

## **MINDING THE GAP BETWEEN POSITIVISM AND HERMENEUTICS IN PSYCHOANALYTIC RESEARCH**

Two quite different cultures are to be found within psychoanalysis, one more clinical in orientation, more focused on meaning and interpretation, and relying primarily on the traditional case study method, the other more research-oriented, focused on cause-and-effect relationships, and relying primarily on methods borrowed from the natural and social sciences. The history of this divide is reviewed and arguments, pro and con, about the potential contributions of specific types of empirical investigation are discussed. Increasingly, it seems, criticisms concerning the scientific status of psychoanalysis are being responded to by empirical research. This has contributed to a growing recognition within the scientific community of the credibility of aspects of psychoanalytic theories and of the effectiveness of psychodynamic treatment. However, some segments of the psychoanalytic community are concerned that this increase in the quantity and quality of empirical research on psychoanalytic concepts risks creating an empirical one-sidedness, while other segments are concerned that not engaging in systematic empirical research can lead to intellectual isolation, fragmentation, stagnation, and orthodoxy. To counter this polarizing tendency, a recommendation is made for methodological pluralism. Adopting this stance could contribute to an enriched understanding of the clinical process and to the development of new research methodologies to investigate complex psychodynamic hypotheses, thus bridging the gap between the two psychoanalytic cultures, as well as the gap between research and clinical practice.

**F**ew topics elicit as much discussion and controversy in psychoanalysis as the debate on the role of empirical research (e.g., Bornstein 2001; Hauser 2002; Sandler, Sandler, and Davies 2000; Westen

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2002a; Wolff 1996). The highly emotional reactions following the negative verdict on the empirical status of psychoanalysis by the philosopher of science Adolf Grünbaum, for example, sent shock waves through the psychoanalytic community (see Grünbaum 1984, 2001; Lothane 2001; Luborsky 1986). While the debate provoked by Grünbaum's critique was primarily between the psychoanalytic community and critics external to psychoanalysis, the role of empirical investigation is today debated within the field. Major psychoanalytic journals including *Journal of the American Psychoanalytic Association* and *International Journal of Psychoanalysis* have devoted special issues to this topic (see, e.g., Blum 1999; Emde and Fonagy 1997; Galatzer-Levy and Hauser 1997; Safran 2001; Westen 1999), and the role of empirical research has been a hotly debated issue at several recent psychoanalytic conferences and meetings (e.g., Fonagy et al. 2002; Safran 2001; Sandler, Sandler, and Davies 2000).

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It is hardly surprising that empirical research has become a central issue in contemporary psychoanalysis. What is surprising is that it has taken so long to become such a central issue, particularly given the repeated criticisms concerning the empirical status of psychoanalysis, both by so-called Freud-bashers and by more serious psychoanalytic scholars. This criticism of the lack of empirical research in psychoanalysis has led to a growing awareness within segments of the psychoanalytic community of the need for systematic empirical evidence to support psychoanalytic assumptions and therapies (e.g., Blatt and Auerbach 2003; Bornstein 2001, 2005; Fonagy 2003; Shedler 2002; Westen 1998). In addition, the advent of evidence-based medicine and managed care in psychiatry has contributed to a growing awareness in some psychoanalytic quarters that the relative lack of systematic empirical research could threaten the future of psychoanalysis both as a science and as a therapy in this age of evidence-based medicine (Bornstein 2001; Fonagy et al. 2002; Gunderson and Gabbard 1998; Safran 2001).

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Although many of the criticisms of psychoanalysis have been increasingly responded to by empirical research, these criticisms have not been fully addressed. To quote Freud's ironic comment on the occasion of his appointment as professor, it is not that "congratulations and flowers are already pouring in, as though the role of sexuality has suddenly been officially recognized by His Majesty, the significance of the dream certified by the Council of Ministers, and the necessity of a psychoanalytic therapy . . . carried by a two-thirds majority in Parliament" (Freud 1985, Letter from March 11, 1902, p. 457). To the contrary, the scientific status of psychoanalysis is still under debate, both outside psychoanalysis (e.g., Grünbaum 2001) and within the psychoanalytic community (e.g., Bornstein 2001; Green 1996, 2000; Wallerstein 2000).

The present paper aims to provide an overview and a critical discussion of this pivotal debate within the psychoanalytic community based on the assumption that the underlying dynamics of the conflict between psychoanalytic practice and training and psychoanalytic research are best understood as a conflict between two seemingly fundamental and diametrically opposed cultures within psychoanalysis (Snow 1959), each culture driven by different assumptions about the nature of psychoanalytic research. This clash between two cultures is, for example, expressed in the debates between André Green, Peter Fonagy, Robert Wallerstein, and Robert Emde (see Sandler, Sandler, and Davies 2000; see also Shedler 2004). It is difficult, however, to describe these two cultures precisely, because many psychoanalysts are situated somewhere between these extremes. But in general this divide involves one culture primarily interpretive in orientation, emphasizing meaning and purposefulness in human behavior, and relying primarily on the traditional case study method as introduced by Freud for theory-building (or on qualitative methods in general) and another culture relying primarily on methods from the physical, natural, and social sciences, which search for sequences of cause and effect and use probabilistic rather than individualistic models of data analysis and explanation. In some cases, as we will see, this divide parallels the distinction between more idiographic approaches, which emphasize the uniqueness of each individual, and more nomothetic approaches, which are more concerned with identifying lawful regularities across individuals. First we briefly discuss the history of this debate, which is partly rooted in and still influenced by criticism from outside the

psychoanalytic community. Next we discuss the ongoing debate within the psychoanalytic community itself concerning the role of empirical research. Arguments pro and con for specific types of empirical investigation in psychoanalysis are discussed from both perspectives. This is followed by a plea for methodological pluralism to bridge the divide between the two cultures in psychoanalysis. Examples from existing research are used to illustrate these issues, as well as to illustrate the potential for research and clinical practice to inform and enrich each other. We close with some conclusions and perspectives concerning the future of research in psychoanalysis.

### **EXTRAPARADIGMATIC CRITICISM OF PSYCHOANALYSIS**

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Psychoanalysis, it need hardly be said, has been under attack since its inception. Ever since Freud's first major publications (e.g., Freud 1900, 1905), psychoanalysis has been described as unscientific, as "old-wives psychiatry," and even as "pornographic" (Kiell 1988; Turner 1996). Freud critics such as Cioffi (1970) and Eysenck (1985) have accused psychoanalysis of being a pseudoscience, because almost no set of testable hypotheses can be derived from its theories. Moreover, these critics claim that the psychoanalytic hypotheses that have been put to the test have all been refuted. Hence, Eysenck (1972) concluded that "there is no evidence at all for psychoanalytic theory" (p. 266). Torrey (1992) concluded his overview of the scientific status of psychoanalysis with an even more dramatic statement: that psychoanalysis has to be situated "on precisely the same scientific plane as the theory regarding the Loch Ness monster" (p. 221). This view of psychoanalysis as outdated is shared by many, especially in academic psychology and psychiatry. Bornstein (1988, 2001), for instance, has documented the waning influence of psychoanalysis in academic psychology and psychiatry, in textbooks, and in many academic training and professional training programs, where it is often negatively depicted as basically unscientific, outdated, and sexist.

