
Group Therapist as Social Scientist, With Special Reference to the Psychodynamically Oriented Psychotherapist

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The recent literature suggests a lowering of tensions between psychotherapy practitioners and researchers in the decades-long “psychotherapy war.” The author analyzes reasons for the thawing and suggests that the time is ripe for clinicians to assume greater authority in the work of developing models of the psychotherapy change process. The article highlights the unique positioning of the psychodynamic group psychotherapist for advancing this work.

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Observers have long noted the tensions between the practice and formal empirical study of psychotherapy, tensions fueled to a large degree by the inherent clash of cultures between these two enterprises (Newton & Levinson, 1973). This decades-long “psychotherapy war,” while long-simmering, dramatically escalated in the 1990s, fueled by sociopolitical forces, including the rise of managed care with its increasing demand for accountability, and culminated in the emergence of evidence-validated, or what is now termed *evidence-based*, practice lists (Chambless et al., 1996, 1998; Task Force on Promotion and Dissemination of Psychological Procedures, 1995). As Wachtel (2010) has posited, the implication in these listings that was most irksome for the practitioner was the hegemony of the randomized controlled trial (RCT) as the exclusive, or at least premier, scientific methodology for determining the efficacy of treatments and dictating what forms of psychotherapy are acceptable (i.e., scientifically respectable and reimbursable via insurance companies) and what forms are not. Other sources of knowledge, especially the wisdom gained from accumulated clinical experience, were clearly disempowered by these documents and regarded merely and disparagingly as “anecdotal evidence.”

What’s Wrong With the RCT?

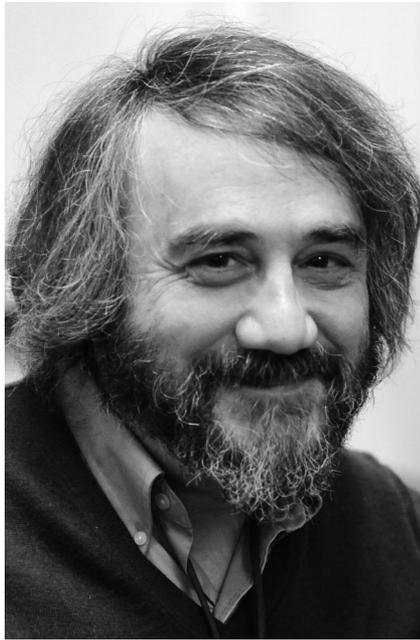
Such a divisive message understandably evoked a vehement backlash from highly regarded clinical scholars, a bombardment of critiques that challenged the supremacy of the RCT from all angles—methodological, clinical, and theoretical. Paradoxically, the RCT design has been sharply

criticized on methodological grounds both for its overriding preoccupation with maximizing internal validity, on the one hand, and for its lack of sufficient internal validity to justify the typical conclusions that are drawn from such studies, on the other hand. With regard to the former concern, critics have pointed out that the very strategies used to strengthen internal validity—particularly the sanitizing of the clinical samples to weed out error-variance-enhancing comorbidity (usually executed, we would add, by clinically unsophisticated research assistants using obvious, nonsubtle questionnaires and checklists with their inherent problems of response biases) and the drawing up of rigid, lockstep treatment manuals to ensure that technicians-as-therapists perform in a standardized manner—have weakened external validity and hence the relevance and meaningfulness of the research to the practitioner. As Westen, Novotny, and Thompson-Brenner (2004) stingingly noted, “There can be no more powerful way to create a gulf between clinical practice and research than to compare laboratory-derived interventions with everything but what clinicians practice in the community” (p. 641).

Many other criticisms beyond this fixation on internal validity have been leveled against what has been disparagingly dubbed the “horse race” mentality of the vast majority of comparative RCT psychotherapy studies that pose the narrow question of which treatment package is the winner. Early comparative designs pitted the treatment approach to which the researchers were wedded against scientifically weak alternatives such as wait-list controls, information-only conditions, so-called treatment-as-usual conditions, or intent-to-fail conditions, which knowingly reflected the suboptimal, catch-as-catch-can eclectic work of harried clinicians, or “non-bona fide . . . treatments that do not control for obvious confounds (e.g., therapist enthusiasm, patient expectancy, and common factors)” (Westen, Novotny, & Thompson-Brenner, 2005, p. 429). The weak hypotheses (Edelson, 1985) embedded in these designs clearly provided an odds-on and no-brainer superiority for

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the researchers' prized treatment but hardly advanced understanding and, worse, cast a pall of skepticism or cynicism over the claimed efficacy of the winning treatment model, as reflected in the witty and telling coining of the term *allegiance effect* by Luborsky et al. (1999). As Westen et al. (2005) asserted,

If researchers compare an experimental treatment they believe to be state of the art with any treatment *not* intended by competent clinicians administering it to be the best treatment *they* have to offer [i.e., any of the weak alternatives described above], the only inference they can draw is that a treatment intended to succeed is superior to a treatment that is not. Any other conclusion is pseudoscience. (p. 430)

But even the newer RCT studies with more legitimate and credible active comparators still levy a host of methodological and scientific challenges. We are repeatedly reminded that the most one can state in the context of a statistically significant difference is that *something* in the superior experimental condition as a whole was causative; scientific logic does not permit the conclusion, often drawn or certainly implied, that it was the touted technical package per se that made the difference. Whether the causative agent was the treatment package as a whole, a component of the treatment package, nontechnical, social-psychological factors such as the mere credibility (remember President Obama's "change you can believe in" campaign motto? [Nakamura, 2011]) of the treatment or persuasiveness of the therapist as perceived by the patient, or interactions among these variables is usually not determinable from the standard RCT. Further, as numerous critics (Borkovec & Miranda, 1999; Krause & Howard, 1999; Westen et al., 2004) have pointed out, efforts to parcel out the so-called active ingredients in the treatment package, either

through dismantling and additive experimental designs or through multifactorial designs to further control or study potential confounding variables, are doomed to fail in the long run; the careful designing and implementation of such tedious follow-up RCTs are simply too time-consuming and cannot keep pace with the ongoing generation of new clinical-theoretical hypotheses, a situation that Krause and Howard (1999) posited merely leads to the development of further "long-shot" treatment packages that also will have inadequate formal testing. And, of course, since not all the confounding variables that might be causally linked to outcome are adequately controlled even through randomization (cf. Krause & Howard, 1999), the faith that a significant finding will likely be significant upon replication with *all things being equal* is just a matter of faith, not science.

