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Jacques P. Barber[a] & Brian A. Sharpless[b]
[a] Derner Institute of Advanced Psychological Studies, Adelphi University, New York, NY, USA
[b] Department of Psychology, Washington State University, Pullman, WA, USA
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HONORARY PAPER

On the future of psychodynamic therapy research

JACQUES P. BARBER[1], & BRIAN A. SHARPLESS[2]

[1]*Derner Institute of Advanced Psychological Studies, Adelphi University, New York, NY, USA &* [2]*Department of Psychology, Washington State University, Pullman, WA, USA*

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Abstract
Objective and Method: Two psychodynamic therapists and researchers from different generations reflected upon the past and present state of psychodynamic therapy research as well as possibilities for the future. **Results and Conclusions:** Several issues (e.g., decreased research funding, increased medicalization of mental health problems, and declining psychodynamic representation among research faculty) were identified as potential impediments for future high-quality research. In addition to encouraging the field to face these challenges directly, a number of specific recommendations were provided. These included not only suggestions for traditional process and outcome research, but also recommendations to modify our current assessment practices, improve our field's cohesiveness, increase our public visibility, and improve relationships with our non-psychodynamic colleagues. In is argued that, if the field confronts these many challenges in a creative and flexible manner, psychodynamic therapy research will not only continue to be relevant, but will also thrive.

Keywords: competence; outcome research; psychoanalysis; psychodynamic therapy; process research

We were asked to reflect upon where we think the field of psychodynamic psychotherapy research should be headed. This is an important question, and one that intrigued us, as little journal space is typically allotted for asking "big picture" questions and making prognostications. As will be apparent below, we have a number of suggestions for where the field "ought" to go (at least from our specific perspectives), and we are legitimately excited at the prospect of future findings and developments, some of which will likely be completely unexpected. However, we also feel a strong sense of responsibility to convey a realistic (what some might term pessimistic) picture of where we are and where we think the field may actually go if certain prevailing trends continue. At the same time, we recognize the very important fact that there has been a great deal of high-quality psychodynamic therapy research in the recent past. Therefore, the potential impediments enumerated below should not be conceived of as death knells for the field, but as challenges to be overcome. Therefore, we as a field should consider modeling good therapist behaviors in our research by honestly confronting these challenges while simultaneously being responsive, creative, and flexible. The emphasis of this paper will be on the future, but as every good psychodynamic therapist knows, past and present are prologues.

Some Core Impediments to Conducting Good Psychodynamic Therapy Research

Although psychodynamic therapy researchers utilize defense mechanisms just like everyone else, it would require some amount of denial for one to overlook the fairly dystopian picture that is being painted for future research. Several trends in academia, economics, and research politics are combining to create a challenging research climate.

Funding

One of the most important impediments is shared with our nondynamic brethren: a lack of research

Correspondence concerning this article should be addressed to Jacques P. Barber, Derner Institute of Advanced Psychological Studies, Adelphi University, Room 301 1 South Avenue P.O. Box 701, Garden City, New York 11530, NY, USA. Email: jbarber@adelphi.edu

funding for most forms of traditional psychotherapy research. It would be nice if this fact engendered increased feelings of solidarity as opposed to an acrimonious competition for scant resources, but this may be overly naive. The economic downturn of 2008 likely only accelerated an already existing trend, and it would appear that the halcyon days of psychotherapy research enjoyed in the 1980s and 1990s are no more. The situation is almost catastrophic in North America (where funding is extremely rare), with a somewhat better (but far from wonderful) climate in Europe. When nonexistent funding is combined with the all-too-real fact that many types of good, state-of-the-art studies have become increasingly complex and expensive to run, one worries that the amount of new and exciting findings, especially those derived from randomized clinical trials (RCTs), will be markedly diminished.

Medicalization

Another worrisome (and somewhat related) trend is the ever-increasing medicalization of mental health treatment. The use of medication to treat even moderate ailments remains on the rise whereas psychotherapy usage is declining (e.g., Clay, 2011). This is in spite of the fact that there is some evidence that this may not be a very effective strategy (Fournier et al., 2010), and that maintenance of treatment gains are usually superior in psychotherapy, at least when looking at cognitive behavior therapy (CBT; Hollon et al., 2005; Spielmans, Berman, & Usitalo, 2011). Perhaps not surprisingly, industry funding is more readily available to conduct drug studies, so it is easy to see the temptation some researchers face when the more complicated psychotherapy funding sources are dwindling. All of the above are problematic for psychotherapy research in general, but the problem becomes even more acute when considering psychodynamic psychotherapy research in particular.