The influence of the so-called Freud-bashers (see Lothane 2001) on the debate concerning the scientific nature of psychoanalysis cannot be underestimated. Over the years, an almost endless series of accusations have been formulated against psychoanalysis and especially against Freud, both as a person and as a scientist. Freud has been accused of

being a brute who forced his ideas upon his patients (Crews 1995, 1998), a cocaine addict with a megalomaniac messiah delusion (Thornton 1983), a paranoid personality (Farrell 1996), and a creator of myths concerning his own person and his theories (Sulloway 1979). According to some of these authors (e.g., Eysenck 1985; Crews 1995; Macmillan 1991), Freud showed a combination of these pathological character traits. More recently, Freud and psychoanalysis were accused of having led many therapists to believe in so-called “recovered memories” of sexual abuse, which resulted in a veritable epidemic of false accusations (see, e.g., Crews 1995; Ofshe and Watters 1995). Yet only a decade earlier Freud and psychoanalysis were accused of exactly the opposite—namely, a disastrous denial of childhood sexual abuse (Masson 1984).

The fact that these and other accusations and allegations often rest on limited evidence (Holt 1999; Köhler 1996; Lothane 1996, 1999; Robinson 1993) may be somewhat surprising because proponents of this kind of criticism are often found within circles that otherwise demand the highest of scholarly standards. Nevertheless, these criticisms have been very influential.

Forrester’s ironic entry for an imagined encyclopedia reflects the opinion of Freud and psychoanalysis typical in many quarters in psychology and its allied disciplines: “FREUD. No need to have any idea of his philosophy, nor even to know the titles of his works, because everyone knows all that. Refer discreetly *either* to the fact that he slept with his sister-in-law . . . *or* to the fact that he made everything up. . . . But preferably not both at once. In uncertain company, it’s always good manners to say he’s rather *passé*, though he once had something useful to say to our parents’ generation (see PSEUDOSCIENCE). . . . If feeling forceful and required to be up-to-date, declare how shameful it is that we’ve only recently learned about all those scandals. And there are still more to come . . .” (p. 12).

This portrayal of psychoanalysis as a pseudoscience has become more and more incongruent given the increasing number of empirical studies of psychoanalytic theories and concepts that have been conducted in the last several decades (for overviews, see Bornstein and Masling 1998a,b; Fisher and Greenberg 1996; Masling and Bornstein 1996; Shapiro and Emde 1995; Westen 1998, 1999). These studies demonstrate not only that psychoanalytic concepts can be tested empirically, but also that solid evidence supports many psychoanalytic

assumptions. Further, psychoanalytic research is increasingly published in prestigious mainstream journals in psychology and psychiatry (e.g., Bateman and Fonagy 1999, 2001; Blatt et al. 1998; Leichsenring, Rabung, and Leibing 2004; Lenzenweger et al. 2001; Levy, Clarkin, and Kernberg in press; Shedler and Westen 2004). In addition, a growing number of studies document both the efficacy and the effectiveness of various forms of psychodynamic psychotherapy (e.g., Bateman and Fonagy 2001; Blatt and Shahar 2004; Fonagy et al. 2002; Leichsenring 2001; Leichsenring, Rabung, and Leibing 2004). Thus, although psychoanalysis continues to be in great need of systematic research, and the empirical basis of psychoanalysis still is relatively meager compared to other forms of psychotherapy, the repeated criticism that psychoanalysis is unscientific because it has not produced empirical data supporting its theories and therapies, and is not even able to generate hypotheses that can be empirically tested, contrasts with the growing empirical basis of psychoanalysis. In addition, the growing convergence between psychoanalysis and other theoretical approaches in psychology, such as cognitive psychology (e.g., Bucci 1997; Erdelyi 1985; Luyten, Blatt, and Corveleyn 2005; Milton 2001; Ryle 1995; Segal and Blatt 1993), developmental psychology and developmental psychopathology, including attachment research (e.g., Beebe et al. 2003; Blatt, Auerbach, and Levy 1997; Diamond 2004; Emde 1988a,b; Fonagy and Target 2000; Main, 2000; Mayes 2005; Slade 2004; Stern 1985), and social psychology (Westen 1991), as well as the neurosciences (e.g., Kandel 1999; Mayes 2003; Olds and Cooper 1997; Schore 2003; Shevrin et al. 1996; Solms 2004; Westen and Gabbard 2002a,b), clearly attests to psychoanalysis as alive and well as a science.

Three decades ago, Lloyd Silverman (1976) published “Psychoanalytic Theory: the Reports of My Death Are Greatly Exaggerated,” an article whose subtitle echoes Mark Twain’s ironic comment upon reading his own death notice in a newspaper. According to Silverman, Twain’s jest also was appropriate in the context of the many reports of the death of psychoanalysis that were then current. Yet today, thirty years later, even many psychoanalysts are often not familiar with, or are uninterested in, the increasing empirical research on psychoanalytic concepts and theories (Bornstein 2001; Westen 1998, 1999).

Aside from the criticisms of the “Freud-bashers,” a large number of more substantial critiques of psychoanalysis have appeared. These critiques have perhaps had an even greater impact on the current debate

on the role of empirical research within the psychoanalytic community (Fonagy and Tallandini-Shallice 1993; Masling and Bornstein 1996). Two criticisms, both by eminent philosophers of science, stand out as particularly important. First is Karl Popper's well-known charge that psychoanalysis is unfalsifiable and therefore unscientific (1959), an idea that continues to influence the opinion of our fellow scientists. Psychoanalysis, according to Popper, is such an encompassing theory of human nature that it is able to explain almost all of human behavior. Although often considered a strength of psychoanalysis, this is actually its weakness, the argument goes, as virtually no behavior can fail to find a psychoanalytic explanation. In Popper's view, the hallmark of scientific theories is an openness to falsification, and because psychoanalysis looks only for verification and confirmation, it is unscientific. For example, a man may have married a woman who resembles his mother, or he may have married someone who resembles her not at all; in either instance, psychoanalysis purports to explain the behavior. In both cases it is argued that the man suffers from unresolved oedipal conflicts: in the first case he has sought a substitute for his mother; in the second his choice is determined by the fact that any resemblance to his mother in another woman is unbearable. Thus, any outcome is considered a confirmation of the theory, and there is virtually no outcome that can be specified as disconfirming it.

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In this context, Popper's distinction between the context of discovery and the context of justification is extremely relevant, because it continues to influence the debate on the scientific status of psychoanalysis (see, e.g., Edelson 1984). The context of discovery refers to the origin of scientific ideas. But according to Popper, ideas or hypotheses become scientific only when they are put to the test (laid open to falsification) and not refuted. This latter process Popper called the context of justification. And indeed, as Fonagy (2003) has recently argued, it seems that psychoanalysis is rich in "discoveries," but has lagged far behind in the justification of these ideas. To be more precise, psychoanalysts have long considered their typical way of testing psychoanalytic hypotheses—i.e., by the in-depth, interpretive study of individual cases—sufficient ground for justification. Yet, as we will argue in greater detail below, it has become clear that the traditional case study method cannot suffice to justify psychoanalytic ideas because, among other reasons, it usually looks only for confirming evidence.