Beyond these methodological problems, critics have also argued that the anointing of the RCT as the *de rigueur* means for gathering clinical evidence is "premature" and "prescientific" in terms of advancing conceptualizations of how people change in psychotherapy. The typical pre-post analysis of variance methodology that analyzes differences averaged over groups of people is simply not capable of addressing such theoretically and clinically core issues as "the optimal timing of an intervention, the mechanisms underlying the effectiveness of the intervention, and for whom such an intervention would be most beneficial" (Pachankis & Goldfried, 2007, p. 761). Further, by focusing on the ideologically steeped and jargon-laced treatment packages, rather than on broad, transtheoretical principles and processes as critics have advocated, the prototypical pre-post outcome designs simply cannot address what optimizes a treatment intervention for any individual patient. As eloquently put by McKinley (2011),

Probabilistic generalizations are purchased at the price of leveling down the complexity of experience by only revealing that which is common across individuals. Admittedly, the aim of experimental studies is to yield nomothetic claims, but what about those individuals who do not trend with the statistical norm? . . . This methodological limitation seems at odds with what is pertinent to practitioners who seek to understand the unique qualities, the idiosyncratic meanings, and the specific contexts of their patients' lives. (p. 28)

When it comes to group psychotherapy outcome research, the methodological problems inherent in RCT designs multiply primarily because of the failure to take into account the nonindependence of group-level data but also because of the heretofore statistical inadequacy in accurately handling missing longitudinal data, as when members drop out or join ongoing, open-ended groups, a practice that characterizes the kinds of groups run in most mental health clinics and private practices. While some recommendations for addressing these statistical challenges have been offered in the past few years (Baldwin, Murray, & Shadish, 2005; Morgan-Lopez & Fals-Stewart, 2007; Tasca, Illing, Ogrodniczuk, & Joyce, 2009), very few studies in the extant literature have incorporated them into their data analyses. Baldwin et al. (2005) compellingly demonstrated that the failure to take into account the de-

pendence in data generated from groups can significantly increase Type I errors. In their reanalysis of the 33 group therapy studies catalogued in the APA listing of empirically supported treatments, they found that the number of significant outcome findings “dramatically” decreased following their estimates of the dependence in the amassed data. Our own inspection of the 17 psychodynamic group studies recently listed in the quality review of RCTs by Gerber et al. (2011) not surprisingly revealed that none of the studies took seriously the violation of the assumption of independence of observations. This flaw is not only statistically naïve but clinically ironic given that these analyses ignore the very construct considered by many psychodynamic theorists (Bernard et al., 2008) to be a key element of therapeutic change, namely, those therapeutic (and antitherapeutic), deindividuating, group-level processes that are responsible for the nonindependence of data! Short of not doing group therapy research, a dismal recommendation by Baldwin et al. (2005) that surely is not a solace to the group therapist, group researchers will need to begin to model their data using the more sophisticated techniques of multilevel growth models if they want their studies to be regarded with greater credence.

A Warming Trend

Fortunately for the mutual advancement of both research and practice domains, recent signs of accommodation are beginning to appear. Not the least significant sign of this softening of positions is a recent document from the APA Presidential Task Force on Evidence-Based Practice (2005) that explicitly broadened the definition of what is to be considered legitimate evidence by its inclusion of clinical observation and case studies. This far more encompassing framework seems expressly designed to supplant the narrow, some would say dismissive (Wachtel, 2010), view taken by the task force of the clinical psychology division (Division 12) of the American Psychological Association (APA) a decade earlier (Task Force on Promotion and Dissemination of Psychological Procedures, 1995).

And, indeed, some critical observers of trends in psychotherapy research confirm that a new generation of research (Pachankis & Goldfried, 2007) seems to be emerging, research that more sensitively balances the needs for both internal and external validity (i.e., efficacy but also clinical utility) and that addresses at least some of the critiques leveled against it. This perceptible shift seems intended not to do away with the RCT—no one is seriously arguing for that—but rather to modify some of the more clinician-unfriendly elements, on the one hand, and to augment and complement it with other useful empirically rooted strategies, on the other hand. One of the more notable changes is the evolution of treatment manuals. Rooted in the scientific logic to develop measurable and standardized interventions and thus allow for the assessment of adherence by clinicians to those standards, manuals have evolved from lockstep rules for therapists-as-technicians to more clinician-friendly, principles-focused guidelines. These newer versions leave considerable room for clinical judgment and, more than this, are seen as being

clinically useful in distilling the essence of a particular treatment; that is, they serve a clinical-educative function in addition to the research mission. With respect to psychodynamic group psychotherapy, as with other specific modes of treatment, some of the earlier, more simplistic and bare-bones efforts (cf. Getter, Litt, Kadden, & Cooney, 1992) are giving way to more theoretically and clinically sophisticated guidelines that allow for idiographic response to the exigencies of the clinical context. The two manuals that show the greatest promise of generalizing beyond the specific pathologies for which they were originally developed are those constructed by Tasca and colleagues (Tasca, Mikail, & Hewitt, 2005), originally designed for application to patients with binge eating disorders, and by Piper and colleagues in their ongoing work for patients suffering from complicated grief (Piper, McCallum, & Joyce, 1995). As pedagogic instruments, these latter guidelines offer a complex, deep, and integrative understanding of the small therapy group, drawing on conceptualizations from intra-individual, interpersonal, and group-as-a-whole perspectives.