Declining Research Faculty and Training Opportunities

One trend, probably more than any other, may lead to a significant decline of not only high-quality psychodynamic therapy research, but also psychodynamic therapy as a flourishing field. The number of core faculty in North American clinical psychology programs who consider themselves psychodynamic has been steadily declining (Levy & Anderson, 2013), and it is our impression that this is occurring in other western countries as well. This trend appears to be much less dramatic in counseling psychology programs (Sharpless, Tse, & Ajeto, 2014). However, along with the general decrease in faculty who are psychodynamic, one also sees an increase in homogenization of faculty orientations in individual departments, with the number of clinical doctoral programs possessing 100% CBT on the rise, and the number of programs with 0.0% psychodynamic faculty also on the rise (Sharpless et al., 2014). Unless researchers and clinicians join forces in order to steer off of this course, the next generation of psychotherapy researchers will not only have far fewer opportunities to learn to *practice* good psychodynamic therapy, but will also have scant opportunities to receive mentorship in dynamic therapy *research*. Should these trends continue, dynamic therapy researchers, at least in clinical doctoral programs, will be slowly bred out of existence whereas clinical CBT programs will reach increasing levels of ascendance.

Matters may be even worse in US psychoanalytic training centers. In a recent article from the *American Psychoanalytic Association*, Robert Pyles writes that, "The evidence is clear. Our recruitment is dwindling. Our membership is declining. Our members are divided. And more than a third of our institutes are on a course to fail" (Pyles, 2014, p. 3). Although many of these centers do not directly contribute to empirical research in the dynamic therapies, this nonetheless signals a decline in opportunities for future researchers to receive solid clinical and theoretical instruction. Therefore we recommend the creation of centers of psychodynamic thinking and therapy where such knowledge can be created, gathered, and taught. It would be important for those centers to be strongly associated with universities.

The various psychotherapy orientations not only differ on techniques, but also differ more fundamentally on *worldview*. They each provide different conceptual lenses through which one can view human phenomena, and a loss of *any* lens can only be experienced as an impediment in the future. Though we are both psychodynamically oriented therapists and assimilative integrationists, we support methodological/theoretical pluralism (Barber, 2009) and feel that no one paradigm should be preemptive of any other (e.g., Feyerabend, 2010). This goes for our own as well. Given the enormously complex task of figuring out how to best alleviate human suffering through a combination of words and human encounter, we can only foresee future difficulties should psychodynamic therapy cease having a pride of place among the major orientations. Fortunately, simultaneous with this, there is a clear trend toward integration (Summers & Barber, 2010).

Recommendations

Having now outlined a nonexhaustive list of some current unavoidable and "real world" challenges to

the future of psychodynamic psychotherapy research, we would now like to devote attention to some of the things we as a field might do to keep psychodynamic research vibrant and viable. We concentrate on more concrete research suggestions as opposed to tackling the more monumental task of providing solutions to these larger (political and macroeconomic) problems. In general, psychodynamically oriented psychotherapy and personality research has been booming in the last few decades. We have witnessed an increase in the quality and the quantity of outcome and process research as documented in the most recent edition of *Handbook of Psychotherapy and Behavioral Change* (e.g., Barber, Muran, McCarthy, & Keefe, 2013; Crits-Christoph, Connoly Gibblons, & Mukherjee, 2013). Therefore, there is much to consider and elaborate upon. In order to expand breadth, we enlisted the aid of a number of colleagues to collect additional ideas about where we "ought" to go. We received (with deep appreciation) 36 very detailed responses from emailed requests. Our original goal was to create a full-blown Delphi poll, but time was too short to go through a second stage of rating all the items that were generated in this initial step. We therefore used our colleagues' comments to support some of our many recommendations below.

Outcome Research, RCTs, and Alternatives

The first author's presidential address to the *Society for Psychotherapy Research* (Barber, 2009) outlined some of the "core conflicts" of psychotherapy research. One of these conflicts could be simply stated as "are RCTs the main gateway to knowledge about the efficacy of psychotherapy, or is the information they provide ultimately misleading?" Examples of both of these views can be found on the pages of *Psychotherapy Research*, but we question whether or not either of these extreme positions is tenable, especially given our hypothesized (grim) future.