This second criticism from philosophy of science was lodged by Adolf Grünbaum (1984, 2001), who has convinced many that the traditional way of gathering empirical evidence in psychoanalysis—i.e., by means of free association and the traditional case study method—fails to meet the criteria for good scientific evidence (Forrester 1996; Lietaer 2001; Lothane 2001). The psychoanalytic method, based on the rule of free association, leads, according to Grünbaum, to a *fundamental contamination* of empirical data in that the analyst influences clinical data by his or her theoretical expectations to such an extent that the data are worthless for testing psychoanalytic hypotheses. To begin with, Grünbaum asserts that the psychoanalyst selectively comments, implicitly or explicitly, on theoretically important aspects of the patient's free associations and, moreover, that when these associations do not lead to a confirmation of theoretical hypotheses, the psychoanalyst directs the associations of the patient by verbal and nonverbal cues "until they yield theoretically appropriate results" (Grünbaum 1984, p. 211). Thus, Grünbaum has convinced many that the psychoanalytic treatment process cannot be a research context, and that only research *outside* the psychoanalytic situation (i.e., in untreated subjects, or in patients who do not receive psychoanalytic treatment) can provide the proper context in which to test psychoanalytic hypotheses. To assess the impact of Grünbaum's criticism in more detail, we now turn to the controversy concerning empirical research within the psychoanalytic community itself.

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### **THE CONTROVERSY WITHIN PSYCHOANALYSIS: THE GREAT DIVIDE**

Somewhat schematically, implicitly or explicitly influenced by Grünbaum's criticism, the psychoanalytic community is currently divided into two groups, or even what can be described in the extreme as two radically different cultures regarding the nature and role of empirical research in psychoanalysis (see Fonagy 2000, 2003; Whittle 2000). One culture maintains that psychoanalysis and psychoanalytic research should focus on meaning, interpretation, and narration, and argues that the traditional case study method, as introduced by Freud, is the only appropriate method by which to investigate psychoanalytic theories (see, e.g., Green 1996, 2000; Wolff 1996). The other culture, characterized more by a neopositivistic stance focused on "hard facts"



and probabilistic (statistical) statements, argues that the traditional case study method does not meet the canons of science and that psychoanalysis should therefore use other methods, derived from the physical and social sciences, including experimental and quasi-experimental methods (see, e.g., Masling and Bornstein 1996). Hence, congruent with methodological assumptions in the physical and social sciences, proponents of the latter position argue that experimental research designs, in which subjects are assigned randomly to either an experimental or a control group or condition, and in which potentially disturbing variables are controlled for and target variables manipulated, are the hallmark of science, because only such designs allow strong causal conclusions to be drawn. So-called quasi-experimental research designs and methods can also be useful in the process of justification of knowledge, though to a more limited degree.

Quasi-experimental designs approximate experimental conditions (“quasi” meaning “similar to”), but fall short in either (or both) of two ways. Either individuals cannot be randomized (think of studies of naturally occurring groups, say, studies of patients as compared to nonpatients from the general population) or theoretically important variables cannot be manipulated (think of naturalistic studies of psychotherapy). Because such designs do not allow for control groups or the manipulation of variables, they are considered limited in their ability to yield causal conclusions; they can only suggest possible causal effects (Campbell and Stanley 1966; Treat and Weersing 2005). For example, group differences in quasi-experimental designs might reflect differences due to a presumed theoretical variable, but may also reflect the effect of a third, unknown variable. In addition, so-called  $N = 1$  studies, i.e., studies of individual cases, are considered scientific only insofar as they use a quasi-experimental design, e.g., systematic manipulation of therapeutic interventions (see Kazdin 2003), but even then they are considered limited in their ability to justify knowledge because of problems associated with the generalizability of a single case.

Other research methods such as psychohistory, ethnography and qualitative/interpretive research, and the traditional case study method (all used for decades in psychoanalysis) are considered useful only in the process of hypothesis generation—the context of discovery—but not in the context of justification, because these methods typically lack randomization and/or the ability to control and manipulate variables. In addition, because these methods typically use qualitative data, any

generalization of findings is considered problematic. Whereas there are clear statistical rules for generalizing from quantitative data, no clear rules exist for generalizing from qualitative data.

As noted, the divide between two cultures in psychoanalysis also reflects in some cases a tension between idiographic and nomothetic approaches toward science, with the former approach interested mainly in understanding individuals and their particular, idiosyncratic history, beliefs, and behaviors, the latter being focused on discovering lawful regularities across individuals.

According to some authors, the more interpretive approach within psychoanalysis is influenced mostly by French psychoanalysis, whereas the more neopositivistic culture is dominant primarily in the anglophone world (Steiner 2000; Stern 2000). However, as Westen (2002a) has pointed out, the reality is probably more complex; both positions, in our opinion, should be seen as extremes, with many psychoanalysts worldwide situated somewhere between them. Moreover, some psychoanalysts evince outright indifference to these issues (Westen 2002a), or restrict their concern to the potential impact of research findings on the future of the psychoanalytic profession. It is our belief that the two cultures are indeed extremes and that many in the psychoanalytic community—both researchers and clinicians—hold less radical views. However, with some justification, the psychoanalytic community can be divided between those who consider psychoanalysis an interpretive science belonging to the humanities and those who believe that psychoanalysis should adopt a neopositivistic paradigm consistent with approaches in the physical, biological, and social sciences.

It is a central assumption of this paper that each of these positions regarding research in psychoanalysis requires careful evaluation. One of the main reasons for the gap between the two cultures is that neither is sufficiently familiar with the assumptions and beliefs of the other. Often they seem not even interested in getting to know each other. As Whittle (2000) has argued, every belief and assumption of the other side appears so wrong, as well as so irrelevant, that, even when motivated, one side quickly loses interest in the other's literature. Hence, any hope of bridging this gap within psychoanalysis requires that both sides initially establish "talking terms," which can emerge only from a detailed consideration of each other's assumptions. In general, three important differences characterize the view of the two cultures

on the nature and role of empirical research in psychoanalysis. We discuss each of these differences in turn.

### **POSTMODERNISM VERSUS NEOPOSITIVISM: CAN WE DISTINGUISH BETWEEN “TRUTHS”?**

First, some analysts are convinced that experimental and quasi-experimental research of psychoanalytic concepts will not improve the scientific status of psychoanalysis, because no evidence indicates that these methods are better than methods such as the traditional case study (Masling and Bornstein 1996; Westen 2002a). Influenced by postmodernistic thought, these analysts argue that science and scientific methods lead to one “story” about “reality,” among other possible stories of this same reality. All these stories have their value, because there is no way to decide which version of reality is most valid. Moreover, proponents of this position often argue that psychoanalysis cannot (indeed, should not) formulate universal “laws” concerning human behavior. A variant of this position is held by Spence (1982), who has argued that narrative truth, not historical truth, is what is important in psychoanalysis.