But more than such modifications to the methodological orthodoxy of RCTs that aim to move laboratory studies closer to the “real world,” there is a crescendo of responses to APA’s call for a broader definition of what constitutes empirical evidence in the assessment and evaluation of psychotherapy. Most recently, Dattilio, Edwards, and Fishman (2010) argued persuasively for no less than a paradigm shift, a mandate for an integrated package of methodological approaches to study psychotherapy that includes both quantitative and qualitative methods, experimental and quasi-experimental strategies, approaches that would allow for the development of both nomothetic, universal cause-and-effect laws and idiographic, context-specific knowledge of limiting and modifying factors that serve to translate the general into the particular. Dattilio et al. pointed out that the combination of approaches that includes RCT group comparison and single case studies is already being applied in other fields, such as program evaluation, and that the inherent complementarity between these approaches provides a kind of checks-and-balances insurance against drawing either too sweeping or too shortsighted conclusions.

Dovetailing with this call for research pluralism are methodological and conceptual developments in three overlapping areas: the case study, the practice research network, and the dual role of clinician-researcher. The most prominent of these is the resurgence of interest in the case study, as best manifested by the recent founding of two journals, *Clinical Case Studies* (Hersen, 2002) and *Pragmatic Case Studies in Psychotherapy* (Fishman, 2005). Importantly, the editors of these new journals, and others (Borckardt et al., 2008; Dattilio et al., 2010), are calling not just for case descriptions and analyses that follow their newly invigorated and explicated rules of logic and epistemology but for the construction of databases, similar to the long-term goals of practice research networks (cf. Borkovec, Echemendia, Ragusea, & Ruiz, 2001). Following Iwakabe and Gazzola (2009), the amassing of cases

replete with comprehensive coded parameters—which, with respect to psychodynamic group psychotherapy, should include client characteristics (e.g., psychological mindedness; defensiveness, character style), structural parameters of the group, technical interventions (e.g., dosage of interpretations), and in-session hindering or facilitating events (e.g., ruptures in the working alliance; transference-countertransference enactments)—could establish an integrative database; such data could then permit fine-tuned comparisons, such as between successful and unsuccessful cases or even between optimally successful cases and cases of only average success. These case-wise comparisons would seem to be particularly relevant to the clinician since, unlike the RCT design that looks at aggregated responses and thus obscures individual performances (cf. Krause, 2011), comparative case studies can help generate ideas about what contributes to optimal or subpar outcome in every instance. That is, these kinds of analyses could inductively facilitate the generation of sophisticated hypotheses of increasing complexity about sequences of variables that influence outcome, hypotheses that could, in complementary fashion, be more formally tested in RCTs. In reframing the clinical situation as a “natural laboratory” for discovery, Westen et al. (2004) reminded us that “many, if not most, of the major clinical innovations in the history of our field have come from clinical practice” (p. 656). Or, as Kazdin (2008) stated it,

Our field would profit enormously from codifying the experiences of clinicians in practice so that the information is accumulated and can be drawn on to generate and test hypotheses. There is no need for clinicians to become researchers and to do complex data analyses. *Yet clinicians already are researchers in the sense of hypothesizing that a particular treatment combination will have particular effects and testing this hypothesis with the individual case [emphasis added].* This work is not evaluated, codified, and accumulated in an archival way and therefore is lost. (p. 155)

What Westen, Kazdin, and others are calling for is a formalization of clinical discoveries, often implicit and unreported, through such means as the written case study and the establishment of clinical databases. Flyvbjerg (2006) offered perhaps the strongest case yet for viewing idiographic approaches such as the case study not merely as a hermeneutic exercise or the crucible for generating hypotheses to be tested in large-scale, quantitative studies but as a legitimate scientific mode in its own right. He provided compelling arguments that counter the conventional wisdom about the perceived inherent limitations and second-class status of the case study, such as the misunderstanding that general, theoretical (context-independent) knowledge is more valuable than concrete, practical (context-dependent) knowledge; that one cannot generalize on the basis of an individual case; that the case study is most useful for generating, but not testing, hypotheses; and that the case study contains a unique bias toward confirming the author’s preconceived notions. He offered compelling arguments that served to dispel these misunderstandings and gave greater weight to the scientific legitimacy of individual, context-specific methodologies.

Beyond the reinvigoration of the case study and the still fledgling development of the practice research network, works that promisingly involve teams of clinicians and researchers in collaborative effort, the literature hints at the increasing legitimacy, albeit with complications and challenges, of the dual role of the clinician-researcher embodied within one individual (Yanos & Ziedonis, 2006). I recently reported (Greene, 2008) on how such a role, in essence a renewal of the Boulder scientist-practitioner model of professional training, can serve the group therapist not only in developing heuristic hypotheses about cause–effect relationships at the end of therapy but also in correcting a treatment trajectory that is going awry.

