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A Critique of the American Psychological Association *Clinical Practice Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults*

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The American Psychological Association (APA) Guideline Development Panel for the Treatment of PTSD in Adults published the *Clinical Practice Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults* in 2017, which is the first of a series of practice guidelines that will roll out over the next several years. Although the *Guideline* is the product of tremendous effort and expertise, it is problematic for several reasons, including hidden and flawed economic assumptions and consequences, exclusive reliance on evidence from randomized clinical trials (RCT), the limited definitions of critical outcomes, the unquestioned virtues of guidelines, inconsistencies with APA's (2012) resolution *Recognition of Psychotherapy Effectiveness* and the emphasis on therapy packages/brands, the formulaic connection between diagnosis and treatment, among other issues. Psychoanalysts and psychoanalytic psychotherapists should be concerned about the absence of psychoanalytic/psychodynamic therapies from the *Guideline*, not only because of the long history psychoanalysis has with treating PTSD, but also because the *Guideline* could become a template for other guidelines and could limit psychoanalytic models of therapeutic practice, exert too powerful an influence over what constitutes ethical practice, and adversely affect funding (both third party payers as well as federal funding for research). Those invested in psychoanalysis must be active in the process of developing, reviewing and approving these guidelines.

Keywords: PTSD guideline, psychoanalytic psychotherapy, medical model, psychotherapy outcome and effectiveness

The American Psychological Association (APA) Guideline Development Panel for the Treatment of PTSD in Adults published the *Clinical Practice Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults* in 2017 (hereinafter *Guideline*; APA, 2017). The *Guideline* was developed according to the standards set forth by the Institute of Medicine (IOM; now the National Academy of Medicine) of the National Academies of Sciences, Engineering, and Medicine for developing high-quality, independent, and reliable practice guidelines (IOM, 2011a, 2011b). The panel was multidisciplinary and had commendable expertise in PTSD and PTSD treatment. Clearly, the *Guideline* is the product of a rigorous process and reflects considerable dedication on the part of the panel.

Nevertheless, I intend to show that this *Guideline* raises many problems. Some of the ways it falls short are alluded to in the *Guideline* or the appendix to the *Guideline*, for example, only reviewing studies published in the English language, only including randomized controlled trials (RCTs) without appreciating the numerous limitations of RCTs, limitations on what constituted critical outcomes, the question of whether IOM standards are really appropriate for psychological treatments, and so forth. Others are based upon faulty economic assumptions, the risk of overselling the recommended treatments and incomplete representation to the public about what the treatments can accomplish, as well as the potential for the insurance industry/third-party payers to pressure clinicians to offer only the recommended treatments (Blue Cross/Blue Shield of Illinois, 2018). Shortcomings include the heterogeneity of patients diagnosed with PTSD, inadequate attention to patient values and preferences, along with the problematic assumptions of how to provide guidance on incorporating patient values and preferences into treatment. Furthermore, the *Guideline* can become one form of justification to limit funding for other forms of research into PTSD treatment.

Following its publication, the *Guideline* quickly generated controversy, for example, Jonathan Shedler's (2017) "Selling Bad Therapy to Trauma Victims" blog post and a rebuttal to *Guideline* supporters (Shedler, 2018); *Protect PTSD Treatments That Work!* Alliance for the Inclusive Integration of Science and Practice in Psychology petition drive concerning the *Guideline* (PsiAn, 2018); Scott Miller's (2017) blog post *Clinical Practice Guidelines: Beneficial Development or Bad Therapy?*; and Gregg Henriques's (2018) blog post, "A Critique of the PTSD Guidelines: Why I Do Not Support the PTSD Treatment Guidelines." Rebuttals ensued (Lilienfeld, McKay, & Hollon, 2018; Mckay, 2017, blog post "Clinical Practice Guidelines: A Clear Public Good, the Doubters Notwithstanding"). Some supporters of the *Guideline* claim that the opposition is "anti-science" or the equivalent. Analogies to

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discarded psychiatric practices, such as blistering, surgical removal of organs, etc. (Lilienfeld et al., 2018) were invoked as "evidence" of the shortcomings of non-RCT forms of knowledge gathering, as if to undertake any therapy not included in the *Guideline* is the equivalent of using leeches to treat depression.

However well founded the concerns articulated by the bloggers may be, I believe it is also important to communicate critiques of greater complexity and offer a more extensive review of the scholarship than can usually be offered in a blog post. Toward this end, Norcross and Wampold (2019) systematically addressed the absence of consideration of the therapeutic relationship and responsiveness in the *Guideline*. I propose to address a number of additional pitfalls to the *Guideline* process and potential consequences.

Hidden and Questionable Economic Assumptions and Potential Consequences

"Because health care costs have exploded, our health care system has dual responsibilities: to ensure that individuals are treated according to best practices and to reduce unnecessary expenditures" (Hollon et al., 2014, p. 214). Hollon et al. (2014) articulated a detailed rationale for the creation of clinical practice guidelines (CPG) in general. In the first paragraph, they noted the rise of health care costs as an important consideration for creating guidelines. Although the *Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults* states, "Treatment costs were not considered in the formulation of the panel's recommendation" (APA Guideline Development Panel for the Treatment of PTSD in Adults, 2017, p. 3), the method of inquiry evaluated only evidence from brief treatments, which are inevitably less expensive than longer term treatments. Costs were not overtly included in the process, but they played a stealth role in the development of the *Guideline*, as most of the authors in Hollon et al. (2014) were on the APA Advisory Steering Committee. So, the issue of costs was not discussed by the team developing the PTSD guidelines. However, cost is clearly a prominent concern from the team charged with providing the rationale for the use of any guidelines. It no longer needed to be spoken; it was an implicit assumption of the specific *Guideline* development process. More significantly, it should be noted that when Hollon et al. (2014) were describing an "explosion" in health care costs, they did not once refer to the cost of mental health care.

