treatments, thus falsifying the psychodynamic theory of symptom formation.

Tryon (2010) observed that "*therapists, and the therapeutic approaches that currently divide us, differ only with regard to what is to be learned and how it is to be acquired*" (p. 10, italics in original). This makes learning and memory basic to our science and profession and should motivate us to search for mechanisms that underlie all effective psychological interventions (e.g., Tryon, 2005; Tryon & McKay, 2009; Tryon & Misurell, 2008).

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Science or Ideology?

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The academic psychology literature is filled with pronouncements about psychodynamic theory, often stated in authoritative tones, that present a picture of psychodynamic treatment that is unrecognizable to me and to other contemporary psychodynamic practitioners. Several of the comments about my article perpetuate this tradition and, I am sorry to say, introduce disinformation into the pages of the *American Psychologist*. Before addressing some specifics, I want to say a few words about my understanding of how such misrepresentations can find their way into scholarly academic journals.

Psychoanalysis developed outside of the academic world. Until the late 1980s, most training in psychoanalysis and psychodynamic therapy took place in freestanding institutes that were open to medical doctors and closed to psychologists. There was little exchange of information between the psychoanalytic and academic worlds, and most academic psychologists were, as a result, entirely unaware of the ongoing evolution of psychodynamic theory and practice. What little they did know of psychodynamic thinking was often based on limited reading of works that were generations out of date and read out of context. The development of psychoanalytic knowledge did not end with Freud any more than the development of physics ended with Newton or the development of the behavioral tradition in psychology ended with Watson.

The medical monopoly on psychoanalytic education, and the arrogance of some within the psychoanalytic community when it was the dominant therapy paradigm (including a dismissive attitude toward empirical research), provoked understandable resentment among academics. A culture developed within academic psychology that disparaged psychoanalytic approaches, or rather, the stereotypes and caricatures that it mistook for psychoanalytic approaches. The self-imposed exile of psychoanalysis from the academic world also left a void, into which were born the alternatives of cognitive and behavioral therapies.

It became a convention for cognitive-behavior therapy (CBT) theorists and researchers to begin articles, papers, and books with comments about how psychoanalytic therapy would supposedly deal with a problem, and then proceed to debunk the psychoanalytic approach and show how much more reasonable and helpful CBT was. From the perspective of those of us who practice psychodynamic therapy, they were debunking fictions of their own invention; they did not actually know how psychodynamic clinicians thought or practiced. After more than two decades of such writings, many people inside and outside the mental health professions have come to believe that these fictions, created as straw men by people who did not practice psychodynamic therapy, *were* psychodynamic therapy.

I do not believe that such fictions were deliberately disingenuous; I think they were often sincerely believed to represent psychodynamic practice. By the late 20th century, however, the divisions between psychodynamic therapists and academic psychologists were so great that there was no opportunity for corrective feedback. The fictions about psychodynamic therapy had taken on a life of their own and were simply passed on as truths by multiple generations of university professors to multiple generations of students. My psychodynamic colleagues who teach in psychology graduate programs can attest to how extensive are the misunderstandings that students, especially those who majored in psychology, have been taught as received truths about the psychodynamic tradition.

Over the past two decades or so, a "master narrative" has emerged in the academic world that psychodynamic therapy has somehow been disproven and that CBT has been scientifically tested against it and found superior. In the prevailing academic climate, the steadily accumulating scientific evidence for psychodynamic therapy has been repeatedly overlooked. To academics who were exposed to fictions about psychodynamic approaches, it may have seemed unthinkable that psychodynamic concepts and treatments could possibly be supported by science, and if that was unthinkable, there was no need to examine scientific evidence concerning them. Also overlooked was the fact that CBT and other "empirically supported therapies" were almost never tested against actual psychodynamic therapy—most of the research simply compared these treatments with no treatment, or with minimal, essentially sham treatments that were not designed to be seriously competitive approaches (what my colleague Drew Westen has called "intent-to-fail" conditions, which are often misleadingly labeled in the research literature as "treatment as usual"). Moreover, the studies that did directly compare CBT with legitimate psychodynamic therapies found no advantage for CBT. No one took notice: The "master narrative" may have been too compelling to academics whose careers, reputations, and funding had now become tied to CBT and related "empirically supported therapies."

Three of the four comments on my article appear to have the intent of reasserting the master narrative by creating a smokescreen of doubt and confusion

(Anestis, Anestis, & Lilienfeld, 2011; McKay, 2011; Thombs, Jewett, & Bassel, 2011). Intentionally or not, they offer misinformation, omit crucial information, cherry-pick findings to create a misleading impression of the research literature, or impose a disingenuous double standard with respect to what constitutes evidence. Such tactics seem to emerge predictably when the master narrative is at stake, as described by Wachtel (2010); his elegantly written exposé of the academic psychotherapy research establishment should be required reading for every psychologist.