RCTs obviously have strengths and limitations (Barber, 2009; Williams, 2010). Although we acknowledge the many problems associated with RCTs, the findings ultimately obtained from multiple, well-conducted trials may provide some of the most convincing (some may say the most convincing) evidence for the value of psychotherapy. This holds not only for clinical scientists and insurers, but also for the general public as well. Thus, it would be fairly unrealistic to assume that we can progress (survive?) as a field without availing ourselves of the argumentative power derived from good RCTs. Although a number of meta analyses supporting the efficacy of psychodynamic therapy have been published in the last decade (e.g., Abbass et al., 2014; Barber et al., 2013; Leichsenring, Rabung, & Leibing, 2004), few could argue that there is not a need for well-powered, well-conducted RCTs. As noted above, the "rules" governing appropriate methodologies for conducing good trials have become more and more rigorous over the last few decades. For the purposes of funding and subsequent publications, it has become increasingly important to conform to the methodologies developed by the CONSORT Group (Moher et al., 2010), and this can be quite expensive in terms of time and money. In spite of these challenges, we as a field will need to follow prevailing guidelines.

In terms of selecting the most appropriate comparison groups, there is a strong need to compare psychodynamic therapies to well-established psychological treatments as exemplified in three recent, large-scale RCTs conducted in Europe that compared psychodynamic therapy to CBT (Driessen et al., 2013; Leichsenring et al., 2009, 2013). They also studied relatively "common" conditions such as generalized anxiety disorder and social anxiety disorder. Within the context of these "non-inferiority" designs, and building on this particular type of research, it is also important to document psychodynamic therapy's efficacy for certain *subgroups of patients* suffering from disorders who may particularly benefit from a psychodynamic focus.

Inspired by the work of Larry Beutler, a group of researchers has addressed the idea that a treatment is not likely to work for all patients with a specific problem. For example, Barber and Muenz (1996) suggested that different therapies may be successful with certain subgroups of depressed patients. More recently, Milrod and colleagues showed that even among patients with panic disorder, those with Cluster C personality disorders were more likely to improve from panic-focused psychodynamic psychotherapy (Milrod et al., 2007). Even more recently, a study found that depressed ethnic minority men (primarily African-American) benefited more from psychodynamic therapy than medication (Barber, Barrett, Gallop, Rynn, & Rickels, 2012). Some of those findings were predicted (e.g., Barber & Muenz, 1996) whereas others were exploratory or *post hoc* (Barber et al., 2012). Regardless, these findings are intriguing. They will help advance our field, and would benefit from extension, replication, and more hypothesis testing derived *explicitly* from psychodynamic theories. A way to make this approach even more sophisticated would be to formulate a widely accepted definition of what actually constitutes dynamic therapy for a specific disorder. A noteworthy step toward achieving this goal was recently published by Leichsenring and

Schauenburg (2014) in the context of dynamic therapy for depression.

Of course it is also important to compare the efficacy of psychodynamic therapy to control conditions, especially in cases where no established treatments exist but for whom psychodynamic therapy could likely be helpful. If one were conducting an RCT for narcissistic or obsessive-compulsive personality disorders, for example, a credible form of nonspecific, face-to-face therapeutic therapy (e.g., Rockland, 1989) would essentially be all that would be presently available. Wait list control or a treatment as usual conditions are also viable alternatives.

Another proposal would be to use RCTs in a somewhat less-traditional way to explore if (and how) psychodynamic therapy may work with those who do not respond to other modalities (including psychopharmacology). Nonresponders are a challenging population often requiring more treatment effort and possibly more flexibility of treatment approach. The rationale for studying a nontraditional treatment sequence like this may come from either a scientific or purely economic perspective. For example, a provider could initially use a less expensive and time-intensive approach (e.g., behavioral activation for depression or escitalopram oxalate [Lexapro] for panic disorder) and then follow up with a more time-intensive treatment (such as a panic-focused form of psychodynamic therapy as in Busch, Milrod, Singer, & Aronson, 2011) for those who either failed to improve or for those for whom interpersonal difficulties made application of the initial approach more difficult. There is a strong need to collect more information on these treatment-resistant patients, as we all know that even in the best-run RCTs, remission rates are not terribly high. Positive findings would underscore the need for, and value of, psychodynamic approaches. Longer follow ups would obviously be necessary here, and they may be more helpful to make the case for psychotherapy over medication.

Relatedly, it remains critical to generate and test sound hypotheses for selecting which patients may be more appropriately helped with long-term dynamic therapy as opposed to short-term dynamic therapy or other modalities. Given the historically longer-term nature of dynamic therapy, it is easy to see the benefits of being able to identify patients who may need more than 24 sessions of a protocol treatment. It remains a challenge for the field to predict which patients would benefit from longer-term treatment vs. those for whom a briefer approach would suffice.