While many would agree that psychoanalysis, like any other science of human nature, is capable neither of formulating “hard,” “universal” laws nor of uncovering historical truth as such (and that indeed it attempts neither), if we really want to convince our patients and the public (as well as ourselves) of the value of psychoanalysis, this necessarily entails theories that propose probabilistic regularities in human behavior. Such theories should not be seen as absolute truths, but, as we argue below in more detail, as “master narratives” that need to be tested and refined, both in nomothetic and idiographic investigations. In fact, many psychoanalysts, starting from Freud, have maintained that psychoanalysis is not (or at least is not only) a science of purely individual, idiosyncratic thought, affect, and behavior, but rather studies regularities that can be observed across human beings (e.g., oral and anal character structure, primary and secondary process thinking, defense mechanisms, psychosexual development). Although every patient has his or her own “idiosyncratic narrative,” any clinician will recognize regularities or “master narratives” in the particular story and dynamics of a patient. To deny this, and to act as if we approach each new patient as a *tabula rasa*, would be naive. Thus, psychoanalysis

produces not just a story among other stories. Although we may study “narrative” and not “historical” truth, to deny that this narrative truth is shaped by the history of the individual, and in turn influences the ways that individual interprets and shapes his or her life, is to deny any regularity in human behavior. As Westen (2002a) has aptly pointed out, when we seek treatment for cancer, we do not expect that the doctor tells us a “story” that makes sense to him or the patient. We expect that the doctor will identify the processes underlying the manifest symptoms and will use empirically tested and supported forms of treatment. At the least, this process involves hypotheses that can be put to the test and proven false, using systematic methods of investigation.

### **CAN QUASI-EXPERIMENTAL RESEARCH DO JUSTICE TO PSYCHOANALYTIC CONCEPTS?**

582 A second area in which the two cultures differ involves the complexity of psychoanalytic concepts. Many psychoanalysts are convinced that the experimental or quasi-experimental investigation of psychoanalytic theories, and especially such research conducted outside the psychoanalytic situation, is not only difficult but impossible (Masling and Bornstein 1996; Westen 2002a; Shedler 2002). Such research would not appreciate the complexities of “real” psychoanalytic concepts and hence would be irrelevant to psychoanalysis. Green (2000) has gone so far as to suggest that such research might even be dangerous, that it might threaten the essence of psychoanalysis. Only the traditional case study method, it is argued, can possibly do justice to the complexities of psychoanalytic concepts and theories (see, e.g., Green 2000; Wolff 1996). It has repeatedly been pointed out, however, that this opinion is simplistic (Fonagy 2000, 2003; Shedler 2002; Westen 2002a). As Masling and Bornstein (1996) have put it, “To dismiss the entire enterprise of experimental testing of psychoanalytic hypotheses is as simple-minded and naive as it is to dismiss the case history method” (p. xviii).

Yet one should also take into account two issues that have reinforced many psychoanalysts in their rejection of this kind of research. First, the quality of research on psychoanalytic concepts and theories has often been very poor and therefore of little relevance to clinicians (Green 2000; Wallerstein 2000). As Spence (1994) has noted, many of these studies are “impeccable ‘studies of nothing very much’” (p. 23).

Thus, many psychoanalysts are disappointed (often repeatedly) in this kind of research, which reinforces their belief that such research is impossible or, if possible, that it has little relevance for their clinical practice (Blatt and Auerbach 2003; Shedler 2002).

A compelling example of the disappointment one can encounter in quasi-experimental research of psychoanalytic concepts is the research on one of Freud's most famous hypotheses, namely, the repression hypothesis. Shevrin and Bond (1993) have shown that almost all research in this area rests on a faulty understanding of the hypothesis. Indeed, they reach the conclusion that until 1993 not a single experimental study ever properly tested it. For instance, in many studies the repression hypothesis was tested by showing subjects stimulus words with sexual or aggressive meanings, together with neutral words. After this task, subjects were asked to recall all of the words. It was assumed that the repression hypothesis predicts that subjects will show a poor recall of words with a sexual or aggressive meaning, as compared with their recall of neutral words. This prediction, however, clearly rests on a misconstrual of the repression hypothesis, as positing that people constantly "repress" sexually or aggressively laden words. But in fact the repression hypothesis proposes that *specific unconscious representations* are not allowed into consciousness and that this process is *motivated* (Freud 1915; Shevrin and Bond 1993). These studies of the repression hypothesis demonstrated neither any motivation to repress these representations, nor the fact that these stimulus words were related to unconscious representations. As pointed out by Shevrin and Bond, and as we discuss in greater detail below, a combination of idiographic and nomothetic methods may be the only way to test hypotheses such as the repression hypothesis using quasi-experimental methods. For example, as Shevrin and colleagues have done, one could initially seek to identify and assess conflict areas in individuals (e.g., by means of an interview or the Thematic Apperception Test) and then construct idiographically tailored stimuli to be used in a subsequent experimental study.

In sum, poorly conceptualized studies that fail to yield meaningful results because they fail to take idiographic meanings into account only reinforce the conviction of many analysts that this kind of empirical research has little to offer.

Yet, at the same time, it is important to note that psychodynamically inspired research is becoming increasingly sophisticated

and clinically relevant (e.g., Beebe et al. 2005; Blatt et al. 1998; Fonagy et al. 2004; Hauser, Golden, and Allen in press; Levy, Clarkin, and Kernberg in press; Miller et al. 1993; Shedler 2002; Shevrin et al. 1996; Talley, Strupp, and Butler 1994). For instance, quasi-experimental research has pointed out that many traditional psychodynamic conceptions of human development are incorrect. As Fonagy (1996) has argued, observational studies have “set limits on psychoanalytic speculations regarding infantile experience by specifying competencies possessed by the child at various stages and ruling out as improbable genetic-developmental propositions that presume capacities outside the developmental timetable” (p. 406, see also Stern 1985). For example, infant research has shown that there is no such thing as a “normal autistic phase” and that the original notion of infantile symbiosis needs to be modified. This has led psychodynamic investigators and clinicians (Pine 2004) to change their views on both normal and pathological development, including the origins and treatment of infantile psychosis (see, e.g., Tustin 1991, 1994).

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A related example of the influence of research on psychoanalytic practice are the studies by Beebe and colleagues (e.g., Beebe and Lachmann 2002) based on microanalyses of videotaped interactions between mothers and infants. Struck by the wealth of information these microanalyses yield, Beebe and colleagues developed a psychoanalytically informed video feedback treatment of mother-infant dyads, which greatly assists in clarifying to mothers their maladaptive ways of interacting with their infants; without such videotaped interactions, this often proves a daunting task (Beebe in press). Moreover, their finding that mother-infant regulation is co-constructed (i.e., that both mother and infant contribute to ongoing interactions) has not only facilitated mother-infant treatment, but has also informed the treatment of adults, in particular drawing attention to the importance of nonverbal interactions between analyst and patient (see Beebe and Lachmann 2002).

Another case in point is the finding of a very high association between borderline personality and sexual abuse. Although psychoanalytic investigators have always been conscious of the importance of early trauma in borderline patients, psychodynamic formulations and treatment clearly underestimated the importance of such traumas in borderline patients, especially in explaining treatment ruptures and precipitate termination (Westen 2002a).

A final example concerns research on the psychotherapeutic process, which has led to a considerable number of clinically relevant insights concerning some of the mutative factors in the treatment process. Blatt (1992) and Blatt and Shahar (2004), for example, found that although prior analyses of the Menninger Psychotherapy Research Project found little differences in the effectiveness of psychoanalysis versus psychodynamic supportive psychotherapy (see Wallerstein 1986), further analyses showed that anaclitic patients, whose problems are mainly focused around relational issues and conflicts, had a better outcome in supportive-expressive psychotherapy and were particularly responsive to the interpersonal dimensions of psychotherapy, whereas introjective patients, who are mainly concerned with achievement, autonomy, and identity, did better in psychoanalysis and were primarily responsive to interpretation.