The rise of health care costs has become conventional wisdom based upon a great deal of economic evidence (from over 10% of gross domestic product [GDP] in 1986 to 17.9% of GDP in 2016; Centers for Medicare & Medicaid Services, 2017). In fact, the country has hotly debated health care because of the rise of such costs and who bears responsibility for the costs. But are the costs of mental health care exploding? Evidence suggests otherwise. Frank and Glied (2006) reviewed data from the Bureau of Economic Analysis and concluded, "The aggregate share that total mental health spending claims of national income has been stable over the past thirty-five years" (p. 601) in contrast to overall health care spending which has risen considerably as a share of GDP over the same time period. Stable is not an explosion.

In fact, there is strong evidence to suggest that the proportion of mental health care spending relative to total health care spending has declined over time. Mental health and substance abuse treatment have shown downward trends in funding relative to total health care spending (both accounting for 9.3% of total health care spending in in 1986 to 7.3% of total health care spending in 2005; Substance Abuse and Mental Health Services Administration [SAMHSA], 2010). We should not envision such expenditures as involving only psychotherapy, psychological assessment, or other psychological interventions. Instead, we should take note of the "explosion" in the use psychotropic medication and percentage of spending for this form of intervention. Prescription drugs only accounted for 7% of mental health spending in 1986 but accounted for 27% of mental health treatment in 2005, essentially a fourfold increase in spending. On top of that, psychiatric hospital treatment accounted for another 27% of mental health spending in 2005 (SAMHSA, 2010). SAMHSA (2014) updated its projections for mental health spending (2010–2020) and concluded, "During the projection period (2010–2020), the average annual growth in M/SUD (Mental and Substance Use Disorder) treatment spending is expected to slow to 4.6 percent, compared with 5.8 percent for all-health spending" (p. 10). Thus, the share of M/SUD treatment was projected to decline as a share of all-health spending (down to 6.5% of total health spending). Again, it is hard to argue that mental health treatment is a cost exploding danger. Although the share of spending for M/SUD declines relative to total health care spending, the percentages of people seeking M/SUD services over time has greatly increased (Mark, Levit, Vandivort-Warren, Buck, & Coffey, 2011).

Thus, over half of the costs for mental health services have not been associated with outpatient psychosocial interventions and outpatient psychotherapy, which is what this *Guideline* specifically addresses. Those who work in independent practice, group practices, community mental health centers, and other social agencies providing psychological services are not responsible for cost increases. Mental health treatment has come to rely more on government funding as well (SAMHSA, 2010). The economic warnings upon which the *Guideline* movement is predicated are shaky, at best, and misleading, at least. Furthermore, the increase in the use of psychiatric medication takes up an increasing share of the relative stable cost of the mental health care pie. The stability of mental health care spending relative to GDP over time does not mean we should ignore cost in considering services. But this stability does not provide evidence for a crisis or explosion in mental health spending. Proclaiming that we are in a spending crisis could lead policymakers to exclude certain practices from consideration when developing guidelines.

Undertaking research for longer term interventions is both very expensive and time consuming. It is hard to envision an early career professional becoming the primary investigator (PI) for studies that could take more time to design, implement, and complete than the tenure clock would allow. Overestimating the history of cost growth could also lead to narrowing our consideration of outcomes, by focusing only on outcomes that are hypothetically achievable in a short timeframe, because government funding agencies would prioritize studies that can be completed within the timeframe of the allocation of funds. Furthermore, conventionality is an advantage in seeking funding (Uzzi, Mukherjee, Stringer, & Jones, 2013) and too much science funding can end up in the hands of too few investigators (Wahls, 2018). In other words, the kind of research previously funded by familiar investigators is much more likely to receive future funding, pos-

sibly to the detriment of scientific progress (Wahls, 2018). As APA embarks on a reorganization (APA Council of Representatives, 2018), let us hope that advocacy bolsters its efforts to increase funding for mental health services, including traditional outpatient psychotherapy and psychological assessment, instead of using cost containment as a partial justification for advocating only for brief treatments that leave remission out of consideration of critical outcomes and gives short shrift to quality of life as an outcome.

Exclusive Reliance on Evidence From RCTs

RCTs provide one method of gauging treatment efficacy. However, there is considerable evidence to suggest that they are not appropriate for evaluating psychotherapy research. In this section, I review that evidence systematically.

Many researchers have raised several concerns about RCTs and the potential harm to the science of psychotherapy research if these methods are viewed as the exclusive approach for producing evidence in assessing the efficacy of psychotherapy for any diagnoses (Gazzillo, Schimmenti, Formica, Simonelli, & Salvatore, 2017; Silberschatz, 2017). The concerns are both practical and analytical. Cronbach (1982) argued that it is a mistake to view the patient and therapist as separate, instead of as a pair, and to view either one or the other as error variance. Further, he asserted that "speaking of experiments and naturalistic case studies as polar opposites is a rhetorical device; evaluation planning is not a matter of choosing between irreconcilables" (Cronbach, 1982, p. 44). RCTs, as currently implemented, do treat therapists' characteristics as well as unstratified patient characteristics as error variance. In medical research, which typically includes far larger *N*s than psychotherapy outcome studies, researchers have noted that differential subgroup responses to treatments complicates the interpretation of RCT results, despite the randomization utilized in group formation (Krauss, 2018). The winning treatment in the horserace can be contraindicated for particular kinds of patients, depending upon understanding different strata relevant to the pathology.