Some examples will illustrate these issues. Two of the four comments (McKay, 2011; Tryon & Tryon, 2011) cite a meta-analysis indicating that there is no empirical support for the concept of "symptom substitution." One of them (McKay, 2011) declares that symptom substitution is "a fundamental axiom according to psychodynamic approaches" (p. 147). I want to state, categorically and unequivocally, that this concept has *nothing* to do with psychodynamic theory or practice. It is irrelevant to psychodynamic therapy and has no meaning in the context of contemporary theoretical models (vs. theories that held currency in the psychoanalytic community more than half a century ago). In fact, most psychodynamic practitioners do not even know what the term means. McKay (2011) cites Wachtel (1997) to support his assertion that symptom substitution is a fundamental axiom, but he fails to mention that Wachtel's book was actually first published in 1977, more than 30 years ago (Wachtel, 1977); that Wachtel, a psychoanalytic theorist, was arguing against, not for, the concept of symptom substitution; and that Wachtel even then noted that the concept was rarely mentioned in the psychoanalytic literature of the time (let alone today; Wachtel, 1997, and P. L. Wachtel, personal communication, September 7, 2010). In fact, the meta-analysis about symptom substitution (Tryon, 2008) begins with the explicit statement that the concept was of interest "half a century ago" (p. 963) and long ago faded from attention. Moreover, my article had nothing to do with symptom substitution. I cannot help wondering whether the intent of McKay's (2011) comment is to engage with my arguments in an intellectually honest way or simply to throw everything but the kitchen sink that might appear to discredit psychodynamic approaches, whether relevant to my article or not.

Two of the comments (Anestis et al., 2011; Thombs et al., 2011) note that the effect size from the meta-analysis by Leichsenring and Rabung (2008)—one of eight meta-analyses showing substantial benefits for psychodynamic therapy reported in my Table 1 (Shedler, 2010)—has been the target of criticism and reflects a computational error. Anestis et al. and Thombs et al. are correct in noting that the computation method was irregular and produced an implausibly large value. However, neither of these comments mentions that the authors of that meta-analysis published a correction (Leichsenring & Rabung, 2009), *more than a year and a half ago*, in the *Journal of the American Medical Association*, and that the corrected, properly calculated effect size was still an impressive .65—comparable to that of any other "empirically supported therapy." I thus find myself skeptical as to whether the intent of the comments' authors is to accurately describe the literature and inform readers or to selectively choose findings that bolster the a priori master narrative.

Three of the comments (Anestis et al., 2011; McKay, 2011; Thombs et al., 2011) imply that the methods used in empirical studies of psychodynamic therapies are somehow inadequate relative to studies of other evidence-based therapies. To make the case, one of them (Thombs et al., 2011) cites a meta-analysis by Cuijpers, van Straten, Bohlmeijer, Hollon, and Andersson (2010) showing that effect sizes for psychodynamic psychotherapy decrease as study quality improves and implies that this limitation is somehow unique to research on psychodynamic psychotherapy. However, Cuipjers himself did not draw this conclusion and does not agree with it; his own conclusion is that the same limitation applies to research on all forms of psychotherapy "including CBT, IPT, behavioral activation, etc." (P. Cuijpers, personal communication, August 11, 2010). The most comprehensive meta-analysis to date on CBT for depression reported an impressively large effect size (Churchill et al., 2001), which I reported in my article without editorial comment (Shedler, 2010). However, the authors of that meta-analysis noted that "the overall quality score of trials appeared to have a considerable effect . . . . Trials with lower scores demonstrated a pronounced and highly significant difference and higher-scoring trials demonstrated no significant differences" (Churchill et al., 2001, p. 82). In somewhat simpler language: The more rigorously conducted studies showed *no* statistically significant benefits for CBT. Nevertheless, that meta-analysis has been subsequently cited repeatedly, with no cautions whatsoever, as providing conclusive support for CBT, and it has had a major influence on health care policies and patient care both in the United States and abroad.

Assuming that the comment's authors (Thombs et al., 2011) hold Cuijpers's meta-analytic research in high regard, they should also be aware of two other meta-analytic studies co-authored by Cuijpers and published around the same period. One (published too late to be included in my article) showed a large effect size of .69 for psychodynamic therapy for the treatment of depression, further contributing to the literature establishing psychodynamic therapy as an evidence-based treatment (Driessen et al., 2010). The second found very substantial publication bias in research on CBT for depression (Cuijpers, Smit, Bohlmeijer, Hollon, & Andersson, 2010); the findings indicated that the effect sizes reported in the literature are inflated by approximately 60% to 75% due to publication bias (i.e., studies with less favorable results are either rejected for publication or never submitted for publication at all). Such a situation may be all but unavoidable when the same individuals routinely trade roles as study authors, grant reviewers, journal reviewers, and members of academic hiring and promotion committees.