Significantly, there are clear indications that treatment studies such as these are increasingly having an impact on psychoanalytic practice. To sample only the recent psychodynamic treatment research, two publications, one concerning the development of Mentalization-Based Treatment (Bateman and Fonagy 2004) and the other on Transference-Focused Psychotherapy (Clarkin and Levy 2003), provide detailed accounts of how the interaction between developments in psychoanalytic theory, ongoing clinical experience, and systematic treatment research have resulted in more effective treatments for borderline personality disordered patients. A similarly fascinating account of the interchange between theory, clinical experience, and research in the development of psychoanalytic time-limited day treatment for personality disorders can be found in Piper et al. (1996). Although, as noted, much more research is needed, these examples suggest ways in which systematic empirical research, theoretical advances, and clinical experience can inform each other.

Thus, recent findings indicate not only that the quasi-experimental study of psychoanalytic hypotheses is possible, but that it is quite relevant to the development of psychoanalysis. Hence, the efforts of psychoanalytic organizations to train psychoanalytic investigators (see, e.g., Wallerstein and Fonagy 1999) and to provide research funds should be stepped up. These efforts can train a new generation of researchers to investigate psychoanalytic hypotheses with sophisticated methodologies that do justice to the complex psychic reality that analysts typically observe in their clinical practice (see also Fonagy 2003).

A second point, often overlooked by proponents of quasi-experimental research, is the importance of conceptual research. Psychodynamic concepts and theories are informed by and rooted in such diverse disciplines as philosophy, linguistics, and cultural anthropology on the one hand, and the careful study of individual lives on the other, leading to rich, often very complex (“thick”) concepts. This richness, however, is often not reflected in empirical studies because researchers sometimes simplify concepts in an attempt to conform to more traditional methodological and theoretical points of view. It is precisely this tendency to simplify psychodynamic concepts in research that has convinced many analysts that the quasi-experimental study of psychodynamic concepts is neither possible nor relevant. Yet a certain degree of simplification of psychoanalytic concepts may not be as problematic as it may seem at first sight. Science always gives an approximation based on probabilistic models, and in fact one should be suspicious of any science, especially a science that is as young as psychoanalysis, that pretends to have an answer to all questions. In any science, one starts, as Freud (1915, 1925) repeatedly pointed out, with imperfect concepts that are subsequently refined through a process of empirical testing. Anyone who attempts to study psychodynamic concepts will immediately experience how difficult it is even to define some of these concepts (see Fonagy and Tallandini-Shallice 1993).

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Thus, we submit that detailed and careful conceptual studies of psychoanalytic concepts are as crucial for the future of psychoanalysis as the operationalization of these concepts in systematic empirical research. These clearly are not separate processes, but in fact two sides of the same coin. As Westen (2002a) has pointed out, psychoanalytic concepts are often vaguely defined and working with them can therefore be detrimental to both clinical practice and clinically relevant research. Perhaps in some circles it is chic or trendy to adhere to vague concepts. But if we cannot even agree on the meaning of basic concepts, how can we expect the field to progress, or even to convince our patients and the scientific community of the value of psychoanalysis? However, if we oversimplify psychoanalytic concepts in order to conduct empirical research, the wide gap between psychoanalytic practice and research will continue to exist. Hence, conceptual and empirical research must proceed hand in hand; they must be considered not as disjoint arenas, but as complementary processes.



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**CLINICAL DATA AND THE CASE STUDY  
METHOD IN PSYCHOANALYTIC RESEARCH:  
CINDERELLA OR PANDORA'S BOX?**

A third and final difference between the two cultures in psychoanalysis concerns the nature of empirical research. What kind of research does psychoanalysis need? According to many psychoanalysts, empirical research with methods other than the traditional case study is unnecessary to validate psychoanalytic concepts and theories (Green 1996, 2000; Wolff 1996). As Fonagy (2003) has pointed out, some analysts are not so much opposed to other kinds of research as prone to seeing it simply as confirming "what they have known all along," and thus as unnecessary.

This issue is probably the primary division between the two cultures in psychoanalysis and the main obstacle to establishing a constructive dialogue between them. Indeed, much is at stake here. It is as if two worlds were colliding. One world considers traditional case studies not only as sufficient for the justification of psychoanalytic theories, but also as providing rich and even unique insights into human nature. The other world considers this method to be devoid of scientific value, except in the context of discovery (i.e., for hypothesis generation).

Grünbaum's influence on the debate concerning empirical research is particularly relevant to this discussion. As noted, according to Grünbaum (1984) the traditional case study method does not meet the canons of science because it involves a fundamental contamination of data by the psychoanalyst. For him, the only way psychoanalysis can become truly a science is (1) to use quasi-experimental methods in studying psychoanalytic hypotheses and (2) to do this *outside* the psychoanalytic situation. Thus, the traditional case study method can play a role only in the context of discovery; it can play no role in the context of justification.

There is a large kernel of truth in Grünbaum's critique, as has been recognized by many psychoanalysts (e.g., Bateman 2004; Fonagy 2003; Galatzer-Levy 1991; Holt 1992; Klumpner and Frank 1991; Spence 1990, 1994; Tasman 1998). Many methodological pitfalls are associated with the traditional case study method, linked to the roles of both patient and therapist. Concerning the patient, research, including (perhaps especially) psychodynamic research, has shown that human

memory can be easily influenced, and it is well documented that even “normal,” well-functioning individuals can produce false memories (see, e.g., Loftus and Ketcham 1994). Precisely because many psychodynamic theories rest on reconstruction of memories of the past, extra-clinical evidence is needed to verify these reconstructions. But there is more. Even if psychoanalysts, and correctly so from a clinical perspective, argue that these independent confirmations are not needed because psychoanalysis deals with the personal meanings of individuals (“narrative truth”) and not historical events, an important limitation of the traditional case study method remains, one related not to the patients involved, but to the way psychoanalysts conduct and report case studies (see, e.g., overviews in Fonagy 2003; Messer and McCann 2005; Spence 1994). Spence, for example, has convincingly shown that in traditional case studies the “raw” material of sessions is rarely reported, making it difficult for readers to judge the extent to which theoretical prejudices and selective memory played a role in the presentation and selection of material (e.g., not reporting or even observing data that contradict or are inconsistent with cherished beliefs). As Bateman (2004) notes, a traditional case study is not a faithful description of an actual clinical encounter, nor is it a systematic study of a number of hypotheses; rather it “is a sophisticated creation, in which the events of a clinical encounter are filtered, shaped, tidied up, reflected upon, romanticized, condensed, and generally tailored to fit theoretical preconceptions, in ways that make it highly unreliable and unreplicable” (p. 162; see also Widlöcher 1994). This lack of raw data and a tendency to tailor data to fit theoretical notions appears to be a general characteristic of much of the psychoanalytic literature. Klumpner and Frank (1991), for instance, reported that of the fifteen most cited papers in psychoanalysis, not a single one included a substantial amount of clinical data. Moreover, the virtual absence of raw data in traditional case study reports makes it impossible to readers to test alternative hypotheses or interpretations. Hence, as Fonagy (2000, 2003) has pointed out, a wide disparity exists between the number of ideas in psychoanalysis and the systematic testing of these ideas.