One core value pervading this apparent paradigm shift in clinical research is a focus on process, which in this article is loosely equated with antecedent variables, that is, all that precedes outcome that affects and effects therapeutic change (cf. Morrow-Bradley & Elliott, 1986). Methodologically, this means identifying and/or investigating those variables that might predict outcome or moderate or mediate cause–effect relationships. Recently there has been an intensifying call for incorporating moderator and mediator analyses within the standard RCT study (Frazier, Tix, & Barron, 2004; Kazdin, 2007; Kraemer, Wilson, Fairburn, & Agras, 2002). Frazier et al. (2004) argued persuasively that “it is a sign of a maturing discipline when, after direct relations have been demonstrated [i.e., efficacy], we have turned to explanation and theory testing regarding those relations” (p. 116). The study of moderators—those variables such as patient characteristics that alter the strength of a main effect—and mediators—those variables that offer understandings of how a main effect dynamically comes about—should have an appeal for the clinician because they help particularize treatment considerations in every clinical situation. In related fashion, these kinds of “discovery” analyses also serve to temper and tone down research conclusions that may often be experienced by therapists as too broad or sweeping and thus can reduce their skepticism or cynicism regarding laboratory-generated findings.

Clinician Meets Research

Whether it is due to this more encompassing view of research and the overthrowing of the empirical imperialism of the RCT as the only legitimate means for establishing evidence, or to other factors such as the capitulation by clinicians as they face the unrelenting sociopolitical pressures to conduct evidence-based practice, or to the rise of a more computer-savvy generation of young clinicians who find the marriage of research and practice less inherently conflictual and problematic, there are a host of signs that clinicians of all stripes (not just cognitive-behavioral therapists, whose approach to treatment has embraced a science-practice model from the very start) are beginning to show genuine investment in research participation. Even with regard to psychodynamically oriented therapies, empirical efforts, once eschewed as being impractical or irrelevant for capturing the nuances, complexities, and subtleties of the therapy situation, are now being invested in

with rigor and clinical relevance (Gerber et al., 2011; Levy & Ablon, 2010; Shedler, 2010).

Supporting Westen et al.'s (2004) recommendations that researchers develop their investigations in close proximity to actual clinical settings and use methods that systematically assess process–outcome patterns, clinicians seem more receptive to research when they are treated as genuine collaborators in bottom-up approaches that generate meaningful clinical hypotheses and that lead to the designing of clinically relevant studies (Castonguay et al., 2010; Lau, Ogradniczuk, Joyce, & Sochting, 2010; Ogradniczuk, Piper, Joyce, Lau, & Sochting, 2010). These subtle signs of change in the longstanding antagonism between researcher and clinician suggest that the tensions may have been driven as much, or more, by latent authority dynamics as by overt content issues (i.e., the perceived irrelevance of the RCT for clinical practice).

The Case of the Psychodynamic Group Psychotherapist

Given these early signs of change in the psychotherapy research landscape, I propose the thesis that the psychodynamic group psychotherapist is in an ideal position to provide practice-based evidence, as much as conduct evidence-based practice. This thesis that the group practitioner can uniquely contribute to clinical science, as I shall argue, is born of the confluence of two additional postulates: the inherent interest in process variables on the part of psychodynamic psychotherapists and the unique vantage point of group work for offering insights into mediators and particularly moderators.

Psychodynamic Group Therapy Outcome Research, Where Art Thou?

Clearly, research in psychodynamic group therapy, whether case study, quasi-experimental field study, or laboratory-based RCT, is relatively scarce. It lags far behind its two chief comparators, cognitive-behavioral group therapy (GCBT), on the one hand, and individual psychodynamically oriented therapy, on the other. This dearth of scientifically acceptable evidence derives from several sources, arguably primarily from the longstanding resistances of psychoanalytically oriented therapists to research efforts (Busch & Milrod, 2010) and to the greater complexities of group life as conceptualized from a dynamic framework, particularly when compared with the reductionistic model of GCBT, which, until most recently, has tended to ignore considerations of group-level process variables. Sadly, the field is shunned to some degree by both of its neighbors, largely on the basis of ideology and myth. Even such highly touted reviews as Shedler's (2010) fail to separate the evidence for psychodynamic group work from its individual therapy counterpart, just as reviews of the group outcome literature fail to sort psychodynamically oriented work from other schools of group therapy (Burlingame, Fuhrman, & Mosier, 2003; McDermut, Miller, & Brown, 2001). As Tschuschke, Anbeh, and Kiencke (2007) recently concluded, "There is still a huge gap between psy-

chodynamically oriented or psychoanalytically-practiced clinical groups and research aiming at evidence-based grounds [sic]" (p. 142). Indeed, I have not found psychodynamic group psychotherapy documented as an evidence-based treatment either on any of the established databases, such as Cochrane's, the Substance Abuse and Mental Health Services Administration's National Registry of Evidence Based Programs and Practices, and the APA Division 12 listings, or in recent treatment reviews (Gerber et al., 2011) and meta-analyses (Burlingame et al., 2003), either because of insufficient rigor or lack of replication of the studies to date. While the research over the past several decades does suggest a trend (cf. Burlingame & Fuhrman, 1994) toward greater methodological sophistication and conceptual complexity, the overall formal research findings regarding this specific modality remain limited in terms of their providing either solid "evidence" or clinical utility.

To illustrate this critique in more detail, consider two recent effectiveness studies of outpatient psychodynamic group therapy (Jensen, Mortensen, & Lotz, 2010; Tschuschke et al., 2007). Both works are distinguished by the comparatively large number of heterogeneously composed groups as well as by the relatively high degree of experience of the multiple study therapists, two improvements over much of the extant empirical work. While a detailed description of the nature of the group work was not provided in either report, both studies explicitly stated that transference interpretation and/or confrontation of resistances were core components of the therapists' roles. From the standpoint of evidence, both of these naturalistic investigations revealed statistically significant improvement in the majority of patients, thus adding to the very slowly accumulating empirical substantiation of this treatment. But more relevant for the practitioner were the finer-grained analyses conducted by both sets of researchers to categorize patients according to level of improvement, ranging from "clearly deteriorated" to "highly successful." This is an important, albeit modest, advance in the statistical treatment of the data if for no other reason than its reaffirming of what clinicians know too well, namely, that the patients they treat can depart considerably from the "average," with some doing far better and some far worse. Unfortunately, an exploration of those factors that underlay differential responsiveness to the treatment was not pursued in either of these two outcome-only studies.

