

**Responses to Public Comments on Draft Version of the**  
***APA Clinical Practice Guideline for the Treatment of Depression***  
***Across Three Age Cohorts***

**American Psychological Association**  
**Guideline Development Panel for the Treatment of Depressive Disorders**

**Public Comment Period: August 23 – October 25, 2018**  
**Total Comments Received = 120**

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### **Introduction**

The guideline development panel owes enormous thanks to the many individuals and groups who have taken the time and energy to write thoughtful and, in some cases, very detailed comments and critiques of the draft American Psychological Association's (APA) *Clinical Practice Guideline for the Treatment of Depression Across Three Age Cohorts*. The panel has revised the guideline, as appropriate, based on the comments and has no doubt that the document is stronger as a result.

Every comment was read by the panel or by the Advisory Steering Committee (ASC) for APA's Clinical Practice Guideline Initiative, but not all comments have individual responses. Given the volume of comments (approximately 120 individuals or groups submitted comments during the public comment period, August 23 – October 25, 2018) and the common themes across comments (including identical comments from different submitters), the panel and ASC reviewed all and organized them by content and then, in many cases, responded to aggregated comments. The panel's and ASC's "response to comments" reflects the collective thinking of panel and ASC members regarding the entirety of received comments. Responses are not based on the identity of the commenter, the intensity of the emotion, or the number of persons who raised a specific comment but rather on the relevance of the comments to the goals and methods of the guideline development effort. If the panel believed that the comment justified a change to the guideline document, a change was made and usually indicated in the response to the comment; if not, no changes were made.

As noted, some comments are from established groups representing multiple individuals whereas other comments are from individuals. Groups that submitted comments included various APA governance groups and committees as well as organizations outside of APA such as the American Psychiatric Association (*Commenter 11*) and the Psychotherapy Action Network (*Commenter 52*).

A number of comments raised broader concerns about APA's Clinical Practice Guideline Initiative rather than the depression guideline specifically. In general, the ASC reviewed and responded to these broader concerns while the depression panel responded to concerns specific to the depression guideline document.

Response to public comments is organized into three sections. The first section contains responses from the ASC to broader concerns about APA's Clinical Practice Guideline Initiative. Representative comments illustrating these concerns are included. The second section contains all the comments from APA Boards and Committees with responses. The third section contains all comments pertaining to the depression guideline itself and responses to them from either the panel or ASC (as labeled). Some comments are fairly general concerning the full guideline document, while others address more specific topics such as research methodology and the guideline development process, legal issues, diversity issues, augmenting treatments, patient values and preferences, recommendations and specific treatments, suicide, population-specific concerns, and more. All comments received are presented verbatim in their entirety at the end of this document as well as in a separate attachment. Please note, while the panel attempted to specify the page numbers of edits in the guideline document, these page numbers may have changed in the final formatting of the guideline. Please use the page number as an approximation for the location of the change.

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## **ASC Responses to Broad Concerns about the APA Clinical Practice Guideline Development**

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### 1. Previous concerns about the PTSD Guideline were not addressed.

**Example quote:** *“PsiAN had offered critique regarding the scientific limitations and usefulness of the APA’s previously-issued Guideline for the Treatment of PTSD, and had hoped that our concerns would be addressed in the Guidelines to follow ... However, as a map for clinicians and their patients as to how to participate in effective psychotherapeutic treatment, this document, like its predecessors, offers virtually no additional insight to the working clinician, and may offer [hindrance] due to its misleading presentation of its findings.”*

#### **ASC response:**

The Advisory Steering Committee (ASC) for the Development of Clinical Practice Guidelines (CPGs) took very seriously the feedback we received about the PTSD Guideline. In addition to discussing the issues raised on our monthly calls, we arranged a 2-day in-person meeting in June 2018 to allow more time to discuss the concerns and ways we could be responsive while still maintaining the necessarily rigorous scientific standards that underlie the development of CPGs.

Below, we outline the different categories of concerns we identified and our proposed responses. Note, also, that we are committed to a transparent process that allows frequent opportunities for input. Along these lines, this information is also available on the APA CPG development web site (<https://www.apa.org/about/offices/directorates/guidelines/meetings.aspx>) and we can always be reached via email at [cpq@apa.org](mailto:cpq@apa.org).

#### **Concerns tied to generalizability**

- *The diversity of samples in the studies that were part of the CPG literature reviewed was not clear.*

#### **ASC response**

APA staff has been working on creating a demographic breakdown of the samples for each study included in systematic reviews for the depression guideline. A similar table was added for the PTSD guideline. Demographic information about the study participants is included in the systematic review utilized by the obesity panel. Moving forward, tables containing this information will be part of the template for future CPG documents, with the expectation that the tables will be updated when guidelines are updated.

- *Comorbidities are not considered.*

**ASC response**

A section will be added to the CPG document template for describing the comorbidities of the samples in the studies included in systematic reviews. The template will also require that CPG documents address the need for practitioners to consider comorbidities in their clinical decision making. In addition, CPG documents will note what information about comorbidities should be included in future research reports. For the depression guideline, please refer to the section, “Comorbidity of samples included in the reviews” for details on comorbidities in the included reviews.

- *Clinical samples don’t reflect the range of severity seen in practice.*

**ASC response**

While this historically was true (individuals were ruled out from participating in clinical trials for many reasons), more recent studies frequently have far fewer exclusion criteria. For instance, individuals with suicidal ideation not requiring immediate hospitalization now often are included in research on depression treatment. Specific to exposure therapy and PTSD, the inclusion and exclusion criteria of RCTs included in the systematic review used for the guideline suggest that most of the patients that participated in studies had a severe presentation of PTSD. Future guideline development panels will be asked to review available information about severity of the disorder/problem area amongst participants in the included studies, and to comment as appropriate in CPG documents.

The range-of-severity issue arises on the other end of the severity spectrum as well. Some patients with subsyndromal difficulties have a severity level that is so low that they would be screened out of many clinical trials, but they still present for treatment in practitioners’ offices. To address this issue, the CPG takes care to point the interested reader to the relevant location within each included review for the criteria used to select participants for the clinical trials. Also, in newly-developed supporting documents, we discuss strategies the practitioner can use to extrapolate from knowledge provided by the clinical trials to patients who do not meet full criteria for the trials.

- *Impact of individual differences is not considered.*

**ASC response**

The ASC discussed the need to be clear in CPGs about whether individual differences in treatment outcomes were examined (but there was insufficient data to draw conclusions, as typically occurred) vs. individual differences were not considered. We also plan to request discussion be added to every CPG document noting the importance of considering patients' individual characteristics as part of clinical decision making.

Additionally, we have prepared a brief statement about the importance of adapting the recommended treatments to meet the needs of the unique patient who is in the practitioner's office in that moment. This statement will be added to the introduction of each CPG, as it was to the beginning of the depression guideline. We have written a more comprehensive document describing strategies the clinician can use to adapt recommended treatments to meet the individual needs of each unique patient that will be posted on the CPG website. With these documents, we hope to make it clearer the ways that individual differences necessarily play a key role in developing a treatment plan that uses CPGs to synthesize the best available research evidence in conjunction with factors tied to each individual's unique values, background, and identities.

- *Only a narrow set of treatment outcomes was examined.*

**ASC response**

This is an important concern that cannot be fully addressed at this time because of limitations in the research base underpinning the CPGs. Although the ASC and depression panel recognize that various outcomes are important to patients, most clinical research focuses on diagnosis and symptom reduction. The ASC will encourage guideline development panels to consider a range of outcomes, including quality of life and improved functioning. The ASC noted that educating researchers and editors about the importance of examining other outcomes is critical so there can be sufficient data for future CPGs to examine a broader range of outcomes. That said, we do believe that symptom reduction is an outcome that patients do typically care deeply about, and this outcome is often tied to other aspects of functioning (e.g., effectiveness at work and in relationships).

In addition, several members of the ASC suggested that guidelines identify principles or processes of therapeutic change, not just symptom or single diagnostic category outcomes. A working group is being established to examine this possibility more deeply and report back to the full ASC. One issue to be considered will be what kinds of evidence will be used as the basis for rigorous characterization of change processes.



**Concern tied to study inclusion criteria**

- *Only randomized controlled trials (RCTs) are considered, which ignores other sources of evidence.*

**ASC response**

Evidence from sources beyond RCTs (e.g., observational data) is used when evaluating potential harms and burdens of interventions as well as patient values and preferences, and these criteria are used along with the efficacy data from systematic reviews to make the guideline recommendations.

The ASC acknowledges that RCTs are the main source of data used in the systematic reviews. The ASC discussed this concern at length and recognized its importance to many members of the community, but ultimately believed it was critical to adhere to best practices in the U.S. and internationally for guideline development as described by the National Academy of Medicine (formerly the Institute of Medicine; IOM), which clearly prioritize the use of systematic reviews for identifying and assessing evidence. The challenge is that while systematic reviews can include studies other than RCTs, RCTs are the types of studies most likely to meet the rigorous quality criteria.

As the working group explores the possibility of creating guidelines that give greater attention to change processes, it is possible that non-RCT evidence will play a different (expanded) role in guideline development.

**Concerns tied to impact of guidelines on practice**

- *CPGs will limit scope of practice and open therapists up to malpractice claims.*

**ASC response**

To date, no documented incidence of malpractice claims tied to a CPG has been reported to APA by psychologists. ASC members discussed whether there was a need to revise the disclaimer language in the CPG document template to enhance its clarity, but believed the current language included clear and prominent statements that the CPG was not intended to limit scope of practice. We also did a comparison of language used in the APA disclaimer and that used by other guideline developers (e.g., American Psychiatric Association, NICE) and found our language provided similar protections. Further, language has been added to the depression CPG document to specify appropriate legal use, and we continue to monitor this issue and seek ways to promote appropriate use of guidelines across settings and practices.

To further address the concern, material from Dr. Jana Martin, CEO of *The Trust*, who presented at the 2018 Practice Leadership Conference on how

psychologists who use guidelines can protect themselves from potential malpractice claims will be disseminated. Dr. Martin's primary advice was that psychologists protect themselves from malpractice by providing clear documentation of treatment rationale and decision making, points supported in APA policy documents as well. Additionally, Dr. Martin and other experts noted that systematic tracking of treatment outcomes can provide a demonstration of progress and the effectiveness of an intervention (which could also counter potential malpractice claims). The materials posted with each CPG include tools that clinicians can use to monitor their patients' progress. ASC members agreed that education on how CPGs could help providers meet insurance companies' expectations may be necessary, but also did not think there was compelling evidence at this time to suggest a significant threat to scope of practice existed.

Further, the Legal and Regulatory Affairs department at APA routinely works on behalf of psychologist members when it becomes aware that third party payers, health care systems, or others are potentially misusing APA policy or not appropriately administering their own policies. In that regard, those with indication of misuse should bring this information to the attention of the appropriate offices at APA to determine a course of action.

We would also like to respectfully point out that an advantage of CPGs is that they inform psychologists who are eager to adopt the most current and evidence-based approaches to treatment about what those treatments are.

- *Some APA members feel devalued.*

**ASC response**

This is an important concern as we believe strongly that APA developing CPGs can be a great benefit to the psychology community (e.g., by securing a stronger voice for psychologists in the healthcare marketplace, and by making it easier for busy providers to learn what the treatment outcome research indicates), and the ASC very much wants this to be a collaborative and inclusive initiative. To address this concern, we are working to help all APA members feel that they are included in the CPG development process and are invited to participate in it. The ASC has been striving to increase communication with APA members and the public regarding the CPG initiative to ensure more opportunities for respectful dialogue about needs and concerns. Useful avenues include presentations, such as the Board of Professional Affairs' "Town Hall" meeting at the 2018 APA convention (and we have submitted a proposal to hold another town hall at the 2019 APA convention); articles in Division, State Provincial Territorial Associations, presentations at APA's Practice Leadership Conference, and Directorate newsletters; and listserv messages. Also, invitations to self-nominate to serve on the ASC and on guideline panels are disseminated broadly.

Additionally, to learn more about the experiences and concerns of APA members who feel devalued by the CPG initiative, we are currently conducting a survey of a sample of APA members to evaluate the reach and impact of the CPG initiative and examine practitioner attitudes toward CPGs (this is being done with Dr. Jonathan Purtle of Drexel University, an expert in dissemination and implementation research, under contract with APA).

- *CPGs will stifle innovation in developing and delivering treatments.*

**ASC response**

The CPGs describe treatments that are studied in randomized controlled trials, and thus do not focus on new treatments that are in the early development stage. However, the description of well-established treatments is not meant to stifle the development of new treatments. In fact, one role of the CPG is to point to gaps in the treatment literature. The ASC very much wants to encourage continued research in treatment development and evaluation. We view the research gaps identified in CPGs as opportunities for development of new approaches and ask APA to work with funding agencies and journals to encourage such work. In addition to development of new treatments, the ASC noted the following aspects of patient care that would benefit from innovative approaches: informed consent, shared decision making between patient and practitioner, and measurement of outcomes throughout the treatment process.

***Concerns tied to over-valuing contribution of specific treatments to outcomes***

- *Need to consider variance accounted for by common factors.*

**ASC response**

To address this concern, the ASC has added a section on the role of common (non-specific) factors into the CPG template to make this a standard component of the guidelines moving forward. Currently, the extent of research evaluating the variance accounted for by common factors varies widely across disorders/problem areas so there is no way to include this information in similar ways across all CPGs. For the depression guideline, the panel included discussion on the importance of common factors (see sections on “Shared versus unique contributions of different psychotherapy models” and “Contributions from Shared and Specific Factors to Treatment Outcome”), and also provided some additional discussion and references on the topic of common factors in the more detailed responses to comments below.

The role of common factors will also be examined by a working group tasked with developing ideas for how change processes can be more fully addressed in CPGs.

- *All three legs (research evidence, clinician expertise, and patient characteristics, values and preferences) of the evidence-based practice stool need to be valued.*

**ASC response**

Guideline development panels will be asked to further discuss the importance of the three legs of the evidence-based practice stool (best available research evidence, clinical expertise, and patient characteristics and preferences) in the beginning sections of each CPG document. This expectation has been added to the document template. Notably, the CPG and numerous supporting documents already explicitly state that best available research evidence, clinical expertise, and patient characteristics and preferences all need to be considered in treatment planning. Further, guideline recommendation decisions are made based on four factors: the overall strength of the evidence for efficacy, the balance of benefits versus harms/burdens (note, clinician and patient representative input are included in the harms/burdens evaluation) of a treatment, patient values and preferences regarding a treatment, and applicability.

ASC members also discussed the need for greater integration on the APA website of related materials: e.g., clinical practice guidelines, professional practice guidelines, and other documents and reports related to evidence-based practice. With input from the ASC and relevant governance boards, staff will work on developing potential models for this integration.

**Concerns tied to implementation of CPGs**

- *Recommended treatments are not feasible to implement in the “real world.”*

**ASC response**

ASC members emphasized the need for future research that develops adaptations of treatments that can readily be implemented in a range of practice settings. Members also recommended that APA share examples of payers that support new forms of CPG-recommended treatments. Such examples include Medicare approving coverage for behavioral treatment of obesity and Optum Behavioral Health covering 90-minute therapy sessions. APA staff and the ASC will work with APA staff attorneys to draft a letter that practitioners can adapt to request reimbursement for extended sessions. Also, we hope the document on individualizing treatment can help practitioners consider how to adapt the recommendations from the CPG in a way that can work in their particular setting.

- *CPGs do not include enough content to help with implementation.*

**ASC response**

Although the guideline documents do not have explicit content on implementation, the PTSD CPG webpage has links to case studies, treatment manuals, audio/visual and textbook resources, and recordings of continuing education (CE) workshops on implementation of guideline recommendations. Similar materials will be provided on the webpages for the overweight/obesity CPG, depression CPG, and future CPGs. The ASC is also interested in enabling access to free or low-cost CE opportunities on recommended interventions and would welcome additional ideas for other ways to support implementation (please email any suggestions to [cpg@apa.org](mailto:cpg@apa.org)).

***Concerns tied to potential iatrogenic effects of CPG-recommended treatments***

We note concerns that have been expressed tied to the PTSD and the Obesity and overweight CPGs here. Similar principles would apply to concerns about CPG-recommended depression treatments.

- *For treating PTSD, patients would be harmed by exposure therapy.*

**ASC response**

Harms and contraindications of interventions are examined in the process of formulating guideline recommendations and addressed in the CPG documents. Notably, the PTSD panel found little evidence of harm following exposure therapy. The ASC felt that education regarding delivery of exposure therapy and its safety would be helpful for practitioners. ASC members and staff will identify practitioners who may be willing to discuss their own initial concerns and current perspectives on providing this type of intervention so that others may learn from peers.

- *Obesity and overweight CPG will promote stigma.*

**ASC response**

APA staff will include content for practitioners and families on the guideline website regarding the stigma associated with overweight and obesity, along with content tied to helpful strategies for talking with individuals that aim to mitigate the likelihood of stigma and promote healthier behaviors, self-concepts, and attitudes.

***Concerns that the process is biased toward certain treatments/outcomes***

- *ASC and guideline development panels are not sufficiently diverse (key constituencies are not represented).*

**ASC response**

Multiple facets of diversity are considered when selecting individuals to serve on the ASC, including theoretical orientations, areas of expertise, research and practice settings, individual identities and backgrounds (e.g., race and ethnicity), and geographic distribution. Efforts are made to circulate calls for nominations across all constituencies of APA and relevant external groups. We welcome ideas for how to further identify qualified diverse candidates willing to serve in these roles.

Also, we encourage interested readers to review the biographies of the current ASC members on the APA website

(<https://www.apa.org/about/offices/directorates/guidelines/steering-committee.aspx>) as we have diverse backgrounds, identities, areas of expertise, work settings, etc. represented among the ASC members.

Moreover, we take very seriously our responsibility to regularly report and discuss potential conflicts of interest that could bias our input and have numerous formal (required) and informal opportunities to share and discuss these conflicts.

- *More opportunities are needed for the community to provide input throughout the process.*

**ASC response**

As noted in all communications about CPGs, community members can email APA staff directly or through the public email address [cpg@apa.org](mailto:cpg@apa.org). Staff shares communications with the ASC and/or relevant guideline development panel. Also, individuals are invited to nominate themselves in response to calls for new members for the ASC and panels. Further, a 60-day public comment period on all draft guidelines is held before documents are finalized.

The ASC also discussed other opportunities to solicit public input, such as creating an “opt-in” email list to receive information and updates on the CPG initiative and holding an annual Town Hall at the APA convention. The ASC will also develop a form to invite public feedback on future CPG panels’ initial scoping of new guidelines. We will continue to seek additional opportunities for the community to provide input at other points in the development process.

**2. Guidelines are based on research that compares therapies to no treatment. Guidelines do not compare therapies with each other.**

**Example Quote:** “... it is important to note that the resources used to create the evidence-base for the Guideline overview aimed to address, for the most part, not which therapies worked better than other therapies, but which therapies worked better than non-therapies. Were a subtitle for the Guideline to be created, ‘Which therapies are better than nothing?’ would be far more accurate than “Which therapies work best?””

**ASC response:**

We understand the frustration when the evidence does not point to a simple answer regarding which ‘treatment works best,’ as is the case for mono-therapies for adults in the depression CPG, but we see it as encouraging that there are multiple treatment options that have good support. It is not the case that treatments were always compared to no treatment. However, data were often not available or not sufficient to make all desired comparisons among treatments. The following comparative questions were included in the scoping of the guideline (underlining added):

- For individuals in each of the three age cohorts with major depressive disorder, persistent depressive disorder, or subsyndromal depression, what is the effectiveness and risk of harms of psychotherapy, somatic treatments (e.g., electroconvulsive therapy, exercise, light therapy, repetitive transcranial magnetic stimulation, deep brain stimulation), or complementary and alternative medicine treatments in comparison either with one another or with pharmacotherapy?
- For individuals in each of the three age cohorts with major depressive disorder, persistent depressive disorder, or subsyndromal depression, what is the effectiveness and risk of harms of combinations of pharmacotherapy, psychotherapy or complementary and alternative medicine treatments compared with inactive or active single or combined treatments?

**3. Despite disclaimer statements from APA about the intended use of the guidelines, they will “reinforce the virtual monopoly of a very narrow range of treatment perspectives in terms of research, training and practice upon patients, to their detriment.” The guidelines will have implications for third party reimbursement, policy, research funding, and training decisions.**

**ASC response:**

We understand this concern but disagree that the guideline supports a very narrow range of treatments. The Depression guideline reviews a wide range of treatments, including Interpersonal psychotherapy, Family therapy, Psychodynamic therapy, Cognitive-behavioral therapy, Play therapy, Problem-solving therapy, Supportive therapy, Problem-Focused couples therapy, Cognitive-Behavioral Analysis System of Psychotherapy, Group Life Review Treatment, Mindfulness-based cognitive therapy, Behavioral therapy, Cognitive therapy, and many others (e.g., other therapies, medications, exercise, and numerous complementary and alternative treatments).

Also, the guideline includes a section on “Limitations of Existing Treatment Research Literature” that highlights the need for RCTs of therapies that are widely used in the community for which we do not currently have sufficient efficacy data. Further, as noted earlier, the current disclaimer language includes clear and prominent statements that the CPG is not intended to limit scope of practice, and our language is similar to that used by other guideline developers. (Please see additional comments above detailing the ways we are sharing information about how psychologists can protect themselves from potential malpractice claims. This is a concern we take very seriously but would note that, to our knowledge, no documented incidence of a malpractice claim tied to the CPGs has been reported to APA by psychologists.)

**4. “None of the treatments included in its analysis are sufficiently effective for a sufficient number of people to merit sustained investments.” Remission rates for these treatments would be considered woefully inadequate in medical meta-analyses.**

**ASC response:**

It is true that remission rates of the most effective treatments are lower than we would like. Nevertheless, many depressed patients are seeking treatment and we want to encourage access to the currently available treatments with the strongest evidence of efficacy based on the best available research. The fact that as a field we need to continue treatment development does not mean that we should not strive to offer the most effective treatments we can to our patients who need help now. A systematic review of the available research can help identify the optimal available treatments.



**5. Restricting the included studies to RCTs biases the recommendations towards specific treatments and excludes treatments with other kinds of evidence (e.g., humanistic, psychodynamic, psychoanalysis, etc.).**

**Example quote:** *“The decision to restrict its data to include randomized controlled trials (RCTs) exclusively, while less directly celebrated in this document than in the PTSD Guideline, nonetheless still shapes the data considered and the recommendations made. Noting it is artificially restrictive does not alter the bias it introduces into the treatment recommendations.”*

**ASC response:**

As described above, evidence from sources beyond RCTs (e.g., observational data) is used when evaluating potential harms and burdens of interventions as well as patient values and preferences, and these criteria are used along with the efficacy data from systematic reviews to make the guideline recommendations. Notwithstanding, the ASC understands that RCTs are the main source of data used in the systematic reviews because they are most likely to meet the quality criteria. The ASC has discussed this concern at length and believes it is important to adhere to best practices in the U.S. and internationally for guideline development, which clearly prioritize the use of systematic reviews for identifying and assessing evidence. Following such objective standards is particularly important given our guideline is issued by a guild.

Some therapies (e.g., psychodynamic psychotherapy) that are supported mostly by non-RCT evidence have been evaluated with some RCTs, and thus are represented in the guideline. Further, we try to make very clear that the exclusion of therapies not supported by data from RCTs does not mean these therapies are ineffective - just that insufficient RCT data are available to demonstrate their efficacy. We hope that the CPGs can encourage funding agencies and others to support future RCTs to study those therapies that were not included due to insufficient evidence.

**6. RCT methodology requiring comparison against wait list or TAU control would be impractical and unethical for therapies of several years' duration. “Epidemiological, outcome and other naturalistic studies are far more appropriate as measures of the effectiveness of many if not most forms of psychotherapeutic treatment” and more accurately reflect the actual experience of consumers.****ASC response:**

We appreciate the concern being raised but it is not clear to us why a Treatment As Usual (TAU) comparison condition would be unethical or impractical. More generally, CPGs are intended to recommend those treatments that have the strongest systematic research support. The methodologies suggested as alternatives to RCTs do not allow for causal inferences to be drawn with a significant degree of confidence. Recommending treatments that have not been systematically evaluated in a way that allows causal inferences is counter to one of the basic goals of a CPG, and a CPG would not be credible if it did not rely on a systematic review of high-quality efficacy data. Thus, it is our strong hope that more treatments will be systematically evaluated in a way that allows for causal inferences to be drawn so that even more treatments can be included in future CPGs.

**7. The construct of treatment as usual is imprecise with “much slippage.”**

A CPG necessarily reflects the current state of the literature (though we believe it can also be a call for future research) and we agree that the concept of TAU is imprecise. Note, this variation in TAUs can be viewed as a strength; if various TAUs are differentially effective, we can then examine the details of the effective treatments to try to learn what elements contributed to the treatment's effectiveness.

**8. Focusing on measures of symptom reduction and not comparing treatments on other measures such as lifestyle factors, relationship and work success, and productivity renders the guidelines less useful for those for whom their use is intended to benefit.**

**Example quote:** *“When lifestyle factors, relationship and work success and productivity are explicitly ruled out as relevant variables in evaluating treatment, a yawning disconnect opens between the purpose of the Guideline and the concerns of those who are intended to use it.”*

**ASC response:**

We agree that focusing on a broader range of outcome measures would make the guidelines even more useful. However, the guidelines are limited by what the scientific literature focuses on, which is typically loss of diagnosis and symptom reduction. We are encouraging guideline development panels to consider a range of outcomes, including quality of life and improved functioning, to the extent possible, but this remains a challenge given the available literature. The ASC noted that educating researchers and editors about the importance of examining other outcomes is critical so there can be sufficient data for future CPGs to examine a broader range of outcomes. That said, we do believe that symptom reduction is an outcome that patients do typically care deeply about, and this outcome is often tied to other aspects of functioning (e.g., functioning at work and in relationships).

**9. APA's guideline development process is informed by IOM standards in an attempt to make psychological and medical research equivalent. This excludes some of the most relevant types of research."**

**Example quote:** *"Epidemiological, outcome and other naturalistic studies are far more appropriate as measures of the effectiveness of many if not most forms of psychotherapeutic treatment, but are ruled out on the basis of their complexity, difficulty to operationalize, and inconsistency with the aspirations of equivalence between psychological and medical research, as dictated by the IOM, that inform the APA's Guidelines Program."*

**ASC response:**

We know that some members of the community are frustrated by the adherence to IOM standards, but this is done because we want to use gold standard methods to evaluate treatment efficacy. This is the gold standard approach across disciplines, including many disciplines beyond psychology and medicine. Especially as psychology is new to developing CPGs and we want our guidelines to be seen as credible and adopted widely, it is important to follow best practices.

**10. Lack of long-term follow up data does not allow the recommended shorter-duration treatments to be compared to the effectiveness of non-recommended longer-term treatments over time. Studies show that patients who achieve benefits from these treatments often experience recurrences, requiring additional treatment and creating a "revolving door" effect. The assumption that the recommended treatments are less time-consuming than the longer-term treatments is not supported by data.**

**ASC response:**

Thank you for raising this interesting discussion point. The panel is constrained in that the literature included and reviewed using the IOM guidelines did not directly address these issues. That said, the panel has attempted to note the importance of considering long term benefits and acknowledge this limitation, while staying consistent with the process that was used to develop the guidelines. Please refer to the sections in the guideline entitled, "Enduring effects of treatment" and, "Long-term outcomes of psychotherapy" for discussion of this topic. Also, while the guideline document notes that long term effects of treatment are not addressed by the included systematic reviews, the panel makes a call for additional future research into this domain and also addresses specific points relevant to long term effects (i.e., of combined treatment, for different age groups, etc.) throughout the document.

**11. The Guideline authors' repeated emphasis on the limitations of the included studies (reflective of limitations of research in general) undermines and weakens the scientific validity and credibility of the recommendations. We believe the only way this document can be useful to clinicians is if no specific treatments are recommended.**

**ASC response:**

We disagree and do not understand the rationale for this argument – there is practically no circumstance where we have perfect data to guide our decision-making. Why would this mean we should therefore make no recommendations? We believe that a frank assessment of the limitations of the included studies strengthens the scientific credibility of the recommendations by taking care not to overstate the claims of the studies included in the CPG. Further, clear statements about the limitations of the studies contribute to the scientific literature by pointing to the need for future studies to address the limitations. We will always be in a position of needing to make recommendations (and do treatment planning) based on imperfect and incomplete information. CPGs synthesize the best available research on efficacy and effectiveness to help with this inevitably challenging process.