So how do we conduct RCTs without sufficient funding? This is an eminently legitimate question, and there are no absolute and perfectly reassuring answers. If contemporary National Institutes of Health (NIH) trends continue, this will be particularly difficult, but there are theoretical possibilities for psychodynamic research through using NIH's new research domain criteria. There are also nongovernmental sources of funding (e.g., foundations). Many universities offer intramural grants, especially to junior faculty, and this may be enough to at least conduct a good pilot study. Certain private sector granting agencies could also provide funds.

Another way one may be more likely to obtain funding is (or perhaps was) through collaborating with other scientists conducting research in nonpsychodynamic modalities (e.g., recent RCTs by Leichsenring et al., 2013). This may be *particularly* helpful, as a prime difficulty with RCTs (and meta-analyses derived from them) is the unavoidable fact that principal investigators often have a strong allegiance to their treatments (Barber, 2009). Through the use of "adversarial collaborations" (Kahneman & Klein, 2009), we could meet several important needs simultaneously. First, we broaden the appeal of the study by placing psychodynamic treatment head-to-head against a well-established treatment (e.g., Driessen et al. 2013). Second, we help ensure methodological rigor and minimize any potential charges of an "allegiance effect" should psychodynamic therapy demonstrate a superior outcome. Third, we expose other modalities to the particular way that we think about patients and conduct psychotherapy and disabuse individuals of the notion that there has been no change or growth in this form of therapy since the early twentieth century. Finally, and this will be discussed in a subsequent section, we can utilize assessment methods more traditionally at home in psychodynamic approaches with nonpsychodynamic treatments and hopefully generate evidence for psychodynamically unique treatment effects.

Given funding limitations, we have a need (and what some may term a great opportunity) to be truly creative in our outcome designs. Along with effectiveness studies (as opposed to RCT efficacy studies), we can utilize qualitative and single-case designs (Fishman, 2000). These are typically much cheaper than RCTs, yet can still answer interesting questions related to outcome. In fact it has been suggested that single-case studies could help promote a treatment to the status of "well established" (Chambless & Hollon, 1998).

We also continue to support the idea of creating large practice-research networks that will enable the field to gather data from naturalistic settings (Barber, 2009; Borkovec, 2002; Castonguay, Barkham, Lutz, & McAleavey, 2013; Castonguay, Youn, Xiao, Muran, & Barber, in press). These can utilize large

numbers of patients/therapists and assess the *effectiveness* of psychodynamic treatments as they are delivered in everyday practice for various conditions and for varying treatment lengths (i.e., short- and long-term). Such networks can also help bridge the ongoing gulf between researchers and clinicians (Grubb, 2014) by engaging clinicians in research and by having researchers assess clinician-relevant questions.

Process Research or (and?) Outcome Research?

There is another RCT-related tension that merits commentary: the intellectual tension between those who focus on psychotherapy *process* and those who focus on psychotherapy *outcome*. For example, the more extreme proponents of the process research camp could state that unless we know the *mechanisms of change* (also termed therapeutic actions) we cannot really improve the overall efficacy of our treatments in a substantial way. Furthermore, the fact that multiple types of treatment can have equivalent outcomes (namely, the dodo remains alive, although the bird is often attacked) appears to lend credence to the belief that understanding the mechanisms of change may be more important than studying efficacy alone. However, adherents in the efficacy camp state that unless we know a treatment is effective, there is no point in really studying the process.

Our position has always been that both goals of psychotherapy research are important and legitimate, and there is no need to dismiss the opponent. It is obviously of the utmost importance for the survival of psychodynamic psychotherapy to study and document its efficacy. This is because the majority of individuals who are *not* psychotherapy researchers (including the general public) are only really interested in "bottom-line" questions such as "does it work" or "can it help me" and could really care less about "how it works." They are just as disinterested in the mechanisms of change in psychotherapy as they would be about understanding how aspirin alleviates pain. Nonetheless, it is also important to better understand the moment-to-moment processes of change, as they unfold, in order to better focus on the most important components of treatment (whether these be common factors or treatment-specific effects). Thus, we should all try to dissolve this false dichotomy and better incorporate our process research questions into therapy outcome trials.