The randomization process does not necessarily create the patient group homogeneity intended (Rice & Greenberg, 1984; Silberschatz, 2017). Blatt, Zuroff, Hawley, and Auerbach (2010) found differing levels of perfectionism among groups of depressed patients in the NIMH Treatment of Depression Collaborative Research Program (Elkin et al., 1989), which appeared to play an important role in therapeutic alliance and outcome. Individual variation is inevitable and should be more the object of study (Kiesler, 1966, 1995) than of control. In addition, the notion of placebos or sham therapies as control conditions in psychotherapy research has generated controversy (Kirsch, Wampold, & Kelley, 2016). "Randomization is a metaphor and not a gold standard," stated Heckman and Vytlacil (2007, p. 4836). Many statisticians from the tradition of William Gosset (pen name Student) hold that randomization presents as interesting on paper, but often is of little use in real world settings (Ziliak & Teather-Posadas, 2014, p. 1).

As Ziliak and Teather-Posadas (2014) articulate, Simpson's paradox (Blyth, 1972; Simpson & Dixon, 1951) remains a risk in RCTs' studies. Namely, "Simpson's paradox occurs when a statistical finding appears in an aggregate analysis yet disappears (or reverses) in all subgroups" (Chan & Redelmeier, 2012, p. 143). Essentially, the potential exists for statistical differences to be present at the between-groups level data, but analysis of subgroups reveals that the original effects do not hold up or are even the opposite for subgroups in the study. This can mislead researchers into making poor recommendations for subgroups, which might not benefit from the intervention or for whom the recommended intervention could be contraindicated. "In summary, if the experiment is not prudently stratified to eliminate heterogeneity bias, the randomized trial can mislead investigators" (Ziliak & Teather-Posadas, 2014, p. 8). Given APA's publicly stated investment in diversity (APA Presidential Task Force on Enhancing Diversity, 2005), the *Guideline* should be much more vocal in communicating the limits of recommending treatments as if they apply equally well to all or fail to mention that some treatments might even be iatrogenic to some subgroups. There is little, if any, subgroup analysis for the treatments recommended in the *Guideline*.

Along somewhat similar lines, Sleigh (1997) argued that the logical-deductive foundations of the RCT has limits that few researchers appreciate. "There may be treatments that are effective but not necessarily provable using RCTs and conversely, that which is proved by RCTs is not necessarily true" (p. 148). Using a computer simulation of RCTs for prophylactic dopamine in cardiac surgery, Sleigh (1997) illustrated how an RCT could produce erroneous results, and the errors are not detected when the researcher is unaware of the underlying mechanisms of pathology. Absent good theories for pathology, the RCT presents as an alluring but potentially deceptive shortcut for basing recommendations. Essentially, failure to detect issues for stratified subgroups can lead to problematic conclusions at the group level. Simply increasing the *N* does not alleviate the errors. "The RCT is a powerful tool but it is not a gold standard and needs to be interpreted with a similar degree of skepticism as results from 'lower' designs" (p. 148).

The *Guideline* has relatively little coverage of possible underlying mechanisms and causes of PTSD; instead there is a near exclusive focus on the results of RCTs. This approach means not only that it is difficult to ascertain treatment efficacy, but also that there is no way to gauge what limitations or contraindications may exist for specific subgroups.

Further, the use of placebo groups in RCTs are not appropriate controls in psychotherapy research. A placebo in medical research is a physical substance that should have no physical effect on the disease or disorder under investigation. The sugar in the proverbial sugar pill has no physical effects on cancer or bone density nor is it necessary for active medicines to work, for example. However, understanding what constitutes placebos for psychotherapy research is thorny (Wampold, Frost, & Yulish, 2016), because the placebos used in medicine try to control for effects produced by anything other than the physical effects of the medicine. Psychotherapy is, by definition, not efficacious via the physical properties of the treatment. Kirsch et al. (2016) argued that "the placebo effect cannot and should not be controlled" (p. 121) and that the construction of control groups is related to the questions that the research is intended to address. They recommend no treatment controls or usual care (nonpsychotherapeutic treatments). RCTs can be valuable in experimentally testing treatment components or ingredients instead of treatment packages.

Brewin and Bradley (1989) stated, "Though patients play an active part in the outcome of all treatments, we suggest that clinical

trials in which they are required to sustain an effortful and demanding role and those in which they are likely to have strong preferences for one treatment need to be considered and conducted differently" (p. 313). Patient motivation for treatment does not necessarily get well "randomized," especially when patient preferences are not accounted for. Which treatment the patient is assigned to will interact with patient characteristics resulting in more or less of a preference fit. In fact, Brewin and Bradley (1989) contended that patient preferences should be considered in the design of all RCTs. Curious in the *Guideline*, however, is the lack of information about patient preferences. The following statement appeared in every assessed treatment including the strongly recommended treatments: "Patient values and preferences were considered but owing to unknown variability did not substantially factor into the recommendation." So, none of the RCTs took patient preferences into consideration, despite the potential problems that have been identified by ignoring this factor.

Of course, other methods, such as naturalistic studies, have limitations. However, the process of developing guidelines should involve weighing the limitations of different methods to reach recommendations and not excluding a large and possibly effective set of studies at the outset. This exclusion is the equivalent of saying that no other study is a scientific study, or why should anyone do any research that is not RCTs. The American College of Physicians weighs evidence from a variety of methodologies, even if it privileges RCTs. Other methods are not automatically excluded. Although evaluating other evidence involves a great deal of judgment and the potential for differences of perspective and opinion among experts, so does evaluating RCTs, which are not all equally strong in design or clear in results. In medicine, observational studies have been essential in demonstrating the efficacy of penicillin for bacterial infections; the efficacy of the smallpox vaccination; the value of vitamin B12 replacement; the treatment of insulin in insulin dependent diabetes; the use of anesthesia for surgical operations, and so forth (Black, 1996). Running RCTs for these and other common medical treatments would be ludicrous. None of these treatments are foolproof or perfect, yet we can assess that treatments can be efficacious without running RCTs. RCTs provide an indication of what can be achieved under the most favorable conditions (Black, 1996) but are not well suited to guide everyday practice, and particularly not to guide everyday psychotherapy practice.