**12. The Guideline authors acknowledge the lack of research specific to mechanisms of therapeutic action. They acknowledge the importance of the therapist-patient relationship, but disregard “all of the parameters,” including time frame, which could support development of this relationship.**

**ASC response:**

We agree that it is important to address the issue of mechanisms of therapeutic action. In response to this critique, we have added a short section on this topic to the Depression guideline and we are working to modify the format of future guidelines to address this issue in more detail in all future guidelines.

Further, we plan to create a working group to explore the possibility of creating a CPG that primarily focuses on principles of change.

**13. The Guidelines Program “perpetuates a view of psychology as reductionistic, scientific, essentializing and even, in its application, dehumanizing.” And highlights the problems inherent in trying to “reduce human distress to a compendia of symptoms.”**

**ASC response:**

The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.

Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.

Expecting any one document to address every consideration that goes into responding to the burden of mental illness and the complexities of the human mind and the social world in which we live and relate to others would doom any initiative to failure; in turn, the argument that because CPGs do not address every aspect of human suffering, CPGs have no value and are dehumanizing seems unfair. We would never claim that CPGs provide a ‘one-stop-shop’ for clinical care needs (and the introductory section of the guideline clearly encourages use of the guideline in conjunction with clinician judgement and information about clients' preferences and values), but CPGs can nonetheless be a very valuable part of clinical decision making.

**14. The Depression Guideline may be most useful because it highlights the inequity in the treatments that have received research funding versus those that have not. This is illustrative of the disparity between clinical psychology as it is researched and as it is practiced.**

**Example quote:** *“For the most part, the Depression Guideline is most useful as it highlights which of the commonly practiced treatment modalities have been most frequently studied and which have been underrepresented on the investigational grid. In the Guideline, endorsements are made solely on the fact that given treatments have been researched, rather than upon evidence that they are better than those that have not been.”*

**ASC response:**

We understand this concern and empathize with the frustration about limited research funding for treatment evaluation. We believe CPGs can be a useful document for advocating to funders for the need to evaluate different approaches. This is one reason we are asking future CPG panels to include a section on needs for future research, which includes the need for research on treatments that are widely used but not yet well tested.

**15. The questions posed by the Guidelines process cannot be answered without a re-visioning of psychological treatment and the mission of psychology as a whole.**

**ASC response:**

This argument suggests that a guideline is premature. We disagree. The magnitude of the problem of depression is undoubtedly huge and will require more and better solutions than those currently available, but as psychologists, we still have a responsibility to offer the best options we currently have available (even as we strive to improve what we can offer). The ASC believes that it is our responsibility to our members and to the public to inform consumers and practitioners about what the research currently says are the most efficacious treatments. Moreover, if we do not do that, other professional associations (including psychiatry, medicine, and others) will do so and our voice will be diminished. Developing guidelines now does not impede or preclude a re-visioning of psychological treatment; CPGs are like progress reports and they are intended to be updated as the evidence base advances.

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## **Responses to Public Comments Submitted by APA Boards and Committees**

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**Board of Professional Affairs (BPA)**

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**Commenter: Board of Professional Affairs**

Comment type: Group Comments

Group name: Board of Professional Affairs

Do you have any other comments about the draft guideline document?

General Comment - The Board of Professional Affairs (BPA) appreciates the opportunity to comment on this proposed guideline. BPA enthusiastically supports the development of clinical practice guidelines that advance and highlight research-based recommendations for the treatment of particular disorders. BPA appreciates that the guidelines offer assessments of the strength of the current scientific evidence for each recommendation and believes that this information sheds new light on additional opportunities for psychology research and practice. BPA commends the guideline panel for its exceptional work on this critical document.

Overall, BPA finds the guideline to be well developed and articulated. At the same time, BPA offers the following issues for review by the development panel. As follows:

- When the guideline addresses suicide among adolescents, it lacks sufficient detail on the rates of suicide among Latina adolescents and LGBT adolescents. It would be useful if this document contained this important information. Lines 178-186.

**Panel response***Thank you for this feedback, we have added this information.*

- When the guideline addresses Prevalence of depression in older adults, it may want to add that cognitive decline, age-associated neurobiological changes, and stressful events are contributing factors. Insomnia is an often overlooked risk factor for late-life depression (<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2852580/>) (<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC3522513/>) Lines 308-315.
  - When the guideline addresses access to care among the elderly diverse population, it lacks sufficient detail on language barriers, an important access issue among this population. Furthermore, family and psychoeducation interventions are of great value among depressed elderly of diverse ethnic and racial backgrounds. Lines 340-47
  - It is important to identify differences among cohorts in the older adult population which is defined as age 50+ in the current draft. In many statistics regarding older adults and depression, subgroupings by cohort can assist with understanding those most at risk. For example, suicidality is highest among the oldest old (80+) and subgroupings can be defined to assist with clarification.
- Thank you for your consideration of this input. Again, BPA appreciates the opportunity to comment on this proposed guideline.

**Panel response**

*We endorse all of the comments from the APA Board of Professional Affairs, concerning the pathogenesis, treatment and management of older adults with clinical depression. There are indeed multiple pathways to depression in older adults, some genetic and neurobiological in nature and others, psychosocial and behavioral. Moreover, the long window of time spanned by old age allows for the operation and interaction of many different factors in the causation of late-life depression (genetic, neurobiological, psychosocial). Heterogeneity also characterizes variability in treatment response, no less than pathogenesis. As BPA suggests, patient-focused but family-centered care is critical to treatment engagement, adherence, and success. Finally, because of the increasing racial and ethnic diversity of the nation's older adult population, sensitivity to cultural factors (including language) is essential to case formulation and implementation of effective, evidence-based treatment.*

*On page 13, line 9 we have added "cognitive decline, age-associated neurobiological changes, stressful events and sleep disturbance are also risk factors for late-life depression." On page 14, line 12 we have added "language barriers" to access to care issues. On page 16, line 11 we have added a sentence "Family and psychoeducational interventions can also be considered as means to reach diverse older adults." On page 13, line 9 we have added the sentence "It is also important to consider cohort differences in the prevalence, presentation, and treatment of depression among older adults, for example, the issues of a typical young-old person in their 60's may be quite different than that of an older-older person in their 90's."*



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**Board of Scientific Affairs (BSA)**

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**Commenter: Board of Scientific Affairs**

Comment type: Group Comments

Group name: Board of Scientific Affairs

Do you have any other comments about the draft guideline document?

General Comment - Below is the minute from the Board of Scientific Affairs regarding CC-5 APA Clinical Practice Guideline for Treatment of Depression: Call for Comments

Board of Scientific Affairs (BSA)

Unapproved Draft Minute

The work conducted on depression treatment relied on appropriate expert review procedures recommended by IOM and NICE. Relevant systematic reviews were used that were produced by experts and appeared in peer-reviewed outlets. The selection of these sources is consistent with practices suggested by IOM and AHRQ and followed by Cochran reviews. The document is very careful to distinguish between “standards” and “guidelines,” and to communicate that the recommendations are not to be considered mandatory.

The report is careful to note patient, therapist and other contextual factors that may be missed by narrative and meta-analytic results. The discussion allows for the possibility that certain treatments might be effective under yet to be determined conditions. The guidelines are very carefully articulated in terms of categories of “proof.” Furthermore, the decision to use the lowest quality coding to determine the strength of evidence was conservative. The limitations of the current evidence and the strength of the report’s recommendations are well-described and balanced. If anything, the authors seemed to “bend over backward” to be restrained in their recommendations noting where the evidence base was small or methodologically weak.

The document can serve to alert clinicians, patients and the public about the limits of current knowledge with respect to some depression treatments. The fact that some treatments lack an evidence base is important to communicate. At the same time, some recommendations, with respect to the three age categories, rest on a reasonably strong evidentiary base, worthy of reporting. All recommendations came with a description of the limitations, unanswered questions, and need for clinicians and patients to take into account the context and factors unexamined by the empirical studies to date.

We acknowledge that conducting a new systematic review/meta-analysis would have been useful, but labor and costs were prohibitive. In any case, there would be too few relevant studies for some treatments to allow for meaningful meta-analytic results. Given that depression guidelines from other professional organizations are dated, this document will serve a useful function.

**Panel response***Thank you very much for the review and feedback!*

Other comments: Some readers had confusion about the reliance on a single reviewer for study quality (page 50 of the pdf). Also, we were uncertain about that coder’s credentials. Perhaps that information was in the document; making that more salient in the main text is advised. Also,

why was only a single reviewer used (if the Board understands the process correctly). Was this because of cost and time?

**Panel response:**

*Thank you for bringing this point of confusion to our attention. The reliance on a single reviewer was used in three meta-analyses for the selection of studies from the searches. The quality of the included studies was rated by two independent reviewers for these meta-analyses, not a single reviewer. We will revise the text to make this point clearer. We also queried the lead author of these meta-analyses, Dr. Pim Cuijpers, and he reported that he served as the single reviewer for the article selection. He reported that decision was made due to lack of capacity in his research team to have two reviewers and because it was not yet the habit in the field at the time to use two reviewers. The guideline development panel decided to relax this criterion because the three meta-analyses had information that was considered essential for the guideline and were otherwise rated as quality reviews. Dr. Cuijpers is a clinical psychologist, methodologist and member of the guideline panel (although recused himself when voting on content specific to the quality of these reviews). We will revise the text to make this process clearer.*

Minor: There are some missing comma's p. 9, line 3; p. 8, line 26.  
"Systemic" appears when "systematic" (p. 26) was intended.

**Panel response**

*Thank you, we are making these edits.*

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**Board of Educational Affairs (BEA)**

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**Commenter: Board of Educational Affairs**

Comment type: Group Comments

Group name: Board of Educational Affairs

Do you have any other comments about the draft guideline document?

General Comment - The Board of Educational Affairs (BEA) reviewed the Clinical Practice Guideline for Treatment of Depression during its Fall meeting. BEA appreciates the diligence and rigor the panel has devoted to this draft of the guideline for the treatment of depression. While understanding that this is proposed as a guideline and not as standards, we expect that a guideline released by APA will be taken very seriously and will be used to shape training as well as practice in psychology. We believe that the strategy of identifying recent, high quality reviews and meta analyses, provided appropriate support for the identification of recommended and conditionally recommended treatments of depression with adult and older adult populations. However, there were fewer reviews and meta analyses related to the treatment of depression with children/adolescent populations and the conclusions from these reviews are more tentative than the recommendations for treatment in adult populations. In light of this, we recommend the following related to the treatment of depression in children/adolescents to, (1) commission a de novo review with a specific emphasis on children/adolescents developmental periods; (2) represent the children/adolescents recommendations as a white paper instead of a guideline; or (3) broaden the criteria for reviews such that a larger number of sources might be used. We are attaching a recent analysis by Weersing et al. 2017 for your consideration.

[Ms. Andrade sent the analysis via email. To access this article, please click on the hyperlinked text]:

Weersing, V.R., Jeffreys, M., Do, M.-H. T., Schwartz, K.T.G., & Bolano, C. (2017). [Evidence base update of psychosocial treatments for child and adolescent depression](#). *Journal of Clinical Child & Adolescent Psychology*, 46(1), 11-43. doi: 10.1080/1537416.2016.1220310

**Panel response**

*To address the issues in these comments would be outside the scope of the guideline and we recognize the limitations of the methodology throughout the guideline. However, we support additional future efforts to expand upon and revise this current guideline, and for APA to commission de novo systematic reviews for a number of domains the current guideline does not address.*

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**American Psychological Association of Graduate Students (APAGS)**

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**Commenter: American Psychological Association of Graduate Students (APAGS)**

Comment type: Group Comments

Group name: APAGS

Do you have any other comments about the draft guideline document?

General Comment - The American Psychological Association of Graduate Students (APAGS) wishes to express their appreciation for being invited to comment on the draft version of the Clinical Practice Guideline for the Treatment of Depression in Children, Adolescents, and Young, Middle-aged, and Older Adults. In general, we found the guidelines to be well-researched, reflecting tremendous effort on the part of the authors. In particular, we wish to acknowledge the utility of considering how depression rates and outcomes are different based on sex and race/ethnicity and found it useful to evaluate treatment efficacy by these distinctions. In addition, we wish to echo the limitations of the guidelines raised by the authors themselves related to the inability to address how depression treatments may differ in under-served or under-represented populations and with respect to co-morbidities. Differences in social, ethnic, cultural, and economic backgrounds, in addition to differences in pre-existing psychopathology, influence the utility of treatment options. Therefore, we thank the authors for making it explicit that there are limitations to systematic reviews and RCTs that fail to include diverse populations.

**Panel response**

*Thank you, we appreciate your feedback. The panel also recognizes that psychotic depression is an important domain and has acknowledged this as a limitation in the guideline.*

In addition, we have a few minor suggestions for improvements for the document that accompanies the guidelines:

- 1) Please consider revising the title of the practice guidelines to “Clinical Practice Guideline for the Treatment of Depression”.
- 2) The section “Adult depression and race/ethnicity” (p. 11) introduces complexities related to defining certain racial and ethnic groups. These complexities include lumping individuals based on origin that may occlude important differences in individuals’ background, including heritage, place of origin, and current country of residence. While important considerations in how race and ethnicity is studied, these considerations are not specific to adults. Are the authors willing to address these considerations up-front within the Introduction, as they apply to all studies across the lifespan?

**Panel response**

*While it is important to consider within group differences related to racial diversity and depressive illness, the current literature base precludes our ability to disaggregate racial/ethnic groups to study differences both within and between racial identifiers. Instead, current literature struggles to adequately address the racial/ethnic diversity that currently exists in the U.S, and abroad such that we can derive generalizable findings about depression treatment (psychotherapeutic and otherwise) relevant for aggregated racial groups. As it stands, the current literature fails to allow for a deep dive into across groups differences which speaks to the need for a far greater focus on increasing the generalizability of study participants and the relevance of depression treatments to more adequately reflect the full population of persons affected by depressive illness.*

3) It would be interesting to see a section detailing sex differences in older adults.

4) While the sections outlining differences in depression across races and ethnicities in children/adolescents and adults were comprehensive, the section on racial and ethnic composition of depressed older adults is overly broad. For instance, it is not clear what ethnic backgrounds have difficulties in treatment accessibility from what is written.

**Panel response**

*APAGS asks about sex differences in response variability to treatment in older depressed adults. This important issue has not yet received adequate attention in the literature (despite the greater suicide rates in older men in the context of depression). APAGS also asks about racial and ethnic disparities in depression treatment access. Older African Americans and Latino Americans tend to be under-represented in clinical trials, making it difficult to draw inferences about race and ethnicity as moderators of treatment response. There is some evidence that if adequate treatment (pharmacotherapy or psychotherapy) is provided and adhered to, response and remission rates will be satisfactory in black Americans, comparable to rates observed in whites. On the other hand, it is also difficult to disambiguate the effects of race/ethnicity and poverty on treatment response variability and race/ethnicity and poverty have some correlation.*

5) The section “Child/adolescent depression and race/ethnicity” discusses discrepancies in the data regarding prevalence across races. Would the authors consider discussing some of the barriers to care and access to mental health issues that disproportionately impact youth of color regardless of prevalence rates? This issue may be broader than just children and adolescents. In addition, treatments are often adapted for youth of color instead of created for them with cultural considerations in mind.

**Panel response**

*The panel speaks to this issue by indicating the need for more research on treatment engagement strategies that seek to reduce barriers to care for youth of color and will make this section more explicit to delineate some of the well-established barriers to care.*

6) We acknowledge how difficult it must be to create a comprehensive guideline inclusive of all clinical presentations. Have the authors considered discussing depression in LGBTQ individuals in more detail given the high rates of depression and suicide? Other groups of interest that are not discussed in detail include individuals with developmental and intellectual disabilities and those who have experienced trauma.

**Panel response**

*The panel noted throughout the process the limitations imposed by numerous factors (e.g., time, resources, quality of available studies, the enormity of the basic focus) that inhibited us from pursuing “everything related to the treatment of major depression.” The basic piece of data we worked with involved (properly executed) meta-analytic reviews of the literature published within 5 years of the beginning of the committee’s work. By that very nature, if a review did not exist, we are unable to comment on a given issue with the type of empirical rigor and objectivity afforded issues related to our main focus.*

7) We suggest that the authors comment on why treatment options for Major Depressive Disorder with Psychotic Features was not considered for the final draft of the guidelines, as many treatment providers would be interested in this.

**Panel response**

*The panel also recognizes that psychotic depression is an important domain and has acknowledged this as a limitation in the guideline.*

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**Committee for the Advancement of Professional Practice (CAPP)**

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**Commenter: Committee for the Advancement of Professional Practice (CAPP)**

Comment type: Group Comments

Group name: Committee for the Advancement of Professional Practice (CAPP)

Do you have any other comments about the draft guideline document?

Recommendations - During the special Fall APA Consolidated Meetings, October 12-14, 2018, the Committee for the Advancement of Professional Practice (CAPP) reviewed Cross Cutting Item CC-05 APA Clinical Practice Guidelines for Treatment of Depression: Call for Comments. As a result of discussion of the item, CAPP has prepared the following statement in response to the CC-05 Main Motion.

CAPP congratulates the committee that put together the clinical practice guidelines for the treatment of depression for a very thorough and systematic analysis of the research data and for the production of a clear and forward document. Further, CAPP recognizes the importance of these guidelines in establishing psychologists' place in healthcare systems and payment models using our foundation in science to guide clinical practice. We can see the effort to include therapeutic approaches beyond those of the traditional medical models which have provided large amounts of research data. We believe that this approach recognizes the complexity of the disorder and its treatment, enriching practitioner's tools when treating this widespread health problem.

We are concerned about the dissension that guidelines are creating among practitioners from different modalities and among practitioners and scientists. The issues raised by members' comments regarding the applicability of the guidelines given the limitations of the studies highlight the need for a comprehensive review of the coordination and support between the four areas of the field: education, practice, public interest and science and among the three "legs" of Evidence Based Practice in Psychotherapy (EBPP)—best research evidence, clinical expertise and patient values, characteristics and preferences. There is a concern about the limitations of research models to capture the long-term history of psychodynamic and vulnerable population treatment of depression.

CAPP supports the importance of the depression guidelines in establishing the current evidence base for practice, while respectfully recommending a bidirectional relationship between clinical science and practice and encouraging promotion of funding for scientific study of psychodynamic and other understudied treatment modalities.

Thank you for the opportunity to review and provide comment on this item.

**ASC response**

*Concern was expressed that the CPG is biased against insight-oriented therapies. We very much hope this is not the case as the goal is to provide as unbiased an evaluation of the evidence as possible. We understand that the concern stems in part from the heavy use of RCT data to guide recommendations. Notably, some insight-oriented therapies (e.g., psychodynamic psychotherapy) that are supported mostly by non-RCT evidence have been evaluated with some RCTs, and thus are represented in the guideline. Further, we try to make very clear that the exclusion of therapies not supported by data from RCTs does not mean these therapies are ineffective - just that insufficient RCT data are available to demonstrate their effectiveness. We hope that the conclusions of this and other CPGs can encourage funding agencies and others to support future RCTs to study those therapies that were not included due to insufficient evidence.*

*CPGs are intended to recommend those treatments that have the strongest systematic research support. Recommending treatments that have not been systematically evaluated is counter to one of the basic goals of a CPG, and a CPG would not be credible if it did not rely on a systematic review of high-quality efficacy data. Thus, it is our strong hope that more treatments will be systematically evaluated in a way that allows for causal inferences to be drawn so that even more treatments can be included in future CPGs.*



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### Committee on Women in Psychology (CWP)

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**Commenter: Committee on Women in Psychology (CWP)**

Comment type: Group Comments

Group name: Committee on Women in Psychology

Do you have any other comments about the draft guideline document?

General Comment - The Committee on Women in Psychology (CWP) appreciated the opportunity to review CC-5: APA Clinical Practice Guideline for Treatment of Depression: Call for Comments. CWP wishes to express concern about the readability and utility of the report in its current form and how to place these guidelines in context. We recognize the committee's stated intent to provide an updated review of the literature but urge the committee to use their expertise to guide readers on how to interpret the large numbers of studies that have emerged but have not necessarily made its way into clinical practice or have added significantly to content knowledge in this area.

**Panel response**

*The panel appreciates the constructive comments from the Committee on Women in Psychology. The panel has revised the guideline based on these and other comments addressing readability and utility. In particular we have addressed how to consider literature that was not included in the guideline.*

*Text to add in the "implementation setting."*

*Incorporation of research not included in the current guideline. The panel recognizes that there is literature that is relevant to decision-making about treatments that has not been incorporated in this guideline. In considering implementation of an intervention that is not reviewed in the guideline, the panel encourages readers to consider number and quality of studies available. Interventions with both well-controlled studies of efficacy and comparative effectiveness relative to other effective treatments should be prioritized.*

We also have the following specific concerns and suggestions:

- a. First, gender and other identity factors are referenced quite generally given that women are far more likely to be diagnosed with MDD than men- prevalence rates are now well established. A related, significant issue is the lack of critical discussion of adequate representation in studies of these groups.
- b. Further, as the committee pointed out, there are significant inequities in which treatments are funded, and not enough differentiation of different psychological therapies. The CWP encourages the committee to comment on what this says about our field vis a vis mood disorders, note when we should interpret findings with caution, and then provide principles of best practice. Similarly, without knowing the funding and recruitment patterns for psychopharm studies, it is difficult to put them into context. It would be helpful for the panel to discuss issues such as the small effect sizes for psychotherapy outcome studies and investigator bias.
- c. Next, while we appreciate the committee did not have the scope to address all the various topics under the umbrella of depression, but the exclusion of suicide studies seems significant especially as suicidal thinking is explicitly part of the diagnostic criteria which is unlike

other disorders. Further, it would be prudent to also address suicidal ideation and some guidelines on suicide assessment/prevention in the guidelines.

d. Due to the complexity of mood disorders and possible co-morbidity with other conditions, we recommend some discussion regarding psychological measurement of depressive symptoms for appropriate diagnosis/treatment outcomes. In these comments, also some discussion regarding misdiagnosis and how depression manifests in other diagnoses like Post Traumatic Stress Disorder, and how there may be differences based on gender expression/cultural mores/situational factors.

e. We did not find any sections addressing situational depression, including life experiences of grief/bereavement.

**Panel response**

*Thank you for your comments. Unfortunately, as you point out, there are multiple areas that were not covered in detail in this guideline. Given the prevalence and ubiquity of major depressive disorder, along with the large number of studies aimed at evaluating various means to address this problem, it is extremely difficult to do justice to all the issues that appear to be related to this topic, including sex differences, comorbidity (e.g., suicide), different mood diagnoses/disorders, and assessment issues. Properly addressing even one of these topics would likely increase the committee's workload by 50% or more. Thus, as stated throughout the guideline, we were, by nature, limited in our ability to address everything we thought important. Our major task was to review the literature, given IOM standards, with an exceptionally objective point of view. This is unfortunate. However, this exercise allows the committee to highlight gaps in the literature and what needs to be done in the future to be able to provide for more meaningful conclusions that can inform clinical practice (e.g., what treatments are more effective for a specific subgroup, such as women, ethnic minorities, individuals with comorbid mental or physical disorders).*

Additionally, variations in cultural expressions of depression was not addressed in this set of guidelines. It was mentioned that "paying attention to diversity issues" was needed but considering this is a set of guidelines, more information is needed for practitioners beyond this statement.

**Panel response**

*The committee appreciates the responder's concerns regarding the import of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The committee was comprised of experts with knowledge and skill in these areas who thoughtfully contributed concerns regarding these issues throughout the process of guideline development. Further, the committee included multiple representatives of the patient and client populations who brought individual knowledge and experiences of intersectionality into the process. The committee agrees that both the conceptualization and expression of depression are important considerations for clinicians to examine as a part of the diagnostic and treatment process.*

CWP appreciated the opportunity to review this document. The committee would like to request feedback on how these recommendations are considered and/or incorporated into the document. The committee also welcomes any requests for additional information to address this area of concern.

If you have any questions or require additional information, please contact staff liaisons Shari E. Miles-Cohen, PhD at [smiles@apa.org](mailto:smiles@apa.org) or Tanya L. Burrwell at [tburrwell@apa.org](mailto:tburrwell@apa.org) or ext. 6044.

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**Committee on Early Career Psychologists (CECP)**

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**Commenter: Committee on Early Career Psychologists (CECP)**

Comment type: Group Comments

Group name: Committee on Early Career Psychologists

Do you have any other comments about the draft guideline document?

General Comment - The Committee on Early Career Psychologists (CECP) reviewed the APA Clinical Practice Guideline for Treatment of Depression during their meeting on October 4th, 2018.

Overall some great concerns given the fact that the guidelines are limited in so many ways by the way they went about ruling in and out recommendations and then the fact that they have to address limitations so much. Also, as they themselves share there is more funding and support for researching just a few approaches than others, which does not necessarily mean that those that are researched most, by the finite way they determine, means they are more efficacious than those approaches that did not have the same funding support for research. Similarly, ruling in of primarily RCTs vs other considerations is potentially problematic as they also acknowledge. The review provides ample evidence that research money needs to be diversified and where the gaps are within the field. This calls to question to utility of producing guidelines when the data is severely lacking. We have significant concerns that RCTs are offered as the only viable evidence, except where the panel decided it was relevant to include other sources of information. Can we not utilize the same exact rationale for including other sources of information in every other section? This practice frequently rules out and excludes many people and may be only implemented in one specific way so it does not apply as much in the real world.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

It would be interesting to know of all of those systematic reviews how many people were excluded from studies and/or dropped out. Relatedly, of serious concern, which is brought up at multiple times in the document, is the lack of a diverse sample across all of the literature reviewed and included that makes the recommendations for specific psychotherapy such as CBT and IPT, and/or specific medications.

**Panel response**

*We are adding summary information about study exclusions/dropouts as well as a table listing diversity information for studies included in the systematic reviews.*

There is a short nod to using the multicultural guidelines along with the depression guidelines but if the depression guidelines do not fully address and include people of diverse identities this seems inconsistent with what the multicultural guidelines advocate itself (using research that shows efficacy among diverse populations). One paragraph on the idea that the depression guidelines should be used and then implement in addition the multicultural guidelines really feels uncomfortable as it may be overlooking some of the procedures of how to select treatments and interventions as shared by the multicultural guidelines.

**Panel response**

*While we appreciate the concern that some procedures on selecting treatments and interventions for diverse populations are not well addressed in this guideline, the necessary literature to address this need was not included in the panel's reviews. Intervention and implementation of this guideline's recommendations will be facilitated through use of the complex and nuanced information in APA's multicultural guideline and other professional practice guidelines addressing issues relevant to diverse populations.*

This process continues to perpetuate the idea that technique is the most important thing in the therapy room. While the panel provides occasional recognition to the factors that actually make therapy work, it is consistently presented in a format that discounts some of the most effective/strongest data that we have about the therapeutic process. Previous versions of guidelines (e.g., PTSD) have been presented in a manner of recognizing the problems, and taking the "we will fix it later" stance. In the meantime, APA stands to compound the problem. Given that ECPs will be the ones to deal with the consequences of these actions on all levels, it is of great concern that we are putting forth a document where nearly 50% of the content pages are discussing the limitations and caveats of the research.

**Panel Response**

*The concern that there is undue emphasis placed by the review on technique as the most important process in therapeutic change is a most important one. The approach of using meta-analyses of RCT's and looking for best treatments is based on a number of assumptions that do need comment and clarification. RCT's do potentially ignore the common or non-specific factors which may be a key ingredient of treatment regardless of technique. There however are no clear experimental studies yet that have been able to make a causal link between the non-specific factors and outcome. There is copious evidence of correlational research on relational factors, such as empathy and the working alliance and outcome, which although not being experimental, is too consistent to ignore. As has been shown in one of the meta-analyses there is evidence that offering a supportive relationship is not significantly different to more technique-based treatments and this lends some support to the helping relationship as an important ingredient of effective treatment.*

Medicine was founded on the idea that human beings are simply mechanisms; processes that can be "fixed" when something goes wrong. This flies in the face of every piece of meaningful psychological construct that has shown up in the literature. We are relational beings. Might it be time to consider the institute of medicine standards are not appropriate or might even be misleading for the complexities of the psychological world? Given that people respond better to medication when they have a positive relationship with the prescribing physician, setting aside the other common factors variables in service of asserting which technique may be better (comparative research shows no differences).