Process: Predictors, Moderators, Mediators

As implied earlier, the study of process reflects a scientific attitude that resonates closely with the prototypical clinical stance, particularly that of the dynamically oriented therapist. Three major kinds of process investigations have been conceptualized in the psychotherapy literature in general and in the psychodynamic group therapy literature in particular, namely, the study of predictors, moderators, and mediators. The primary questions driving these studies go beyond whether a particular treatment works; rather, they address what early signs predict outcome, for whom and under what conditions the interventions work, and how they work. I argue that familiarity with the scientific and

statistical logic of these kinds of investigations can aid the psychotherapist's capacity to develop complex formulations and clinical hypotheses. That is, while I agree with Kazdin (2008) that the clinician need not know the intricacies of hierarchical linear modeling nor collect quantitative data under the "illusion" (cf. Shedler, Mayman, & Manis, 1993) that they are more valid than clinical observation, the understanding of such notions as scientific logic, inference making, and conditionality are crucial to clear-headed clinical thinking.

The study of predictor variables in psychotherapy research is, in essence, the exploration of independent effects beyond the primary variable of interest, typically a treatment package. Within the group therapy literature, the most studied group-level predictor by far is cohesion, selected primarily because it has long been regarded as analogous to the therapeutic alliance in individual psychotherapy, although its predictive value (Crowe & Grenyer, 2008) and its construct validity (Hornsey, Dwyer, Oei, & Dingle, 2009) have come under scrutiny in recent years. The prototypical study of cohesion as a predictor of outcome asks whether cohesion measured in snapshot fashion in some early session or, more dynamically, as increasing over early sessions directly predicts (and, by inferential leap, causes) clinical outcomes. In a recent meta-analysis, Burlingame, McClendon, and Alonso (2011) demonstrated evidence of its direct correlational relationship to treatment outcome across the prominent schools of group therapy. The 40 studies in their review taken together yielded an overall weighted aggregate correlation between cohesion, operationally defined in multifarious ways, and outcome that was statistically significant, which suggests that this group process generally foretells the treatment outcome independent of the treatment intervention itself. In clinician-friendly terms, the correlation suggests that if there are two groups identical in all ways except for early levels of group cohesion, the one with the higher initial cohesion is likely to generate better outcomes. The investigation of predictors serves both practical and theoretical ends. Specifically with regard to cohesion, its role in the therapeutic change process beyond any effects of technique highlights the importance of considering nonspecific, nontechnical, or relationship factors, long and well understood by psychodynamic group therapists and much more recently acknowledged by their cognitive-behavioral counterparts (Bieling, McCabe, & Antony, 2006).

If the logic of the predictor analysis is to study a main effect (e.g., Is greater cohesion, independent of other variables, associated with better treatment outcomes?), the moderator analysis is essentially a study of the interaction of variables. More specifically, the moderator analysis explores whether the strength of some obtained relationship, such as the significant correlation of cohesion with outcome, is affected—enhanced or diminished—by the interactive effect of another variable. Frazier et al. (2004) pointed out that the identification of moderators "indicates the maturity of and sophistication of a field of inquiry . . . and is at the heart of theory in social science" (p. 116). Not only crucial for theory building, moderator analyses have

clinical relevance because they particularize the conditions—aspects of the situation and the person—under which an association is more or less applicable. To illustrate, Burlingame et al. (2011), extracting additional variables from the 40 studies they reviewed, were able to identify several moderators of the association between cohesion and outcome, including group duration and size and participant age: The association was stronger in groups that met for more than 12 sessions and were composed of from five to eight members than it was in groups with different structural properties. They also discovered that the cohesion–outcome relationship was stronger in groups with younger members. While intriguing, these findings are difficult to integrate into a theoretical framework largely because they were necessarily selected out of convenience and are not theory driven.

Fortunately, there are other recent studies that demonstrate the value of moderator analysis when variables are selected on the basis of theoretical relevance and substance. Perhaps most relevant to the study of cohesion in psychodynamic groups is the recent large-scale ($N = 327$) investigation by Dinger and Schauenburg (2010) on inpatient groups. They found, preliminarily, that the groups effected symptomatic change. But more than this expected finding, their regression analyses showed that mean level of cohesion endorsed by patients at 12 weeks as well as increasing levels of cohesion in early sessions also were significantly associated with positive outcome. But finally, at the heart of the study, the researchers found, as hypothesized, that these latter associations depended on patients' views of their own affiliative needs. For those who complained that they were too interpersonally distant, the experience of increasing group cohesion was linked to comparatively greater treatment outcome. Conversely, for patients who viewed themselves as becoming too close and losing themselves in interpersonal relationships, the experience of *decreased* cohesion over time led to comparatively more positive outcome. This interaction is a nice reminder for clinicians and researchers alike that there are, as aptly stated in the vernacular, different strokes for different folks, or that one size doesn't fit all, highlighting the utility of the moderator analysis (as well as posing a unique challenge for group psychotherapy).