Let's briefly consider a study not included in the *Guideline*, because it was a naturalistic study: Levi, Bar-Haim, Kreiss, and Fruchter's (2016) "Cognitive–Behavioural Therapy and Psychodynamic Psychotherapy in the Treatment of Combat-Related Post-Traumatic Stress Disorder: A Comparative Effectiveness Study." Note that the study does use psychodynamic psychotherapy. Assignment to treatment was not random but was based upon an extensive intake process. The study is not a RCT. Those with more phobic-like symptoms (e.g., avoidant of places) and strong negative beliefs (e.g., feeling it is not safe anywhere) were assigned to CBT, whereas those with extensive interpersonal dysfunction and possible developmental and personality issues were assigned to psychodynamic therapy. Both treatments are routinely used with Israeli Defense Force veterans, consistent with typical practices. Both treatments were time limited and probably far less extensive than most psychoanalysts and psychoanalytic psychotherapists would consider optimal. At the end of the treatment, there were no statistically significant differences between the groups (35% of CBT patients had remitted and 45% psychodynamic psychotherapy remitted). We note that most patients did not remit, but this study actually measured remission as an outcome. The outcome of this study is on a par with other therapy studies. Although there are a number of aspects of the study that limit generalization and clarification about the mechanisms of efficacy, at the same time, it is a "real-world" study of how treatment takes place *in situ*. The strengths and weaknesses of the study could be evaluated. Instead those who wrote the *Guideline* chose to exclude all naturalistic studies.

Krauss (2018) analyzed the 10 RCTs in medicine with the highest number of citations and found every one of them wanting in significant ways that limit the generalizability of the findings and the causal explanations. Flaws included not looking carefully enough at a host of possible confounding background variables, aside from demographics, that could conceivably affect outcome measures and not collecting enough baseline data for psychological or other factors that could affect outcome measures. He also criticized a number of medical RCTs for having small *N*s, but most relevant for the current analysis is that he considered a small *N* to be a few hundred per group! Are there any psychotherapy researchers who would consider a few hundred subjects to constitute a small experimental group? In psychotherapy research, having a few hundred per group would be considered gigantic. Furthermore, psychotherapy RCTs do not do a good job of accounting for confounding variables, especially confounding variables of a psychological nature, such as personality characteristics. From the standpoint of most RCT research, uncontrolled variance in the form of personality characteristics could create confounding factors to the purity of the RCT. However, psychoanalytic therapists would view understanding personality as central to the therapeutic enterprise.

RCTs for psychotherapy are, of course, not double-blind, the very fact that makes them valuable in medicine (Kirsch et al., 2016). In medicine, the physician (or research aide) dispensing the pill does not know what the pill contains. Therapists, however, do know what treatment they are providing or if they are running a sham treatment or a treatment not under study and considered inefficacious. There is a long history in social psychology involving experimenter or expectancy effects, aka Pygmalion or Rosenthal effects. Therapists conducting the treatments under study are surely invested in the outcome and must be excited to be part of the project. This appears to be related to the so-called allegiance effects in psychotherapy research (Gaffan, Tsaousis, & Kemp-Wheeler, 1995; Jacobson & Hollon, 1996; Luborsky et al., 2002; Smith, Glass, & Miller, 1980).

Although the *Guideline* mentions the need for studying allegiance effects, the "horse" of recommendations has already left the "barn." The creation of the *Guideline* provides a template for future revisions. In other words, the treatments recommended by the *Guideline* face an easier time getting funded because of their supposed success. Federal agencies want to place bets on winners, so that Pygmalion effects have significant consequences for funding. Wampold et al. (2010) have specifically noted allegiance effects and other confounds for treatment research of PTSD.

It is important to consider that other RCT-centric, systematic reviews in medicine have led to poor, even dangerous, recommendations. For example, Stradling and Davies (1997) documented the

fact that a highly effective treatment for sleep apnea (nasal continuous positive airway pressure) was omitted from treatment guidelines because it lacked RCT research, whereas the RCT research that had been conducted did not examine a multitude of problematic outcomes associated with sleep apnea. Given the risks involved with sleep apnea, for example, increased risk of falling asleep at the wheel, leaving off an effective treatment from recommendations created potential harm. The problem for those recommendations was created at the outset, that is, exclusive reliance on RCTs for evidence and little consideration of the underlying mechanisms of pathology.

There are many routes to scientific progress, and limiting the scope of inquiry, as the *Guideline* does, can lead to mistaken confidence in our analysis instead of tolerating and helping others recognize the inherent ambiguity and complexity of working therapeutically with any individual, regardless of the diagnosis. Moreover, it is important for researchers to consider and look for possible negative consequences associated with the treatment, beyond those that cannot be ignored because of their severity. The exclusion of other than RCT evidence becomes a pretense that simply having evidence from RCTs does our thinking for us, notwithstanding the cautionary, legalistic language in the *Guideline*. In fact, the panel declined to use its considerable expertise to even consider the scientific merits of non-RCT research. Wouldn't the experts be able to review non-RCT research and weigh the relative merits of different studies? Doing so would have necessitated weighing the strengths and weaknesses of a variety of types of evidence, a common practice in medicine. That would have been a beneficial activity to model for our profession and ultimately more useful for the public. It really is what individual clinicians need to do unless the intentions of CPGs are to discourage thinking.