Other detailed concerns:

It seems irresponsible to not address psychotic depression in some way as it is an experience with MDD and the guidelines are stating these recommendations are for MDD. Understandable to leave out treatment for affective psychosis under the realm of a schizoaffective presentation but not with MDD if these are proposed for MDD.

## ES-1 lines 16-19

Some of the areas the document says it leaves out such as addressing screening for depression, associated comorbidity with suicide, and response to treatment seem irresponsible to leave out. It makes sense that the guidelines can't address everything but these few areas at least should have some relatively succinct space in the document.

**Panel response**

*The panel also recognizes that psychotic depression is an important domain as well as comorbidity and suicide. and has acknowledged the limitations in the guideline. The panel supports future efforts to expand upon the current guideline to address these important domains.*

## ES-2 lines 2-3

These lines address what defines depression solely by diagnostic information of the DSM-V. Later this is addressed via different manifestations across gender but there should also be a line about this in here.

**Panel response**

*We do not understand the meaning of this comment. Page ES-2 lines 2-3 discuss process and methodology of guideline development, not depression diagnosis.*

## ES-4 lines 3-6

The statements earlier on ES-1 about not including monitoring of response to treatment in the guidelines while the group shares here on ES-4 that "selecting which outcomes were most critical for deciding upon recommendation, the panel decided that response to treatment and serious associated harms/adverse events were critical" seems inconsistent with not providing some guide to providers about how to monitor response to treatment and any associated reduction in suicidality.

**Panel response**

*The panel defined the critical outcome of response to treatment as reduction in depressive symptoms (i.e., at post-treatment). This is a qualitatively different concept than monitoring response to treatment which refers to continuously obtaining client feedback throughout treatment in order to determine whether treatment is helping the patient as expected.*

## ES-5 lines 5-8

See this as a great concern that studies exploring efficacy of psychotherapy are not conducted equally across modalities. This potentially rules out psychotherapies that are potentially as efficacious but the inclusion and recommendation criteria did not include them. Also, inpatient samples were excluded which may have ruled out people with the most serious treatment resistant depression and associated suicidality and/or MDD with psychotic features, which is concerning.

ES-5 line 23

There is a nod to shared-decision making starting here and throughout the article; there should be an explanation of this model and a nod to the developer Pat Deegan as a citation and reference. Does not seem this is there.

**Panel response**

*The panel is using the term shared decision-making in a much broader manner, not referring to a specific model.*

ES-6 line 3

Inpatient care not included. Again; most serious cases of depression and suicidality maybe not included?

**Panel response to ES-5 lines 5-8 and E-6 line 3**

*This is an important concern with which the panel struggled. The panel ended up including treatments that did not have efficacy studies in the literature reviewed but did have comparative effectiveness studies with comparisons to treatments that did have well established efficacy. This is an inclusive approach to recommendations. The panel felt this provided the most useful guidance to readers while maintaining scientific rigor.*

*Limitations due to not including inpatient populations and suicide were addressed in prior sections.*

ES-6 Line 14/15

“Further research is needed examining what components of psychotherapy are effective and processes of change.” Research in this area is already robust, this statement is misleading given the common factors and other research that clearly shows what works in psychotherapy.

**Panel response**

*The panel decided to remove this particular line. The comment was made with the goal of reflecting that while there is research in these domains, translating that research into an actionable guideline is difficult due to the limitations of the research design in these domains, the panel decided that an elaborated discussion of these issues would be more confusing than helpful.*

ES-9 line 6

This line should qualify if suicidality and psychotic features were rule outs.

**Panel response**

*The Zhou et al. (2015) review notes that psychotic depression was a rule out but does not include suicidality as a rule out. The Cipriani et al. (2016) review does not list either suicidality or psychotic features as rule outs, however they stated that there was a paucity of data on suicidality and antidepressants. We are adding information about the psychotic depression rule out to the indicated location in the guideline.*

ES-15

This table talks about switching from anti-depressant medication alone to cognitive therapy alone broadly; it seems like this table should consider carefully there pointed recommendations and also share what caveats are known about how suicide risk may factor in here for treatment resistant depression.

**Panel response**

*The panel did not have adequate information to comment at a more detailed level for this recommendation.*

There is no review of literature on use of ECT or not, but medications are recommended. Consistently, with people that have treatment resistant depression ECT is used in the “real world.” There should be some acknowledgment of this and sharing of the literature if APA is similarly recommending pharmacological treatments, even if it is a recommendation to consult for the possibility of this helping.

**Panel response**

*The panel has identified this (and the lack of information regarding somatic and interventional treatments) as a limitation of the guideline.*

This area also has recommendations for older adults with cognitive impairment and psychotherapies. Was augmentation with cognitive remediation also evaluated?

**Panel response**

*Augmentation was not addressed.*

PG 2

Line 6

After line 6 the document seems like it should address how depression presents differently across various cultures and gender, etc.

**Panel response**

*The committee appreciates the responder’s concerns regarding the import of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The committee was comprised of experts with knowledge and skill in these areas who thoughtfully contributed concerns regarding these issues throughout the process of guideline development. Further, the committee included multiple representatives of the patient and client populations who brought individual knowledge and experiences of intersectionality into the process. The committee agrees that both the conceptualization and expression of depression are important considerations for clinicians to examine as a part of the diagnostic and treatment process.*

Line 22/23

This reference is quite old, are there no newer studies that discuss prevalence rates?

**Panel response**

*We are adding a newer reference here.*



Pg. 5 Lines 1-6

This information seems largely redundant with what has already been said.

**Panel response**

*The goal of this sub-section is to emphasize characteristics of depression that are unique to children and adolescents.*

Pg. 6

Sex differences section and throughout manuscript The document begins by stating sex is the topic, but then discusses gender in several places. This suggests a conflation of biological sex and gender identity that is extremely concerning for those who have target group identities. The social context differences and gender role identity expectations are a critical component of how “depression” gets expressed, including what individuals will endorse on the commonly used measures.

It is unacceptable that an APA guideline would reinforce biological sex and gender identity as interchangeable.

**Panel response**

*We are checking on our use of sex and gender throughout the document to be consistent.*

line 23

This statement should really be qualified about a greater percentage of white youth vs AA youth experiencing depression. So there are many problems with the research here as culturally it is also more off limits for many AA men to talk about having depression or ever seek treatment for it. So many men of color may be suffering in silence and there is increasing evidence of this as Black men are coming out and speaking about this.

**Panel response**

*The committee appreciates the responder’s concerns regarding the import of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The section on depression and race ethnicity within the child adolescent guidelines portion provides the references used by the committee to support the ideas regarding the equivocal nature of prevalence estimates on depression in youth of varied racial/ethnic groups.*

Pg. 10

This area should share statistics that men underreport depression for fear of seeking MH treatment and it’s stigma. Interesting with the literature that shares though women attempt suicide more men complete suicide more; what is the role of untreated depression and gender role conflict?

Pg. 11 lines 10-12

This should also reflect the burgeoning literature on the likelihood of Black men to not identify or seek help for depression but may later complete suicide.

Pg. 16 line 20-21

What is considered a minimum does of treatment. The reference without discussing the concept does not make sense.

**Panel response**

*Thank you for your comments.*

*The committee continued to lament throughout the process regarding the limitations imposed by numerous factors (e.g., time, resources, quality of available studies, the enormity of the basic focus) that inhibited us from pursuing “everything related to the treatment of major depression.” The basic piece of data we worked with involved (properly executed) meta-analytic reviews of the literature published within 5 years of the beginning of the committee’s work. By that very nature, if a review did not exist, we are unable to comment on a given issue with the type of empirical rigor and objectivity we could afford to issues related to our focus. Thus, we are unable to comment with any “authority” on the types of issues you raised.*

Pg. 17 line 3 , 20-25

This area shares that these guidelines are substantially different because they offer pt expertise and their preferences and values. This seems pretty inconsistent with the area of the document in the beginning that makes a disclaimer that the guidelines overall make recommendations primarily on symptom reduction or resolution rather than on person-centered things that are valued such as quality of life, social connection, etc. (which also greatly relate to reduction in depression).

**Panel response**

*Reduction of depressive symptoms was one of the critical outcomes considered by the panel. However, in forming recommendations regarding treatments to reduce depressive symptoms the panel considered four main areas, one of which was patient preferences and values about treatment.*

P. 18 line 6-8

This area shares that they involved consumer stakeholders in the process but if person-centered values were not considered and the entire recommendations are focused on symptom reduction this is likely hugely inconsistent with the claim that consumer stakeholder opinion was involved.

**Panel response**

*Reduction of depressive symptoms was one of the critical outcomes considered by the panel. However, in forming recommendations regarding treatments to reduce depressive symptoms the panel considered four main areas, one of which was patient preferences and values about treatment.*

P. 38

There are concerns about the applicability of use of primarily RCTs due to differences in the real world from very specific samples and real world application. This area is problematic and there

are many concerns with this, as addressed somewhat, however, to proceed and not find some other way to integrate other knowledge seems like an oversight.

### **Panel response**

*The American Psychological Association's clinical practice guidelines follow the Institute of Medicine's (IOM; now National Academy of Medicine) standards for developing clinical practice guidelines, and thus rely on data from high quality, independent systematic reviews (also conducted according to IOM standards; Institute of Medicine, 2011a, 2011b; and, on occasion, include reviews that have been evaluated as sufficiently high quality following the Assessing Methodological Quality Of Systematic Reviews, or AMSTAR, process; Shea et al., 2007). These systematic reviews largely use data from RCTs to determine efficacy of interventions, though the option exists to include some high-quality observational studies. (Note, while carefully constructed single-subject designs may be used to provide context for the panel's recommendations, these designs are not typically part of systematic reviews that follow Institute of Medicine standards.) There are several advantages to this strategy. First, the RCT design allows the investigator to conclude that differences in outcomes between treatments that have been randomly assigned to participants are very likely caused by the treatments rather than by extraneous factors.*

*Second, the use of IOM standards is consistent with best guideline development practices across health care professions, lending credibility to the clinical practice guidelines. In fact, APA's clinical practice guidelines are submitted to the [ECRI Guidelines Trust](#), a database established in 2018 by an independent, nonprofit healthcare organization to continue the legacy of the Agency for Healthcare Research and Quality's National Guideline Clearinghouse. Through the submission process, ECRI rates for how well the guidelines follow the IOM standards utilizing the [TRUST \(Transparency and Rigor Using Standards of Trustworthiness\) Scorecard](#). Health care administrators, insurance companies and other important stakeholders consult the ECRI Institute's Guidelines Trust when making coverage, service delivery, reimbursement and other decisions.*

*Nevertheless, RCTs do not answer all questions that consumers and providers want to answer. RCTs best answer the question: Which of the treatments studied in the RCT is most efficacious for the average patient who met the selection criteria for the trial? However, the RCT does not fully answer the question that the provider typically wants to answer: What treatment is likely to be most effective for this patient who is in my office at this moment (and who might not meet the selection criteria used in the RCTs)? Another limitation of RCTs is that they typically study changes in symptoms or diagnostic status, whereas patients frequently have additional idiographic symptoms and goals. Moreover, RCTs emphasize efficacy and typically do not test the mechanisms underlying the treatment approach, so guidelines leave unanswered critical questions about how treatments achieve their effects. This is a critical issue facing the field and an important consideration for future guidelines because relatively little is known about whether the mechanisms that are theorized by the specific treatments to be responsible for the effects of the treatment actually are the mechanisms that produce the effects. It is also worth noting that RCTs generally evaluate interventions as a whole. As a result, when clinical best practices involve multicomponent interventions, it is often unclear which are the necessary and sufficient sub-components (for this reason, Comparative Effectiveness Trials can be especially useful for guiding the selection of specific components).*

*Thus, as outlined by the Institute of Medicine (2001) and the American Psychological Association's Presidential Task Force on Evidence-Based Practice (2006), in addition to considering efficacy data from reviews of the best available research evidence (typically RCTs), it is also important to consider the individual patient and their particular needs, background, and wishes, and the provider's training, expertise, and judgment when selecting and implementing a treatment. We also recognize the role of nonspecific factors in therapy outcomes, and the role of patients' individual differences in determining clinical outcomes.*

#### References

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- Shea, B.J., Grimshaw, J.M., Wells, G.A., Boers, M., Andersson, N., Hamel, C., & Bouter, L.M. (2007). Development of AMSTAR: A measurement tool to assess the methodological quality of systematic reviews. *BMC Medical Research Methodology*, 7(10). <https://doi.org/10.1186/1471-2288-7-10>

P. 46 line 10-12

Appreciate the nod that CBT condition was compared to a waitlist, so this may show greater effect size; however, just another major concern for how some of the evidence was gathered and now recommended.

#### **Panel response**

*Effect size inflation of treatments when compared with waitlist is an issue for many treatments, not only CBT.*

Pg. 48-50

ECT should be addressed for use with treatment resistant depression or not. Seems like a major oversight given how much it is used.

#### **Panel response**

*The panel has identified this (and the lack of information regarding somatic and interventional treatments) as a limitation of the guideline and has made this more explicit in edits.*

Pg. 59

This area gives a short paragraph or nod to the idea that consideration of a pt's diverse background, identities, and comorbidities should be considered however the guidelines in their entirety have a problem in that they do not have literature that has enough diverse samples.

**Panel response**

*The panel appreciates the responder's concerns regarding the import of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The panel was comprised of experts with knowledge and skill in these areas who thoughtfully contributed concerns regarding these issues throughout the process of guideline development. Additionally, though outside the scope of the committee's charge, members of the panel thought it important to compile a list of articles that might help address these very concerns and the panel worked with APA staff to complete a detailed review of the current literature on depression treatment. This was done to determine the extent to which these issues of race/ethnicity in particular have been studied. APA staff compiled a long list of relevant literature in this area and determined that very little literature exists (that specifically fit the IOM criteria with which the panel was charged) to assist in the generation of substantial background in these issues. While the panel agrees that many studies exist that provide theoretical, pilot studies and larger studies relevant for intersectionality issues, the panel was unable to identify any more than a few studies that incorporated intersectionality and met the stated criteria for inclusion in our review.*

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**Committee on Socioeconomic Status (CSES)**

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**Commenter: Committee on Socioeconomic Status (CSES)**

Comment type: Group Comments

Group name: Committee on Socioeconomic Status

Do you have any other comments about the draft guideline document?

General Comment - The Committee on Socioeconomic Status (CSES) appreciates the opportunity to make comments on the Clinical Practice Guideline for Treatment of Depression. CSES commends the attention that the panel has paid to the crippling effect that depression has on individuals in three distinct life stages: childhood and adolescence; adulthood and; advanced age. The impact of depression, when it remains untreated, also negatively impacts family members and, in locations where access to culturally competent and accessible services is limited, hardship is particularly acute.

**Panel response***Thank you for your feedback.*

While the Panel on Developing APA Clinical Practice Guideline for Treatment of Depression has completed many sophisticated reviews of the existing literature when compiling its guidelines and treatment recommendations, it has acknowledged that the existing science is vastly deficient in representing rates of depression and related disorders, as well as treatment effects for marginalized populations (race, ethnicity, sexual orientation), including those of lower SES. Notwithstanding the panel's candor and call for greater research inclusion of diverse populations, CSES believes that the proposed guidelines would be strengthened by the integration of more culturally competent themes and information throughout this paper. For example, certain symptoms of depression may present, be experienced, or understood distinctly in the context of cultural heritage, race, or ethnicity.

**Panel response***The panel appreciates the responder's concerns regarding the import of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The panel was comprised of experts with knowledge and skill in these areas who thoughtfully contributed concerns regarding these issues throughout the process of guideline development. Additionally, though outside the scope of the committee's charge, members of the panel thought it important to compile a list of articles that might help address these very concerns and the panel worked with APA staff to complete a detailed review of the current literature on depression treatment. This was done to determine the extent to which these issues of race/ethnicity in particular have been studied. APA staff compiled a long list of relevant literature in this area and determined that very little literature exists (that specifically fit the IOM criteria with which the panel was charged) to assist in the generation of substantial background in these issues. While the panel agrees that many studies exist that provide theoretical, pilot studies and larger studies relevant for intersectionality issues, the panel was unable to identify any more than a few studies that incorporated intersectionality and met the stated criteria for inclusion in our review.*

Moreover, discussion of the stigma felt by those experiencing depression should incorporate the fact that through social constructs, mental health has too often been defined differently for persons and populations based on race, socio-economic status, class and sexual orientation.

As such, seeking mental health services may have different risks associated with it for those who are disadvantaged versus advantaged. Opportunities for mental health services, and the likelihood of treatment completion, is not associated simply with availability but true accessibility of services. It is suggested that the guidelines more strongly emphasize factors that may enhance treatment such as availability of culturally competent caregivers, trauma informed settings, appointments during non-traditional hours, and opportunities for affordable transportation, and appropriate child care. As currently included in the guidelines, the discussion of contextual variables that hinder treatment may be misinterpreted by some readers as “choices” that are to be overcome through the therapeutic alliance.

In addition to the multi-focal call for inclusivity in research design and funding to better address the needs of diverse populations, including those that experience lower SES, CSES recommends that specific reference to the 2017 Multicultural Guidelines be central to the panel’s guidelines wherever relevant.

**Panel response**

*Thank you we are adding this.*

**Intended Use of Guidelines - P.VII – ‘Individualizing Treatment’ Section**

- Line 18 add “socio-economic status, culture and/or heritage”
- Line 20 add “accessibility: location, hours of operation, available appointments; public transportation and child care when needed.

**Executive Summary -**

**ES-6**

- Line 9 add “cultural heritages” after diverse racial/ethnic backgrounds

**ES-7**

- Line 3 add “treatment for racially, ethnically socio-economically diverse populations as well as marginalized communities”

**Recommendations - P.2**

- Line 14 - Add sentence: “In marginalized populations and those of lower SES, rates are likely to be significantly higher.” (Panel included citations).
- Line 17- add “families” after burden on individuals.
- Line 23 - “Rates among same aged youngsters of low SES, or who are members of marginalized populations are believed to be significantly higher” (Panel included citations)

**P.3**

- Line 17 - Add sentence: “These data may also not include significant representation of children of color or other minorities less likely to receive evaluation and treatment.”

**P.4**

- Line 7 - Add sentence. “Children and youth living in rural remote and other underserved locations are least likely to receive needed mental health treatment. “
- Line 13 - add “childhood and adolescent depression”.

## P.7

- Line 3 - Add sentence at the end: "Given the profound impact of SES on the developmental trajectory and the strong association between poor mental health outcomes and the sequelae of poverty (Panel included citations), further research that better depicts rates and presentation of depression among marginalized populations is necessary."
- Line 22 - Add sentence. "Furthermore, such research must be based on an understanding of multicultural as well as practical barriers that limit access to care."

## P.8

- Line 5 - Insert sentence. "Rates of suicide among Native American Youth are the highest across all populations and are at epidemic levels "(Panel included citation).

## P.12

- Line 2 - Add sentence:" Research that fully explores rates of depression amongst multicultural, and diverse populations inclusive of SES, gender, sexual orientation, cultural heritage and geographical is needed."

## P.20

- Line 17 - add "race, socio economic status ".

## Considerations for Treatment Implementation - P.56

- Structural barriers include all related financial costs, affordable transportation, clinic hours and locations, child care.
- Attitudinal barriers include: experiences of racial, ethnic, and cultural bias,
- Line 10 - Add SES
- Line 23 - Add acceptance of diversity, and cultural competence.

**Panel response**

*Thank you, we are making the above edits.*



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**Ad Hoc Committee on Psychology and AIDS (COPA)**

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**Commenter: Ad Hoc Committee on Psychology and AIDS (COPA)**

Comment type: Group Comments

Group name: Ad Hoc Committee on Psychology and AIDS (COPA)

Do you have any other comments about the draft guideline document?

General Comment - The Ad Hoc Committee on Psychology and AIDS (COPA) wishes to express its appreciation for being invited to review and provide feedback on Guidelines for Treatment of Depression. Overall, we find the guidelines to be well-written and of great value to mental health providers. We have a few recommendations to offer:

1. In providing background on the prevalence of depression, there are sub-sections for sex and race/ethnicity. Given increased rates of depression and suicidality among sexual minorities throughout the lifespan, we urge the authors to include information on depression among sexual minorities in those sections of the guidelines.
2. On Page 12, where you cite the impact of depression on co-morbidities such as diabetes, we recommend you include a statement on the impact of depression on HIV/AIDS risk behavior, treatment, and mortality, for which there is an established literature.
3. On Page 20, line 17, we request that you add “sexual minority” as a category. We appreciate that gender identity is already listed.
4. There are numerous places where the report references the importance of providing culturally sensitive services to diverse populations, focusing primarily on gender and race/ethnicity. We recommend these discussions also include sexual minority populations, especially when there are mental health professionals who may hold negative views of sexual minorities and who may continue to provide “conversion” therapy.

Introduction - On Page 12, where you cite the impact of depression on co-morbidities such as diabetes, we recommend you include a statement on the impact of depression on HIV/AIDS risk behavior, treatment, and mortality, for which there is an established literature.

Introduction - On Page 20, line 17, we request that you add “sexual minority” as a category. We appreciate that gender identity is already listed.

**Panel response***Thank you, we have made all the edits requested above.*

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**Committee on Psychological Tests and Assessment (CPTA)**

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**Commenter: CPTA (Lois Condie, Ph.D.)**

Comment type: Group Comments

Group name: CPTA (APA Committee)

Do you have any other comments about the draft guideline document?

General Comment - Consider shortening the title to: Clinical Practice Guidelines for the Treatment of Depression Across the Lifespan.

**Panel response**

*The panel has modified the title to be more concise.*

General Comment - Consider adding another limitation to the "aspirational" section that is meant to dissuade attorneys from using the guidelines as though they are mandatory. Individuals who have attended COPPS workshops have expressed concern that although the guidelines are aspirational, they are presented in courts (e.g., in malpractice suits) as though they are mandatory, and the judicial body treats the document as mandatory for the purpose of judging the behavior of the psychologist.

**Panel response**

*Thank you for this suggestion. We are making this edit to the Intended Use of this Document disclaimer section at the beginning of the guideline document.*

General Comment - The word judgement has two official spellings:

Judgment

Judgement

Choose one and make sure the document is consistent.

**Panel response**

*Thank you we are reviewing the document for consistency in spelling.*

Process and Method - Consider adding a caveat about the use of assessment measures in diagnosing depression that reminds psychologists and other practitioners that a significant score on a depression measure is not synonymous with a clinical condition of depression. Multimodal assessment and carefully considered interpretation are needed.

**Panel response**

*Although your comment is sound clinical advice, please note that this guideline focused on treatment of depression and not its assessment.*

Introduction - Minimize the use of "also" and "further" in the description of the document. Those words are used excessively, and it detracts from comprehension of the content.

**Panel response**

*Thank you, we are making this edit.*

Executive Summary - "Treatment as usual" is a broad term that might have unintended consequences. I work in a setting that provides me access to multiple records by multiple psychologists and other treatment providers. I review a significant number of therapeutic notes and documents. "Treatment as usual" is sometimes tantamount to the use of outdated treatment procedures, or worse, engaging in activities that involve listening as a friend might listen but providing very little in the way of actual treatment. Consider changing the definition of Treatment as Usual as follows:

Change "...care that is customarily provided in a particular situation" to

"...empirically supported treatment that is customarily provided in a particular situation."

**Panel response**

*The panel appreciates these comments. The stylistic modifications will be taken into account in review of the draft. The use of the term treatment as usual does have limitations as CPTA and Dr. Condie indicate. However, the panel has decided to retain it given that it was often the term used by researchers to describe the control condition in studies.*

General Comment - Consider expanding the Special Population sections of the treatment recommendations. There is a section that addresses treatment of individuals with both dementia and depression, but the document is silent on other special populations (e.g., those with Intellectual Disabilities or other characteristics that might compromise their comprehension of treatment).

**Panel response**

*Unfortunately, the panel did not have information to expand addressing special populations. The panel supports future efforts to provide targeted guidelines for special populations.*

General Comment - The section on Taking Into Account Patient Values and Preferences offers some good suggestions for tailoring treatment to individual patients and giving considerations to their barriers to treatment. Consider tightening this section a bit so that clinicians understand that they should use empirically guided judgment (not personal judgment or proclivities) where judgment is indicated and flexibility is needed.

**Panel response**

*Thank you for this feedback. We have worked to tighten this section by adding information about the use of empirically guided judgment in several locations throughout the section.*

Similarly, the section on Adapting Treatment to Fit the Individual, though important, is phrased somewhat awkwardly and thus makes the document seem internally inconsistent--as in, "here are some guidelines that you should follow," and "it won't work sometimes, so do whatever you want." It does appear the authors are trying to encourage clinicians to use sound clinical judgment but the sentence with the Webb et al. 2010 reference on p. 58 seems to unravel the entire document. I'm over-emphasizing what the writers did, but the purpose is to make the point that this section needs to be reworked a bit.

**Panel response**

*Thank you for this feedback, we are reworking this section.*

General Comment - The document is well prepared, overall. Congratulations.

**Panel response**

*Thank you very much!*

General Comment - I have re-written this comment because it contained a typo on the first submission. The word "not" was missing from sentence 1.

In the section in Table 1 on Complementary and Alternative Treatments, it may be worthwhile to point out that those procedures are not covered by insurance. In their current form, it seems there is room for misunderstanding. It would be a mistake for someone to believe that APA has endorsed yoga as a "therapy," and thus it is billable (to insurance companies). Psychologists have sometimes found their way to Ethics Committees because they have billed insurance companies for those or similar non-therapy techniques and then have been charged with fraud. I cannot give you specific information for confidentiality reasons, but please be clear about the intent of the writers--that alternative therapies are meant to be adjunctive, not the main goal. If you are, indeed, endorsing these techniques as treatment techniques via the guidelines, then please do so more specifically.

General Comment (ORIGINAL) - In the section in Table 1 on Complementary and Alternative Treatments, it may be worthwhile to point out that those procedures are covered by insurance. In their current form, it seems there is room for misunderstanding. It would be a mistake for someone to believe that APA has endorsed yoga as a "therapy," and thus it is billable (to insurance companies). Psychologists have sometimes found their way to Ethics Committees because they have billed insurance companies for those or similar non-therapy techniques and then have been charged with fraud. I cannot give you specific information for confidentiality reasons, but please be clear about the intent of the writers--that alternative therapies are meant to be adjunctive, not the main goal. If you are, indeed, endorsing these techniques as treatment techniques via the guidelines, then please do so more specifically.

**Panel response**

*The issue of reimbursement is an important one as well, but outside the scope of the panel.*

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**Committee on Legal Issues (COLI)**

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***Commenter: Committee on Legal Issues (COLI)***

Comment type: Group Comments

Group name: Committee on Legal Issues (COLI) Do you have any other comments about the draft guideline document?

General Comment - As described in the Executive Summary, the purpose of the guideline is to provide recommendations for the treatment of depressive disorders (including major depression, subsyndromal depression, and persistent depressive disorder) based on systematic reviews of the evidence. The guideline addresses the efficacy of psychological, somatic, and complementary and alternative medicine treatments, the comparative effectiveness of psychotherapy in combination with pharmacotherapy as well as compared to pharmacotherapy, somatic, and complementary and alternative treatments. It further examines efficacy of medications for the child and adolescent population only. To accomplish this, APA's Advisory Steering Committee issued a call for nominations (including self-nominations) for individuals to serve as panel members from a variety of backgrounds (patient, psychology, psychiatry, general medicine) with content knowledge or methodological expertise. The resulting panel used clearly defined criteria to evaluate systematic reviews and meta-analyses for quality to make recommendation or conditional recommendation for or against each particular treatment or make a statement that there was insufficient evidence to be able to make a recommendation for or against.

**Level 1: Legal & Risk Management Considerations**

The panel was cautious in making recommendations and clearly noted limitations to their review. They are presented as aspirational guidelines only, so they are explicitly not intending for this to serve as a requirement for practice. Thus, as stated in the introduction, "It is not intended to limit scope of practice in licensing laws for psychologists or for other independently licensed professionals, nor limit coverage for reimbursement by third party payers." Given this, COLI did not identify any legal or risk management considerations in terms of exposing members to legal liability or any other Level 1 concern.

**Level 2: Level II: Other Considerations**

The document is lengthy and provides a comprehensive review of depression research as it could be applied to clinical practice. It is well-written, and COLI commends the panel for producing a document that will be useful to clinicians to guide the clinical treatment of depression in both adults and children.