My article (Shedler, 2010) reported eight meta-analyses, all of which showed consistent and substantial treatment benefits for psychodynamic therapy. Anestis et al. (2011) accuse me of "ignoring crucial findings that run counter to [my] position" (p. 150), noting that I did not include one meta-analysis, two decades out of date, that had not shown such favorable results (the meta-analysis of Svartberg & Styles, 1991). I did not include that particular publication because it was superseded four years later by a more rigorous meta-analysis that I did include (that of Anderson & Lambert, 1995), which reexamined all the same studies. The authors of the later meta-analysis (whose own allegiance was not, incidentally, to psychodynamic approaches) found that the earlier meta-analysis had mistakenly classified as "psychodynamic" treatments that were *not* psychodynamic, including behavioral, cognitive, and psychoeducational approaches as well as a bogus treatment that involved only eight 30-minute sessions, which no psychodynamic practitioner then or now would consider adequate treatment. I am thus dismayed to be accused by Anestis et al. (2011) of "glossing over key methodological details" (p. 150).

Unlike the other comments, the comment of Tryon and Tryon (2011) appears to be a sincere effort to engage with my arguments. I noted in my article (Shedler,

2010) that there are seven distinctive features of psychodynamic therapy that, in empirical research, reliably distinguish psychodynamic therapy from other therapies. Tyron and Tyron suggest that the seven features are not specifically psychodynamic, but merely common factors in all effective therapy. However, the empirical research is unambiguous. It is not that some therapists trained in other approaches do not draw on some of these same features or that the features are incompatible with other therapy approaches. It is that psychodynamic therapists do these seven things more regularly, consistently, and deeply than practitioners of other forms of therapy. Moreover, psychodynamic theory and treatment models explicitly place these features at center stage, in ways that other approaches do not. They are at the heart of practice-oriented books and articles on psychodynamic therapy but are scarcely mentioned in the manuals for many of the treatments that are actively promoted and marketed as "empirically supported therapies."

What disturbs me about the three other comments is not that the authors disagree with my conclusions but that they portray themselves as objective investigators who desire only to promote good science. I have little doubt that that is how they genuinely see themselves. People speaking from a dominant paradigm often assume that they are speaking obvious truths, while people in more marginalized groups tend to experience those in power as self-justifying, self-serving, and blind to important information that does not comport with their own worldview (much as academic psychologists experienced many of the medical psychoanalysts in the middle of the 20th century). From my perspective, the authors of these comments essentially offer tendentious arguments aimed at promoting an ideological agenda. I would go so far as to say that their writings betray a troubling disrespect for scientific evidence. While Anestis et al. (2011), McKay (2011), and Thombs et al. (2011) imply or explicitly state that I am the one who marshals evidence selectively, from my angle of vision they appear to value only evidence that supports an a priori agenda while ignoring, dismissing, or attacking evidence that does not. If so, this is not science, but ideology masquerading as science.

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Integrative Perspectives on Acculturation

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Schwartz, Unger, Zamboanga, and Szapocznik (May–June 2010) are to be commended for their attempts "to propose an expanded, multidimensional model of acculturation and of the demographic and contextual forces that can influence the acculturation process" (p. 238). In their article, they called attention to key factors such as the generational status of immigrants and their children; the role of location, particularly in ethnic enclaves; and the context of reception that immigrants enter, including the potential discrimination they may face. These variables are the crucial backdrop for the authors' call to "focus on the higher order construct of receiving-culture acquisition as well as on the individual dimensions of this higher order construct—practices, values, and identifications" (p. 246). As a sociologist trained in social psychology, I am pleased by their incorporation of some of the sociological literature on these processes. However, I was surprised by important gaps in their discussion of Portes and Rumbaut's (1996, 2006) work and by their neglect of one of the most widely used terms employed by sociologists to hypothesize outcomes for the very questions Schwartz et al. were posing.

Although Schwartz et al. (2010) thoroughly detailed Berry's (1980) four acculturation models, they failed to discuss Portes and Rumbaut's (1996, 2006) *selective acculturation*. In their own four models, Portes and Rumbaut discussed whether children of immigrants acculturate or resist acculturation in consonance or in dissonance with their parents, which in turn affects their level of acculturative stress, and thus their mental health outcomes, in part through the potential for role-reversals with