Valuable Research Into Psychoanalytic Work Was Not Considered

In addition to Levi et al. (2016) above, other psychoanalytic research for treatment of PTSD was ignored by the Task Force. Space limitations and the focus of this article preclude me from presenting an exhaustive review of psychoanalytic empirical literature on the treatment of PTSD, but it is important for a journal of psychoanalytic psychology to address the absence of psychoanalytic perspectives from the *Guideline*. Schottenbauer, Glass, Arnkoff, and Gray (2008)[1] comprehensively reviewed evidence illustrating that psychodynamic therapy can lead to improvement in a variety of functions, including "improved self-esteem, increased ability to resolve reactions to trauma through improved reflective functioning, increased reliance on mature defenses with concomitant decreased reliance on immature defenses, the internalization of more secure working models of relationships, and improved social functioning. Additionally, psychodynamic psychotherapy tends to result in continued improvement after treatment ends" (p. 13). They acknowledged the lack of RCT-based research strategies for psychodynamic psychotherapy, with the exception of Brom, Kleber, and Defares (1989), a short-term dynamic therapy that showed effectiveness, and Gersons, Carlier, Lamberts, & Van der Kolk (2000), which contained elements of psychodynamic therapy with CBT. More recent efforts include Steinert et al. (2017), who used RCT methodology integrating psychodynamic therapy with EMDR and showed high levels of remission for the treatment group, including those with comorbid symptoms of depression and anxiety. This psychodynamically based therapy was efficacious despite not actively confronting trauma memories.

Although psychoanalytic therapists value the kinds of improvements described by Schottenbauer et al. (2008), these would not constitute critical outcomes (see below) in the opinion of the PTSD Task Force. Sharpless and Barber (2011) undertook the kind of evaluation I recommend here. They concluded,

> Taken together, the available empirical base of psychodynamic therapy, while often lacking in empirical controls, appears compelling enough to warrant its use. This may especially be the case with PTSD clients who are unwilling to undergo exposure techniques early in treatment, clients with Axis-II pathology, or in other complex cases where interpersonal themes predominate. (p. 7)

Their assessment is well reasoned and devoid of boosterism. It represents the kind of thoughtful evaluation of the strengths and limitations of various research strategies that all researchers should strive for.

Inconsistencies With APA's Resolution on Psychotherapy Effectiveness

Because RCTs, as usually implemented, study brands or packages of therapy, it is useful to revisit APA's (2012) resolution *Recognition of Psychotherapy Effectiveness*:

> These large effects of psychotherapy are quite constant across most diagnostic conditions, with variations being more influenced by general severity than by particular diagnoses—that is, variations in outcome are more heavily influenced by patient characteristics e.g., chronicity, complexity, social support, and intensity—and by clinician and context factors than by particular diagnoses or specific treatment "brands" (Beutler, 2009; Beutler & Malik, 2002; Malik & Beutler, 2002; Wampold, 2001).

Yet brands of therapy are exactly what the *Guideline* offers. It is not offering useful recommendations about therapeutic processes nor emphasizing the importance of the therapeutic relationship. By recommending brands or packages of therapy, patients are asked to think about psychotherapy in much the same way they think about vaccinations, namely as a passive recipient of a procedure instead of a collaborator in a relationship. It reflects a capitulation to medical model thinking and forsaking psychological thinking. The *Guideline* implicitly frames psychotherapy as an activity that therapists *do to* patients instead of one they *do with* them. Considering the "brand" approach to studying treatment for PTSD that was embraced by the panel, it seems that it should only include studies that had good treatment fidelity measures in the review process. But that was not the approach taken.

Much current research into psychotherapy emphasizes the need to tailor interventions in consideration of many factors, with formal diagnosis being only one factor that does not trump others (Norcross, 2011; Norcross & Wampold, 2011; Silberschatz, 2005).

[1] I would like to thank an anonymous reviewer for comments on this section.

> Rather than seeking to compile a list of empirically supported treatments for a particular symptom, the field would be better served if resources were directed toward understanding more fully the processes of therapeutic change—toward identifying what occurs in those patient–therapist dyads that lead to therapeutic gain that is sustained over time and that eventually enables the patients to deal effectively with their lives, including the ability to deal with subsequent stressful life events. (Blatt, 2001, p. 639)

The *Guideline* process undermines this by emphasizing brands and packages implemented via manuals. Furthermore, lacking a model of psychopathology to test, the manualized treatments create the impression of a process in social engineering instead of the amelioration of human suffering and the widening of the range of options in living.

Clinical, Legal, and Educational Implications of the *Guideline*

Guidelines are intended to have several benefits. They hold the potential to reduce problematic variability in services for individuals with identical diagnoses in the hope of improving outcomes. They can discourage problematic practices for diagnoses. They also hold the potential to make certain evidence-based services more widely available for consumers. The publication of guidelines serves as a means of educating the public about available treatments for a diagnosis and could prompt some consumers to seek treatment when they otherwise might not. It is important in advertising guidelines to the public to accurately reflect the state of the field and not oversell the benefits of recommended treatments. Guidelines are intended to provide professionals and consumers alike with information on the best ways to treat different diagnoses. They can identify gaps in the literature about treatment and understanding of diagnoses. They also aim at cost efficiency.