1. This is a comprehensive and systematic review of research on treatment outcomes, which is used to recommend treatments for various populations with a range of depressive disorders. The Panel was clear about the methodology it used to make decisions about efficacy of various treatments. The Panel was appropriately cautious in making recommendations and careful in identifying limitations of their conclusions. COLI did not identify any concerns about the methodology used to reach these recommendations.
2. The Panel was also careful to ensure that panel members did not have any conflicts of interest that would prohibit participation. Candidates completed a conflict of interest form that was reviewed by the Advisory Steering Committee or APA staff, and the result was that no panel members were identified with particular approaches to intervention nor had significant

known financial conflicts. This was a careful process that COLI believe avoids any concerns about possible ethical conflicts of the panel members.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

3. Editorial: As noted, this is a well-written review. COLI offers the following suggestions for minor corrections or clarifications.

Page 14-Line 3-6: This sentence appears to equate "under-served and disadvantaged" with race and ethnic diversity. The sentence now appears to convey that minorities are under-served and disadvantaged. COLI suggests this should be revised they need to change it to reflect that they are two separate groups, minorities and disadvantaged populations. Suggested re-write: Models employing lay health counselors of similar ethnic and racial backgrounds to the patient increasingly seem to be a rational and cost-effective use of resources "to reach diverse reach diverse racial and ethnic groups in under-served and disadvantaged older adults."

**Panel response**

*Thank you for this feedback, we are re-writing the sentence per your suggestion.*

Page 33-11/12. It isn't clear why the Driessen et al. study is an exception.

**Panel response**

*The Driessen et al. review is an exception because it included non-RCT data in addition to RCT data. However, the panel used only the RCT data from the Driessen review. We are revising the noted sentence to be clearer about this point.*

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**Committee on Ethnic Minority Affairs (CEMA)**

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**Commenter: Committee on Ethnic Minority Affairs (CEMA) [EMAIL]**

**M E M O R A N D U M**

**DATE:** November 2, 2018

**TO:** APA Board of Professional Affairs (BPA)/Practice Directorate

**FROM:** APA Committee on Ethnic Minority Affairs (CEMA)

**SUBJECT:** CC-05 Clinical Practice Guideline for the Treatment of Depression in Children, Adolescents, and Young, Middle-aged, and Older Adults

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The APA Committee on Ethnic Minority Affairs (CEMA) during its conference call meeting on Friday, November 2, 2018 reviewed and discussed possible CEMA action regarding the Clinical Practice Guideline for the Treatment of Depression in Children, Adolescents, and Young, Middle-aged, and Older Adults (CC-05) being proposed by the APA Board of Professional Affairs/Practice Directorate.

CEMA expressed reluctance to recommend the adoption of these clinical practice guidelines, at this time without important modifications. The Committee believes that the Guidelines are indeed somewhat thorough in its attempt to address the complex nature of depression treatments that may be most beneficial to different developmental groups (i.e., children, adults, etc.). However, it is CEMA's belief that the Guidelines as currently written do not adequately incorporate mediating and/or moderating variables of treatment efficacy with marginalized communities and underserved populations, such as people of color.

The Committee commends the various writers and contributors to the development of these Guidelines. The attempt to address the importance of race and ethnicity is acknowledged. However, the blanket comment made throughout the document to "obtain multicultural competence" is insufficient. CEMA is mindful of the importance of using rigorous criteria in making decisions, but the stringent requirements for including specific studies has resulted in further perpetuating the indivisibility of underserved populations. Having done such a thorough review of the empirical literature, the drafters of these Guidelines could have included an appendix, with appropriate caveats, summarizing the evidence without necessarily making a specific recommendation to increase access to this information to mental health care providers.

CEMA is unable to recommend that the APA Council of Representatives (COR) adopt these Guidelines, as written without important modifications. The Committee is available to discuss this action and provide any additional information that may be needed. Please contact CEMA via the staff of the APA Office of Ethnic Minority Affairs (OEMA).

CEMA stresses the importance and value of including a multicultural/ethnic minority perspective in the development of all APA related projects, activities, initiatives, and policy. Accordingly, CEMA's comments and recommendations in this memorandum reflect another example of the importance of ethnic minority representation, expertise, and participation on all APA task forces, work groups, initiatives, and other APA projects. CEMA strongly recommends that as a matter of conventional practice, commitment to multicultural/ethnic minority concerns and inclusion of ethnic minority expertise shall be consistently a high priority in all APA related endeavors.

cc: BAPPI

Clinton Anderson, PhD/Donella Graham

Tiffany G. Townsend, PhD/Alberto Figueroa G., EdD, MBA

**Panel response**

*The committee appreciates the responders' concerns regarding the import of race/ethnicity, gender and all issues of intersectionality. We hope that CEMA might have the opportunity to review the backgrounds of the committee members as posted on the APA Depression Guideline Committee website describing those persons tasked with developing treatment guidelines. The committee is particularly pleased to see that CEMA has expressed a concern that the committee shared regarding a commitment to diversity and inclusion. The committee was comprised of persons of varied racial and ethnic backgrounds and individuals with explicit expertise in the needs of communities of color, people of diverse socioeconomic backgrounds, and LGBTQIA+ populations including committee members who represented patients, patient advocacy and caregivers who were themselves of intersectional backgrounds.*



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**Committee on Sexual Orientation and Gender Diversity**

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**Commenter: Committee on Sexual Orientation and Gender Diversity  
(Matthew Skinta, PhD, ABPP)**

Comment type: Group Comments

Group name: APA Committee on Sexual Orientation and Gender Diversity

Do you have any other comments about the draft guideline document?

Considerations for Treatment Implementation - Thank you for the opportunity to review CC-5 APA CLINICAL PRACTICE GUIDELINE FOR TREATMENT OF DEPRESSION. While the scope and limitations of such a review are well-described and supported, the guidelines would benefit from a more thoughtful inclusion of references to work with understudied minority groups. Specifically, while clinicians are urged to consider cultural contexts and minority identities, no information is provided regarding well-established disparities in rates of depressive disorders among population groups. Some language is inconsistent, such as the use of LGBTQI in one place, and reference to sexual orientation and gender identity elsewhere. Gender identity and depression are highlighted among cultural concerns that require further study, though sexual orientation is not included in the latter recommendation despite being referenced previously as a group lacking sufficient study for recommendations. While a thorough review of what specific cultural considerations may be incorporated into treatment, we encourage the inclusion of reference to other APA Task Force reports that may provide further, clearer guidance, including:

American Psychological Association. (2017). Multicultural guidelines: An ecological approach to context, identity, and intersectionality.

American Psychological Association. (2015). Guidelines for psychological practice with transgender and gender nonconforming people. *American Psychologist*, 70(9), 832-864.

American Psychological Association. (2012). Guidelines for psychological practice with lesbian, gay, and bisexual clients. *The American Psychologist*, 67(1), 10.

**Panel response**

*Thank you for this feedback. We are making the suggested edits as well as adding references to the noted guidelines.*

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**Committee on International Relations in Psychology (CIRP)**

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**Commenter: Committee on International Relations in Psychology (CIRP)**

Comment type: Group Comments

Group name: Committee on International Relations in Psychology (CIRP) Do you have any other comments about the draft guideline document?

General Comment - CIRP has reviewed this document and appreciate the work and effort put into it, however, we are concerned with the lack of attention to diverse populations, culture and culturally-based and indigenous practices, and would recommend that attention to such issues are incorporated throughout the guidelines. In addition, we are concerned with how the authors have incorporated, addressed and acknowledged the use of international samples in this document.

**Panel response**

*The panel is adding a table of demographic data to the appendix in which characteristics of studies included in the reviews are noted, including national origin. Included studies consisted of both United States and countries outside the United States. Please refer to the summary of demographics information in the methods section as well as detailed tables in the appendix for further details. While the data did not generally allow the panel to make recommendations specific to particular cultural and other sub-groups, the panel included much discussion of issues relevant to diverse populations throughout the introduction and discussion sections of the guideline document. Further, the panel hopes that CIRP might have the opportunity to review the backgrounds of the committee members as posted on the APA Depression Guideline Committee website describing those persons tasked with developing treatment guidelines. The committee is particularly pleased to see that CIRP has expressed a concern that the committee shared regarding a commitment to diversity and inclusion. The committee was comprised of persons of varied racial and ethnic backgrounds and individuals with explicit expertise in the needs of communities of color, people of diverse socioeconomic backgrounds, and LGBTQIA+ populations including committee members who represented patients, patient advocacy and caregivers who were themselves of intersectional backgrounds, as well as members from countries outside the United States.*

On page ES-1, the scope of the document is addressed. It would be helpful to clarify whether the scope is only for practitioners in the United States. We wonder this because of statements made on pages ES-2 and 10, regarding depression being a leading cause for disability world-wide. With such statements, it seems like the document could be used by an international audience. Similarly, on pages 33-34, the authors refer to number of studies that consist of international samples, and state that among these, 46.67% included non-white children. It is unclear whether these 46 studies are the international studies and whether some of these non-white children are children residing in other countries. Are the findings of these studies generalizable to a U.S. population? The authors address limitations on pages 45 and 83, but there is no acknowledgement of the inclusion of international samples, or a lack of attention to global mental health. On page 11, there is a statement about needing to be careful regarding lumping various subgroups together, and hence we wonder if U.S. samples are lumped with international samples and serve as foundation for some of the conclusion drawn?

**Panel response**

*This guideline is intended for a broad international audience, not only for individuals in the United States. We will clarify this point in the document. However, although APA's clinical practice guidelines are intended for an international audience, APA's dissemination/implementation efforts are focused on the U.S. healthcare environment. The panel is adding a table of demographic data to the appendix in which characteristics of studies included in the reviews are noted, including national origin. Included studies consisted of both participants from the United States and countries outside the United States. Please refer to the summary of demographics information in the methods section as well as detailed tables in the appendix for further details. While the data did not generally allow the panel to make recommendations specific to particular cultural and other sub-groups, the panel included much discussion of issues relevant to diverse populations throughout the introduction and discussion sections of the guideline document.*

Introduction - pp. 5-7, (adapting Tx to fit the individual) add research that recommends combining cultural practices with western, evidence based practices

**Panel response**

*We are unclear the specific location the commenter is referring to as the indicated pages do not appear to correspond with the noted content. However, we are going back through the guideline and making sure to add mention of culture when discussing the three legged stool of evidence based practice. We are also adding explicit mention of culture to the Individualizing Treatment section of the guideline. We hope this addresses the commenter's request.*

vii, Lines 17-18, add culture (e.g., Lewis-Fernandez, Diaz Cultural Case Formulation)

**Panel response**

*We are adding culture to this line.*

p. 17, # 3: Patient expertise regarding preferences and values – add patient's cultural values and preferences

**Panel response**

*Thank you, we are making this edit.*

p. 20 report lines 10 – 15 Guideline recommendations for underserved populations – incorporate global mental health and medical anthropological literature and research

**Panel response**

*The panel appreciates the comment on incorporating these research domains. However, the current section is referencing the need for randomized controlled trials, the basis for the recommendations, to better address these populations, and the suggested literature does not address this need. The section has been edited to clarify the focus*

p. 20, please add nationality of origin and generation status to the examples of diversity.

**Panel response**

*We are making this edit.*

Considerations for Treatment Implementation - p. 56 report, lines 9, 10 (treatment compatibility)  
add cultural values and beliefs

**Panel response**

*Excellent suggestion we are making this edit.*

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**Committee on Children, Youth, and Families**

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**Commenter: Committee on Children, Youth, and Families**

Comment type: Group Comments

Group name: Committee on Children, Youth and Families

Do you have any other comments about the draft guideline document?

General Comment - CYF reviewed the Clinical Practice Guideline for the Treatment of Depression in Children, Adolescents, and Young, Middle-aged, and Older Adults at the October 18, 2018 virtual meeting.

CYF appreciates that the guidelines authors examined children and adolescents as different populations than adults in their review of prevalence, diagnostic criteria, presentation, and treatment of depression. We recognize, and appreciate that the authors recognize, the limitations of the guidelines based on the available literature and systematic reviews (e.g., that there are only two reviews available for children and adolescents, and there is limited research on cross-cultural applications and adaptations of clinical interventions).

While we see that a developmental perspective is well integrated into the first half of the guidelines, we have concerns about how a developmental perspective is represented within the second half that begins with practice implications. The second half reads as though specific to working with adults. For example, in the discussion of informed consent- how are parents of guardians integrated into informed consent in child treatment? How is this process negotiated with an older adolescent? With respect to barriers: What are the unique barriers children and adolescents experience in accessing treatment? How are change mechanisms different among children and adolescents?

Given that the review includes children 0-18, we also feel that the integration of family into the treatment of children and adolescents is hugely important, and as stands, under-represented in the guidelines.

**Panel response**

*We agree that the integration of family in the treatment of children and adults is important. However, given the empirical studies that are the basis of the guidelines, to address the family issues would be outside the scope of the guideline (primarily because the available literature meeting AMSTAR and IOM criteria did not include family treatment studies). The committee recognizes the limitations of the methodology throughout the guideline.*

Finally, we believe authors could make a stronger point with regard to the caution to exercise in applying the guidelines, since they are based on only two reviews of children and adolescents.

**Panel response**

*We appreciate the reviewer's suggestions and have referred to the limitations of the guideline in the executive summary.*

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**Committee on Aging (CONA)**

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**Commenter: Committee on Aging (CONA) (Dr. Alexander Watts)**

Comment type: Group Comments

Group name: APA Committee on Aging (CONA)

Do you have any other comments about the draft guideline document?

General Comment - The APA Committee on Aging (CONA) expresses its strong support and endorsement of the comments submitted by APA Division 12 Section II (Clinical Geropsychology). CONA strongly encourages the guideline development panel to seriously consider and address these comments and those of the American Psychiatric Association related to older adults prior to the adoption of these guidelines.

CONA applauds the inclusion of a specific section on subthreshold/minor depression in older adults given the higher prevalence of clinically significant subthreshold symptoms compared to major depression in older adults and the negative outcomes associated with subthreshold depression

Meeks, T. W., Vahia, I. V., Lavretsky, H., Kulkarni, G., & Jeste, D. V. (2011). A tune in “a minor” can “b major”: A review of epidemiology, illness course, and public health implications of subthreshold depression in older adults. *Journal of Affective Disorders*, 129(1), 126-142. doi:<https://doi.org/10.1016/j.jad.2010.09.015>

Executive Summary - The APA Committee on Aging (CONA) expresses its strong support and endorsement of the comments submitted by APA Division 12 Section II (Clinical Geropsychology). CONA strongly encourages the guideline development panel to seriously consider and address these comments and those of the American Psychiatric Association related to older adults prior to the adoption of these guidelines.

Introduction - The APA Committee on Aging (CONA) expresses its strong support and endorsement of the comments submitted by APA Division 12 Section II (Clinical Geropsychology). CONA strongly encourages the guideline development panel to seriously consider and address these comments and those of the American Psychiatric Association related to older adults prior to the adoption of these guidelines.

Process and Method - The APA Committee on Aging (CONA) expresses its strong support and endorsement of the comments submitted by APA Division 12 Section II (Clinical Geropsychology). CONA strongly encourages the guideline development panel to seriously consider and address these comments and those of the American Psychiatric Association related to older adults prior to the adoption of these guidelines.

Discussion of Clinical Recommendations - The APA Committee on Aging (CONA) expresses its strong support and endorsement of the comments submitted by APA Division 12 Section II (Clinical Geropsychology). CONA strongly encourages the guideline development panel to seriously consider and address these comments and those of the American Psychiatric Association related to older adults prior to the adoption of these guidelines.

In the section on prevalence rates for depression in older adults, some discussion of the prevalence of subthreshold depression would be useful. For example, Meeks et al. (2011) reported that subthreshold depression in older adults is generally 2–3 times more common than

major depression, with a median point prevalence of 9.8% in the community. This review also noted that approximately 8–10% of older adults with subthreshold depression developed major depression each year.

Meeks, T. W., Vahia, I. V., Lavretsky, H., Kulkarni, G., & Jeste, D. V. (2011). A tune in “a minor” can “b major”: A review of epidemiology, illness course, and public health implications of subthreshold depression in older adults. *Journal of Affective Disorders*, 129(1), 126-142. doi:<https://doi.org/10.1016/j.jad.2010.09.015>

**Panel response**

*We thank CONA and Dr. Alexander Watts for the many comments and citations to the peer-reviewed literature. We endorse these comments. On page 13, line 9 add “Subthreshold depression is 2-3 times more common than major depression in older adults and 8-10% of those with subthreshold symptoms develop major depression each year (Meeks et al., 2011).*

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**APA Policy and Planning Board**

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**Commenter: APA Policy and Planning Board**

Comment type: Group Comments

Group name: Policy and Planning Board

Do you have any other comments about the draft guideline document?

General Comment - The Policy and Planning Board (P & P) reviewed the Clinical Practice Guideline for the Treatment of Depression in Children, Adolescent, Young, Middle-aged, and Older Adults guideline and offers the following comments from a policy perspective. P & P appreciates and applauds the substantial work in creating this guideline and acknowledges that the movers have been successful in achieving the original intent of their charge.

Noteworthy is the group's careful effort in articulating its decision-making criteria, its use of best practices for conducting a systematic and comprehensive literature review, its use of methodological rigor in the guideline development, and its detailed discussion of the limitations, of both the guideline and the foundational research. P & P also appreciates the Guideline Development Panel's attention to integrating research, clinical judgment as well patient values and preferences to extend the existing knowledge base about appropriate therapeutic interventions geared at populations ranging from children to older adults. Further, P & P acknowledges the Panel's attempt to consider the appropriateness of recommendations pertaining to underserved populations. For the reasons stated above, P & P finds the Panel's process for the development of this guideline is in keeping with APA's mission of integrating science with policy.

**Panel response***Thank you very much, we appreciate the review and feedback!*

It is P & P's hope that as a larger policy issue, APA includes crosswalk products (e.g. <https://www.apa.org/ptsd-guideline/index.aspx>) as a follow-up to these types of guidelines. It is a meaningful way to bridge the gap between science and practice and the challenge of ensuring that guidelines such as this, which are good for the field of psychology but have varying impacts for individual and/or groups of psychologists. Further, it is P & P's hope that APA continues to refine mechanisms for the creation of practice guidelines that are increasingly useful to both the field of psychology and those psychologists and other providers engaged in healthcare service delivery.

**Panel response***Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*



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**Panel Responses to Public Comments on  
Draft Version of the  
*APA Clinical Practice Guideline for the  
Treatment of Depression***

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**1.****Commenter: Silvia Sara Canetto**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Reviewing and integrating a literature as large as that pertaining to the treatment of depression in children, adolescents, and young, middle-aged, and older adults is a major endeavor. I commend the panel for the effort, and for producing this major report. I realize that it is easy to miss content or perspectives given how vast the literature is on depression in children, adolescents, and young, middle-aged, and older adults. It is good that there is an opportunity to provide feedback about this APA report so its frameworks and content can be expanded and inadvertent errors corrected. My two main concerns with regard to frameworks and content are 1. How medical-model, individual-focused the guidelines are; and

**ASC response**

*We know that some members of the community are frustrated by the adherence to IOM standards, but this is done because we want to use gold standard methods to evaluate treatment efficacy. This is the gold standard approach across disciplines, including many disciplines beyond psychology and medicine. Especially as psychology is new to developing CPGs and we want our guidelines to be seen as credible and adopted widely, it is important to follow best practices.*

2. The unsystematic attention given to issues of diversity in depression, and also the fact that when they are considered, issues of diversity are discussed one at the time instead of in intersection (e.g., ethnicity separately from sex/gender instead of ethnicity intersecting with sex/gender). The latter issue is particularly problematic given that in the United States, the epidemiology of depression and suicidality varies significantly by age, ethnicity and sex, in intersection. For example, in the adolescent depression and suicidality sections (pp. 7-8), there is no mention of the gender paradox of depression, suicidal ideation and suicidal behavior among adolescents in the United States, that is, that in this country, girls have higher rates of depression, suicidal ideation, and nonfatal suicidal behavior (also called suicide attempts) but lower rates of death by suicide than boys (for content on this topic, see Canetto, S. S. (1997). Meanings of gender and suicidal behavior during adolescence. *Suicide and Life-Threatening Behavior*, 27, 339-351). Similarly, in the adult depression and suicidality sections on p. 12, there is no mention of the U.S. gender paradox of depression and suicidality (for content on this topic, see Canetto, S. S., & Sakinofsky, I. 1998. The gender paradox in suicide. *Suicide and Life-Threatening Behavior*, 28, 1-23), and what the paradox means for the “treatment” of adult suicidal women and men (for content on this issue, see Canetto, S. S. 1994 Gender issues in the treatment of suicidal individuals. *Death Studies*, 18, 513-527).

**Panel response**

*The committee appreciates the responder's concerns regarding the importance of race/ethnicity, gender and all issues of intersectionality in the patient conceptualization, clinical diagnosis and treatment of depression. The committee was comprised of experts with knowledge and skill in these areas who thoughtfully contributed concerns regarding these issues throughout the process of guideline development. Additionally, though outside the scope of the committee's charge, members of the committee thought it important to compile a list of articles that might help address these very concerns and the committee worked with APA staff to complete a detailed review of the current literature on depression treatment. This was done to determine the extent to which these issues of race/ethnicity in particular have been studied. APA staff compiled a long list of relevant literature in this area and determined that very little literature exists (that specifically fit the IOM criteria with which the committee was charged) to assist in the generation of substantial background in these issues. While the committee agrees that many studies exist that provide theoretical, pilot studies and larger studies relevant for intersectionality issues, the committee was unable to identify any more than a few studies that incorporated intersectionality and met the stated criteria for inclusion in our review.*

Finally, in the older adult depression and suicidality sections (p. 14) the high suicide rates of older "white" men are mentioned but explained simplistically as an issue of too easy access to firearms and insufficient access to care for depression. The U.S. ethnic and gender paradox of older adult suicide is a complex phenomenon related to ideologies of white masculinity, aging and suicide in the United States. For content on this topic see Canetto, S. S., 2017 Suicide: Why are older men so vulnerable? *Men and Masculinities*, 20, 49-70. The fact that older men have similar rates of depression as older women should be mentioned in the older adult depression section. It should also be noted there that dominant explanations of older adult suicide (e.g., that older adult suicide is a response to chronic illnesses, financial problems, and widowhood/living alone) do not account for the highest rates of suicide mortality of older White men.

**Panel response**

*We thank Dr. Silvia Canetto for her comment about risk and protective factors in late-life suicide, especially among older white men. We concur with her view that there is scientific evidence supporting the need for multiple approaches to caring for suicidal patients (including but not limited to older white men). Considering this further, it appears to us that there are at least six evidence-based elements for the effective treatment of suicidal patients:*

- (1) explicit focus on suicidal risk at each visit (to address factors that contribute to suicidal urges versus those that contribute restraint or resistance to such urges); (2) treatment of the underlying mental and/or substance use disorder(s);*
- (3) assessment and management of contributory medical comorbidity (pain, insomnia, dyspnea, traumatic brain injury);*
- (4) use of interventions shown to reduce suicide risks (e.g., removal of lethal agents from the patient's home);*
- (5) addressing contextual factors (e.g., social support, victimization, family discord, LGBT issues); and*
- (6) care co-ordination (e.g., 24-hour crisis line, integration of behavioral health into general medical services).*

A third concern with regard to frameworks is that the information presented in this document does not address the variability in depression rates and depression manifestation that has been recorded by nation, within nations by culture/age/sex, and over time. The national origin of the studies cited in the report is not specified so it is not clear who the findings apply to. I recommend adding the national origin of the studies. If all of the evidence cited is from the United States. I recommend specifying from the start that the guidelines are based on, and are meant to apply to the United States.

**Panel response**

*While the analyses included in the reviews do not allow for information to be presented on treatment recommendations by national origin, we are adding a table to the guideline with all available information on national origin of individual studies included within each review. Please refer to Tables K1- K4 located in the appendices for this information.*

Finally, I have a few recommendations for language. Terms like “completed suicide” and “attempted suicide” have come to be recognized as problematic and are being abandoned. Among the reasons are that the term completed suicide inadvertently implies that a suicide is a more positive and stronger act than a suicidal act that the person survives (for a discussion of language issues see Meanings of gender and suicidal behavior during adolescence. Suicide and Life-Threatening Behavior, 27, 339-351).

**Panel response**

*The panel appreciates the comment and the agrees with the importance of choosing language carefully. We have found and replaced one instance of "completed suicides" with "deaths by suicide." However, panel members felt that the overwhelming popularity of the term "attempt" among those with clinical, research, and lived expertise, as well as the lack of a suitable alternative, make it the most relevant term for this report.*

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## 2.

**Commenter: Laura Groshong, on behalf of Clinical Social Work Association**

Comment type: Group Comments

Group name: Clinical Social Work Association

Do you have any other comments about the draft guideline document?

General Comment - Comments on the APA Draft Practice Guidelines on Depression October 25, 2018

The Clinical Social Work Association thanks the American Psychological Association for the opportunity to present comments on their draft Practice Guidelines on Depression. As one of the most common mental health disorders, depression impacts about 25% of our citizens at any given time, while only 20-30% of those afflicted get professional clinical services to treat the condition (SAMHSA, 2016).

While the Practice Guidelines offers some guidance on available treatments, the focus is on treatments that are transactional and designed to relieve symptoms, rather than relational and designed to resolve the underlying causes of the depression. While such an approach may be consistent with the treatment of depression that insurers would encourage us to take, i.e., that the purpose should be to end acute episodes, at the same time, the failure to treat the ongoing

problems which cause depression would be a serious breach of clinical social work therapeutic ethics, as it would severely limit help for those who have chronic depression.

**Panel response**

*The guideline panel acknowledges the concerns of the Clinical Social Work Association, and appreciates the effort put into responding to the guideline draft. The panel also wants to acknowledge that the domain of evidence reviewed focused on psychotherapies, pharmacotherapies, and complementary and alternative interventions, and this does not represent the full range of potential domains that could be determined to be of benefit when considering how to address depression. We have attempted to acknowledge these limitations in the draft.*

As currently drafted, this Guidelines creates a dilemma for responsible clinicians. Although the panel states on page 6 that “this Guidelines is intended to be aspirational and is not intended to create a requirement for practice,” APA on the same page endorses just the opposite, quoting the statement from the British National Institute for Health and Clinical Excellence (NICE): “...When exercising their judgement, professionals are expected to take this Guidelines fully into account...”

**Panel response**

*We have removed the NICE statement from the guideline document.*

CSWA believes that it is important to note that the “evidence-base” referenced in the creation of the Guidelines did not focus on which therapies worked better than other therapies, but on which therapies worked better than non-therapies. CSWA is concerned that the way in which the Guidelines approaches the wide variety of treatments for depression limits these treatments to a manualized short-term perspective. The depression caused by PTSD or other trauma requires much more nuanced long-term methods. The value of a treatment relationship is ignored, or at best, minimized in these Guidelines. We strongly urge the APA to expand the range of types of depression and the need for relational methods to treat the more chronic forms. Evidence as defined in the Guidelines is so limited as to redefine the concept of this disorder and the treatments that are needed to lead to improvement, if not cure.

CSWA appreciates the acknowledgement of the authors that there are limitations to the Guidelines and would encourage them to provide the final document with detailed information about the range of available treatment methods for depression, the equivalencies of methods for different kinds of depression, and the importance of the treatment relationship as a therapeutic component.

We would like to point out that the panel recommends on page 17 two treatments for childhood depression (cognitive behavioral and interpersonal psychotherapy). The research studies actually showed that they were better than no treatment, but there was no evidence to support that they were better than other therapies. On page 18, the panel states that if neither recommended psychotherapy is available or neither is acceptable to the patient, an alternate model could be considered. The panel then warns that there is insufficient evidence to recommend one over another, yet promptly touts the two psychotherapies appearing in the preceding paragraph for which no evidence had been found for the superiority over any others.

The panel did the exact same thing for the treatment recommendations for older adults: recommending two treatments over other treatments without any research evidence on which to

base such a comparative conclusion. Here again, the two recommended treatments happened to have been ones included in the admittedly sparse number of studies from this age group in the systematic review, in which the RCT included only treatment versus no treatment, NOT comparative effectiveness research. Responsible clinicians cannot accept what is essentially unsubstantiated clinical guidance.