Consistent with Dinger and Schauenburg's (2010) conclusion that "numerous other personality variables may be of importance" (p. 27) in studying moderator effects in group research are works by Piper's (Piper, Ogrodniczuk, Joyce, & Weideman, 2011) and Tasca's research teams (Tasca et al., 2006) that well epitomize the search for moderating personality constructs theoretically linked to their models of psychodynamic group therapy. In a long-term series of studies, Piper et al. (2011) examined the role of patients' level of object relations (as well as psychological mindedness) in influencing the effectiveness of their short-term model of psychodynamic group therapy. In analogous fashion, Tasca et al. (2006) studied how patients' attachment styles—avoidance and anxiety—modulated the impact of Tasca's dynamic model of group therapy. Consistent with theoretical expectation, their findings

showed that patients with more developmentally advanced levels of object relations and those with higher levels of attachment anxiety do better in these dynamically oriented groups than do those with lower scores on these variables and that they also do better than similar patients in less psychologically deep and more structured forms of therapy, such as supportive group treatment or cognitive-behavioral groups. These kinds of findings have practical utility in terms of homing in on the idea of optimal fit or match between patient characteristic and form of treatment. But more than this, they also have heuristic value for the building of models of therapeutic change processes in groups by virtue of their linking the intrapsychic world with the social environment.

Uniquely within the therapeutic group setting, the study of personality as a moderator can go beyond its assessment at the individual patient level. Group composition—the mix of patients along some theoretically guided personality construct—can also be considered as a potentially moderating influence on outcome. There are now a few provocative studies that highlight a kind of contagion or contamination effect, a variation of the “one apple spoiling the applecart” maxim, based on group composition. To illustrate, Dishion, McCord, and Poulin (1999), in their interpretation of the complex and equivocal findings on group work for high-risk adolescents, suggested that interventions within homogeneous groups can inadvertently produce unintended, harmful effects through a covert group process they termed *deviancy training*, whereby group members subtly reinforce each others’ antisocial behaviors and attitudes. However, adding some prosocial youths to the group mix had the effect of neutralizing or muting the negative effect of deviancy training in their group work for at-risk youths. Here, group composition, specifically the presence or absence of prosocial youths, moderated the effects of the group treatment intervention. Similarly, Piper et al. (2011) reported that group composition—the addition of at least some patients with higher levels of object relations—could enhance overall group performance. As with Dishion et al.’s work, Piper et al. suggested that these higher functioning patients could serve as “role models” for others in the group. In yet another example of contagion or contamination effects, Cloitre and Koenen (2001) found that having some patients with a comorbid diagnosis of borderline personality disorder in groups for women suffering with posttraumatic stress disorder (PTSD) adversely affected outcomes in the entire group. Anger in groups with even one borderline patient intensified in the group as a whole, compared with those PTSD groups without borderline patients. These intriguing findings nicely illustrate how group ecology, the mix of personalities along a single dimension within any group, can affect—enhance or diminish—treatment effects for the group as a whole. This, of course, should not be a surprise for psychodynamic group psychotherapists, who understand well how the valencies among group members can magnify both therapeutic and antitherapeutic dynamics.

Beyond the study of predictors and moderators, psychotherapy process research also entails the study of me-

diators, variables that explore the underlying pathways of therapeutic change. As with moderator analysis, there are formal statistical procedures (Kenny, 2011) for testing mediator effects by assessing, in essence, whether an obtained empirical relationship between a treatment intervention (as in cognitive-behavioral group therapy for social phobia) and outcome (such as reduction in phobic symptoms) is diminished when the mediating variable (say, increases in cohesion over the course of the group) is included in the equation. As I have recently argued (Greene, in press), mediator analysis serves several important goals. Practically, it can serve to optimize treatments by eliminating technical strategies that are not associated with outcome and enhancing those in-session or in-treatment processes that are positively related to outcome. The study of mediators could also serve to lower the acrimony and “horse race” mentality among competing schools of therapy by identifying transtheoretical core mechanisms of change, thus moving away from the ideologies of prized or “proprietary” treatment packages. And, as argued earlier, the study of mediators, precisely because it is essentially an exploration of process, may lessen the historic divide between researcher and clinician.

The recent study by Stice, Rohde, Seeley, and Gau (2010) nicely illustrates how mediator analysis can serve these ends. These researchers applied a sophisticated and stringent five-step model for assessing mediating variables within a four-way comparative efficacy study of two group treatments (GCBT and supportive-expressive therapy), bibliotherapy, and a control condition for depressed adolescents. While the literature reflects some minor variations in specific procedures for conducting mediator analyses, the logic across the methods aims at (a) generating a series of inferences about temporal and causal connections, first between an independent variable and a putative mediating variable and then between this mediating variable and the outcome variable, and (b) assessing whether the strength of the relationship between the independent variable and the outcome variable decreases when the mediating variable is introduced into the statistical equation. The scientific elegance of the work lies in the effort to identify and validate theoretically driven and theoretically specific mediators for particular treatment modalities. In the Stice et al. (2010) study, the presumptive mediators were decreases in the experience of loneliness and increases in emotional expressiveness over the course of treatment in the supportive-expressive condition (which can be considered a distant cousin to psychodynamic group work in terms of both therapies sharing an emphasis on an unstructured format and the encouragement of self-disclosure and empathic listening). Highlighting how our treatment models are only approximations of the actualities of the treatment situations, only partial supporting evidence was obtained from Stice et al.’s mediator analyses. These researchers found, as expected, that the supportive-expressive group yielded an improvement in depressive symptoms when compared with the do-nothing control condition. More meaningful, and directly relevant to the issue of mediation, is that this treatment-outcome relationship was no longer significant

when either change in loneliness or change in emotional expressiveness was statistically controlled, which thus provides the inferential basis for the view of these two variables as mediators. However, further analyses revealed that this decrease in the strength of the treatment–outcome relationship was significant only when controlling for loneliness. Also problematic were crossover effects in which a mediating variable presumed to be specific to one school of therapy was implicated in the change process within the comparator group treatment. The final step in Stice et al.’s analysis was to demonstrate that change in the mediator occurs *before* change in the outcome variables, consistent with scientific logic and the very definition of a mediator variable. Unfortunately, findings for the putative mediators in the supportive–expressive groups did not support the theoretical expectation of this temporal sequence. Taken together, then, these results and the limited empirical support for the role of their selected variables as mediators in their two group modalities suggested to the researchers that other variables—particularly such nonspecific therapeutic factors as instillation of hope and universality—may play more robust roles as mediators in the treatment process within both group modalities. As I have concluded previously on the basis of these new kinds of drill-down studies, the lack of robust, straightforward, and consistent findings speaks not to the futility of the task but rather to the real limits to our understanding of the core mechanisms of change within any treatment. “It may indeed be easier to demonstrate change (particularly when comparisons involve weak alternatives such as wait lists, reading lists or treatments-as-usual) than to uncover or discover hidden, underlying processes, especially when they do not necessarily derive from one’s preferred theoretical framework!” (Greene, in press).