In summary, something is fundamentally wrong in psychoanalysis with the way data are gathered and justified, and this has contributed to the negative perception of psychoanalysis by the public, and particularly by the scientific community. This has led, for instance, to the criticism that psychoanalysts are “thought readers” (Meehl 1994), a

field exemplified by endless debates settled not by systematic empirical research, but by relying on authority, persuasion, anecdote, and the selective release of data that confirm each author's favorite theory. This has led to isolation, intellectual stagnation, fragmentation, and orthodoxy. Thus, the often hailed theoretical pluralism within psychoanalysis might in part disguise a fundamental problem: the inability to give up cherished theories and to find ways of systematically evaluating them. If psychoanalysis continues to gather data and justify knowledge in ways not accepted by the scientific community, it could spell the death of psychoanalysis in this age of evidence-based medicine and managed care (Bornstein 2001; Fonagy 2000; Gunderson and Gabbard 1998). But does this mean that clinical data and the case study method are completely worthless in testing psychoanalytic hypotheses? Many, even within the psychoanalytic community, believe this is so. We believe, however, that this conclusion is somewhat premature. First, as we pointed out earlier, all "raw" data are theory-driven (as are all observations) and therefore somewhat "contaminated" by theoretical expectations. Thus, every therapeutic endeavor, like every empirical investigation, regardless of the therapist's (or investigator's) orientation, is based on theoretical conceptions. One powerful example of the way data can be contaminated by theoretical expectations is the finding that the investigator's theoretical or clinical allegiance is highly associated with the results in psychotherapy studies. Luborsky et al. (1999), for example, found that about 70 percent of the variability in effect sizes reported in outcome studies could be attributed to researcher allegiance to a particular therapy. In other words, the researcher's theoretical persuasion frequently leads to better outcomes for his or her favored treatment (possibly by delivering the treatment with more enthusiasm and conviction) than for the other treatments being evaluated.

Hence, dismissing clinical data completely on the basis of the argument that these data may to some extent be contaminated is not justified. As Forrester (1997) has argued, such a view would be based on an unreachable "ideal of purity," namely "the notion that science must have pure materials to work with—much as analytical chemistry is based on the purity of the materials used for testing and experimenting" (p. 223).

In addition, systematic empirical research has contradicted Grünbaum's assertion that clinical data are *fundamentally* contaminated

by suggestion and theoretical expectations and are thus completely worthless in testing scientific theories (see, e.g., Luborsky 1986; Miller et al. 1993; Shedler and Westen 2004). For instance, studies have consistently shown considerable similarity in the typical relational pattern or Core Conflictual Relationship Theme (CCRT) measured in the psychoanalytic situation and before and outside it. In addition, clinical data allow the predicting of a wide variety of extraclinical variables (Luborsky 1986). For example, Shahar et al. (2004) found that patients' self-critical perfectionism disrupted the development not only of a good therapeutic alliance, but also of social relationships outside treatment, thus demonstrating the parallel between processes occurring both inside and outside the clinical situation. Shedler, Westen, and colleagues (for an overview, see Shedler and Westen 2004) have shown that clinicians may not be good at combining clinical data (e.g., combining personality characteristics to define personality disorders), but can observe characteristics of patients, whether manifest (e.g., symptoms) or more latent (e.g., typical defense mechanisms) in a reliable and valid way. Interestingly, Westen and Shedler (1999) did not find any differences in this respect between therapists from different theoretical orientations (e.g., psychodynamic, cognitive-behavioral, biological). An even more interesting finding was that clinicians, regardless of theoretical orientation, could rate patients reliably on psychodynamic concepts such as defense mechanisms, if these psychodynamic concepts were described in a clear, jargon-free style.

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Thus, these findings from systematic empirical research show that clinical data need not necessarily lead psychoanalysts into an epistemological swamp (see also Lothane 1999). Clinical data cannot be dismissed as totally invalid, but can lead to important theoretical and therapeutic discoveries and insights (for an overview, see Lambert and Ogles 2004), especially if systematically studied using questionnaires, observational research (e.g., videotaping and coding sessions), psychological assessment instruments like the Rorschach, the Thematic Apperception Test (TAT), and the Object Relations Inventory (Blatt and Auerbach 2003), and evaluation procedures based on psychodynamic concepts like the Social Cognition and Object Relations Scale (Westen 2002b), the Mutuality of Autonomy Scale (Urist 1977), and the Concept of the Object on the Rorschach (Blatt and Auerbach 2003). Q-sort methodology (Jones 2000; Westen and Shedler 1999), to be discussed below, is another useful tool.

But is there still a place for the case study method in this story? Many seem to believe there is not. According to Holt (1984), for instance, psychoanalysts have to accept “that their primary and typical form of research, the uncontrolled case study, is devoid of scientific value *except* as a source of hypotheses” (p. 13, cited in Masling and Bornstein 1996, p. xii; see also Michels 2000). Masling and Bornstein (1996) go on to say that despite this “there should be no confusion about the relative contribution of experiment and case history method: relatively few truly creative ideas have originated in laboratory studies of personality. The raw material of psychoanalytic thought, the good stuff, has come primarily through the analyst-patient interaction and the case method of presenting these ideas” (p. xxii).

However, if much of the “good stuff” in psychoanalysis comes from the case study method, why should we consider it as suited “only” for the purpose of generating hypotheses? Rather, shouldn’t we try to improve this method, so that it becomes more scientific? It seems that dismissing the traditional case study method is like throwing out the proverbial baby with the bathwater. Research over the last decades has shown that the traditional case study method can be adapted to confirm to appropriate scientific standards, and thus play an important role in empirical research on psychoanalytic hypotheses (Britton and Steiner 1994). Although notable attempts have been made to develop and introduce more rigorous qualitative (as well as quantitative) case study methodology into psychoanalysis (e.g., Boston Change Process Study Group 2005; Edelson 1984, 1988; Fonagy and Moran 1993; Fridhandler, Eels, and Horowitz 1999; Hauser, Golden, and Allen in press; Kächele, Eberhardt, and Leuzinger-Bohleber 1999; Messer and McCann 2005; Pole and Jones 1998; Horowitz et al. 1993; Tuckett 1994; Wallerstein 1986), relatively little use has been made of these developments. This is remarkable for at least two reasons. To begin with, as we have noted, if many psychoanalysts believe that the case study method is the most appropriate way to investigate psychoanalytic theories, why have they not made greater use of these developments in case study methodology? Second, developments in the methodology of both qualitative (e.g., Denzin and Lincoln 1994; Forrester 1996; Miles and Huberman 1994; Yin 1989) and quantitative case study research (Bailey and Burch 2002; Kazdin 2003) have resulted in the increasing use of case study methodology and qualitative research in general in other branches of psychology, including clinical psychology and

psychiatry (e.g., Crawford et al. 2002; Elliott, Fischer, and Rennie 1999; Fossey et al. 2002; Hauser, Golden, and Allen in press). The fundamental difference between this *controlled* case study methodology and the traditional *uncontrolled* case study method is that the former (and good qualitative research in general) uses a rigorous design, which includes clear hypotheses, a good description of the methodology used (e.g., participants, procedures, data collection procedures, analysis methods), and a clear separation of results from their interpretation. Although controlled case study methodology holds strong promise for psychoanalytic research, especially for those who believe that other methods do not do justice to psychodynamic hypotheses, it has been rarely used.