CSWA sees the Guidelines in their current form as leading to misinformation about the wide variety of treatment methods that can be beneficial in treatment of depression, basing evidence on the types of depression that are not chronic or the acute phases of chronic depression. The Guidelines privilege transactional symptom reduction over relational treatment which demonstrated its effectiveness in long-term change – but does not fit neatly into an RCT research model. We hope the final document will be more balanced and give the public a more accurate guideline about what works in the treatment of depression.

Sincerely,

Melissa Johnson, LCSW, President  
Clinical Social Work Association  
[mjohnson@clinicalsocialworkassociation.org](mailto:mjohnson@clinicalsocialworkassociation.org)

Laura Groshong, LICSW, Director, Policy and Practice Clinical Social Work Association  
[lwgroshong@clinicalsocialworkassociation.org](mailto:lwgroshong@clinicalsocialworkassociation.org)

**Panel response**

*Thank you for the careful review of the guideline draft. In regard to the concerns regarding the recommendations of CBT and IPT in the context of both the child and adolescent cohort and the older adult cohort, the commenters are correct to indicate that there was no comparative effectiveness data in the literature reviewed demonstrating CBT or IPT to be superior to other models. However, this was because there was no comparative effectiveness data available in these cohorts that adequately included other approaches. The panel required there be such data for considering treatments equivalent. This is reflected in the recommendations for the adult cohort, in which there are a number of appropriate comparative effectiveness trials available and which therefore led to the inclusion of a broader range of treatments.*

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**3.****Commenter: Tori Branch**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I wholeheartedly agree with the comments posted by Jonathan Shedler and Stephen Soldz (below for reference). This does feel like a guideline for practice, but rather a gathering of research with no clear indication that any one type of treatment should be privileged over others. There is indication in the document (see Shedler and Soldz's comments) that therapies being suggested here are no more effecting than not having treatment, and/or that patients experienced worsening of symptoms after treatment. Using this document as a guideline will be extremely confusing to the general population, and I believe this type of confusion could lead to patient harm. Research has bounds (needing exact repetition, limited length of treatment, artificial restrictions on who may provide what type of treatment, etc) that are not present in clinical practice. Having the expectation that practice look like research is a

dangerous and unnecessary burden on practitioners. Expecting patients to read through a 100 page document that is put for as a treatment guideline, and expecting them to understand the minutia of the research findings, is unreasonable and has the potential to put patients in harms way. Please do not issue this document under the guise of treatment guideline. That is misleading use of the information it contains.

[COPIED AND PASTED DRS. SHEDLER & SOLDZ COMMENTS]

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**4.**

**Commenter: Denise Lensky, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - As a clinical psychologist and psychoanalyst with close to 30 years of experience as a clinician, I am writing to strongly support the well-reasoned arguments of the following individual psychologists who have written extensive prior comments: Jonathan Shedler, Stephen Soldz, Virginia Shuiller and James Wilk.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**5.**

**Commenter: Salam Soliman**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am endorsing the comments made by Jonathan Shelder, Virginia Shiller, Larry Rosenbering and Stephen Solz which I have attached here.

General Comment - The guidelines document offers a careful and detailed critique of the research literature considered for the guidelines, and in fact provide a compelling argument for why APA should NOT be issuing guidelines.

There are two major concerns: 1) the guidelines do NOT offer practical clinical guidance, and 2) The guidelines will be misinterpreted by the public, policy maker, and many psychologists. If the guidelines fuel public misunderstanding, this would be a serious public disservice.

1) the guidelines do NOT offer practical clinical guidance

According to the information provided by the guidelines document itself:

-There is no basis for recommending any bona fide form of therapy for adult depression over any other (even supportive therapy, which I imagine was not designed to treat the causes of depression per se). (p. 47)

-Approximately 70% of patients who receive the "recommended" therapies remain depressed or relapse quickly (p. 90)

-The measured outcomes do not necessarily dovetail with what patients regard as treatment success (p.57).

-It is not at all clear that research on therapy "brands" is a sound investment of research dollars (or that a focus on therapy brands is a sound basis for developing guidelines), since only 17% of improvement is attributable to specific brands/technique, versus 50% for other factors that the studies essentially treat as error variance (p. 62).

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

## 2) The guidelines will lead to widespread misunderstanding

to no evidence that any brand of therapy is more effective than another. Thus, on p. 47 the Guideline states: "Comparative effectiveness studies indicated similar effects across different models of psychotherapy." (See also ES12: "The panel found that effectiveness studies demonstrated similar effects across psychotherapy") The major issue is that when APA formally designates a treatment as "recommended for use," most people (who will never read the original source) will take this to mean that the recommended treatments work-- that is, that patients who receive them can expect to get better. And those in our profession who have a partisan agenda will encourage that misunderstanding.

This has already happened with the PTSD guidelines. In the PTSD guidelines, prolonged exposure therapy (pet) and cognitive processing therapy (cpt) are designated "highly recommended." However, the overwhelming majority of patients who receive these treatments (approximately two-thirds) still have PTSD after treatment.

But this is not how APA has been presenting the findings to the public or to psychologists. A recent article in the APA monitor, for example, stated: "For providers, [the guidelines] offer recommendations that... quickly summarize which treatments have been shown to work for hundreds or even thousands of patients." Given that two-thirds of patients still have PTSD after treatment, it would be at least as accurate to say that the recommended treatments have been shown NOT to work for hundreds or even thousands of patients. In short, the PTSD guidelines are already being used in ways that mislead and misinform.

Now APA plans to officially "recommend for use" therapies that also fail most patients most of the time (given that ~ 70% of patients in the included studies either don't improve or relapse quickly). The likelihood is extraordinarily high that the "recommend for use" designation will be misunderstood by the public. A "reasonable person" (including the general public, the media, policy makers, and many psychologists) will take this to mean that patients who get these treatments can expect to get better. If terms like "recommend" inadvertently fuel such public misunderstanding, that would be a significant public disservice.

In fact, the literature offers no basis to recommend \*forms of therapy.\* What the guidelines panel was really evaluating was the methodology/quantity of research studies. That crucial distinction will quickly be lost when the findings and recommendations are disseminated.



Instead of issuing recommendations about \*therapies,\* the guidelines document is primarily evaluating methodological strength of research, not strength of treatments. That suggests that the document should not be issues as a "guidelines" document at all, and should more accurately be presented as a "Methodological Review of RCT research on depression." Such a title and designation would dramatically reduce the risk of widespread public misunderstanding, and the risk of misuse of the document.

General Comment - I am impressed with the amount of effort that went into the development of this Guideline. It is an excellent summary of the findings from a certain type of research, namely RCTs, on depression. I am impressed that the panel essentially replicated the most common finding in psychotherapy research, the Dodo Bird result: 'everybody has won, and all must have prizes.' That is, that all psychotherapies are reasonably effective and that there is little

Given that the Dodo Bird result has occurred in hundreds, if not thousands, of studies and meta-analyses, surely it is time to stop comparing brands of therapy to each other. The evidence is fairly overwhelming that this is not an effective research strategy if the goal is to identify effective treatments for patients. Even more important, is to stop the process of recommending specific therapies, not because they are shown to be significantly (in the sense of clinical significance not statistical) better than other well-conducted active therapies, but simply because they have been included in more RCTs. Inclusion in research studies is hardly a criterion for quality of therapy and the public is not well served by having us recommend therapies larger than the basis of how many studies they have been included in, much less the creativeness of researchers in designing ineffective alternative treatments to compare them with.

Further, the literature review here demonstrates the major gaps in our understanding of how to successfully treat depression. As noted, rates of response, at approximately 50%, were relatively low across treatments in the relapse rate in responders with relatively high. These facts suggest that we desperately need better treatments and also, perhaps, the treatments need to go on longer than occur in most RCTs. Thus, rather than more comparisons of therapy brands, we desperately need process research that elucidates the mechanisms whereby therapies work. Assuming these mechanisms can be identified, we then need research on whether and how they can be taught. Only when we better understand therapy process can we stop creating new brands and start improving real-world treatments.

Another concern I have with the whole endeavor is that it seems extremely problematic, indeed unscientific, to be making policy recommendations without good evidence that having such policies implemented leads to improve practice. Thus, while careful implementation of treatments described in manuals makes sense from an RCT angle, this is far from how therapy is practiced in the real world. In most clinics, therapists have an "orientation" that they practice, based on something they learned at some earlier time point. In some settings, they may receive a few hours of in-service training. However, is a well-known phenomenon that therapists fairly quickly deviate from the model included in any manual. After all, this is why RCTs needs such compliance mechanisms. If therapists didn't deviate from the manual, there would be no need for these compliance examinations. Thus, I failed to see the logic of recommending that treatments that passed some RCT test be implemented in settings where we know they will not be implemented in the manner that they were in the RCT. This seems more like voodoo than science to me. It is a major problem with the whole current approach to "evidence-based practice" in psychotherapy.

In conclusion, I commend the task force for creating this literature review that so clearly elucidates the major problems with the whole Guidelines approach to improving psychotherapy. I would hope that APA would commit to real science, which requires us to understand the processes and mechanisms whereby treatments work and stop this pseudoscientific comparison of therapy brands.

While not directly relevant here, I will note that the situation in psychotherapy is quite different than that in prevention. In prevention research, a common finding is that well-meaning prevention interventions are ineffective at accomplishing the stated objectives. And it is not uncommon to find that they are even harmful, increasing rather than reducing the targeted behavior. Thus, the RCT approach makes a lot more sense in prevention than it does in psychotherapy, where the Dodo Bird finding is been replicated over and over and over and over and over again. As the RCT approach is now dominated therapy research for the last 30 years, isn't it time to move on to a research approach which we stands a chance of improving our knowledge?

General Comment - 1) We question the notion that the only useful data comes from RCT's:

A) Authors of a 2013 study argued "There is evidence that quasi-experimental and observational studies do not yield effect sizes that systematically differ from those of RCT's" (p. 864 Abbass et al.); and also: "RCTs and non-RCTs did not differ with regard to between-group effect sizes" (also see p. 870.) (Abbas, A., Rabung, S., Leichsenring, F., Refseth, J. S., & Misgley, N. (2013). Psychodynamic Psychotherapy for Children and Adolescents: A Meta-Analysis of Short-Term Psychodynamic Models. Journal of the American Academy of Child & Adolescent Psychiatry, 52, 863-875.)

[Regarding this point, Abbass et al. also cite: Leichsenring, D. Randomized controlled vs. naturalistic studies. A new research agenda. Bulletin of the Menninger Clinic. 2004; 68, 115-129; Shadish, Q., Matt, G., Navarro, A., Phillips, G. The effects of psychological therapies under clinically representative conditions: A meta-analysis. Journal of Consulting and Clinical Psychology, 2000, 126, 512-529.]

B) Further, the methodologies behind RCT's do not take into account the vast array of individual differences which arguably are even more pronounced among young people. There is no consideration of the "What Works for Whom and Why" paradigm. None of these studies fully take into account underlying characteristics of the individual for which we have ample research evidence suggesting their importance (e.g. temperament, attachment style, personality style, locus of control, etc.)

**Panel Response**

*Among the central features of RCTs are inclusion of comparison conditions, randomization of participant assignment to conditions, and masking of assignments. As a result, RCTs reduce various forms of experimental bias and provide the clearest evidence for the efficacy or comparative effectiveness of interventions within a population. Also, RCTs often have large samples, which enhances the ability of the study to detect effects of interventions.*

*RCTs have been criticized for including only certain types of participants (e.g., those of particular demographic backgrounds or without comorbid conditions). However, over the last 20 years, many RCTs have included a wider range of participants in an effort to make the results more applicable to real world clinical practice.*

*Despite having more diverse samples, RCTs still may not be able to pick up differential effects across subgroups of participants. To answer questions about differential effects, either much larger RCTs or RCTs focused on particular subgroups are required.*

*Also, although RCTs can indicate whether an intervention works or not, they generally will not be informative about the mechanism by which the intervention works. And RCTs will provide limited information about factors that interact with the intervention to produce specific outcomes in individual patients.*

*Of course, not all RCTs are of high quality. As described above, RCTs are assessed in systematic reviews using a variety of criteria (risk of bias, consistency, directness, precision). Lower ratings on these criteria may be due to issues related to the design, conduct, or reporting of an RCT. In developing APA's guidelines, only high-quality RCTs were used to determine the strength of evidence for the efficacy or comparative effectiveness of interventions.*

*Researchers are exploring other designs, in addition to RCTs, for assessing the efficacy or comparative effectiveness of interventions. These include historical control, case control and single case designs. However, studies using such designs often still carry a high risk of bias and lack generalizability and are not broadly accepted as providing strong support for efficacy or comparative effectiveness.*

2) The draft guidelines include only one meta-analysis of treatment of children and adolescents (Zhou, X., Hetrick, S. E., Cuijpers, P., Qin, B., Barth, J., Whittington, C. J., ... Xie, P. (2015). Comparative efficacy and acceptability of psychotherapies for depression in children and adolescents: A systematic review and network meta-analysis, *World Psychiatry*, 14, 207-222.) This review combines studies of both children and adolescents. The average age of participants in the studies was 14.7 years. Only 5 of the 52 studies included children as young as 7 or 8 years; some of these studies including 7- and 8-year-olds had small N's overall, so the actual number of children at the younger end of the age range in the individual studies isn't clear. It therefore seems important to clarify that the Zhou et al. meta-analytic results cannot be presumed to apply to children under approximately 9 years. Also, replication of the results of this meta-analysis would be important before it was used to reach major conclusions about recommendations for therapy.

#### **Panel response**

*The reviewer makes an important point. We will clarify that although the studies included children as young as 6 years old, most of the children included in the clinical trials were 9 years or older, thus the findings may not be applicable to youth under the age of 9 years of age. We have also revised the guideline to provide separate recommendations for children versus adolescents.*

3) While the Zhou et al. study concluded that "at post-treatment, only interpersonal therapy (IPT) and cognitive-behavioral therapy (CBT) were significantly more effective than most control conditions" (p. 207,) the authors of this meta-analysis failed to note that one solid study (Trowell et al., (2007), see below) that included children aged 9-15 years showed that both psychodynamic psychotherapy and Family Therapy resulted in significant decreases in depression (to the point that youngsters were no longer clinically depressed.) This study had the benefit of also showing that treatment resulted in a decrease in co-morbid conditions. We feel it

is limiting to require that studies include a control condition to be considered as evidence of treatment effectiveness.

Trowell, J., Joffe, I., Campbell, J., Clemente, C., Almqvist, F., Soininen, M., Koskenranta-Aalto, U., Weintraub, S., Kolaitis, G., Tomaras, V., Anastasopoulos, D., Grayson, K., Barnes J., & Tsiantis, J. (2007). Childhood depression: a place for psychotherapy An outcome study comparing individual psychodynamic psychotherapy and family therapy. *Eur Child Adolesc Psychiatry*, 16, 157–167 DOI 10.1007/s00787-006-0584-x.

Important conclusions reached by Trowell et al.: “The results of this study suggest both Individual Therapy (response rate 74% by End of Therapy) and Family therapy (response rate 75% by End of Therapy) may be more effective in the treatment of depression than other forms of treatment. Previous studies have found a response rate in the region of 60% to CBT [5] and 52–56% to Fluoxetine [13, 14] and 71% to CBT and Fluoxetine combined [29].” (p. 166)

Also: “A significant number of cases in both therapy groups had co-morbid conditions. Almost a third of cases in the study had 3 or more co-morbid conditions. Following therapy, there was a decrease in comorbid conditions, particularly anxiety disorders and conduct disorders, which are often associated with depressive disorders. This occurred in both therapy groups.” (p. 166)

**Panel response**

*We understand that it is desirable to see the Trowell et al. study as demonstrative of the effects of these treatment approaches. However, it is important to acknowledge research methodology experts who state that inclusion of a control condition is the only way to know whether these youth are getting better because of these specific treatments or whether they would have gotten better with time alone or any other treatment. The panel appreciates the commenter’s perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

4) One important study was not included in Zhou et al meta-analysis but is relevant to effectiveness of psychodynamic therapy. This study was a methodologically rigorous trial, had a non-inferiority/equivalence result, and included long-term follow-up:

Goodyer, I. M., Reynolds, S., Barrett, B., Byford, S., Dubicka, B., Hill, J., Holland, F., Kelvin, R., Midgley, N., Roberts, C., Senior, R., Target, M., Widmer, B., Wilkinson, P., & Fonagy, P. (2016). Cognitive behavioral therapy and short-term psychoanalytical psychotherapy vs a brief psychosocial intervention in adolescents with unipolar major depressive disorder (IMPACT): A multicentre, pragmatic, observer-blind, randomized controlled superiority trial, *The Lancet Psychiatry*, 4, 1-11.

This study randomly allocated 470 children and adolescents aged 11-17 years with a DSM-IV diagnosis of major depressive disorder to either CBT or short-term psychoanalytical psychotherapy (as well as a brief psychosocial intervention group.) CBT and short-term psychoanalytical psychotherapy were equally effective in decreasing self-reported depression symptoms at end of treatment, and continued to prove equally effective at a follow-up 86 weeks after initiating treatment.

**Panel response**

*We agree that this is an important study, however, it was not published in the timeframe of our research methods. The panel appreciates the commenter's perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

5) In considering limitations of the Zhou et al. study, we are concerned that current research has not emphasized long-term follow-up. In the Zhou et al study, only 10 of the 52 studies followed patients at least 12 months, and no studies followed children more than 24 months. As the draft guidelines note “The impairments associated with adolescent depression also have found to persist into adulthood and reflect the significant morbidity and lifelong impairment associated with the disorder” and “children with subsyndromal depression are at risk for developing depression diagnoses later in childhood and adolescence.” (p. 3 draft guidelines). Should the goal of treatment be simply to work to remit current symptoms, or might it be more beneficial to offer a longer intervention that not only results in remission of symptoms but addresses underlying factors that may result in new episodes of depression later in childhood, adolescence, or adulthood? While we do not have the data to address the question as to which treatments are most likely to limit or prevent further episodes of depression beyond two years, we should be careful to acknowledge that this is an important issue and that current knowledge should not discourage provision of longer-term or more in-depth treatments that may prove beneficial for long-term mental health.

**Panel response**

*The reviewer brings up important points about the course of depression in youth and the limited knowledge that we have about long-term effects of the different treatment approaches. The question regarding the benefits of offering a longer-term intervention to prevent new episodes in the future remains an empirical question. We agree that understanding which treatments are most effective in preventing further episodes is still in need of investigation. Reviewing this literature was outside the scope of this guideline.*

6) Given the paucity of studies of psychodynamic therapy focusing solely on depression, it seems reasonable to look at studies that treat other diagnoses, or individuals with multiple diagnoses which include depression, to gain relevant information regarding the efficacy of psychodynamic psychotherapy. A study by Abbass et al. (Abbas, A., Rabung, S., Leichsenring, F., Refseth, J. S., & Miskley, N. (2013). Psychodynamic Psychotherapy for Children and Adolescents: A Meta-Analysis of Short-Term Psychodynamic Models. *Journal of the American Academy of Child & Adolescent Psychiatry*, 52, 863-875.) looked at 11 studies (9 were RCT's) that compared Short Term Psychodynamic Therapy with either other active treatments or minimal contact controls and standard care. Diagnoses ranged from borderline personality disorder, depression, anxiety, eating disorder, internalizing disorders, and mixed disorders. This review found “preliminary data indicating that STPP may be effective for a range of conditions in children. Moderate to large sustained within group gains were seen across all dimensions examined (i.e., general psychopathology, anxiety, mood disorders, somatic complaints, interpersonal functioning, and personality/behavioral problems) except interpersonal problems, which showed small gains only in follow-up. These changes were also reflected in high remission rates in serious mental disorders where these rates were provided, and where treatment was not ‘restricted’.... The effects of STPP were similar overall to those of what were generally robust treatment comparators (p. 871). Importantly, the finding that all within-group effects increased in size in follow-up matches what has been found in studies of TPP with adults; the effect of this intervention appears to be not only sustained over time, but increased in

what some have referred to as a ‘sleeper effect.’ This supports the hypothesis that changes in this brief therapy are persistent and that certain blocks to personal and psychological development are positively affected by these interventions.”(p. 873.)

**Panel response**

*We appreciate the reviewer’s strong feelings about the effectiveness of psychodynamic therapy. It is a reasonable point to consider general studies of treatment efficacy, however, the panel’s scope and methods for developing this guideline focused on treatment of depression specifically and therefore, included studies of individuals with a depression diagnosis. Finally, we refer you to Pg. vi of the guideline, where the panel indicates that a lack of evidence about a treatment does not imply that that treatment is not efficacious.*

7) As noted earlier, there is really little or no guidance for children younger than 9, who in the “real world” are often seen either in play therapy or dyadic therapy with parent/caregiver. We would argue that to gain some information, one should look at meta-analyses of play therapy or dyadic therapy with younger children, who may have a variety of diagnoses.

E.g., the Bratton et al. study does this. (Bratton, S., Ray, D., & Rhine, T. (2005). The efficacy of play therapy with children: A meta-analytic review of treatment outcomes. *Professional Psychology: Research and Practice*. 36, 376-391). From abstract, p. 376: “A meta-analysis of 93 controlled outcome studies (published 1953-2000) was conducted to assess the overall efficacy of play therapy and to determine factors that might impact its effectiveness. The overall treatment effect for play therapy interventions was 0.80 standard deviations. Further analysis revealed that effects were more positive for humanistic than for non-humanistic treatments and that using parents in play therapy produced the largest effects. Play therapy appeared equally effective across age, gender, and presenting issue.”

**Panel response**

*We agree that there are limited studies of interventions for youth under 9 years of age. However, the methodology of this guideline precludes us from including data from meta-analyses that are not specific to depression. We acknowledge the limitations noted in this guideline.*

8) We are concerned that there are no guidelines for infants and young children. These children display a variety of symptoms that may not be well described by the DSM depression criteria; very young children’s adjustment is often assessed by measures of attachment security. Given that children with insecure attachments are at greater risk, compared with children with secure attachments, for later receiving diagnoses of internalizing disorders (Groh et al., (Groh, A. M., Roisman, G. I., van IJzendoorn, M. H., Bakermans-Kranenburg, M. J., & Fearon, R. (2012). The Significance of Insecure and Disorganized Attachment for Children’s Internalizing Symptoms: A Meta-Analytic Study. *Child Development*, 83, 591–610.), a preventative approach to depression would potentially emphasize early interventions. There are a number of evidenced-based treatments (some of these are RCT’s) that provide attachment-focused interventions for infants and toddlers that show treatment can help children move from insecurity to a secure attachment; these attachment-focused treatments might be included in the guidelines. Evidence-based interventions are reviewed in Steele & Steele (2018) (Steele, H. & Steele, M. (2018). *The Handbook of Attachment-Based Interventions*, New York: Guilford Press.)

**Panel response**

*The reviewers make good points however, this guideline is not focused on preventive treatments but rather focused on interventions for youth who already have a diagnosis of depression. The guideline followed the parameters outlined for the review in the section on scope of review, which do not include studies on infants nor studies not specific to depression. We recognize the limitations of the guideline throughout the document.*

9) In Summary, in Table 1 of the Draft Guidelines, regarding Recommendations for the Child and Adolescent Population, we have two significant concerns.

- A. Most importantly, we are concerned that information about the complexities of the research questions still to be addressed are not reflected in the Table 1 Guidelines, and that those interested in a simple “take-away” will be misled. Indeed, in the text the authors spend considerable time speaking to the weaknesses of, limitations of and caveats to their claims, nearly as much as they spend presenting methods and conclusions. So, the simplistic guidelines in Table 1 do not reflect the issues raised in the text. The risk that insurance companies and other seeking to limit treatment will ignore the nuances of the text in favor of the “headlines” is quite worrisome.

**Panel response**

*We recognize that there are limitations to how we can present the information in the tables and the risks identified by the commenter. We will try to make the footnotes and table explanations more visible to prevent the reader from being misled. We have also revised Table 1 to provide recommendations for children versus adolescents in separate tables.*

Currently, Table 1 states that for “Initial Treatment,” two psychotherapies, Cognitive-behavioral therapy and Interpersonal therapy, earn the term “Recommend Use” (pp. ES 12-13). We recommend that the Table 1 headings be changed to better reflect the data. We suggest that the guidelines might replace “Recommend Use” with “Treatments which have been the focus of the greatest number of studies regarding their effectiveness, and which show efficacy.” Then, for “Additional psychotherapy recommendations of initial treatment,” instead of “Insufficient evidence for recommendation” we believe that the current state of affairs would be better reflected by a statement such as “Therapies that have been less intensively studied but which show evidence of effectiveness.” This recommendation is based on the fact that there is definitely some evidence that psychodynamic as well as family therapy are effective; there are simply significantly fewer studies which have been conducted using these modalities.

**Panel response**

*We understand the reviewer’s viewpoints. We believe that the footnotes accompanying the tables makes the same points as the reviewer does regarding the limitations of the data. We have edited the table to better highlight the limitations of the data in a manner making these footnotes easier to find by readers. We have also revised Table 1 to provide separate recommendations for children versus adolescents in separate tables.*

- B. Secondly, we feel that Recommendations for the Child and Adolescent Population should be clarified, that these pertain to older children (i.e. age 9 and up) and not to younger children. Whether there should be another section providing preliminary guidance regarding treatment for children younger than 9 (where there aren’t RCT’s specifically for treatment of depression) is a question.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10). As stated in response to other reviewers who have made a similar comment, we will clarify that the recommendations pertain largely to youth 9 years and older. We do not have review data at this time to make recommendations regarding youth under 9 years of age.*

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**6.****Commenter: Jane Tucker, PhD [3 submissions]**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I support the comments already submitted by PsiAN, Jonathan Shedler, and Stephen Soldz.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**7.****Commenter: David Goldberg**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I endorse the comments made by Stephen Soldz, PsiAN, and Jonathan Shedler.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**8.****Commenter: Marguerite Stewart [2 submissions]**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I support all sections of the comments submitted by Jonathan Shedler.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*



**9.****Commenter: Mark Siegert, PhD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I endorse the comments of Stephen Soldz and PsiAn.

Do you have any other comments about the draft guideline document?

Recommendations - I strongly recommend adopting Virginia Schiller's post on the problems with the Guidelines, parts which have been mischaracterized and need a change in names, and the near absence of studies that have a large enough N to capture what is effective for children 9 and under.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**10.****Commenter: Stephen Soldz**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am impressed with the amount of effort that went into the development of this Guideline. It is an excellent summary of the findings from a certain type of research, namely RCTs, on depression. I am impressed that the panel essentially replicated the most common finding in psychotherapy research, the Dodo Bird result: 'everybody has won, and all must have prizes.' That is, that all psychotherapies are reasonably effective and that there is little to no evidence that any brand of therapy is more effective than another. Thus, on p. 47 the Guideline states: "Comparative effectiveness studies indicated similar effects across different models of psychotherapy." (See also ES12: "The panel found that effectiveness studies demonstrated similar effects across psychotherapy")

Given that the Dodo Bird result has occurred in hundreds, if not thousands, of studies and meta-analyses, surely it is time to stop comparing brands of therapy to each other. The evidence is fairly overwhelming that this is not an effective research strategy if the goal is to identify effective treatments for patients. Even more important, is to stop the process of recommending specific therapies, not because they are shown to be significantly (in the sense of clinical significance not statistical) better than other well-conducted active therapies, but simply because they have been included in more RCTs. Inclusion in research studies is hardly a criterion for quality of therapy and the public is not well served by having us recommend therapies larger than the basis of how many studies they have been included in, much less the creativeness of researchers in designing ineffective alternative treatments to compare them with.

**Panel response**

*The fact that most research finds equivalent effects of different treatments is not evidence that all treatments are indeed equally effective. It is very well possible that the equivalence is an artifact resulting from dozens of differences between patients, therapists and the interaction between them (if all differences are mixed in a blender, like in a trial and meta-analyses, you will find no differences, but that does not imply they are not there). Furthermore, equivalent effects is not evidence that the effects are realized in the same way.*

*Furthermore, for some therapies there is more evidence for the effects. Not only in the sense that there are more trials, but also in terms of long-term effects, the combination and comparison with pharmacotherapy, specific target groups, the quality of the trials, etc. And when all therapies appear equally effective, we should still give patients the therapies with the best evidence (e.g., Cuijpers, 2013).*

**Reference**

Cuijpers, P. (2013). Effective therapies or effective mechanisms in treatment guidelines for depression? *Depression And Anxiety*, 30, 1055-1057.  
<https://doi.org/10.1002/da.22205>

Further, the literature review here demonstrates the major gaps in our understanding of how to successfully treat depression. As noted, rates of response, at approximately 50%, were relatively low across treatments in the relapse rate in responders with relatively high. These facts suggest that we desperately need better treatments and also, perhaps, the treatments need to go on longer than occur in most RCTs. Thus, rather than more comparisons of therapy brands, we desperately need process research that elucidates the mechanisms whereby therapies work. Assuming these mechanisms can be identified, we then need research on whether and how they can be taught. Only when we better understand therapy process can we stop creating new brands and start improving real-world treatments.