Relations or interactions among moderator, predictor, and mediator variables can also be studied and may contribute to an increasingly nuanced theory of the change process in groups. Johansson and Høglend (2007) expanded on this point: “For example, if it is shown that gender moderates a treatment effect [i.e., that men improve more in Treatment A and women do better in Treatment B], it is likely that different mediators are in operation for men and women” (p. 2), what Kenny (2011) referred to as moderated mediators. Johansson and Høglend (2007) continued,

This chain of thought can be reversed. If a good outcome is achieved through changing the patient’s level of insight [a mediator], it is conceivable that certain pre-treatment characteristics, such as level of insight before treatment [a moderator variable] might be of importance for the effect of the treatment [a mediated moderator in Kenny’s terms]. So the same construct, in this case insight, can be both a moderator and a mediator. (p. 2)

Over the years, there have been a smattering of process studies beyond the investigations of predictors, moderators and mediators, and their interactions that have attempted to build sophisticated models of group therapy (Bakali, Baldwin, & Lorentzen, 2009; Johnson, Burlingame, Olsen, Davies, & Gleave, 2005; Sexton, 1993;

Shechtman & Leichtenritt, 2010). As displayed in Table 1, these works, unfortunately all isolated studies, have attempted to explore sequences among process variables that tap three primary domains: content (especially focusing on therapeutic work and goals vs. relational or bonding issues); a structural dimension (entailing a focus on member–member, member–leader, and member–group relationships); and a general affective quality (positive vs. negative). The table reveals only modest overlap in derived process variables across these four methodologically sophisticated studies. Clearly, much more is needed in the development of empirically supported models of therapeutic change. Further, by no means have all the processes that have been theoretically linked to outcome within different schools of group psychotherapy been empirically studied. Take, for example, the construct of containment, a core therapeutic notion derived from object relations theory and referring to the complex back-and-forth projective processes so rampant in unstructured groups in which “raw” cognitive-affective material (initially too hot to handle within the individual, who thus needs to externalize it into a suitable “container”) is tempered or “metabolized” through empathy and secondary process verbalization, making its truth more available to be reclaimed and acknowledged. While theoretically useful, such a construct has perhaps been too complicated or abstract to operationally define. Other process variables, such as cohesion, lack consistency in operational definition to the point where their value for theory building is seriously weakened (Hornsey et al., 2009). Most of all, I agree with and underscore the assertion by Piper et al. (2011) regarding the limits of an exclusively quantitative/positivist approach to science: “The study of the process of group psychotherapy will never be a readily controlled deterministic science; the interactions in groups are complex and sensitive to many different conditions” (p. 157). And, conversely, I do not at all share the optimism of a purely positivist approach as expressed, for example, by the assertion of Orlinsky, Rønnestad, and Willutzki (2004) that all it takes is a [very] multidimensional scheme that “would effectively organize the body of [process-outcome] research to provide a detailed and complex answer to the well-known specificity question: What aspects of therapy and what kinds of therapy, provided how and by what kinds of therapist, under what circumstances, for what kinds of patients with what kinds of problems, are likely to lead to what kinds of results?” (p. 362). In my view, a qualitative/interpretative approach (cf. Dattilio et al., 2010), precisely the analytic stance taken by the psychodynamic clinician in studying process, will likely always be a part of the advancing of ever more refined, nuanced, and complex models of how group therapy works.

What’s the Psychodynamic Group Therapist to Do?

All of the trends and developments identified above would seem to converge on the idea that this is a particularly opportune time for the clinician to help advance models of

Table 1
Group Therapy Process Variables Linked to Outcome in Multivariable Process Studies

	Bakali et al. (2009)	Johnson et al. (2005)	Shectman & Leichtenritt (2010)
	Patient-rated variables		
Insight	Member–leader alliance (agreement on tasks and goals, positive affective bond)	Positive working relationship (sense of shared goals and tasks with leader and other members)	
	Positive bonding relationship (cohesiveness and engaged with group; positive bond with therapist)	Positive bonding relationship (cohesion; engagement; bonding with and empathy from members and leader)	Bonding with group and therapist
Negative experience (feeling irritation and rejection)	Negative relationship (group avoidance and conflict)	Negative relationship (sense of group conflict; empathic failure with leader and other members)	
Emotional intensity			Open communication (e.g., talking about relationships, asking for feedback, expressing positive and negative emotions)
Dysphoria			Perceived therapist’s help (e.g., support, reflection)
	Therapist-rated variables		
Session importance			Open communication (same as above)
Positive feelings (warmth; belief that patient would improve; emotional bond with patient)			Cognitive exploration by client
Negative feelings			Affective exploration by client Therapeutic change in client

treatment and for the psychodynamically oriented group therapist, uniquely among clinicians, to be in the forefront of this movement. Psychodynamic group therapists, precisely because of their inherent interest in process (Greene, 2005), can take advantage of the nonindependence of observations or “data” of group members by asking what it is—in the way of predictors, moderators or mediators, or their interrelations—that leads to deviations from “average” performance, in both optimal and subpar directions. It is precisely the variation in treatment responsiveness, on the one hand, and the constancy of at least some treatment parameters across patients in the group, on the other hand, that uniquely advantage the group therapist for identifying variables that differentiate performance. Simultaneous formal observations and analyses of two or more patients within the same group would seem to offer the likelihood of somewhat greater internal validity than would the cross-sectional study of individual therapy cases. The group, after all, has a unique ecology that in some important and robust ways is a constant across all the patients who constitute its membership. The group members share a common space and time; they witness the same “real,” in vivo events of the group (filtered, of course, through their own idiosyncratic lenses); and they all relate to the same here-and-now cast of characters—the same therapist using the same technical interventions and the same fellow patients, in the same time, and in the same group culture.