So what are the potential mechanisms of change in psychodynamic therapy, and which have some degree of empirical support? The clinical literature is replete with discussions of putative mechanisms of change (Boswell et al., 2011), but there is a relative paucity of supporting empirical data. Barber et al.'s (2013) chapter reviewed what is known about five particular mechanisms of change of psychodynamic therapy which had been somewhat operationalized and undergone testing (i.e., insight, increasing adaptive defenses, decreasing rigidity, improving the quality of object relations, and increasing reflective functioning/mentalization). By way of very brief summary, their review revealed that: (1) all five mechanisms changed with psychodynamic therapy, (2) maladaptive defenses and a poorer quality of object relations are associated with greater symptomatology, (3) insight, defenses, and the quality of object relations were found to change along with symptoms (see also Crits-Christoph et al., 2013), (4) changes in insight and reflective functioning appeared to be unique to psychodynamic therapy, and (5) defenses and the quality of object relations moderated therapy outcome. Again, this is not an exhaustive list of possible therapeutic actions for dynamic therapy, but is an enumeration of those with operationalized measures and empirical tests. We should also note that changes in attachment style (a construct arising out of analytic theory, but adopted by clinical psychology more broadly) have also been found to result from psychodynamic treatment (Levy et al., 2006).

Given these very promising findings, we have a number of suggestions for future psychodynamic researchers. First, it is *imperative* that we continue to theorize about and empirically tease apart the unique and nonunique mechanisms of change that could be operative in psychodynamic therapy. We also have to be observant, creative, and not blinded to new possibilities (as best as we can be) by potentially erroneous assumptions about change mechanisms that were bequeathed to us from current or classical theory. Much like mechanisms of change in psychopharmacology, we may not know *why* psychodynamic therapy works in spite of the fact that we know *that* it works. Regardless, competent, thoughtful, and programmatic research into these various mechanisms of change will not only allow us to gain basic knowledge, but will also help us establish and promote the particularity of psychodynamic therapy. For instance, the finding that psychodynamic therapy may increase mentalization/reflective functioning (whereas other tested therapies do not appear to lead to equivalent increases, as in Levy et al., 2006), could make quite a compelling argument for using psychodynamic therapy with patients who display deficits in this capacity (assuming those with low capacity to mentalize would benefit more from psychodynamic therapy, which has not yet been demonstrated). This may allow us to expand the

already extensive range of problems that psychodynamic therapy is already applied to (e.g., autistic spectrum disorders, Drucker, 2009; attention deficit hyperactivity disorder, Conway, 2014).

Along with these suggestions described above, it will be useful to explore other mechanisms of change that have been discussed in the literature. Sharpless and Barber (2012) summarized many of these (e.g., abreaction, expanding the domain of the *observing* ego, corrective emotional experience, reducing overly harsh egos), but others are likely important as well (e.g., internalized representations/identifications with the therapist, as in Geller & Farber, 1993). If these mechanisms receive empirical support, we should obviously modify our theories accordingly. These modifications will give our theories additional explanatory power so we have a "progressive research program" (Lakatos & Musgrave, 1970).

In addition to these mechanisms of change, we should also look at the specific techniques that likely facilitate therapeutic changes (Summers & Barber, 2010). Regardless of the subtype of dynamic therapy one utilizes, we all share certain interventions in common. For instance, the therapeutic use of silence, questions, clarifications, and interpretations are all found in the traditional psychodynamic armamentarium (Langs, 1973). However, we realize that the particular application of techniques could look quite different if one were a relational analyst versus an ego psychologist. Much could be done to tease out effects of "dose" (e.g., frequency), appropriateness, etc. for each of these tools. As one example, some data exist showing that psychodynamic interpretations are beneficial (e.g., Orlinsky, Rønnestad, & Willutzki, 2003), particularly those that are "appropriate" interpretations, but that use in high frequencies may not be beneficial for certain types of patients (Crits-Christoph, Gibbons, & Mukherjee, 2013). McCarthy, Keefe, and Barber (in press) also documented the "Goldilocks effect" where *moderate* amounts of psychodynamic and experiential techniques yielded the best treatment outcome. Some researchers have even looked at the specific *impact* of interpretations, and found that this is moderated by the quality of object relations (e.g., Connolly et al., 1999; Høglend et al., 2006, 2008).

Facilitation of affect is also important. An interesting meta-analysis (Diener, Hilsenroth, & Weinberger, 2007) analyzed the effect of psychodynamic therapists' focus on affect in session. Specifically, they found a moderate effect (mean $r = .30$) between outcome and the therapists' facilitation of affective experience/expression.