**Panel response**

*These are good points. We do need research on the mechanisms of therapy. However, this research is complicated, expensive, and requires a long-term, large research program instead of a study. It is relatively easy to show that a therapy works, but very complicated to show how it works (e.g., Cuijpers et al., 2019; Kazdin, 2007).*

**References**

Cuijpers, P., Reijnders, M., & Huibers, M.J.H. (2019). The role of common factors in psychotherapy outcomes. *Annual Review of Clinical Psychology*, 15, 207-231.  
<https://doi.org/10.1146/annurev-clinpsy-050718-095424>

Kazdin, A.E. (2007). Mediators and mechanisms of change in psychotherapy research. *Annual Review of Clinical Psychology*, 3, 1-27.  
<https://doi.org/10.1146/annurev.clinpsy.3.022806.091432>

Another concern I have with the whole endeavor is that it seems extremely problematic, indeed unscientific, to be making policy recommendations without good evidence that having such policies implemented leads to improve practice. Thus, while careful implementation of

treatments described in manuals makes sense from an RCT angle, this is far from how therapy is practiced in the real world. In most clinics, therapists have an “orientation” that they practice, based on something they learned at some earlier time point. In some settings, they may receive a few hours of in-service training. However, is a well-known phenomenon that therapists fairly quickly deviate from the model included in any manual. After all, this is why RCTs needs such compliance mechanisms. If therapists didn’t deviate from the manual, there would be no need for these compliance examinations. Thus, I failed to see the logic of recommending that treatments that passed some RCT test be implemented in settings where we know they will not be implemented in the manner that they were in the RCT. This seems more like voodoo than science to me. It is a major problem with the whole current approach to “evidence-based practice” in psychotherapy.

**Panel response**

*Evidence-based practice emphasizes the importance of the three-legged stool in which clinical decisions and practice are based on the intersection of the best available research with clinician expertise and patient characteristics, values and preferences. The guideline treatment recommendations are based largely on the best available science for efficacy and effectiveness (which includes RCTs) and thus are linked primarily to one of the legs of the stool. The work of clinicians can be informed by the guideline but do not need to be entirely determined by it.*

In conclusion, I commend the task force for creating this literature review that so clearly elucidates the major problems with the whole Guidelines approach to improving psychotherapy. I would hope that APA would commit to real science, which requires us to understand the processes and mechanisms whereby treatments work and stop this pseudoscientific comparison of therapy brands.

While not directly relevant here, I will note that the situation in psychotherapy is quite different than that in prevention. In prevention research, a common finding is that well-meaning prevention interventions are ineffective at accomplishing the stated objectives. And it is not uncommon to find that they are even harmful, increasing rather than reducing the targeted behavior. Thus, the RCT approach makes a lot more sense in prevention than it does in psychotherapy, where the Dodo Bird finding is been replicated over and over and over and over and over again. As the RCT approach is now dominated therapy research for the last 30 years, isn’t it time to move on to a research approach which we stands a chance of improving our knowledge?

**Panel response**

*In principle this is true. Many RCTs do not contribute very much to our knowledge. But that does not mean that RCTs in psychotherapy have not considerably contributed to our knowledge on treatment. The guideline would not have been possible without this research.*

**11.**

**Commenter: Jennifer Medicus, on behalf of the American Psychiatric Association**

Comment type: Group Comments

Group name: American Psychiatric Association

Do you have any other comments about the draft guideline document?

General Comment - We recognize the immense amount of work that has gone into a guideline of this magnitude and appreciate the impact of the constraints imposed by the guideline development recommendations of the Institute of Medicine (now National Academy of Medicine). Nevertheless, we find it difficult to read this document and determine a logical sequence of treatments or combinations of treatments to offer to patients based on these guideline statements. Ideally, readers should be able to develop a mental algorithm of this sequence of recommended and suggested treatments that can then be used in shared decision making. As written, however, this seems extremely difficult if not impossible to do based on the current text and tables.

We are also concerned that readers of the guideline will not be able to know which patients are best served by specific treatments because the statements are developed using very broad groupings of depressed patients ranging from subthreshold depression to minor depression to major depression at all levels of severity (albeit without psychotic features). For a number of treatments, evidence is only available for some of these subgroups of illness severity. We also believe that the exclusion of psychotic depression warrants further explanation given the fact that it is often under-recognized, under-treated, and associated with poorer outcomes, including mortality.

As another issue related to patient subgroups, the age categories used in the document may lead to some confusion about the most appropriate intervention for an individual. For example, children and adolescents are grouped together but the most appropriate treatment for a child, in terms of psychotherapies or medications, may be quite different from that for an older adolescent based on physiology, medication side effect profiles, and levels of cognitive and psychological development (for participating in psychotherapies). For adults, the overlap in recommendations between adults and older adults is especially confusing for those aged 50 to 65.

**Panel response**

*Thank you for these constructive comments. The panel has attempted to address each in turn:*

**1) *It is difficult if not impossible to develop a mental algorithm to decide on treatments.***

*Thank you for this concern. Based on feedback, the panel has edited the document to better support clinicians in decision-making. In addition, APA plans to produce a number of support resources based on the full document to facilitate decision-making.*

**2) *Readers will not be able to know who are best served due to broad groupings and lack of inclusion of psychotic depression.***

*The commenter brings up an important concern. When possible, based on the information reviewed, we have tried to clarify where treatments are appropriate for specific subgroups. The panel also recognizes that psychotic depression is an important domain and has modified the guideline to clarify why it was not included and acknowledge this as a limitation.*

**3) *The age groupings may contribute to confusion for providers.***

*The panel recognizes that the groupings and overlap between age cohorts can lead to confusion. Based on further review and consultation, the panel has changed the age for older adults to 60 and above and clarifying that there are some studies included in the older adult review that incorporated younger participants. The panel has added language to the document to clarify these issues and make recommendations on how to address individuals where guidance is inadequate. Moreover, recommendations have been separated for the child versus adolescent populations.*

In terms of the document format, we have difficulty in figuring out which specific aspects of the evidence relate to each of the guideline statements; information in the appendices (such as the guideline grids) does not provide much assistance in that regard. It would be helpful for the justification column to include direct citations (particularly for the statements on older adults) and/or cross-references to the appropriate sections of the guideline that describe the specific evidence for a given statement. Later in the document, where the evidence is discussed further, it would be helpful to include the text of the actual statements that correspond to those elements of the evidence. This will help the reader synthesize and review the evidence in the context of the final guideline statement(s).

**Panel response**

*While the evidence and decision-making process for each recommendation for children/adolescents and general adults is detailed in the corresponding grid we realize this information may not be as easy to locate for the unfamiliar reader. We have thus added additional information about each grid and how to understand the information presented in each grid. We have added direct cross-references to the appropriate decision table outlining the evidence and decision-making process for each older adult recommendation. Moreover, we have added references to the specific reviews in the section discussing the recommendation statements.*

We are concerned that readers may draw incorrect conclusions about recommended or conditionally recommended treatments because of significant gaps in the evidence used to construct these guidelines. In particular, the guideline includes efficacy studies for some treatments but does not include this information for studies of pharmacotherapies in adults. Given the relative paucity of comparative effectiveness studies of medications relative to efficacy studies, this introduces substantial gaps in the ways that statements are worded and introduces the possibility of a selective bias against pharmacotherapies due to a lesser amount of available evidence that met inclusion criteria. We see this as a fundamental flaw in the document, particularly if it is intended for any use by clinicians who would be prescribing medications alone or using medications in combination with pharmacotherapies.

This approach may be acceptable if the guideline makes clear from the start and at key locations in the guideline text that the document is primarily aimed for use by individuals who would be providing psychotherapies and not medications. As such the nuances of medication related decisions and the literature review that would support such decisions were not part of the evidence review or construction of guideline statements. Comparisons between psychotherapies and medications and statements related to combination therapies would still be appropriate in alerting the psychotherapist to the possible need for additional referral and consultation and in helping patients and families understand their options in the context of shared decision-making.

Approaching the guideline in this way would also eliminate several other gaps in the current recommendations. In particular, the current statements do not address the issue of patients whose initial preference for treatment is for medications alone. Many of the comparative statements are not helpful because they do not define treatment as usual (TAU) and in many studies TAU includes pharmacotherapy. Without knowing the specific medications, if any, that are included in TAU, it is difficult to draw conclusions to guide care. As noted, the statements about antidepressants are quite general and don't provide sufficient information to allow for a choice among specific antidepressant medications or specific approaches to augment antidepressant treatment (except with psychotherapies).

Discussion of neuromodulation therapies (e.g., electroconvulsive therapy, transcranial magnetic stimulation) does not seem to be included in the document although these were part of the initial PICOTS questions. It would still be useful to include some background information about these treatment modalities as well as information about the reasons that no statements were made or why they were otherwise excluded from the PICOTS scope.

**Panel response**

*We thank Ms. Jennifer Medicus for her thoughtful comments on behalf of the American Psychiatric Association. She notes in particular that the Guideline provides limited coverage of the literature on pharmacotherapy and none on neuromodulation approaches to the treatment of depression. We note in response that the guideline document is intended for clinicians who primarily administer psychotherapy and not antidepressant medication. In this context, the literature reviewed by the Panel was largely determined and constrained by IOM guidelines used to determine which studies, meta-analyses, and systematic reviews were included as evidence. Neuromodulation strategies were never a part of the charge to the Panel. The guideline does note that under many circumstances the best supported treatment is a combination of depression-specific psychotherapy and pharmacotherapy. Further, the Panel also emphasized the importance of shared decision making between clinician and patient, including the use of pharmacotherapy and referral to psychiatrists for consultation on matters pertaining to starting, continuing, or discontinuing psychopharmacology.*

The literature review structure is also confusing because the section on older adults provides recommendations based on specific studies whereas other sections rely on systematic reviews. Because these reviews are of varying ages, although recent, and have their own inclusion and exclusion criteria, this introduces gaps in the coverage of the literature. This is another issue that reduces confidence in the document's conclusions.

**Panel response**

*Recommendations for all age cohorts, including older adults, are based on systematic reviews of the literature. However, in some cases within a particular systematic review there were some interventions for which a small number of trials were identified, thus only that study(s) may be listed. We agree it would be ideal to have a single large review covering all interventions of interest over the exact same time period. However, because the panel was not able to identify such a review and in order to address gaps in the original review used by the panel, the panel elected to use additional reviews that met quality standards in order to provide coverage for these gaps. Unfortunately given the large number of reviews used by the panel, the reviews have slightly varying time periods covered. However, all reviews were current within five years at the time the panel examined the data.*

A number of issues occur throughout the document and executive summary tables. These are also noted and discussed below in some instances but we would suggest reviewing the document as a whole for the following:

1. Use of the words "consider" or "considering". These terms are generally viewed by guideline development groups as confusing to readers and implementers of guidelines because the process of "considering" is not possible to measure or express in operational terms. These words are typically used in the context of conditional recommendations but the reduced certainty implied by the word "consider" is already implicit in the fact that a recommendation is conditional.

**Panel response**

*Thank you for this comment. The panel has decided to retain the term "consider" for conditional recommendations because it captures the lower confidence in the recommendation.*

2. Discussion of specific antidepressant medications in the recommendation statements or justification. This occurs primarily in sections where the available evidence used an antidepressant medication that is no longer a preferred agent (typically due to side effect profiles). Including this discussion in each justification distracts from the evidence discussion and may be better placed in a footnote. In some instances, the text is very specific in recommending specific antidepressant agents in lieu of the medication that was studied whereas in other sections a more general approach is used. We would suggest that a consistent approach be taken that considered the evidence and side effect profiles of the full range of antidepressant medications.

**Panel response**

*Concerning Ms. Medicus' comments about our mention of specific antidepressant medications in the recommendations (followed by caveats in the discussion to the effect that practice has shifted from the use of tricyclic antidepressants to SSRI's and SNRI's): we followed this practice in keeping with IOM recommendations for the construction of modality- specific practice guidelines and recommendations, particularly as related to comparative efficacy and effectiveness. The panel followed this practice consistently, especially in our discussion of benefits and harms.*

3. Many of the statements or justifications emphasize the need for shared decision-making with the patient. We concur with the importance of shared decision-making but found it hard to determine why this information was included in some statements and justifications but not others. We feel that it would be preferable to emphasize the importance of shared decision-making across all clinical and treatment related decisions and emphasize that shared decision-making about options may need to be more detailed when the balance of benefits and harms for an intervention is less clear, such as with conditional recommendations. This information could then be removed from the actual guideline statements and justifications.

**Panel response**

*Thank you for the comment. We are addressing this in the revisions.*

4. In formatting the tables in the Executive Summary, the left column of the summary table is titled "Recommendation" and the middle column is titled "Strength of Recommendation." This is somewhat confusing because some of the statements have insufficient evidence for a recommendation to be made. It would be preferable to label these column headings as "Guideline Statement" and "Strength of Guideline Statement".

**Panel response**

*Thank you. We have discussed and decided to retain our current labeling due to a few factors including consistency with other APA clinical practice guidelines as well as semantics.*

5. The word "suicidality" is used in a number of places in the guideline. Although we recognize that this term is frequently used in the literature, we found that its use can contribute to confusion on the part of readers as it can refer to a range of suicide-related thoughts and behaviors. Often, however, readers infer that the word relates to suicide deaths, per se, or risk for suicide death, when that is not always the case. Whenever possible, we suggest that the text use specific language (e.g., suicide ideas, suicide attempts, non-suicidal self-injurious behavior, suicide) rather than using the less precise term of "suicidality."



**Panel response**

*While the panel appreciates and agrees with this recommendation, we decided to use “suicidality” to cover the full spectrum of suicidal experience, particularly due to the limited reporting on harms and burdens in the literature. Because the research contained vast inconsistencies on which kinds of suicidal experience were tracked or reported—whether ideation, planning, attempts, self-injurious behaviors or other suicidal experiences—panelists feel that any delineation of suicidal experiences would create further confusion, requiring further disclaimers about the limitations of the research available to us.*

*Where possible, the panel has specified the suicidal experiences as requested. Elsewhere, the term “spectrum of suicidal ideation and experiences” was used. In the instances in which “suicidality” was required, a footnote defining “suicidality” in this context is added.*

Intended Use of Guidelines - p. vi, 3—We suggest changing “it is not intended to limit” to “it is not intended to modify”.

p. vi, 6 ff – The word “endeavor” seems oddly phrased and the actual meaning is unclear in this context. Although the document says that these guidelines “may be useful for other clinicians”, the intended audience of this guideline seems inconsistent as the rest of the statements focus on psychologists.

Executive Summary - Overall there are a lot of places that aren’t clear in the Executive Summary (ES) without reading the rest of the guideline, but readers are likely to only read the ES and the table of recommendations, therefore it’s critical the right information is included up front and easy to read and understand.

**Panel response**

*Thank you for the suggestion. We prefer to use “limit” to be clear that the intention is not to place limitations on scope of practice. Our disclaimer (intended use of guideline) does focus more heavily on psychologists because we would like to be clear as the professional association for psychologists that the intention is not to place limitations on psychologists by their professional association. The document however is broadly useful for other clinicians besides psychologists. We use the word “endeavor” to be consistent with American Psychological Association policy documents which define guidelines in this same manner.*

Scope: We understand the need to follow a rigorous evidence-based approach to guideline development and have a body of evidence that is manageable. Nevertheless, limiting the guideline evidence-base to these systematic reviews alone could severely impact the usefulness of the guideline. Key studies may not have met the criteria for the reviews due to the review date or focus of the review, rather than methodological weaknesses of the studies. For the adult population (general or older), the inclusion of only comparative effectiveness data and exclusion of data on efficacy of medications leaves a very large gap in the available information. This, in turn, influences the recommendations for clinicians to use in making treatment decisions, particularly if a patient expresses preference for medication rather than psychotherapy. Additionally, because of limiting the guideline scope, very important areas are missing, including treatment response, timing/duration of treatments, maintenance treatments and treatment of depressed patients with suicidal ideas or behaviors. Although there may not

have been resources or high-level evidence for these topics, they are key to a complete treatment approach of a patient with depression.

Footnote 1: This exclusion limits the severity of depression covered by this guideline and may, in turn, limit generalizability and lead to misinterpretations of the guideline by users. This exclusion should be emphasized throughout the document as individuals may read some but not all sections of the guideline. The distinction between severe depression especially melancholic depression and/or depression with catatonic features is often hard to distinguish from delusional depression. The point at which depressive ruminations become delusions, for example, is not always easy to determine. In clinical practice, missing the diagnosis of psychotic depression is common and often associated with poorer outcomes and poorer quality of care. (For a recent review see Rothschild AJ 2013 <https://www.ncbi.nlm.nih.gov/pubmed/23599251/>). Relegating this important topic to a footnote seems problematic.

Footnote 2: It's not clear why the panel would shift the whole age boundary due to a single study. This seems like a very low end of the range for "older" adults. Many of the issues that are common to older adults (e.g., pharmacokinetic changes, life stage issues related to retirement, development of physical comorbidity, and frailty) would not be applicable to those at the low end of this age range. See further comments throughout the document, particularly in relation to the overlap with the general population that goes up to 65 years old.

**Panel response**

*Similarly, concerning Ms. Medicus' concerns about the elimination of pharmacotherapy efficacy studies (Scope and Footnote 1), this was not a part of our original charge because the guideline document is intended for clinicians who primarily use psychotherapeutic and behavioral interventions for depression, not antidepressant pharmacotherapy. Nor was the panel charged to deal with the treatment of psychotic depression. Concerning Footnote 2, which deals with the inclusion of studies with a lower age-to-enter of 60, rather than 65 or higher, we note in the document that much of the data on late-life depression is based on studies of the "young old" (people aged 60-75), with underrepresentation of participants above the age of 75 or 80. This is an important limitation of the literature available to us, as Ms. Medicus points out. In addition, it is appropriate to note that people age at different rates: some people at age 60 are frail and carry heavy burdens of medical comorbidity, while other people at age 75 or 80 remain spry and actively engaged in life.*

Footnote 3: We support the decision and rationale for the use of the term "patient" throughout the guideline.

**Panel response**

*Thank you.*

Background: ES-2, Line 21. The guideline uses the term dysthymia (now called persistent depressive disorder) several times, particularly in the beginning of the document. Later, in discussion of the specific studies, it is usually referred to as persistent depressive disorder. Ideally this should be written as persistent depressive disorder throughout the document, as this is the current term. With first use, the document could include "persistent depressive disorder (formerly called dysthymia)" for purposes of clarification.

**Panel response**

*Thank you, we are checking the document for consistency.*

ES-3, Line 1-2. The wording of this sentence implies that the mood can be irritable in persistent depressive disorder in children and adolescents. However, the inclusion of irritable mood relates more broadly to the diagnosis of major depressive disorder in children and adolescents and is not simply limited to those who have a persistent depressive disorder. This wording should be corrected to avoid giving misinformation about the DSM-5 criteria.

**Panel response**

*Thank you, we have re-ordered the wording in that sentence to make it clearer that irritable mood is not simply limited to those who have persistent depressive disorder.*

ES-3, Line 9. The guideline actually covers medications (as compared to psychotherapies) as well as some complementary and alternative therapies. This sentence should be updated accordingly. Alternatively, if psychotherapies were meant to be the focus of the guideline, then some of the information on scope and some of the recommendations should be reconsidered to reflect that perspective.

**Panel response**

*Our intention with this sentence was to highlight aspects covered by this guideline that are not extensively covered in other clinical practice guidelines for treating depression (i.e., psychotherapeutic interventions). While our guideline covers treatments beyond psychotherapeutic interventions the goal in this sentence was to highlight ways in which it complements the existing knowledge base.*

ES-3, Line 10. Technically it is now the National Academy of Medicine.

**Panel response**

*Thank you. We have adjusted the footnote to reflect this.*

Process and Method: ES-5, L 8-9. Depending upon the scope of the reviews included, this means that there may be gaps that could have been filled (at least partially) by some consideration of older reviews or updating the search subsequent to the reviews that were included.

**Panel response**

*Thank you for sharing this concern. The panel did extend the parameters beyond those originally established to better capture the literature available, but the adoption of specific criteria does potentially exclude some literature that may have addressed gaps. The decision was based on best guidance on guideline development practices from the IOM.*

ES-5, Line 15...Having read the rest of the guideline, it's clear that the decision tables were done only for the older population as those reviews were analyzed further by individual studies, but it's not clear why the decision was made for this population only.

**Panel response**

*Decision tables were used for the older adult population, which was the first population about which the panel made recommendations. The panel then made a modification to its methodology and switched to using the “grid” for the general adult and child/adolescent populations. Please refer to the methods sub-section called “The development and use of decision tables and the grid,” for details about this methodological decision.*

## Discussion:

ES-6, Line 1-3. The exclusions noted here (e.g., individuals with co-occurring physical conditions, inpatient and collaborative care settings) lead to significant limitations in the applicability of these guidelines. A sizeable fraction of individuals with depression have co-occurring physical conditions and there have been significant increases in the use of collaborative and/or integrative care models to provide treatment for depression in primary care settings. Although the bulk of care for patients with depression and the bulk of clinical research related to depression occurs in outpatient settings, the lack of consideration of inpatient studies also reduces the applicability of the guideline to individuals with severe depression with or without significant suicide risk. Individuals using the guideline may not read the detailed discussions of these limitations in the guideline text and these limitations on applicability should be made clear in any summary tables of condensed listings of guideline recommendations.

**Panel response**

*We are adding a note to the recommendation tables reminding the reader to refer to the scoping discussion in the document for a list of what was included/excluded in the guideline.*

ES-6, Line 4. Although there is discussion of the limitations on adhering to the IOM/NAM recommendations in a later section of the guideline, the discussion in the ES is vague and uninformative. At the very least, this section should include a cross-reference to later sections of the guideline where the challenges with the IOM/NAM approach are described more fully.

**Panel response**

*We are adding a cross reference to this in the executive summary.*

ES-6, Line 14-15. We support the need for more research on the effectiveness of the components of psychotherapies.

**Panel response**

*Thank you.*

ES-6, Line 16-17. Although the dependence on federal funding is likely true of psychotherapy research, pharmacological and other somatic treatments tend to be funded primarily by pharmaceutical companies or device manufacturers, with federal funding being less common. This funding pattern introduces additional biases in the available research including the potential for bias in study design due to commercial interests and a paucity of well-designed studies (including comparative effectiveness studies) once a treatment has obtained approval by the Food and Drug Administration. For older medications and older somatic therapies, this is particularly problematic because standards for study design and rating of evidence quality have undergone significant changes in recent decades.

**Panel response**

*Thank you for the insight. We are adding a note to specify this sentence is particularly true of psychotherapy research.*

ES-6, Line 20: It's unclear what is meant by differences across diverse groups regarding access to funding. "Diverse groups" is usually used to talk about patient populations but this does not seem to be the case in the wording of this sentence. It would be more helpful to explicitly describe the problem with funding, or just say funding is difficult to get.

**Panel response**

*We agree that the phrase "differences across diverse groups regarding access to external funding" is unclear. We have eliminated this phrase. On page 15, line 8 we have eliminated the phrase "if not most". We have added the sentence "Multiple factors influence these preferences including insurance coverage, availability of specific treatments, transportation, and reduced patient mobility and make input from patients and families important".*

Recommendations: ES-8, Line 12-17. The guidance here for recommended and conditionally recommended statements is worded more clearly than the definitions on the preceding page (ES-7 line 8-16). It would seem preferable to consolidate the information on recommendations and conditional recommendations in one portion of the text, particularly because there seem to be subtle differences in the criteria being described in the current sections. On p. ES-7 line 12, the concept of a "conditional suggestion" is also introduced.

**Panel response**

*The panel appreciates this observation and agrees with the judgement of the commenter. The definitions for recommended and conditionally recommended on page ES-7 have been removed given the better clarity of the definition of page ES-8.*

ES-8, Line 23. It would be preferable to divide this item into two discrete parts, one related to treatments that were not recommended and one related to treatments that were recommended against using.

Introduction - Page 2, MDD and persistent depressive disorders are defined but subsyndromal depression needs to be defined as well. In some places in the document, the phrases "minor depression" or "subclinical depression" are also used. It may be useful to point out (either in the definitions of disorders or in a discussion of the document scope) that the guideline is not including depressive episodes that occur in individuals with bipolar disorder, substance/medication-induced depressive disorder, depressive disorder due to another medical condition, or depressive symptoms in the context of disruptive mood dysregulation disorder, premenstrual dysphoric disorder, adjustment disorder with depressed mood, or bereavement. Because the document has recommendations that apply to subsyndromal depression, there may also be a need to distinguish between depression as a symptom and distress or demoralization that patients or others may label as "depression." Such distinctions, among others, may affect whether treatment is even indicated.

**Panel response**

*Thank you, we are adding the definition of subsyndromal depression. Discussion of document scope is in the section called, "Guideline Purpose and Scope: What the Guideline Does and Does Not Address."*

Child and Adolescent Prevalence, Page 3, Lines 8-11. This sentence is very unclear. The original article includes a table that shows individuals with all of these disorders have greater odds ratios for major depression, particularly severe MDD. It may be preferable to say "Furthermore, adolescents with anxiety disorders, attention-deficit/hyperactivity disorder, substance use disorders and behavior disorders (i.e., oppositional defiant disorder, conduct disorder) have significant rates of comorbid MDD and higher risks of developing MDD, particularly severe MDD (Avenevoli et al., 2015)."

**Panel response**

*Thank you, we are making this edit.*

Older Adults prevalence, Page 13, Line 4. The data from the Epidemiological Catchment Area study are not the most recent information available. Haigh et al. (Am J Geriatr Psychiatry. 26(1):107-122, 2018; <https://www.ncbi.nlm.nih.gov/pubmed/28735658>) gives a brief description of current figures along with the associated study references.

**Panel response**

*Thank you, we are updating this reference.*

Suicide, page 14, lines 17-19. In reducing suicide, increasing accessibility and utilization of treatment and reducing firearms access are applicable for all age populations and not just older adults.

**Panel response**

*We agree. Each age section notes the importance of treatment for suicide reduction and we are adding a comment about the importance of reduction of access to lethal means to each section as well.*

Treatment issues, page 15, lines 8 ff. The literature on treatment preferences of patients with depression is complex, with the findings in different studies being influenced by multiple factors including demographic features (e.g., sex, race/ethnicity), prior treatment history, illness severity, and perceptions of illness stigma. (For example, see: Gum et al. Gerontologist. 46(1):14-22, 2006 <https://www.ncbi.nlm.nih.gov/pubmed/16452280> for a study in older adults; Dunlop et al. Am J Psychiatry. 174(6):546-556, 2017, <https://www.ncbi.nlm.nih.gov/pubmed/28335624> for a study of adults; and Gelhorn et al. Prim Care Companion CNS Disord. 13(5) pii: PCC.11r01161, 2011, <https://www.ncbi.nlm.nih.gov/pubmed/22295273/> for a systematic review of treatment preference impact on outcomes). It is reasonable to say that "many" older adults prefer psychosocial interventions without extending such a preference to "most" older adults. It would also be useful to highlight that multiple factors can influence these preferences and that individualized input from the patient is important in matching interventions to patients' wishes with consideration of logistical issues such as insurance coverage, geographic availability of specific treatments, transportation to visits, reduced mobility making attendance at appointments more challenging, or other factors.

**Panel response**

*Ms. Medicus' comments about patients' treatment preferences are accurate. We agree that while many patients (including older adults) prefer psychotherapy to medication, treatment preferences are informed by such factors as prior treatment, insurance coverage, and care-related burden (such as access/transportation to treatment) and vary by patient characteristics such as racial and ethnic group status.*

Page 15, line 11-12. Defining treatment goals in terms of returning to functioning, engagement, and well-being is not unique to psychosocial interventions and is a goal of most patients, regardless of the treatment approach that is used.

**Panel response**

*We are modifying this sentence to be clearer about this point.*

Page 15, line 13-14. Although there is a particular paucity of clinicians who are trained to provide care to older individuals, the challenge discussed in this section (lack of access to psychosocial treatment) is not unique to older individuals. Many of the other topics discussed in this section are also shared by individuals of other age groups and not unique to older adults. (See also p. 16, lines 7-11.)