Guidelines hold risks as well, which can be difficult to guard against in the "real world" of individual decision making. They involve recommendations for the average patient, but treatment is always for the individual patient. The Disclaimer section of the *Guideline* states, "This guideline is intended to be aspirational and is not intended to create a requirement for practice." and "Guidelines are not intended to be mandatory or exhaustive and may not be applicable to every professional and clinical situation. They are not definitive and they are not intended to take precedence over the judgment of psychologists and other professionals" (p. vii). However, although the *Guideline* contains language that limits its applicability for specific clinical situations, such disclaimers do not necessarily restrain insurers or public payers. In medicine, health spending by private and public funders is often influenced by guidelines (American Academy of Actuaries, 2008; Woolf, Grol, Hutchinson, Eccles, & Grimshaw, 1999), and behavioral health insurers are invested in guidelines as a means of containing costs (Clay, 2014).

Under pressure to manage costs, private and public funders hope to use guidelines as a way to create standardization of services and a predictable cost structure. Standardization can be especially problematic in mental health as mental health diagnoses differ in many ways from other medical diagnoses. If the *Guideline* were to be used in malpractice law as a way of creating a standard of care against which to measure or assess the provider, it could become onerous to justify treatment decisions made by the therapist (defendant) that are not part of the recommendations (Moses & Feld, 2008). Malpractice is about an individual case, while guidelines are about the average case. In appraising practice guidelines in medicine, Sox (2014, p. 200) notes that ". . . health insurance companies use guidelines to help decide their coverage policies." The APA *Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults* involves recommended psychological treatment packages or brands as opposed to psychological processes or therapeutic techniques to utilize in psychotherapy for PTSD. In other words, a psychotherapist could be faced with questions about whether and how they used a brand or package of therapy with a patient instead of focusing more on the specific processes involved in the psychotherapy of a unique individual. Thus, not using the brand could be viewed as a deviation from a standard of care. Although the panel added the legal disclaimers to the *Guideline*, as quoted above, individual clinicians, including APA members, might not see the *Guideline* as their friend in a legal proceeding. Furthermore, the *Guideline* recommendations are based upon manualized treatments. How well does the average practitioner follow the manual? Even practitioners who identify as CBT might not follow a manual with anywhere near as much fidelity as therapy undertaken in a research study. Would a CBT therapist need to justify departures from a manual in court?

Guidelines incrementally contribute to the deprofessionalization of the field, by narrowing our clinical work to following manuals and packages instead of focusing on the developing the therapy relationship, processes that promote change, and thinking and decision making by the psychologist in a complex relationship all of which have proved important to therapeutic outcome in earlier research. They contribute to the fallacy that emotional recovery should be a simple linear upward trend from week to week of a brief therapy process. Guidelines produce a menu of recommendations based upon manualized treatment packages with no guidance about applying the accumulated knowledge in the field to individual cases. But the unspoken proviso of the guidelines to patients is—If you do not see the treatment on the list, you're getting snake oil.

The role of the *Guideline* and others to be promulgated by APA must also be considered in light of APA's (2018) decision to develop an accreditation system for master's programs in areas that APA already accredits (clinical, counseling, and school). Although master's programs already exist, APA's decision to create a path for MA accreditation changes longstanding policy of recognizing the practice of psychology as a doctoral profession. By definition, a master's degree in a practice area is not a research degree, and those with MAs will not receive the extensive training in the production and evaluation of research received by doctoral level psychologists. The *Guideline* could be seen as a tempting answer to the complexities of working therapeutically. In fact, the RCT-only approach can be seen as sparing those without extensive research training the strain of evaluating the strengths and weaknesses of different research and methods of treatment.

In fact, by eschewing the consideration of non-RCT studies in the review process, the panel makes it more difficult for all clinicians to use other forms of research and expertise in undertaking treatment with patients diagnosed with PTSD. In the event that a clinician was confronted with the need to justify their intervention strategies to third parties, reviewers would have strong grounds to discount any justification that did not include

RCT-backed research, despite the importance in the treatment of individual patients. After all, it could be argued, the experts wouldn't even consider such research. The expert opinion held that non-RCT research was not even worth reading. So, how can an individual clinician claim such research as part of the justification for their approach? "Clinicians should consider well-established guidelines in their practice and should be prepared to justify in court any departure, if so required" (Samanta, Mello, Foster, Tingle, & Samanta, 2006, p. 321). Guidelines that are developed by national organizations, such as APA, would be more likely to be used in court settings. CPGs have been shown to have an effect in malpractice cases involving accusations of negligence (Hyams, Brandenburg, Lipsitz, Shapiro, & Brennan, 1995; Mackey & Liang, 2011), for both inculpatory and exculpatory purposes. "Proponents of systems for healthcare reform, clinicians and guideline developers should all be aware that guidelines are a double-edged sword" (Samanta et al., 2006, p. 343).

Given the role clinical guidelines are playing in making treatment decisions, the recent Findings of Fact and Conclusions of Law (Order) by Magistrate Judge Joseph Spero of the Northern District of California against the insurance corporation United Behavioral Health (UBH) is noteworthy (*David V Witt et al. vs. United Behavioral Health, Case 3:14-cv-02346-JCS Document 418*, 2019).[2] The pivotal finding in Judge Spero's decision involves UBH's guidelines: "the record is replete with evidence that UBH's Guidelines were viewed as an important tool for meeting utilization management targets, 'mitigating' the impact of the 2008 Parity Act, and keeping 'benex' [benefit expense] down" (p. 93). Furthermore, Judge Spero noted, "It is a generally accepted standard of care that effective treatment requires treatment of the individual's underlying condition and is not limited to alleviation of the individual's current symptoms" (p. 33). It is important to note that the American Psychiatric Association's clinical practice guideline "Treatment of Patients With Substance Use Disorders, Second Edition" (Kleber et al., 2007) and their "Practice Guideline for the Treatment of Patients With Major Depressive Disorder" (Gelenberg et al., 2010) reflect generally accepted standards of care. This illustrates how influential clinical practice guidelines are, but I contend there is significant reason for concern.