In addition, researchers should continue to take seriously the fact that the timing of the many process and outcome variables is important (e.g., Barber, Connolly, Crits-Christoph, Gladis, & Siqueland, 2000; Crits-Christoph et al., 2013). Demonstrating that the change in outcome occurred *after* the assessment of the measurement of the putative mechanism of change or specific techniques is critical (e.g., Barber et al., 2014; Zilcha-Mano, Dinger, McCarthy, & Barber 2014).

From our perspective, these close analyses of techniques and timing effects are very exciting, yet remain relatively untouched research areas. This is in spite of the fact that they possess enormous implications for psychotherapy research and the training of future therapists.

Assessment

The results of any sort of efficacy, effectiveness, or process research are predicated upon the availability of valid and psychometrically sound assessment instruments. This is another instance in which the past continues to significantly impact the present. Given the (from our perspective, unfortunate) historical fact that a robust empirical research tradition came to psychodynamic therapy much later than behavioral and cognitive approaches, the trailblazers (i.e., cognitive-behavioral therapists) were forced to make the rules for the not-yet-existing field of psychotherapy research. These early researchers came from their own therapy traditions and viewed their patients (and the process/assessment of change) through their specific lenses. Not surprisingly, they developed and chose instruments to maximize detection of the phenomena that were most lens-consistent. This included, among others, various observable symptom measures, the operationalization of behavioral avoidance, and subjective units of fear/anxiety/discomfort. They were not focused on dynamic change and the modification of unconscious psychodynamic processes, so they did not develop measures relevant to psychodynamic work. The conundrum is, therefore, "how to play the game when the rules may not be as directly relevant to us."

Symptoms only, dynamic constructs only, or both? We would of course expect psychodynamic therapy to alleviate troublesome symptoms, and the outcome literature shows that it does (Barber et al., 2013), but this does not exhaust the true import of dynamic therapy. Thus, we wonder whether our field is potentially selling itself short by only speaking this language of symptoms? Freud purportedly noted that an ability to love and work was the aim of treatment (Erikson, 1963), and we think this is a fairly useful, albeit rough, indicator of health. More specifically, though, would freedom from repetitive and maladaptive relationship patterns, a felt and

consistent feeling of wholeness in one's self (Kohut, 1977), or appropriately exercising ones ego capacities (Hartmann, 1939) be any less important that the absence of depressive symptoms? Clearly not, but current RCT assessment conventions do not include these constructs. Similarly, is an increased sense of well-being or similar constructs relevant? We found that, before Bonferroni corrections, a component of quality of life improved more in supportive-expressive therapy than in medications (Zilcha-Mano, Dinger, McCarthy, Barrett, & Barber, 2013) in an RCT (Barber et al., 2012). Therefore, developing and testing measures that showcase dynamic therapy's specific impacts is critically important.

This is not to say that these measures do not exist. There are good measures currently available on defensive functioning (e.g., see Perry & Lanni, 1998), object relations (the *Quality of Object Relations Scale*; Huprich & Greenberg, 2003), reflective functioning (the *Reflective Functioning Scale*; Fonagy, Target, Steele, & Steele, 1998), and measures of repetitive relational patterns and responses, such as the *Central Relationship Questionnaire* derived from the *Core Conflictual Relationship Theme* method (McCarthy, Connolly Gibbons, & Barber, 2008). More measures are needed to capture the nuances of specific approaches, but we also need to use and continue to validate the measures we have. Whether one is conducting basic research on psychopathology, process research, intensive case studies, or efficacy research, we recommend using "traditional" (i.e., symptom) assessment instruments in combination with these instruments derived from dynamic traditions.

Traditional symptom-focused instruments obviously do not reveal the full complexity of changes attributed to dynamic therapy, but are nonetheless important to utilize. This is because their use allows psychodynamic research to: (1) maintain a relevance for the wide number of researchers who are more familiar with symptomatic measures, (2) provide data that is comparable across different therapy trials regardless of orientation (i.e., the increasing importance of meta analytic data to our field), and (3) perhaps most importantly and related to the preceding points, help psychodynamic therapy maintain a pride of place in psychotherapy research as opposed to becoming more insular in our methods and models such that they bear little resemblance to what other approaches are doing (more on this point in the next section).

By retaining a mainstream approach, we may expand the audience for findings derived from more psychodynamic-specific measures. Should these measures demonstrate clinical utility (e.g., finding that the quality of object relations is an important measure of healthy functioning), we could even see increased integration of these constructs into nondynamic modalities. We have seen this before (e.g., attachment; the therapeutic alliance) and, at least from our perspective, this would lead to an increased recognition for psychodynamic therapy and its methods. More importantly, it may lead to a greater leveling of the playing field for psychotherapy research, and possibly even the creation of new research conventions to better capture the many aspects of human change functions (i.e., be "fairer" for all orientations).