**Panel response**

*We concur. Many topics discussed in each of the age cohort sub-sections (e.g., suicide, co-morbidity) are not unique to that age only. Rather the goal of each age cohort sub-section is to highlight treatment issues for these various topics as they relate to that age group.*

Page 16, line 3-4. The sentence that reads "Many frail older adults who are home-bound, and who either cannot or will not come to a clinic, must live with symptoms of depression and cognitive impairment." would benefit from rephrasing. For example, "Many frail, home-bound older adults who have symptoms of depression and cognitive impairment have difficulties in obtaining appropriate care."

**Panel response**

*Thank you, we are rephrasing this sentence.*

Page 18, line 14. The text should note that the decision tables were used for the older adult population, whereas the grids were used for other portions of the review.

**Panel response**

*Thank you, we are noting this.*

Page 18-19, line 19 ff. The last paragraph on this page and of the section talks about the reviews for child/adolescent and general population but not the older population. This section should also be clearer about the inclusion and exclusion criteria for these reviews and the fact that medication efficacy was not a part of the reviews.

**Panel response**

*The goal of this section is to highlight ways in which our guideline differs from others currently available. Thus, panel members chose to focus on more unique aspects of the methodology of the child and general adult review selection/inclusion and not the older adult reviews. Likewise, the inclusion/exclusion criteria for each review are beyond the scope of this particular section but are discussed in more detail in the methods section of the guideline.*

Page 19, Line 2-4. With the text as written, the reader assumes that the statement on AMSTAR quality criteria is in reference to studies of the child/adolescent literature and per details further in the document, the reviews completed by evidence-based practice centers were not reviewed with AMSTAR. However, in the tables summarizing the guideline recommendations, references to the AMSTAR criteria are also made with respect to other population subgroups. These sentences and other portions of the document may require review and revision to assure accuracy and clarity about when and to what bodies of evidence the AMSTAR criteria were applied.

**Panel response**

*Thank you, we are making this distinction clearer in the document.*

Page 21, Guideline Purpose and Scope. This section should be clear on aspects of depression treatment that are not included in the guideline scope such as severity of MDD, treatment resistant depression (although it was an inclusion criterion for the reviews), treatment of suicide risk, and psychotic depression. Additionally, the clinical questions specifically mention ECT and TMS, however these are never addressed in the guideline at all, except noting that ECT is recommended by other organizations in certain situations. At the very least, the document should include information about the reasons that these treatment modalities are not considered further (e.g., absence of studies meeting inclusion criteria). And although locus of care isn't specifically looked at, the studies do say which populations are covered, such as outpatient, inpatient, etc. and that information could and should be included as it affects generalizability. There is no discussion of why APA chose to not do its own systematic review or at least a search of studies since the reviews were published to make sure there wasn't anything that could affect the strength of a recommendation.

**Panel response**

*Thank you, we are adding information to the document about the absence of studies meeting inclusion criteria for ECT and TMS. While it is not feasible for the list of items not addressed by the guideline to be completely exhaustive of all possibilities, we are changing the language of this section to note items that were not explicitly searched for in the underlying reviews. It was not practical to do our own update of the ten included reviews across the lifespan.*

Page 21, line 17. Although early literature used the term "repetitive transcranial magnetic stimulation" or rTMS, more recent literature tends to use the simplified phrase "transcranial magnetic stimulation" or TMS.

**Panel response**

*Thank you, we are making this modification.*



## Process and Method - Conflicts of interest.

Page 24, line 24. The text notes that "No panel members were to be singularly identified with particular interventions." However, at the end of the guideline, the disclosures of the group members suggest that a number of group members have written royalty-generating books on specific psychotherapies and/or have been involved in the development of specific psychotherapies. This would seem to be a significant source of potential intellectual bias.

**Panel response**

*Indeed, while the Advisory Steering Committee did not appoint individuals they deemed to be "singularly [and strongly] identified with a particular intervention," some panel members have contributed to the development of specific interventions. The ASC reviewed these individuals' completed COI forms before recommending that they serve on the panel in the belief that the potential intellectual bias could be managed. A primary strategy for managing such intellectual bias was to ensure a balance of perspectives amongst panel members to foster "adversarial collaboration."*

Page 25, lines 6 ff. The document notes that panel members completed an educational module that included information on the importance of identifying and managing conflicts. In addition, it notes that actual or potential conflicts would be verbalized "at request." However, it is not clear how conflicts were actually managed (e.g., did individuals with perceived conflicts on a given topic recuse themselves from those discussions). In addition, it is not clear who would make the request for conflicts to be verbalized and if these conflicts were ever verbalized and discussed with the group as part of the group discussions or process.

**Panel response**

*Multiple strategies were used to identify and manage COI. Panel members (and ASC members and associated staff) all completed a disclosure form on an annual basis that was reviewed by the panel and ASC chair. Panel members were expected to disclose potential COI at all face to face meetings and on phone calls whenever new COI emerged. This was structured into the agendas for the meetings. Several strategies were used to manage COI and typically these involved some combination of recusing from the discussion of a particular topic, recusing from voting on certain issues or a combination of the two.*

Search. Page 27, Lines 1-2. As described under general comments, limiting the evidence to these systematic reviews limits the inclusion of more recent literature. Even if newer studies were unable to be incorporated into the meta-analysis, they could at least be discussed, and it could be noted if they would potentially affect the strength of recommendation.

**Panel response**

*Thank you for sharing this concern. This issue of the limits of the process in part has been addressed in response to the comment regarding ES-5 L 8-9. In relation to discussing other literature, the panel has in several areas made reference to additional literature. In doing so, the panel has aspired to maintain the integrity of the guideline process and not incorporate research that was not vetted appropriately into the recommendations, but rather to identify areas for future guidelines to address (for example, long term benefits of treatment for preventing relapse and recurrence).*

Page 27, Line 14. It should be noted what definition of depression was used for the review of psychotherapies for depression. It would also be helpful to include a brief description of exclusion/inclusion criteria for both reviews.

**Panel response**

*We are adding the depressive diagnoses that were included in the reviews and adding in page numbers for the interested reader to see the original inclusion/exclusion criteria for each review.*

Page 27, line 20 ff. The description of the two general population systematic reviews should include the depressive diagnoses that were included in the reviews and other information on inclusion and exclusion criteria. For the additional reviews used to supplement the two primary reviews, the reasons for selecting these specific reviews is not clear. For example, were reviews on other psychotherapies unavailable or were such reviews found but excluded for a specific reason.

**Panel response**

*We are adding the depressive diagnoses that were included in the reviews and adding in page numbers for the interested reader to see the original inclusion/exclusion criteria for each review. These additional reviews were used to address gap areas that the panel felt to be missing from the original review.*

**Strengths/Limitations of Review**

Page 31, Line 13 ff. In the section on older adults, it is not clear why a separate analysis was undertaken for the older adult population only with extraction of data from reviews into evidence profiles. This approach seems to have resulted in very specific but potentially less useful recommendations for this population group. It also seems to have led to a lower than typical age threshold for older adults and overlap with the general adult population. The differing levels of detail across age groups makes the document appear unbalanced. Furthermore, the difference in recommendations for adults and older adults becomes especially problematic given the overlapping age ranges. Even without the overlap in ages, readers would typically like to understand whether there is something specific to older age that makes a treatment more or less effective (and thus, more or less indicated) or whether the differences in recommendations simply reflect the studies that have been funded in a particular age group.

**Panel response**

*Concerning Ms. Medicus' discussion of the strengths and limitations of the review of treatment in older adults, we note that treatment response variability in older adults is associated with factors that are frequently prominent in the clinical presentation of late-life depression and therefore relevant to treatment considerations, including a range of medical, neurologic, cognitive, and psychosocial burdens that affect the elderly more than younger people. For these reasons, we view it as scientifically and clinically inappropriate to generalize treatment recommendations from data originated in younger people to the care of older adults who present with age-dependent challenges.*

*In response to her comment on the Strength and Limitations of the Reviews and why a separate analysis was undertaken only for the older adult literature- we were able to use evidence profiles generated by AHRQ and VA/DoD reviews that were not available for the older adults and thus had to be independently provided. Also, because there was often only one study of a particular comparison (e.g., CBT vs. TCA), the resulting recommendations are more specific versus the more general recommendations of the general adult section.*

Page 32, Line 10. Although each of the major reviews provided a comprehensive literature search based on their specified scope, there were still gaps in the literature search for the guideline as a whole. Some of these are described elsewhere in the document and our general comments also discuss the difficulties inherent to this approach. We appreciate the difficulty of identifying the reasons that particular studies were excluded from consideration, particularly given the reliance on existing systematic reviews. Nevertheless, it would be helpful to know why a number of major federally funded studies (e.g., pharmacotherapy and psychotherapy trials in children/adolescents; treatment of depression with ECT, including in geriatric patients) do not seem to have been considered.

**Panel response**

*We have explained the methods for the searches in the guideline (p.23-29). It was not possible to identify all studies and all treatment methods (such as ECT), because of the huge number of studies. We do understand the frustration that major federally funded studies were not included, but there is no systematic way of selecting the best available studies, except through assessment of meta-analyses (or new literature searches).*

*This is an important constraint. The inclusion of reviews was determined by the PICOTs questions as well as the determination to comply with IOM guidance on the development of clinical practice guidelines.*

Page 39, Line 1 ff. It is generally recognized that important items to the development of practice guidelines, mostly harms and patient preferences, will not be collected in RCTs and therefore there must be other methods used to gather than information. It would have been reasonable to set up the process for this a priori, and to use it for future guidelines. The process used to search for and assess any additional studies should be included for purposes of transparency and reduction of possible bias.

**Panel response**

*The methods used to gather information on harms/burdens of treatment and patient values and preferences are detailed in the methods section below page 39, in the sections labeled, "Assessing magnitude of harm/burdens," and "Assessing patient values and preferences."*

Page 39, Line 10. Again, for purposes of transparency, it would be helpful to know the specific ways in which these considerations influenced the final guideline statements.

**Panel response**

*The methods used to gather information on harms/burdens of treatment and patient values and preferences are detailed in the methods section below page 39, in the sections labeled, "Assessing magnitude of harm/burdens," and "Assessing patient values and preferences." Details of specific harms/burdens and patient values and preferences for particular interventions and their role in influencing final guideline statements are included in the decision tables and grids.*

Page 40, Rating strength of evidence. Although this process conforms to GRADE, readers may not appreciate that ratings are reduced to the "lowest common denominator", which is almost always quite low due to the paucity of high-quality data on harms. This aggregate rating makes the information less useful to readers than reporting strength of evidence separately by outcome or reporting separate strength of evidence for benefits vs. harms. Information other than these aggregate ratings were apparently included in making decisions as there were still a number of statements with a strong strength of recommendation.

**Panel response**

*Quality of evidence is reported separately by outcome in the decision tables and grids. We concur however with the issue that the "lowest common denominator" very frequently makes the overall rating very low/insufficient due to the paucity of high-quality data on harms and will explore other ways to report strength of evidence for future guidelines.*

Page 42, Line 1. It is not clear why additional searches were only done for the older adult population and not to supplement other reviews. In addition, it is important to include all search terms used, including search strings and numbers of results retrieved. If the search was limited to the listed terms (line 3), it seems likely that relevant articles would have been missed. In addition, other databases (e.g., PubMed, Cochrane, EMBASE) sometimes identify additional references that are not found in PsycINFO. It would also be important to include information on the process for screening these articles as well as inclusion/exclusion criteria.

**Panel response**

*The additional supplemental searches conducted by staff were a focused review, not a systematic review by any means and they thus do not contain the same level of detail as would a systematic review. These additional searches focused only on PsycINFO®. For the general adult and the child/adolescent populations, harms and burdens were only extracted from the included articles due to resource limitations.*

Page 42, Line 10. Although it is possible to infer the name of the child psychiatry expert from the acknowledgements section of the document, it would be helpful to be more specific about the role of this individual in developing the guideline (e.g., discussion of child/adolescent

recommendations, crafting of recommendations, after the fact review for potential concerns) and whether processes related to conflicts of interest were similar for this individual as for other panel members. (If not, were disclosures discussed with the panel?)

**Panel response**

*The child psychiatry expert did an after-the-fact review of recommendations for potential concerns and clarity but did not contribute to the drafting of recommendations. All decision-making was done by panel members and the expert was thus not required to undergo the same conflict of interest procedures as the panel members.*

Page 44, Line 11-13. It would be important to include further information on how consensus was achieved and assessed if not through formal voting. In addition, it is not clear what circumstances would signal a need for a formal vote. For each guideline statement, it would be important to note whether it was agreed upon through consensus or through a blinded or open vote. When vote did occur, the number of members, if any, who disagreed with a specific statement should be noted for purposes of transparency.

**Panel response**

*Consensus was achieved via discussion. We are adding a note to refer the interested reader to the appendix where additional information on the open voting procedures is detailed and will consider providing additional information in future clinical practice guidelines.*

Discussion of Clinical Recommendations - General comments Page 44. We are pleased to see the inclusion of a discussion section in the document. Nevertheless, as noted previously, it is very hard to figure out which guideline statements in the initial tables of recommendations are associated with the relevant discussion sections. At the very least, it would be helpful to add additional subheadings that would help the reader navigate this portion of the document. Alternatively, each statement could be reproduced here and immediately followed by the relevant discussion or this section could follow the table of recommendations. References must be included for many of the statements in this section.

**Panel response**

*Thank you, we are adding in additional sub-headings and references for clarity.*

Some important areas are already highlighted (e.g., racial diversity in IPT-A for youth, paucity of studies in children relative to older youth). It would also be helpful to reiterate some of the other gaps in knowledge and gaps in the literature review in this section, to provide the reader with an appropriate context for interpreting the recommendations.

**Panel response**

*Several critical gap areas are highlighted in this section however a more extensive discussion of gaps in the knowledge and literature review are provided in the sections that follow.*

Some of the guideline statements seem to have an associated discussion, whereas others do not. It would be useful to include some discussion of each guideline statement.

**Panel response**

*The panel provided discussion for a number of guideline statements individually, focusing on those that it felt warranted discussion beyond the rationale discussed in the recommendation tables and grids/decision tables. The effect sizes of several therapies were comparable in direct comparisons, thereby not indicating major differences between the therapies. Thus, several therapies are discussed more generally as a group with the panel encouraging shared decision-making with the patient for these therapies.*

Page 44, line 23 ff. We understand that screening and prevention were not within the document scope but it may be worth mentioning the importance of early identification and treatment of subsyndromal depression given the current and future impact of untreated symptoms on youth and their families. It would be helpful to address the potential differences in treatment for different severities of depression, associated comorbidities or presence of suicidal ideas or behaviors. If such information is lacking, that could be specifically mentioned.

**Panel response**

*Discussion of the potential differences in treatment for different severities of depression and associated comorbidities was outside the scope of this guideline. However, we acknowledge your concerns but due to the constraints of the methodology and scope, we cannot elaborate on these topics in this guideline.*

Page 46, Line 22. It would be helpful to have more information on why escitalopram wasn't included, even as a conditional recommendation. There should also be some description of what the FDA pivotal trials showed and the specific reasons that these trials were not viewed as sufficiently rigorous to inform the guideline statements.

Page 47, Lines 1-3. In the discussion of nefazodone, it would be helpful to mention that nefazodone is rarely used (in adults as well as in children/adolescents) because of the rare occurrence of hepatotoxicity, which has been fatal in a small number of cases (<https://livertox.nlm.nih.gov/Nefazodone.htm>). This section should also include discussion of the other medications that were not recommended for use in this population.

**Panel response**

*We believe that the appropriate response to Ms. Medicus' query about escitalopram (and other antidepressant agents) is that the guideline was not intended to be a comprehensive review of psychopharmacological treatments of depression. We concur with her comment about nefazodone's hepatotoxicity.*

**General Adult Population**

Page 47, line 24-25. It would be helpful to note why comparative effectiveness was emphasized for the adult literature, particularly as this approach leads to significant gaps in the evidence related to pharmacotherapy, with limits on the generalizability and utility of the conclusions. As noted above, issues of depressive severity also would benefit from additional discussion because this affects choice of treatment and shared decision making for individual patients.

**Panel response**

*The panel chose to focus on comparative effectiveness data for the treatment of depression in adults and older adults when it made its initial scoping decisions for the guideline. Part of this decision pertained to the availability of other clinical practice guidelines already focused on the efficacy of pharmacotherapy for treatment of depression in adults. The data did not allow the panel to make further recommendations pertaining to severity beyond what is noted regarding diagnosis though the panel includes some discussion of this in the text*

Page 48, Line 6. In terms of the section on complementary and alternative treatments, apart from the conditional nature of the recommendation, it is not clear how these approaches are prioritized relative to other treatments. Certainly, given the many other health benefits of exercise, suggesting exercise is reasonable (assuming that the patient does not have any major physical health issues that would make exercise problematic). On the other hand, with St. John's Wort, there should be a more detailed discussion of potential side effects, drug-drug interactions, differences in preparations (including potential for adulterants) and other factors related to use. There is an incorrect assumption by many patients, and even by many health professionals, that natural products are necessarily safer than products developed and manufactured by pharmaceutical companies.

**Panel response**

*Ms. Medicus' caveats and cautions about the use of St. John's wort are entirely appropriate. Similarly, her emphasis on the importance of assessing treatment adherence before inferring failure of a particular pharmacotherapy is also on point. Her call for greater precision and consistency of language (continuation therapy for relapse prevention and maintenance therapy for prevention of recurrence) is also consistent with the literature on depression in both midlife and older adults.*

Page 48, Line 7. Unless the guideline statement is restated before each section of text, it would be helpful to specify the alternatives mentioned here.

**Panel response**

*Thank you, we have made this edit.*

Page 48, Line 10. It would be useful to note the importance of identifying barriers to adherence, determining whether antidepressants are being taken as prescribed, and whether dosing has been optimized before changing treatments unless tolerability is also an issue.

**Panel response**

*The importance of noting barriers to treatment are discussed in the section on patient values and preferences and general treatment adherence is discussed. However, the scope of the panel's work did not include efficacy of antidepressant medications for the general and older adult populations. Thus, the panel did not detail information about medication dosing optimization, tolerability, etc.*

Page 48. Line 15. Some portions of the document make reference to maintenance therapy whereas this guideline statement focuses on relapse prevention. It would be helpful to use consistent terminology throughout the document. If the panel sees relapse prevention as distinct from maintenance therapy, these distinctions should be spelled out in terms of their definitions and clinical implications. Unless the statement is modified (based on our earlier comments), it is

crucial to caution readers that discontinuation of antidepressants can be associated with increased risk of relapse, particularly if done abruptly or in individuals with recurrent depressive episodes. Decisions about medication discontinuation must also consider the severity of prior episodes and association with other symptoms such as significant functional impairments or suicidal ideas.

**Panel response**

*In the context of continuation and maintenance therapy, risk for relapse and recurrence, respectively, is usually increased if antidepressant medication is stopped.*

Page 48, Line 18. In the sentence on effect sizes, it is not clear which specific therapies are being discussed. It is also not clear whether the statement is referring only to psychotherapies or also includes consideration of antidepressant treatments or other interventions.

**Panel response**

*A table listing brief descriptions of different types of treatment by age group is included in the appendix. We are adding a more prominent note about this to the main guideline document. Brief justifications for recommendations made are included in the justification column of each recommendations table with additional details for each available in the decision tables and grids. We are making this clearer by adding the grid or decision table the interested reader can reference for further details for each recommendation. The decision tables and grids also contain information about harms/burdens and patient values and preferences. The data included in the systematic reviews did not allow the panel to make recommendations about enduring effects. We are adding in references to the original reviews for the interested reader to view forest plots.*

Page 49, Line 1. It is not clear whether negative effects are being viewed differently than side effects. It would be preferable to standardize the use of terminology throughout the document for consistency.

**Panel response**

*The guideline retains the term “negative effects” in one discussion as there is a difference between side effects (typically understood to be possible adverse consequences that might occur with psychotherapy, such as deterioration) and “side effects” or the typically physiological effects of various medications. We agree that these terms are not always well defined, and we particularly support more research and reporting of negative effects, along with the more routine reporting of side effects.*

Page 49, Lines 6-11. It would be useful to provide citations to this statement and specify the subpopulations (if other than patients with chronic and treatment-resistant depression). In addition, it is not clear why combined treatment is noted to be “usually recommended” here for these subgroups but not as part of the guideline statements. Other treatments that are effective in individuals with treatment resistant depression (e.g., ECT, TMS) would benefit from being mentioned in this context.

**Panel response**

*It is appropriate to note, as Ms. Medicus suggests, that neuromodulatory treatments (ECT, TMS) should be considered in the context of treatment-resistant depression.*



Page 50, Line 18. We agree with the observation that psychotherapies may have enduring effects after the cessation of treatment and that the potential enduring effects of treatment are important to discuss with patients in the context of shared decision making (about initial treatment and maintenance treatment approaches). Based on the section as written, it is not clear whether the effects of antidepressants are lost due to medication discontinuation or due to loss of efficacy with continued treatment. (It makes intuitive sense that a medication would no longer exert an effect once it is no longer in one's system.) In terms of the enduring effects of psychotherapy, it would be helpful to describe the extent to which and the time period for which benefits persist after the end of treatment and whether less frequent sessions or "booster" treatments are needed to maintain or optimize benefit, if such information is known.

**Panel response**

*Thank you. While these questions are beyond the scope of the data examined by the panel, discussion of long term effects of treatment are included in the sections of the document.*

**Older Adult Population**

Page 51. As previously discussed, the reliance on comparative effectiveness data is problematic in terms of making specific statements about pharmacotherapy in older adults. There is also no delineation of treatment choices according to the severity of depression or the presence of comorbidities (e.g., cognitive impairment) that may influence participation in psychotherapy. Other issues related to the content in this section (e.g., issues related to antidepressant choice, over-specificity of some recommendations for very small patient subgroups, overlaps in age ranges with adult populations and disparities in recommendations with adults, comparisons with treatment as usual) have also been discussed above. There are a number of references in this section to "nuanced situations"; these references are difficult to understand without more specificity or inclusion of the full guideline statement.

**Panel response**

*Concerning antidepressant pharmacotherapy in older adults with varying degrees of severity and treatment-resistant depression, we indicate in the text that the question is still open regarding the comparative effectiveness of augmentation versus switching strategies. PCORI is currently sponsoring a multi-site clinical trial (the OPTIMUM study, coordinate by Professor Eric Lenze at Washington University in St. Louis, and with participation by sites at Pittsburgh, Toronto, Columbia, and UCLA), to examine this very issue and using a variety of agents (including bupropion, aripiprazole, nortriptyline, and lithium) in comparative effectiveness studies of augmentation and switching. The study's primary hypothesis is that augmentation will likely bring about greater remission rates than switching but will also lead to greater side effects (especially in those over 75).*

Page 51, Line 4. This initial sentence is confusing and could be simplified by stating "The panel's recommendations and suggestions for older adults are largely consistent with other reviews of depression treatment in this population."

**Panel response**

*The panel respectfully disagrees that the sentence on page 51, line 4 is confusing and finds the suggested revision less explanatory.*

Page 51, Lines 7 and 13. It is not clear whether life review treatment and life review course are the treatment. If so, consistent use of the same terminology is suggested. If not, the distinctions in the treatment should be explained.

**Panel response**

*The guideline was changed to consistently reference "life review treatment."*

Page 53, L 3-6. The use of evidence-based integrated care models such as the collaborative care model can be effective. Additional references for the older population would be useful to include for the interested reader, given the importance of this issue.

**Panel response**

*Ms. Medicus recommended that we provide more references to integrated/collaborative care on page 53; these references were mentioned on page 15 of the document.*

Page 53, Lines 7-15. The issues of medication choice in older individuals are important ones and have been commented on above.

Page 53 Lines 15-20. It is not clear why this review was not included in developing guideline statements since supplementary reviews of psychotherapies were incorporated into the guideline development process

Considerations for Treatment Implementation - Consent Page 54. Because the guideline includes treatment of depression in children and adolescents, it would be useful to mention the issue of assent for treatment in adolescents. (The American Academy of Child and Adolescent Psychiatry practice parameters/guidelines often have language that could be helpful. has resources materials available relating to this topic.)

**Panel response**

*The committee appreciates the commenter's important concerns regarding assent in the process of obtaining clinical care. The committee agrees that generally speaking, since most often the uptake of psychotherapy is a voluntary process, providers will confer with the parents, guardians or caretakers of children and adolescents brought in for care. It is believed that given the voluntary nature of help-seeking for psychotherapy, assent is implied, though it is important for any provider to carefully review his/her qualifications, approach to treatment and initial course of care at the outset. So, the panel has added the following language to the relevant section of the document.*

*"When working with children and adolescents, the provider should convey this same information to the parent/guardian who must provide consent for treatment along with conveying this information to adolescents who must assent to treatment. For children, it also would be helpful to convey some of the information regarding interventions to be provided and the rationale in an age-appropriate manner."*

Patient Values/Preferences –

P. 54. This section should be renamed to include other factors influencing treatment selection. Much of the discussion actually focuses on barriers to receiving specific treatments and barriers to care more broadly. Although these limitations can influence patient choices, they are often distinct from their treatment related values and preferences if such barriers didn't exist.

**Panel response**

*Thank you for your comment. We are making this edit accordingly.*

P. 55. In the table of potential barriers, other possible barriers include lack of access to childcare. Insurance related issues are complex and can include availability as well as financial considerations, particularly when patients are unable to find any clinicians who participate in their insurance plan. (This seems different from insurance not covering treatment.) Treatment availability also includes lack of access to specific expertise ("I was told to look for someone who does interpersonal therapy, but the closest person is 2 hours away.")

**Panel response**

*The panel appreciates this response and has made some of the suggested additions. That said, this table is not meant to be an exhaustive list.*

Enhancing Therapeutic Alliance and Other Principles/Processes of Change - Page 65. Lines 20-24. We would suggest deleting this paragraph and having the section on change mechanism focus on processes of psychological change. The changes produced by antidepressive treatments are extremely complex and our understanding of those changes is evolving. There is no way to convey these complexities in a few lines and to attempt to do so is likely to be misleading rather than helpful.

**Panel response**

*We are revising this paragraph to note the complexities of changes produced by anti-depressive treatments and that our understanding is evolving.*

How the APA Clinical Practice Guideline Compares to Other Clinical Practice Guidelines for Treatment of Depression - Page 69 lines 14-17. It is not clear what aspects of these studies (including TADS) were unconvincing to the panel, particularly given the many strengths of the study and fact that additional large comparative effectiveness or combined treatment studies are unlikely to be forthcoming.

**Panel response**

*The panel appreciates the commenters concerns and notes that TADS was an important, though singular study with some noted safety concerns related to the specific medication used. As a result, the panel felt that it was prudent to consider all of the evidence related to the TADS trial within the context of the parameters of the panel's charge (i.e. a focus on psychotherapeutic interventions for the treatment of depression).*

Challenges in Developing the Guideline and Recommendations for Future Efforts - Page 85-86. We concur with the importance of addressing treatment resistant depression in older adults and, indeed across all age groups. It is not clear why the meta-analysis cited here was not used as a supplement to the existing studies as it seems that it would support use of antidepressant medications in older adults given the strikingly low value for NNT.

**Panel response**

*The scope of the panel's work did not include efficacy of antidepressant medications for the general and older adult populations. Thus, reviews with antidepressants were only included if they assessed comparative effectiveness of antidepressants with psychotherapy or somatic or complementary and alternative medicine treatments. The meta-analysis referenced focused on antidepressant medication.*

Pages 86 to 91. We support the principles outlined in these sections related to a better taxonomy for psychotherapies, more rigorous comparisons of treatments, and improvements in methodologies, reporting, and knowledge of treatment mediators/moderators. The need for improved research funding is also critical to improving our understanding of effective treatments and providing personalized care to address patient-centered outcomes.

**Panel response**

*Thank you.*

Author Disclosures - The disclosure statements are not consistent in whether they specify a lack of conflicts in developing the guideline. Because issues of intellectual bias or conflict can be difficult to identify, it may be preferable to report that the individual has no financial conflicts of interest with work on the guideline.

**Panel response**

*The following introductory comment for the disclosures section will be added:*

*"None of the panel members had any financial conflicts of interest. The following points, drawn from panelists' disclosures, were among the information noted in assessing and managing potential intellectual conflicts of interest."*

Appendices - We did not conduct a detailed review of the appendices.

