1990). *Understanding transference: The core conflictual relationship theme method.* New York: Basic Books.

Macmillan, M. B. (1980). Introduction: Freud and Lindner on pleasure sucking. *Storia e Critica della Psicologia,* **1**, 111–116.

Macmillan, M. B. (1991). Freud evaluated: The completed arc. In *Advances in psychology* (Vol. 75). Amsterdam: North-Holland.

Masson, J. M. (1985). *The complete letters of Sigmund Freud to Wilhelm Fliess, 1887–1904.* Cambridge, MA: Harvard University Press.

Pennebaker, J. W., & Beall, S. K. (1986). Confronting a traumatic event: Toward an understanding of inhibition and disease. *Journal of Abnormal Psychology,* **95**, 274–281.

Pribram, K. H., & Gill, M. M. (1976). *Freud's 'Project' re-assessed: Preface to contemporary cognitive theory and neuropsychology.* New York: Basic Books.

Rapaport, D. (1959). The structure of psychoanalytic theory: A systematizing attempt. In S. Koch (Ed.), *Psychology: A study of a science* (Vol. 3, pp. 55–183).

Rapaport, D. (1960). The structure of psychoanalytic theory: A systematizing attempt. *Psychological Issues,* **2**(2) (Monograph 6).

Rapaport, D., & Gill, M. M. (1959). The points of view and assumptions of metapsychology. *International Journal of Psycho-Analysis,* **40**, 153–162.

Schafer, R. (1976). *A new language for psychoanalysis.* New Haven: Yale University Press.

Schimek, J. G. (1987). Fact and fantasy in the seduction theory: A historical review. *Journal of the American Psychoanalytic Association,* **35**, 937–965.

Sears, R. R. (1947). *Survey of objective studies of psychoanalytic concepts.* New York: Social Science Research Council.

Shweder, R. A. (1982). Fact and artifact in trait perception: The systematic distortion hypothesis. In B. A. Maher & W. B. Maher (Eds.), *Progress in experimental personality research* (Vol. 11, pp. 65–100). New York: Academic Press.

Singer, J. L. (Ed.). (1990). *Repression and dissociation: Implications for personality theory, psychopathology, and health.* Chicago: University of Chicago Press.

Spence, D. P. (1982). *Narrative truth and historical truth: Meaning and interpretation in psychoanalysis.* New York: Norton.

Spence, D. P. (1994). *The rhetorical voice of psychoanalysis: Displacement of evidence by theory.* Cambridge, MA: Harvard University Press.

Sulloway, F. J. (1979). *Freud, biologist of the mind: Beyond the psychoanalytic legend.* New York: Basic Books.

Wegner, D. M. (1992). You can't always think what you want: Problems in the suppression of unwanted thoughts. In M. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 25, pp. 193–225). San Diego: Academic Press.

Wegner, D. M. (1993). Ironic processes of mental control. *Psychological Review,* **101**, 34–52.