### **AN ILLUSTRATION: Q-SORT METHODOLOGY**

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However, as noted, there are important exceptions. For example, one promising methodology for rigorously studying both individual cases and groups is the Q-sort. Basically, this methodology uses a set of descriptions printed on separate cards, which have to be sorted into piles by clinical judges. Hence, Q-sort methodology need not rely on self-reports from patients; often it relies on expert judgments. The items of the Q-set can vary depending on the research question. Jones (2000), for instance, developed the Psychotherapy Process Q-set (PQS), consisting of 100 descriptions reflecting a wide variety of treatment processes. Using the PQS, Ablon and Jones (2005) showed not only that there was high agreement among expert psychoanalysts concerning what constitutes an ideal psychoanalytic process if they used the PQS, but also that this prototype of an ideal psychoanalytic process was different from the prototype of ideal cognitive-behavioral process, lending support to the idea that psychoanalytic process is distinct. In addition, Ablon and Jones (2005) found that when this prototype of psychoanalytic process was rated on actual therapy sessions, it was present more often in sessions of psychoanalysis than in analytically oriented long-term therapies and brief dynamic treatments. Further, when applied to two individual cases, the PQS also revealed unique treatment processes in each case, demonstrating the ability of the PQS to tap into both nomothetic and idiographic processes, and enabling a clinically rich description, based on systematic data, of therapeutic change in these cases (see also Jones and Windholz 1990).

Westen and Shedler (1999; Shedler and Westen 2004) similarly developed the Shedler-Westen Assessment Procedure (SWAP), a Q-sort set containing 200 statements of symptoms and attributes typical of patients with personality disorders. Westen and colleagues were able to demonstrate not only that clinicians, regardless of theoretical orientation, could describe patients in a reliable and valid way using the SWAP, but also that these descriptions provide a clinically more relevant and theoretically more valid way of classifying personality disorders than the DSM approach. Further evidence for the clinical importance of research with the SWAP has come from studies showing that patients with eating disorders form three distinct personality clusters, each with a different course and prognosis. Significantly, Westen and colleagues showed that therapists used cognitive-behavioral or psychodynamic interventions more or less often, depending on the personality type of the patient they were treating (for an overview, see Shedler and Westen 2004). Like the PQS, the SWAP may be used both in group research and in individual cases (Shedler 2002). Josephs et al. (2004), for example, described a single case study of therapeutic change in a female schizoid patient who had been in analysis for more than thirty years, using assessments of clinical change by the treating therapist, patient self-report, and independent systematic analysis of transcripts of audiotapes of sessions over a four-year period in the treatment. This study also included the SWAP. Josephs and his colleagues showed how the SWAP ratings of this patient evidenced therapeutic progress during treatment in various areas of personality functioning. For example, she became less depressed, was less emotionally dysregulated, and showed fewer dependent masochistic features, which was confirmed both by self-report and by the treating analyst, hence demonstrating that even patients with severe character pathology may make clinically significant improvement. Another recent single case study of the psychodynamic treatment of a female borderline patient (Lingiardi, Shedler, and Gazillo 2006) demonstrates the ability of the SWAP to rigorously capture changes that are considered typical of psychoanalytic treatment with these patients (enduring changes in ways of relating to others, impulse regulation, etc.), that are hard to capture with traditional self-report measures.

Over the years, similar nomothetic measures have been developed to tap both content and various structural aspects of personality functioning, measures that can be used in both nomothetic and idiographic

investigations (Blatt and Auerbach 2003; Huprich and Greenberg 2003). Some of these measures can be used with data from different sources including self-report, clinical interviews, narratives, projective techniques (e.g., the Rorschach, the TAT, and stories told to the picture arrangement subtest of the Wechsler Adult Intelligence Scale–Revised), early memories, transcripts of psychotherapy sessions, and even responses to experimental stimuli.

The use of these measures in controlled case study research or “case study plus” methodology (Josephs et al. 2004) illustrates how psychoanalytic researchers can adopt and adapt existing research methodologies, and develop new research methods, to study the complexity of psychic life in clinically relevant ways. In particular, such studies are likely to lead to a further realization that studies using quasi-experimental methods should focus more on meaning and interpretation (Hauser, Golden, and Allen in press), and that clear rules for interpretation in psychoanalysis should be developed because, despite many theoretical efforts (e.g., Edelson 1988; Ricoeur 1965; Rubovits-Seitz 1998; Siegel, Josephs, and Weinberger 2002), little consensus has been achieved in that area. Because the absence of such clear rules has often been considered the Achilles’ heel of psychoanalysis, future research should be directed to developing guidelines for assessing the validity of interpretations.

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### **BRIDGING THE GAP: METHODOLOGICAL PLURALISM AND THE FUTURE OF PSYCHOANALYSIS**

To summarize, we believe that no single method is “uniquely suited” to test psychoanalytic hypotheses. Neither the traditional case study, nor quasi-experimental or experimental methods, can fill this role. Instead, in line with many other investigators (e.g., Alvarez 2000; Ablon and Jones 2004; Fonagy and Moran 1993; Leuzinger-Bohleber and Bürgin 2003; Hauser, Golden, and Allen in press; Rustin 1989), we believe that the future of psychoanalytic research lies in methodological pluralism. Studies using different methodologies, ranging from  $N = 1$  studies, multiple case studies, and the study of narratives, to questionnaire research, observational research, and experimental studies, can all potentially contribute to approaching the complexity of psychoanalytic hypotheses and, ultimately, of human nature. Thus, instead of considering one method as alone suited to test psychoanalytic hypotheses, or adopting one particular methodology (e.g., experimental science), psycho-



analysts should play a role in the development of methodologies that facilitate investigation of the complex clinical reality. For example, methods such as growth curve modeling and survival analysis facilitate the modeling and testing of both idiographic and nomothetic trajectories over time (see, e.g., Willett, Singer, and Martin 1998). In addition, methods that allow for a more detailed investigation of complex dynamic processes, such as experience sampling (Corveleyn and Luyten 2005) and the study of narratives (Main 2000; Hauser, Golden, and Allen in press), might be used and further developed by psychodynamically oriented researchers.

Methodological pluralism might also bridge the gap between the two cultures in psychoanalysis. The divide between interpretive and neopositivistic cultures within psychoanalysis is not only unfruitful and unproductive, it is also, to a large extent, false. Any type of research, but especially research on aspects of human nature, involves interpretation and meaning, just as any type of research should include a process of systematic testing and falsification, no matter the methods used. Idiographic and nomothetic approaches should also not be seen as opposite perspectives, as is currently often the case; instead they can be viewed as complementary, because ultimately they share a common aim—both seek to understand human nature. As demonstrated by several psychoanalytic researchers (e.g., Ablon and Jones 2004; Hauser, Golden, and Allen in press), nomothetic findings should always be tested at the idiographic level and vice versa. Such an exchange between the idiographic level, which aims at assessing, understanding, and treating individuals, and nomothetic research, which aims at discovering probabilistic laws or “master narratives,” can only lead to better theories, a better understanding of individual patients, and thus a gain for everyone involved. In particular, “master narratives” or general trends that have been identified in nomothetic research can be refined or qualified in idiographic research and then reevaluated in group designs. As noted, several methods have already been developed that can be used in both group studies and individual cases, but more work is needed in this area.