The work for the group-therapist-as-scientist is to attempt to isolate factors that make a difference in individual performances—within any session, in any developmental phase of group treatment, or over the course of the entire group. There are, of course, an indeterminate but not infinite number of potential factors. Writing from the perspective of individual psychotherapy, Dattilio et al. (2010) proposed that individual differences in treatment trajectory or final outcome can be analyzed by exploring the role of such factors as

vulnerabilities related to the client’s history; treatment credibility; client expectation of success; client commitment to treatment and motivation; the therapeutic alliance; the development of an individualized case formulation based on the treatment model; adaptation of the treatment manual to the client’s presentation; the unfolding of the therapeutic process; and the impact of aspects of the client’s life situation. (p. 434)

This list is far from exhaustive and, more problematic, represents a hodgepodge of technical and nonspecific factors, varying in complexity and clarity. Orlinsky et al. (2004) developed an equally complex and complicated cataloguing of potential process constructs from a transtheoretical perspective, based on an encyclopedic classification of process studies over the past 50 years. The psychodynamically oriented group therapist, can, of course, consider all of the variables identified in the process research studies described above and need not be limited only to those constructs that have been well researched, such as the warhorses of cohesion and alliance. As I have suggested above with regard to the construct of containment, psychodynamic theories—and not just those directly

pertaining to group psychotherapy—can also help in identifying variables that may play a role. Empirically studied and operationally defined constructs derived from psychodynamic formulations about the internal world, personality, and developmental psychopathology, such as Blatt’s (2011) notions of introjective versus anaclitic styles, and dynamic processes manifested in the social context, such as transference (Andersen & Chen, 2002), the core conflictual relationship (Luborsky & Crits-Christoph, 2006), and unconscious motivation and perception (Bargh & Williams, 2006; Westen, 1998), can all have meaningful applicability to the development of increasingly sophisticated models of dynamic group therapy. Equally important, constructs from neighboring domains such as group dynamics and social psychology—ingroup versus outgroup perceptions, entitativity, and deindividuation, to name a few—can also usefully be applied to the thinking about how group therapy works at the individual patient level.

And, finally, the field of psychodynamic group psychotherapy could be enriched by systematically exploring the relevance of process constructs within individual psychotherapy, a suggestion offered against a backdrop of parochialism and compartmentalization within the psychotherapy literature as a whole, with each theoretical school or modality tending to focus only upon its own conceptual framework. In this vein, Kivlighan (2008) argued persuasively that psychodynamic group therapists’ “resistances” to cross-fertilization of ideas across both practice and research domains can impede theoretical advances. Similarly, Billow (2002) criticized the rigid balkanization of psychodynamically oriented individual and group modalities, which makes it appear as if the two practices have nothing to offer each other. Sadly, what Hill (1990) declared over two decades ago, namely, that “the jury is still out” on whether the therapeutic processes in individual therapy differ from those in group therapy, is still true today.

Concluding Thoughts

The rich interplay and reciprocity of the intrapsychic (conscious and unconscious wishes and needs, self- and other-schemata, unconscious motivations) and the blank screen of the unstructured group create the development of such social structures as idiosyncratic roles, pairings, splits, and group-as-a-whole representations and the dynamic unfolding of collusions, complicities, compliances, and alliances that can serve either therapeutic ends or defense and resistance (Bernard et al., 2008) for the individuals involved and/or the group as a whole. In the effort to optimize treatment for each group patient, the group therapist as social scientist can uniquely add to our understanding of the therapeutic process, a uniqueness afforded by the opportunity to simultaneously consider the differential treatment pathways of more than one patient at a time within the same treatment milieu. As such, the comparison of differentially responsive patients in the same group is thus situated between the large-scale RCT and the $N = 1$ case study and thus is consistent with Iwakabe and Gazzola’s (2009) idea of examining cases with differential outcomes in order to shed light on processes that facilitate or hinder thera-

peutic effectiveness. The present call, geared specifically to the psychodynamic group therapist, goes beyond Iwakabe and Gazzola precisely because of my advocating for the *in vivo* comparison of differentially responsive patients in the same group, a method that can, at least partially, control for some treatment parameters and should thus facilitate the identification of moderating and mediating variables. Even if not engaging in the development of clinical databases, as Iwakabe and Gazzola suggested, at the least, clinicians should make their observations of and hypotheses about treatment processes known through their writings, which, as it turns out, researchers actually read (Beutler, Williams, Wakefield, & Entwistle, 1995). The opportunity seems ripe for the group therapist, in general, and for the psychodynamic group psychotherapist, more particularly, to become an authority on identifying aspects of the therapeutic process that make a difference at the individual level and thus serve well the science of building nuanced theories and models of change. And, as Wachtel (2011) reminded us, "What is needed [for advancing the interests of prospective patients] . . . is research [broadly defined] that illuminates the processes and principles . . . that account for meaningful therapeutic change" (p. 463). Now is the right time for the psychodynamic group therapist, from the unique vantage point of this role, to contribute to the growing interest in and developing of more complex understandings of therapeutic change processes.

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