We also need to explore more experimental methods in assessment validation. For example, recent work by Bornstein challenges the traditional conceptualization of test-score validity (e.g., Bornstein, 2011). In contrast to the traditional approaches that solely utilize *correlational* methods to quantify the relationships between test scores and relevant criteria, Bornstein advocates the use of *experimental* methods as part of the validation strategy. This will enable researchers to draw more definitive conclusions about the underlying psychological processes that are hopefully reflected in the test scores. We believe that this methodological approach is particularly relevant for the future of psychodynamic research and our need to more strongly operationalize our more nuanced constructs into measures.

Assessment of adherence and competence. Mention should also be made of measuring adherence to treatment protocols and intervention competence. These are both core components of treatment integrity (Perepletchkova & Kazdin, 2005), and also help ensure that results are replicable across assessment sites. Although one large meta-analysis found no relation between these two constructs and outcome regardless of treatment modality (Webb, DeRubeis, & Barber, 2010), a more recent review which assessed dynamic therapy outcome specifically indicated a moderate association between adherence and outcome and some evidence that competence may be associated with outcome as well (Crits-Christoph et al., 2013). However, the results are not terribly robust, and more research is needed.

One potential impediment to clearer results may be a result of instrumentation. With a few exceptions, adherence and competence have been conceptually conflated in many measures (e.g., see Barber, Sharpless, Klostermann, & McCarthy, 2007; Sharpless & Barber, 2009). Muddying these crucial distinctions likely leads to a heterogeneity of results and a reduction in our ability to detect the true import, if any, of these therapist/therapy effects. Future researchers should closely attend to the

distinctions between competence and adherence in terms of the *types of clinical knowledge* that they each demonstrate (e.g., see Sharpless & Barber, 2009). As we all know, the timing and nuance (i.e., competence) of an interpretation is as crucial as the fact that it is actually made and the frequency with which it is made (i.e., adherence). We should also not necessarily assume linear relationships. Researchers should consider the possibility that there be curvilinear relationships between adherence, competence, and outcome (e.g., Barber et al., 2007, McCarthy, Keefe, & Barber, in press). Researchers could also assess flexibility of adherence, a construct related to competence. One way to gauge flexibility would be to look at standard deviations in adherence. If one sees an increase in standard deviations over time, this may be related to an increased flexibility, and possibly responsiveness (Barber, 2009). An example of this approach was recently reported. It showed that therapists making a flexible use of techniques seemed to have better outcome than less flexible therapists (Owen & Hilsenroth, 2014). Regardless of whether one is conducting RCT research or an intensive case study using a standard protocol, ratings of adherence and competence are critical for potential replicability.

Other future directions. In ending this section, we briefly note two other potentially important assessment trends for psychotherapy research. First, we recommend continued attempts to combine psychodynamic research with functional magnetic resonance imaging and general neural/metabolic change work while simultaneously recognizing that these findings may not exhaust the full scope of changes wrought by dynamic therapy.

Second, various newer technologies may potentially allow for gathering interesting process and outcome data. For instance, the near universality of smart phones in contemporary society allows for the creation of applications which could be used to supplement traditional "in house" therapy assessments. For instance, smart phones have been used in research to collect moment-to-moment data on interpersonal interactions and emotions (e.g., Roche, Pincus, Rebar, Conroy, & Ram, 2014). We foresee a number of possibilities, ranging from the simple completion of online symptom checklists at regular intervals to reminders to apply fairly sophisticated insights learned in face-to-face therapy sessions.

Issues Involving Public Perceptions of Psychodynamic Therapy

Another issue that is unavoidable, at least in our estimation, is ensuring that we present psychodynamic therapy to the general public in a way that is simultaneously accurate, accessible, and compelling. Psychodynamic research occasionally receives popular press attention, but not consistently, and most of the public likely has an image of our work derived from Woody Allen movies and reruns of *The Sopranos*. These are likely not the best representations that our field could have. We wonder if more accurate depictions could help the public realize the unique contributions psychodynamic therapy could make to their lives beyond symptom reduction. Whether this occurs through movies or short documentaries, we see only good consequences (including potential increases in funding) arising from more image consciousness on the part of our field. Similar to what Neil Degrasse Tyson has done for public interest in astrophysics, we could use a compelling public figure for psychodynamic therapy and research.