If UBH hypothetically adopted the APA PTSD *Guideline* as its own internal guideline, mental health providers and patients would face highly restrictive treatment options for PTSD. The PTSD *Guideline* is not based on an overarching model of the psychopathology of PTSD guiding the treatment research, that is, a model of the underlying condition. Given this, how could the treatments be said to address the underlying condition, as opposed to the alleviation of current symptoms? Furthermore, the *Guideline* does not address services such as residential treatment. The court's finding specifically noted that UBH fell short of accepted standards of care in coverage of residential treatment. It's difficult to see how the PTSD *Guideline* will improve this situation.

The Definitions of Critical Outcomes Are Narrow

How effective or helpful are the approved treatments? Well, the majority of people do not recover using the treatments in the timeframe studied, and some of the most important outcomes are not the subject of scrutiny or not considered important enough to consider for the *Guideline*. For example, Shedler (2018) noted that one of the best RCTs upon which the *Guideline* was developed (Schnurr et al., 2007) did not result in the kind of improvement a patient would likely expect, that is, not having PTSD at the end of treatment and all patients continued to exhibit clinical depression at the end of treatment. It is common for dropout rates to be high in trials, which cannot count as a treatment success, and remains an area of concern buried in the minutia of the *Guideline* document.

The *Guideline* chose to define *critical outcomes* in a way favorable to certain forms of evidence and not consider outcomes that the average consumer might find critical or important to their situation. So, symptom reduction and serious harm were considered critical outcomes. By themselves, we can understand these as important outcomes of concern to any patient entering treatment. However, outcomes that were not seen as "critical" included remission, quality of life, disability or functional impairment, prevention or reduction of comorbid medical or psychiatric conditions, adverse events leading to withdrawals (treatment discontinuation), and other adverse events. Surely, a potential patient learning about the recommended treatments for PTSD would think that remission is what the *Guideline* is referring to and not simply a reduction in symptoms. Why is this called the APA *Clinical Practice Guideline for the Treatment of Posttraumatic Stress Disorder (PTSD) in Adults,* instead of the APA *Clinical Practice Guideline for the Treatment of Symptom Reduction and Serious Harm in PTSD*? The title promises more than the fine detail delivers. The *Guideline* declares symptom reduction and serious harm as the critical outcomes without a clear rationale for why the other "important" outcomes were not considered "critical." It is hard to imagine that patients think about treatment outcomes with the same categories or academically nuanced distinctions as the researchers. What is the researcher going to say, "Well I never promised to do anything about your depression"?

Furthermore, as Shedler (2018) has noted, most patients do not recover in these well designed research studies. Buried in the minutia is the cruel fact that, in studies across the board for various disorders, most patients continue to experience significant symptoms even if there are statistically significant differences between treatment and control groups. Whereas in medicine, we are informed how much a treatment improves blood pressure, cholesterol levels or whatnot, in psychological treatment research, we tend to be informed merely that one group was statistically significantly better than another (Shedler, 2018). Such differences do not inform us much about the reduction in suffering or improvement in the quality of life. Much research was excluded from consideration in the development of the *Guideline*, and what was included shows some helpfulness for some people some of the time but hardly something to hang our hats on and to argue that this is the only scientific and ethical way to practice.

Confounds in the Connection Between Diagnosis and Treatment

Numerous mental health professionals and *DSM* critics have raised concerns about mental health diagnoses in general. Many of these concerns would fall into the broad category of the trend in medicalizing many aspects of the human condition (see Conrad,

[2] I would like to thank an anonymous reviewer for suggestions that enhanced this section.

2010, for a sociological critique of the trend in medicalization). Prior to addressing these concerns in more detail, it is important to emphasize that all of the research that forms the basis for the *Guideline* use criteria from earlier versions of the *DSM* than the current *DSM–5* (American Psychiatric Association, 2013a). "Of note, all of the studies included in the RTI-UNC systematic review that served as the evidence base for that report used *DSM–IV–TR* or earlier *DSM* criteria and are those discussed throughout this guideline" (APA Guideline Development Panel for the Treatment of PTSD in Adults, 2017, p. ES-3). Considering the attempts made by the guideline team for precision and the concerns expressed about recommending treatments based upon certain forms of evidence, it should be emphasized that no treatment recommended by the *Guideline* was researched using criteria from the *DSM–5*. This flaw is of particular significance when we note that the American Psychiatric Association's *Highlights of Changes From DSM–IV–TR to DSM–5* states, "*DSM–5* criteria for posttraumatic stress disorder differ *significantly* from those in *DSM–IV*" (American Psychiatric Association, 2013b, p. 9, italics added for emphasis).[3]

The entire *Guideline* is predicated on the assumption of needing specific treatment packages for specific diagnoses (in this case PTSD). However, when a clinician makes a *DSM* diagnosis in 2019, they are using a diagnosis (*DSM–5*) that the American Psychiatric Association explicitly says is different than the diagnostic criteria used in studying the recommended treatments in the *Guideline* (pre-*DSM–5*). In essence, the *Guideline* must view the differences in diagnostic criteria between versions of the *DSM* to be of trivial import in recommending treatments for PTSD. We are forced to conclude that diagnostic precision, via the *DSM* approach, is not necessary for recommending treatments based upon diagnosis, despite the claims of the *Guideline*. The *Guideline* implicitly tells a clinician who makes a *DSM–5* diagnosis of a patient to use treatment packages based upon pre-*DSM–5* criteria. Yet, that would seem completely inconsistent with the philosophy of a *Guideline* that promulgates the notion that there is a close tie between *DSM* diagnostic criteria and recommended treatment packages.