The interpretive and neopositivistic cultures within psychoanalysis are complementary in yet another way. Each provides a basis for bridging the gap between psychoanalysis and other disciplines. The interpretive culture is the bridge to the humanities, while the neopositivistic culture is the bridge to the natural sciences. Thus, methodological

pluralism implies an openness to research and theory from other theoretical and methodological perspectives, including, but not limited to, linguistics, philosophy, developmental psychopathology, cognitive-behavioral research, the neurosciences, and psychiatric genetics (Beutel, Stern, and Silbersweig 2003; Fonagy 2003; Hauser 2004; Luyten in press; Mayes 2003). As Fonagy (2003) has put it, “The mind remains the mind whether it is on the couch or in the laboratory” (p. 220).

Why, then, has psychoanalysis not adopted methodological pluralism, and instead remained largely divided into two cultures? Many forces are at work here. First, especially as psychoanalysts, we need to be aware of the psychological forces—our personal preferences and dislikes—that maintain this divide. As noted earlier, these two cultures are relatively isolated, and, as in all human interactions, processes of both idealization and denigration can be observed in how the two cultures depict themselves and each other. Moreover, the prospect of having to give up cherished ideas, an inevitable correlate of research and dialogue with individuals of other persuasions, may engender fear—in clinicians that research will increasingly intrude on their “old ways,” and in researchers that reverting to methods other than quasi-experimental designs risks losing the hard-won and still precarious respectability of psychoanalysis as an empirical science. The intense, highly complex, and often philosophical nature of this debate, which many clinicians consider merely academic, might also play a role. In addition, issues of power, politics, and economics are involved. Many researchers hold academic positions and must struggle for survival in an environment dominated by “hard” science; in a time when neuroscience and evidence-based medicine dominate psychiatry, it can be hard to “out” themselves as interested in more clinical, qualitative, and interpretive methods. Although the pendulum may have started to swing toward these “softer” approaches, investigators still experience pressure to obtain research funds and academic recognition. As for practicing clinicians, many have had little exposure to research in their training and have built a professional identity around a model that emphasizes meaning, interpretation, the study of individual cases, and supervision as methods of scientific research. Moreover, they feel threatened by managed care and evidence-based medicine, which seems based on research that does not fully appreciate the complexities of clinical reality.

Hence, the inclusion of psychoanalytic research in psychoanalytic training programs, the creation of research funds, the inclusion of clin-

icians in funding agencies, and the establishment of practice research networks consisting of both clinicians and researchers (see, e.g., Westen and Shedler 1999) are only the first steps that must be taken in getting these two cultures on “talking terms” and bridging the gap.

### CONCLUSIONS AND PROSPECTS

What can be learned from this overview of the current debate concerning the role of empirical research in psychoanalysis? First, we believe that this debate shows that psychoanalysis is flourishing, despite the many claims of its impending death. The last decades, in particular, have witnessed a considerable increase in well-conducted empirical studies of psychodynamic theories. Although much more research is needed, psychoanalysis is responding to its many critics and assuring its future both as a theory and as a form of treatment. Further, much of this recent research is clinically relevant and has changed theoretical conceptions and clinical practice in various ways and has increased the scientific status of psychoanalytic theories and the effectiveness and credibility of its treatment modalities in the scientific community (Bateman and Fonagy 2004; Gabbard, Gunderson, and Fonagy 2002; Leichsenring, Rabung, and Leibing 2004).

At the same time, psychoanalysis will have to resolve some important issues concerning empirical research. Debate still rages within the psychoanalytic community regarding the kind of research methods that are most suitable to investigate psychoanalytic hypotheses. In particular, the psychoanalytic community appears to be divided to some extent into two different cultures, each with its own view of the nature of psychoanalysis and psychoanalytic research. As our overview demonstrates, the method most cherished by many psychoanalysts, the traditional case study method, is clearly not always the most appropriate method. This does not mean, however, that the case study method has outlived its usefulness. Traditional case studies should continue to play an important role in the generation of hypotheses and in the teaching of psychoanalytic concepts. In addition, *controlled* case studies as compared to the traditional *uncontrolled* case study hold particular promise for psychoanalytic research. Controlled case study methodology avoids many of the methodological pitfalls of uncontrolled studies while still focusing on individual cases, and thus may do more justice to the complexities of individual cases. As Fonagy and

Moran (1993) have pointed out, case study methodology may provide access to “unique data that may not be accessible outside of this long-term, intimate, and confidential relationship” (p. 62). Case studies also provide the opportunity to present exemplifications, qualifications, or exceptions to nomothetic principles identified in nomothetic probabilistic research, and thereby show the way to more refined empirical investigation.

Our overview of the debate about empirical research in psychoanalysis reveals the many dangers that attend the shift toward systematic research that psychoanalysis has witnessed in recent decades. In an attempt to test the validity of psychodynamic hypotheses, systematic research of psychoanalytic hypotheses risks falling into simplistic conceptions of psychoanalytic notions and research methods that do not do justice to their complexity. Concerns have been expressed that the pendulum now could swing too far in the other direction. As Pick (2000) has pointed out, “The current desperate rush to survive by achieving ‘respectability’ may, in fact, jeopardize the very specific and very powerful capacity for research that psychoanalysis has in fact demonstrated and that it continues to embody” (p. 118). The danger is that methodology “conceived originally as a means to the end of scientific knowledge . . . may come to be an end in itself” (Mishler 1979, p. 6). This would indeed lead to research “about nothing very much,” which is hardly relevant for clinicians. Erdelyi (1994) has convincingly argued that the opposite should happen. Psychoanalysis should contribute to the development of new and more complex research methodologies that do greater justice to the complexity of psychodynamic thinking: “When experimental psychologists want to get more serious about truly complex and emotionally invested psychological phenomena . . . they will have to go to the clinic—or they will have to study their subjects in their laboratories for hours, weeks, and months, which is tantamount to transforming the hit-and-run lab into the clinic” (pp. 677–678). In this context, we would like again to enter a strong plea for a methodological pluralism that integrates adaptations of existing research methods with the development of new research methodologies, ranging from controlled case studies to quasi-experiments to epidemiological and neurobiological studies (Bornstein 2005). Hence, we agree with Wallerstein (2000) that “the challenge for psychoanalytic research is to accomplish its necessary work by methods devised in such a way as not to do violence to the nature, or ‘spirit’, of the enterprise being

studied” (p. 29). In order to do that, the current divide within psychoanalysis needs to be bridged, and proponents of both cultures need to begin to respect each other and discuss what future psychoanalytic research should look like. This reconciliation could not only end our “not-so-splendid isolation” from other branches of science (Fonagy 2003), but also our isolation from each other within psychoanalysis (see Levy 2004). In the end, both cultures within psychoanalysis must come to realize that much more is to be gained from dialogue than from opposition, and from complementarity rather than competition and conflict.

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