We also believe that *public health in general* could benefit from our research. More cost-effectiveness studies would be helpful, as would studies linking dynamic therapy to the "real-world" impacts favored by funding agencies and insurance companies (e.g., mortality rates, work productivity, and decreased medical utilization/cost offset studies as in Town, Abbass, & Bernier, 2013).

Things to Avoid

Along with the suggestions above describing the positive things researchers might do in order to push the field forward, we also have suggestions for things that might be better avoided. One that comes to mind is avoiding unnecessary "turf" wars.

In any environment where different groups compete for limited resources, whether these resources are research monies, journal space, or public attention, it is expected that occasional conflicts will occur. However, some of these conflicts may not evince good outcomes. Although both of us identify as psychodynamic therapists, we were also trained in, and have a legitimate respect for CBT. Further, we are aware of the literature demonstrating its efficacy with certain problems and feel that it is just as intellectually illegitimate to dismiss CBT as it is for their proponents to dismiss our findings. One way to avoid this, as mentioned above, is through adversarial collaboration. The term "adversarial" could be limited to orientation allegiance alone, and not be adversarial in tone. Given the limited research monies that are available, we need all the friends we can get.

However, avoiding acrimony does not necessarily mean that these two very different orientations are equivalent or require an identical set of assessment

protocols. We believe that some of the acrimony between these two camps may be the result of the assessment differences previously noted. Dynamic therapists do not have to play completely by the other team's rules, though some degree of overlap is likely necessary for publication purposes.

We also question the purpose and wisdom of many of psychodynamic therapies' *internecine* battles. To an extent, these stimulate healthy debate and often lead to a better clarification of what may otherwise be unquestioned assumptions. They can also turn nonproductive when disputants view their theories as narcissistic extensions as opposed to useful lenses through which to view the world. Although differences in clinical focus/proposed mechanisms of change/etc., clearly run the gamut and are sometimes in stark opposition, psychodynamic therapists are nevertheless united by certain core presuppositions such as the presence of a dynamic unconscious and the importance of a developmental perspective on the development of psychopathology (Boswell et al., 2011). Therefore, emphasizing the foundational similarities of psychodynamic treatment, searching for empirical evidence for our claims (unconscious or otherwise), and even developing more unified protocols (Leichsenring & Schauenburg, 2014) could help avoid some unnecessary conflicts and lead to increased recognition of the efficacy of psychodynamic therapy.

At the other extreme of the argumentativeness continuum, we need to avoid a "circling the wagons" mentality whereby dynamic therapists and researchers only associate with like-minded individuals and self-consciously avoid disputants. From our informal observations, this has become increasingly easier to do in our technological age than in previous times. This is because not only do we have more of an ability to maintain connections with like-minded people, even at great distances, but we also have increasing pressures to (prematurely?) specialize in one's research. It can obviously be seductive and easy to primarily talk to people who study the same problems and think in similar terms.

Conclusion

In conclusion, therapists/researchers from two different generations have briefly reflected upon where we came from, where we are, and where we should go. Although legitimate dangers for the field of psychodynamic therapy research exist, there are also a number of exciting possibilities on the horizon. If there's one thing we *empirically* know about psychodynamic therapy, it is to expect the unexpected. Psychoanalytic thought has not only weathered strong challenges (e.g., two world wars, managed health care, behaviorism, the cognitive revolution, and the decline of the traditional analytic institutes in North America), but it has also taken root in a number of other places that were perhaps not expected (e.g., Latin America, China).

Much like trends in popular music, intellectual ideas may also wax and wane according to unpredictable whims of the prevailing zeitgeist. We would appear to be at a relative low point. This unfortunate fact is all the more surprising given that we have never had the amount (or quality) of psychodynamic research in the field's entire 100+ year history. Thus, as mentioned above, future researchers need to model good psychotherapy practices in their empirical endeavors by being creative, flexible, and responsive to the particularities that are in front of them. We must accept the fact that these are the particular times we do research. Some words from Nietzsche, a depth psychologist of amazing perception, may provide some "realistic" encouragement. "I assess the power of a will by how much resistance, pain, torture it endures and knows how to turn to its advantage" (Nietzsche, 1967, p. 206). Put in less hyperbolic terms, Nietzsche appears to be saying that, regardless of the particular times and real-world limits that one is bequeathed, there always remain opportunities to maximize the usage of these prevailing trends to not only survive, but also thrive.

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