In addition, by tying recommended treatments to the *DSM* diagnosis, the *Guideline* communicates that a *DSM* diagnosis is sufficient information for developing a treatment plan. All clinicians know that this is not the case, and psychoanalytic clinicians consider a different approach to diagnosis by taking the whole person into consideration and not simply a set of behavioral criteria (McWilliams, 2011). The *Psychodynamic Diagnostic Manual* (2nd ed., Lingiardi & McWilliams 2017) notes acute stress disorder and PTSD "to be highly variable, multifaceted psychophysiological and spiritual conditions that can take different courses at different times." (p. 185). The *Guideline*'s emphasis on the recommended treatments does not inform the public about the complexities of developing treatment plans and the fact that the symptoms of a psychiatric diagnosis involve great subjective complexity by the patient, and yet it is the subjective experience of the patient that is the main "target" of the intervention process. Furthermore, the *DSM–5* does not include all of the dimensions of Complex Posttraumatic Stress Disorder (CPTSD), which includes a variety of developmental impacts which make an individual vulnerable before the trauma of the PTSD unfolded (Lingiardi & McWilliams (2017). In this light, epidemiological studies have found extremely high rates (>90%) of lifetime comorbid mental disorder in those diagnosed with PTSD (Kessler, Sonnega, Bromet, et al., 1995; Sareen, 2014). In the *Guideline*, the issue of comorbidity is handled with a light touch and does not assist in treatment planning for PTSD with present or past comorbid disorders. Instead, the *Guideline* makes it seem that a *DSM* diagnosis of PTSD is all that's needed to undertake a treatment process.

For example, the For Patients and Families website for the *Guideline* says, "Together, you and your mental health care provider can use the guideline to determine which treatment, or combination of treatments, will work best for you." It implies that only treatments from the list should be considered and focuses on the treatment label as opposed to the process of therapy. This provides little basis for deciding treatment process aside from the diagnosis of *DSM–IV–TR* (or earlier) PTSD. It fails to communicate that there are many other things to consider in addition to a *DSM* diagnosis, as if the patient's difficulties are adequately captured this way. In effect, the *Guideline* says to patients, "Here is the menu. You can pick anything on the menu, but there isn't anything else good, and you shouldn't really want to have anything else anyway."

The menu of options patronizes patients by simplifying the process of picking a therapy approach. It is as if the *Guideline* is promoting the patients' excitements of choice while simultaneously trying to protect them from its anxieties (Dauphin, 2006; Schwartz, 2016). By limiting the options the way Steve Jobs hyped new Apple products and reduced complexity of the MacIntosh product line when he resumed leadership of Apple in the late 1990s (Isaacson, 2011), the *Guideline* functions more like the launch of a product line than as a guide to working on complex human problems. Given the investment in research using therapy manuals and the mechanistic processes assumed to produce effective psychotherapy, an analogy to the computer industry is apt. It's also interesting that the For Patients and Families website suggests patients and therapists could determine which combination of treatments would work best for the patient. Have all of the combinations been subjected to RCTs? Isn't the website suggesting a process which has not been empirically validated? As if to put an exclamation point on the *Guideline*'s enchantment with the medical model, in an era of psychiatrists prescribing medication cocktails of unproven efficacy, the *Guideline* advocates that psychologists offer therapy cocktails.

Conclusions

The *Guideline* is no doubt the product of a great deal of work and expertise. The recommended treatments can be of some help to some people some of the time. Yet, the fact that most patients do not achieve recovery is minimized to the point of oblivion. The *Guideline* approaches recommendations via therapy packages, which is an approach that directly contradicts APA's (2012) resolution *Recognition of Psychotherapy Effectiveness*. There is no clear overarching model of the psychopathology of PTSD guiding the treatment research, and ignoring the mechanisms underlying pathology has led to problematic recommendations in medical research. The advertising of the *Guideline* is not modest; of course, little advertising ever is. A great deal of

[3] As the depression guidelines have been open for public comment, it should be noted that the criteria for major depressive disorder experienced no substantial changes between versions of the *DSM*.

scientific research and accumulated clinical knowledge was deemed unworthy of serious review. Taking that stance is the equivalent of saying no treatment research should be published except RCTs, notwithstanding the limitations, practical and epistemological, for that methodology. The *Guideline* has all the trappings of consumer products that create buzz, promise a lot, but underwhelm. It encourages the notion that patients should focus on brands (something quite superficial) instead of focusing on therapy relationships, which is what is most important in successful therapy.

摘要

美国心理学会在2017年发布了《美国心理学会创伤后应激障碍治疗临床实践指南》(APA创伤后应激障碍治疗临床实践指南,2017),这是接下来几年将陆续推出的一系列实践指南的第一个。尽管该指南是巨大的努力和专业的产物,由于某些原因,它仍是有争议的,包括隐藏的有缺陷的经济假设和结果,完全依赖于随机临床试验(RCT)的证据,关键结果的有限定义,指南的无可争议的优点,与美国心理学会对心理治疗有效性的决议和对治疗组合/品牌的强调不一致,在诊断和治疗之间的公式化连接,以及其他问题。精神分析师和精神分析治疗师应该关注到该指南中缺少了精神分析/精神动力取向治疗,不仅因为精神分析有着很长的治疗创伤后应激障碍的历史,也因为该指南会成为其他指南的模板并限制精神分析模式的治疗实践,会对伦理方面施加过多的影响,并对资金的资助也会有不利影响(既包括第三方支付者也包括联邦政府的研究资金)。那些在精神分析上投资的人们必须积极参与到制定、审核和批准这些指南的过程中。

关键词: 创伤后应激障碍指南, 精神分析心理治疗, 医学模式, 心理治疗的结果和有效性

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