Recommendations - Table 1: Child and Adolescent Population ES-9. In this initial statement, the recommendation for CBT or IPT is based on an extremely broad definition of depressive disorders (as defined in footnote 5). It would be preferable to note the specific evidence available for each subgroup of depressed individuals (based on severity and chronicity) and for specific guideline statements to be developed for each subgroup based on that evidence.

**Panel response**

*The panel did not have sufficient evidence to warrant claims for subtype-specific treatments. Studies typically enroll participants with major depressive disorder, but there is great heterogeneity in this diagnosis. There is relatively little understanding of moderators of treatment response derived from adequately powered subgroup analyses.*

ES-9. The subsequent statements on this page that are listed under "Initial Treatment" do not follow a standardized structure for the statement. For example, for the medication statement related to use of fluoxetine, there is no information included in the justification column related to the initial treatment of children and adolescents although the statement itself notes that it was based on "considering medication options and research between different medications." Nevertheless, this justification is non-specific and of little help to the reader.

**Panel response**

*The panel concurs that this language is confusing and has attempted to edit for clarity.*

ES-9. With this statement on fluoxetine, it is unclear whether the panel intends for fluoxetine to be used only in the subgroup of individuals with major depressive disorder or whether this statement is intended to apply to the broader group of individuals with a depressive disorder (as defined in footnote 5).

**Panel response**

*The panel understands that the fluoxetine trials were conducted in participants with major depressive disorder and the recommendations in this case are specific to major depressive disorder.*

ES-9. The third statement notes that there was insufficient evidence on whether to recommend either psychotherapy or fluoxetine over the other treatment. However, no mention is made of whether combination treatment (i.e., fluoxetine plus psychotherapy) is recommended either as an initial treatment approach or a subsequent approach if initial treatment does not produce response. This omission is particularly striking given the availability of evidence from the federally funded Treatment of Adolescents with Depression Study (TADS) and Treatment of SSRI-Resistant Depression in Adolescents (TORDIA) trials, which showed apparent superiority of combined treatment (e.g., March JS and Vitiello B, Am J Psychiatry. 166(10):1118-23, 2009; Brent et al., JAMA. Feb 27;299(8):901-13, 2008).

**Panel response**

*We concur with this comment: both the TADS and TORDIA studies demonstrated the efficacy of combined treatment in patients not initially responding satisfactorily to monotherapy with psychotherapy or medication.*

ES-10. Under "additional psychotherapy recommendations for initial treatment" of children and adolescents, the recommendation column notes that "the panel suggests considering an alternative model." Most guideline developers try to avoid use of the words "consider" or "considering" as these terms are vague and unable to be assessed in operational or measurable terms. In addition, use of the word "suggests" makes it seem as if the listed therapies are being conditionally recommended, though there is no evidence for one over the others. The next column, however, notes that there is "insufficient evidence for recommendation" and this is consistent with the information listed under justification. This would make sense for not recommending one treatment over the other but the suggestion itself, should be a conditional recommendation for use and a corresponding justification should be inserted. If there is no evidence to actually support using one of the other therapies regardless, then either there shouldn't be a statement, per the APA process, or there should be additional information on why it was included.

**Panel response**

*Thank you for this comment. The panel has decided to retain the term "consider" for conditional recommendations because it captures the lower confidence in the recommendation.*

ES-10. Under "Additional pharmacotherapy guidance for initial treatment, the panel "recommends" a shared-decision making approach, but this seems inconsistent with the strength of recommendation, which is only conditional, and with the preceding statement, which is noted as having insufficient evidence to make a statement.

**Panel response**

*The panel respectfully disagrees. Shared decision-making seems particularly appropriate, and important, in clinical circumstances where available data are inconclusive to strongly support one intervention over another; and where patient and caregiver preferences are particularly important.*

ES-10. Under "Additional pharmacotherapy guidance for initial treatment," the wording of the justification does not convey the complexities of decisions related to the benefits and risks of antidepressant use nor does it convey other nuances of the available information for the different medications, such as differences in the evidence base (e.g., findings with different antidepressants, absence of studies in children and adolescents for many medications) and differences in FDA approval status of antidepressants in children and adolescents. As written, the justification implies greater suicide risk with other medications as compared to fluoxetine and focuses primarily on suicide risk as a safety issue. We are concerned that these aspects of the justification are misleading as written. We understand that antidepressants other than fluoxetine may have additional (or different) risks of side effects than fluoxetine and that these must be balanced against more limited evidence of efficacy from clinical trials, but these points are not conveyed in the justification as currently worded. In addition, the statement implies that these medications have an increased risk of suicide, per se, whereas the bulk of the available evidence relates to suicidal ideas, attempts, or other suicidal behaviors. The vast majority of studies did not include any individuals who died by suicide and, when suicide deaths did occur, these were small in number making statistical comparisons more difficult and less precise. Other evidence from observational or administrative database studies suggests that reductions in antidepressant prescribing after the FDA black box warning may have been associated with increases in suicide deaths. Importantly, all of these studies have a number of experimental confounds (e.g., observational design, exclusion of suicidal individuals in randomized trials). Unfortunately, none of these issues would be able to be inferred from the current text. Finally, the justification should be more specific and refer to "other antidepressant medications" rather than referring to "other medications" more generally.

**Panel response**

*All of these points are well taken but would seem to be more appropriate for psychopharmacology treatment guidelines. Our guideline is clear that the antidepressant of choice in adolescents is fluoxetine, based upon best available and extensive data. We also acknowledge that the black box warning about activation of suicidal ideation in patients under the age of 25 may have had unintentional effects of being a barrier to appropriate antidepressant use in children and adolescents, based upon analyses by Robert Gibbons and colleagues.*

ES-11. There is an implication, based on the recommendation against use, that these medications are more problematic than either fluoxetine or other unnamed antidepressant medications. As with the statement on the previous page, however, it is not clear what differences or specific safety concerns exist for these medications, at least based on the listed justification. It is also not clear why these medications were included on this list whereas others with similar properties are not. The tricyclic antidepressants imipramine and clomipramine are included whereas other tricyclic antidepressants are not; monoamine oxidase inhibitors presumably would fall into a not recommended category for children and adolescents as well due to risks of food-drug and drug-drug interactions and tolerability considerations. Also, the justification refers to use of these medications in children but presumably intends to comment on use in children and adolescents. As worded, it is not clear which elements and available

evidence should be incorporated into the shared decision-making process. We suggest deleting the second portion of this statement (i.e., recommending paroxetine over clomipramine and paroxetine over imipramine). These medications are not recommended whereas a number of other antidepressants are not included on this list and would presumably be tried first. Listing these comparisons gives these medications more weight than indicated.

**Panel response**

*We appreciate the comments, but they are more appropriate for detailed psychopharmacology guidelines, which was not the mission or charge to the APA panel. We have made recommendations based upon the studies obtained our commissioned literature reviews of comparative effectiveness.*

Recommendations - Table 3: Older Adult Population

ES-17 to E23. It would be helpful to have a footnote that describes the differences in the systematic review for this population as compared to the child/adolescent and adult populations. The recommendations in this portion of the guideline seem more confusing, perhaps because the analysis occurred at the level of individual studies rather than being based on prior meta-analyses. The level of specificity in terms of patient subgroups (e.g., under the section on MDD with medical or other complications) also seems to be based on limited data.

**Panel response**

*We agree that the data available for treatment of major depression in older adults are limited and that published subgroup analyses examining moderators of response remain scarce.*

ES-17 to E23. The rationale for having the "older adult" age range begin at age 50 is perplexing and not consistent with usual conventions. It appears to be based on the inclusion criteria for a specific study, however, the age range of 50-65 has considerable overlap with the general adult age range in terms of other studies. Consequently, it is difficult to know which set of recommendations to apply to those from age 50-65 and the recommendations seem rather different in the included comparisons and interventions. An individual over 80 years of age is likely to be quite different than a 50-year-old in terms of the physiological effects of medications, sensitivity to medication side effects, and frequency of physical health issues, as well as in terms of phase of life issues that may be addressed by specific psychotherapies (e.g., life review treatment).

**Panel response**

*We have previously dealt with this issue and concur that the threshold for defining older adults should be age of 60. We also concur that there is much age-dependent variability in the efficacy and tolerability of different treatments.*

ES-17 to ES-23. We found the recommendations regarding medication use to be confusing. The guideline specifically mentions studies that included paroxetine and nortriptyline as interventions but then gives detailed descriptions of the reasons that second-generation medications other than paroxetine are preferred. In other sections of the table, sertraline and escitalopram are specified but the table does not explain why citalopram would not be a viable alternative. It is also not clear why other non-tricyclic medications (e.g., SNRIs, mirtazapine) are not mentioned for use in specific clinical circumstances. It would be helpful to have a much broader discussion of the available treatments, the evidence for their efficacy and effectiveness and their relative side effect profiles. In some contexts, specific "side effects" can be beneficial (e.g., increased

appetite and sedation with mirtazapine in an older patient with significant insomnia and prominent weight loss). We agree that, in older individuals and in adults more generally, tricyclic antidepressants are not first-line medications and that paroxetine is less preferable to other SSRIs due to its short half-life, tendency for associated withdrawal symptoms, and greater degree of anticholinergic effects. It would also be worth noting that fluoxetine can be problematic in older individuals due to its long-half life, presence of active metabolites and potential for drug-drug interactions that could further affect its metabolism. It should also be emphasized that there are some circumstances in older individuals (e.g., treatment resistant depression particularly after a successful course of ECT) in which nortriptyline is beneficial. Also, the ability to monitor nortriptyline serum levels is useful in assessing the adequacy of the medication dose and minimizing toxicity.

**Panel response**

*We concur with all of these points, but they are more appropriate for treatment guidelines focused primarily on psychopharmacology. Such a focus was not the mission or charge of the panel, nor are these nuances clinically useful to the probable greatest users of this guideline (i.e., primarily clinical psychologists and other disciplines that provide psychotherapy).*

ES-17. For the first statement under initial treatment, the justification section is confusing in referring to efficacy evidence for medications although other portions of the guideline note that efficacy evidence was not reviewed for medications. The justification also says that "sufficient evidence is found to recommend between some treatment comparisons," however, these comparisons are not discussed further.

**Panel response**

*We refer to efficacy evidence for medication within the specific context of trials examining medication versus psychotherapy or versus combined treatment. Our guidelines were never intended to focus primarily on psychopharmacology per se.*

ES-17 to ES-18. It is difficult to use the information from the conditional recommendations in making any treatment decisions because many of the treatment arms are not well-defined (e.g., supportive care, non-specific talk therapy, usual care) and because of the variety of interventions and comparison conditions that are listed.

**Panel response**

*We concur and note that comparator conditions need to be better defined in future research. We felt that conditional recommendations were appropriately conservative, given the imprecision with which control conditions were defined.*

ES-19. In the statement on subthreshold depression, the justification section notes that there is no efficacy data, yet the panel makes the limited suggestions that are noted. It is not clear whether there is comparative effectiveness data available or why the panel seems to be deviating from its protocol for making recommendations on the basis of the available evidence. Statements recommending the providers treat with "usual care" are not helpful as this treatment arm is not defined and could include a medication, another psychotherapy treatment, no treatment, or other interventions or combinations of interventions.



**Panel response**

*It has been difficult to show drug-placebo differences in patients with subsyndromal or subthreshold depression. There is a general consensus that behavioral or psychotherapeutic approaches can be helpful in reducing symptoms and in reducing the risk that patients with mild depression will develop major depression.*

ES-20. In the second statement on subthreshold depression treatment, it is unclear how subthreshold and subclinical depression differ, if at all, in the description of the treatment. In addition, the strength of recommendation is noted to be "Insufficient"; this wording should be updated to be consistent with the wording in other locations in the tables.

**Panel response**

*Thank you. We inserted a footnote defining Other Specified Depressive Disorder noting that the systematic reviews and studies that are discussed in this guideline use the broader definitions "subclinical" or "subsyndromal" depression. We have modified the strength of recommendations for consistency.*

ES-20. The first statement under MDD or minor depression + cognitive impairment/dementia uses the word "considering." This is problematic for the reasons discussed above and should be reworded.

**Panel response**

*Thank you for this comment. The panel has decided to retain the term "consider" for conditional recommendations because it captures the lower confidence in the recommendation.*

ES-20. There is no mention of medications in the section on MDD or minor depression + cognitive impairment/dementia. This seems like a major omission given the prevalence of depression and the frequent use of and need for antidepressant medications in this population. If medications were not included in the search of the literature on this topic, there should be a specific statement as to the reasons this was not included. There should also be a specific notation that medications may be indicated even though they were not in the scope of this particular review so that the reader does not erroneously assume that they should not be used. There is also no mention of the severity of the patient's dementia in these studies. This would be essential to note given the wide differences in patients' ability to have meaningful participation in psychotherapies depending on the magnitude of their cognitive impairments.

**Panel response**

*The weight of available evidence suggests that antidepressant medication is not superior to pill placebo in relieving depressive symptoms in the context of dementing illness. Behavioral management and supportive care appear to be critical in reducing depressive symptoms in dementia.*

ES-21. In the footnote, the definition of "executive dysfunction" is not helpful or specific and that it may be misleading. We suggest that this be revised or deleted.

**Panel response**

*"Executive dysfunction" covers a broad range of cognitive impairments, including response inhibition and reductions in cognitive flexibility. In the accompanying text we note that these may influence the strength of treatment response to antidepressant medication in older adults. It is important that readers of the manual be aware of the range of executive impairments and implications for treatment.*

ES-21. The statement on combined behavioral activation therapy plus treatment as usual needs to be rewritten in a way that notes that the panel had insufficient evidence to recommend this approach. In addition, the strength of recommendation is noted to be "Insufficient"; this wording should be updated to be consistent with the wording in other locations in the tables.

**Panel response**

*The panel had insufficient evidence to recommend the combination of behavioral activation therapy (individual) and treatment as usual over treatment as usual alone for depressive symptoms in older adults with mild to moderate cognitive impairment.*

ES-21. The statement on persistent depressive disorder should also be reworded to avoid using the word "consider." Also, the statement says that this conditional recommendation applies to individuals with "MDD or minor depression" but should be changed to persistent depressive disorder. It is not clear if the phrase "in the context of cognitive impairment or dementia" is due to a copy and paste error, or if the PDD section also applies to individuals with cognitive impairment. If the former, it should be deleted, if the latter, the heading should be updated. Additionally, we agree that other meds would be preferred to paroxetine, but we would not limit to these two as implied by the "i.e." as other SSRIs and other newer agents (e.g., SNRI, mirtazapine) may also be acceptable. Also, it's not clear why the panel is only including a precise specification of individual antidepressant in this statement although there is similar language in other statements (and per this guideline no specific evidence for these medications).

**Panel response**

*We concur with these comments but again would note that our intention and charge never extended to detailed pharmacotherapy recommendations.*

ES-22. The level of specificity of the comparisons listed under MDD with medical or other complications seems to be based solely on the unique designs of several studies. This information is not very helpful, particularly because there is no mention of the use of medications in these populations and their pluses or minuses implying that these treatments would be preferable. There is no description of the multi-component intervention and likely to be no access to such services, in any case.

**Panel response**

*We used the best data available to make recommendations for treatment of MDD in the context of co-occurring medical disorders. This is an area where much additional research is needed. We concur that multidisciplinary team-based approaches can be helpful in optimizing treatment outcomes for complex patients.*

ES-22. The section on prevention of recurrence does not define a "history of depression". Indications for using any treatment may be quite different depending on the recency of the history of depression and the number and frequency of prior depressive episodes. As with other

recommendations in this section of the guideline, the statements seem to be driven by specific studies rather than by a body of consistent evidence. Although we would agree that a combination of an evidence-based psychotherapy and pharmacotherapy may be appropriate in individuals with a recent late-life depression or a past history of frequency recurrences or severe depression (e.g., requiring hospitalization, associated with significant suicidal risk), this recommendation seems discrepant with than for adults more generally.

**Panel response**

*This comment addresses an area where clinical judgment and shared decision making are particularly important. Naturalistic studies of the course of depressive illness have shown that risk for recurrence increases with a history of two or more episodes and with shortening of inter-episode intervals. There is also an increase of the risk for treatment resistance with each succeeding episode.*

Recommendations - Table 2: General Adult Population ES-12. For a number of reasons, we are concerned that clinicians will have difficulty in interpreting these recommendations and applying them to individual patients

1. Footnote 8 notes that the recommendations apply to "the full range of depression diagnoses identified by the panel for inclusion unless a recommendation specifies otherwise." However, it is not clear, at least in looking at this table, what diagnoses were included and why the evidence was not assessed separately based on illness severity, chronicity, or other factors that may influence treatment.

**Panel response**

*The diagnosis of major depression covers a broad spectrum of severity and chronicity. Clinical depression does not occur in isolation but, rather, in differing psychosocial, medical, and sociocultural contexts. All of these need to be taken into account in treatment planning to optimize engagement in treatment and outcomes.*

2. The use of the term "second generation antidepressant" is potentially confusing. Antidepressants are more often referred to in groups based upon their presumed pharmacological mechanism (e.g., selective serotonin reuptake inhibitor, serotonin norepinephrine reuptake inhibitor, tricyclic antidepressant) rather than referring to first or second generation agents. Even within these categories, evidence for the efficacy and effectiveness of some medications is quite different than for others (e.g., evidence for the SSRI fluvoxamine is very limited for treatment of depression as compared to evidence for other SSRIs) and the side effects and tolerability of specific medications can be quite different, even within the same class of medications. Thus, we would suggest listing the medications by name rather than referring to them as second generation antidepressants.

**Panel response**

*The general point here is correct, namely, that "second generation antidepressants" like SSRI's and SNRI's vary considerably with respect particularly to tolerability. The level of granularity recommended by this comment is beyond the scope of our guideline, which were never intended to be a psychopharmacology manual.*

3. As discussed above under general comments, the literature included in the guideline does not include information on the efficacy of antidepressants in adults, which constitutes a large part of the available evidence base. Consequently, the only information available on these

medications is from the included comparative effectiveness studies. These studies often state treatment as usual, which frequently includes medication, but the studies don't list which medications. This makes it difficult to infer what the treatment should be. Some providers will be very familiar with antidepressant medications, including their side effect profiles, and therefore will be able to engage with the patients on all treatment options, and particularly if a patient prefers medication. However, other providers are less familiar with the pharmacotherapy option and treatment as usual provides no information or direction which antidepressants or other medication are available and which to try.

**Panel response**

*Our charge was to focus on comparative effectiveness studies and not specifically on the efficacy of antidepressants in adults.*

4. It is not clear how the initial statement (designated by the numeral 1) differs in principle from the second statement. It seems that the two statements could be combined (e.g., "For initial treatment of adult patients with depression, the panel recommends either a second generation antidepressant (i.e., list of specific antidepressants) or psychotherapy. General models of psychotherapy that appear to have comparable effects include "list". The panel is unable to recommend specific monotherapies for initial treatment because effectiveness studies demonstrated similar effects across psychotherapies. The panel recommends basing treatment choice on shared decision-making with the patient.")

ES-13. There is no information given in the justification on using combination therapy. It is not clear whether it is even being recommended for use, possibly because of how the table breaks across pages, but also because the recommendation is qualified by saying "if considering combined treatment." No information is provided on when or why combined treatment should be considered. (As noted, previously most guideline developers try to avoid recommendations that includes words such as "consider" or "considering".) Other guidelines recommend initial use of combined treatment for moderate to severe depression or recommend combined treatment after an initial lack of response to monotherapy, but these guideline statements do not provide any breakdown of the treatment recommendations by depression severity, chronicity, or degree of treatment resistance.

**Panel response**

*The available data support the need for shared decision-making both depression-specific psychotherapies and antidepressant medication may be effective in the initial treatment of major depression. The use of combined treatment may indeed become appropriate, in the absence of a satisfactory response to monotherapy. It seems reasonable to take a stepped approach to treatment, driven by patient preference and response variability.*

ES-13. In the statements that have a conditional recommendation for use, a number of issues would benefit from clarification. Given the broad range of psychotherapies listed on p. ES-12 and the large number of potential antidepressants that could be tried, it seems unlikely that all recommended treatments would be unacceptable and unavailable (or that problem-focused couples therapy would be more acceptable and/or available). The implication is that this statement would apply to individuals with difficulties in intimate relationships who would be treated in a "couples" format. However, individuals can have relationship distress in other contexts (e.g., parent and adult child who lives at home). It would be helpful to specify the types of relationships that would be included in this guideline statement.

**Panel response**

*This level of granularity is not supported by the available evidence. Clearly, interpersonal and role conflict provide important contextual factors for depression and need to be taken into account in treatment planning.*

ES-13. It is not clear whether the next statement that relates to selecting between treatments is also applicable to individuals with relationship distress or whether it refers to depressed patients more broadly. (If the latter, the order of the statements should be switched; otherwise, the language should probably be revised. This statement also uses the word "considering" and in the second bullet "suggests cognitive therapy plus antidepressant medication to improve likelihood of full recovery in treatment." It is not clear why this qualifier is included here and not in any other statement, when it would likely be a goal of most people to experience a full recovery.

**Panel response**

*Personalization or tailoring of depression treatment needs to take into account the interpersonal and social context in which the illness occurs. This may lead to a dyadic or family-based intervention in some instances to deal with issues such as role conflict or inadequate communication.*

ES-13. In the justification of treatments that are given a conditional recommendation for use, the listed interventions are noted to have "demonstrated efficacy when compared with no treatment (i.e., waitlist) or control." However, this approach is biased against pharmacotherapies as efficacy studies of pharmacotherapies are not included in the review. It is also not clear how a waitlist or control comparison would have led to a suggestion to use behavioral therapy over antidepressant medication alone.

Complementary and Alternative Treatments. There isn't any discussion in the process or methodology sections of the document that explains the reasons that a treatment would be downgraded if it only has efficacy data. It's also unclear why this would be done as it is not a standard procedure in other guideline developers' processes. Typically, studies of treatments such as exercise or St. John's Wort would only include individuals with mild or perhaps moderate depression. Given the lack of stratification of recommendations by illness severity, the reader is left to assume that these treatments would be conditionally recommended regardless of severity, which seems problematic. Additionally, if St. John's Wort is not any different than antidepressants (based on comparative effectiveness studies), it's not clear why it wasn't considered a first line treatment. On the other hand, most St. John's Wort studies are problematic in terms of dose comparisons (with inadequate antidepressant doses in most trials), limited standardization of commercially available St. John's Wort preparations (in terms of content and ratios of active ingredients), and significant potentially harmful drug-drug interactions (e.g., with oral contraceptives, HIV medications, antirejection medications post-transplant). Thus, the complexities of suggesting St. John's Wort to a patient should be fully understood in light of the available evidence and this should be incorporated into the justification.

**Panel response**

*Our charge did not encompass a review of efficacy studies of pharmacotherapy. We concur with the description of study limitations addressing complementary and alternative interventions. Clinicians should be aware of these limitations and address them in shared decision-making with patients.*

ES-14. The considerations for the conditionally recommended treatments should also specify the type and severity of depression that was studied in the included trials. For example, if bright light therapy used samples in which depression was associated with a seasonal pattern, this would be important to specify. Similarly, if depression severity was limited in studies of yoga (e.g., to mild or mild-to-moderate depression), this would be important to note.

**Panel response**

*We concur with this comment. We believe that the majority of the evidence supporting the use of bright light is directed at seasonal affective disorders; and that studies of yoga have empaneled primarily people with milder depression. Again, the heterogeneity of the clinical phenotypes encompassed under the rubric of “major depression” presents a challenge for formulating precise treatment recommendations.*

ES-14. The conditional recommendation related to acupuncture suggests adding acupuncture to antidepressant treatment, however the guideline does not seem to consider other adjunctive treatments (e.g., antipsychotic medication added to antidepressants, addition of lithium to antidepressants). The relative benefit of adding acupuncture to antidepressants is also unclear as compared to antidepressants alone making it difficult to engage in shared decision making with patients related to issues such as treatment cost and convenience.

**Panel response**

*We concur with this comment, but it goes beyond our charge with respect to the use of adjunctive medications like atypical antipsychotics or lithium.*

ES-15. This section is problematic in conflating partial response with non-response and the issue of treatment-resistant depression is only addressed later in the discussion section of the guideline. In addition, there is not a parallel section of recommendations for partial or non-response to initial psychotherapy. It is also unclear whether the determination of partial response is based on any assessment of treatment adequacy (e.g., dose, frequency, adherence). The justification implies that the statements were developed based on a lack of difference in effect between the listed interventions; however, it is not clear whether the study population included only depressed patients who were initially treated with an antidepressant or whether the studies included depressed patients regardless of prior treatment or response. Obviously, the latter scenario would raise questions about the validity of the guideline statements.

**Panel response**

*We respectfully disagree. We note that there is a continuum of response variability in the treatment of depression. The important point is to help patients get well (i.e., achieve symptomatic remission and functional recovery) and to stay well. Optimal management of treatment resistance is an active and ongoing focus of research, examining such strategies as medication switching versus augmentation. This is well beyond the purview of our charge.*

ES-15. In the conditional recommendation, the list of psychotherapies includes cognitive behavioral therapy whereas in the options for switching treatments (under recommendations) cognitive therapy alone is recommended. Given the frequent combination of cognitive and behavioral strategies in clinical practice, it would be helpful to know whether the apparent distinction between cognitive therapy and cognitive behavioral therapy is intended. (If cognitive

therapy alone is recommended, it is unclear why cognitive therapy added to an antidepressant would not be part of the conditional recommendation along with cognitive behavioral therapy.) In the justification, there is a statement against adding CBASP or brief supportive therapy to an antidepressant medication. This seems like a statement against that should have been included as a full statement with a recommendation or conditional recommendation against use. The footnote implies that CBASP is not recommended due to additional information that suggests increased burden without corresponding benefit. However, CBASP was developed and primarily used in treatment of individuals with chronic depression for whom other treatments may be less likely to produce a response, thereby justifying some increase in burden. Furthermore, the nature of the additional information and the magnitude and cause of the increased burden are unclear. It is also unclear why issues of burden are considered here but do not seem to have been incorporated into other aspects of the guideline. (Some patients view psychotherapy as more burdensome than medications, particularly if their ability to make appointments is limited by factors such as work schedules, childcare, or transportation, yet these considerations do not seem to have been incorporated into the ratings of strength of recommendations.) ES-15. The third statement on the page (which is continued on p. ES-16) refers to patients with major depressive disorder (rather than depression) and specifies patients who have "not responded or only partially responded to initial adequate second-generation antidepressant." This statement is more specific than the preceding statements in mentioning treatment adequacy and specifying the presence of a diagnosis of MDD. It would be preferable to specify the antidepressants fully, either in the guideline statement or in the footnote, rather than making the reader go to Table E22 for medications other than bupropion, sertraline, and venlafaxine.

**Panel response**

*The panel members were frankly surprised to come to this conclusion, based on the number of well-known studies. In reviewing the evidence that was included in the guideline development process, unfortunately several of the studies cited by the commenters above ended up excluded because they did not meet the minimum threshold necessary (i.e., they were rated as very low quality or insufficient quality based on the review process).*

ES-15. The statement on relapse prevention suggests that patients who have achieved a depressive remission be offered one of three types of psychotherapy for relapse prevention rather than receiving antidepressants or treatment as usual. This statement is confusing and worrisome for a number of reasons. First, it implies that a patient who has just achieved remission should have antidepressant medications stopped with a change to one of these therapies. Also, treatment as usual is ill-defined and non-specific, but often includes medication as well. Given the risks of abrupt medication discontinuation for prompting relapse and the risks of medication discontinuation for individuals with severe and/or frequently recurring episodes of depression, this statement would benefit from revision or clarification. The inclusion of mindfulness-based cognitive therapy is also curious as this treatment modality does not seem to have been mentioned as an option for acute treatment or treatment of individuals with non-response or partial-response. Thus, it is unclear whether there is something unique about mindfulness-based cognitive therapy in relapse prevention or whether this is just a reflection of the available evidence.

**Panel response**

*The literature on prevention of relapse and recurrence of depression shows the efficacy of depression-specific psychotherapies and of antidepressant medication. There is room for shared decision making about what type of continuation and maintenance treatment is appropriate. It was never our intention to suggest discontinuation of medication, especially abrupt discontinuation. If patients have needed medication to get well, the data suggest they need it to stay well. At the same time, it is clear that learning based approaches like CBT do have staying power and are effective in relapse and recurrence prevention.*

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**12.****Commenter: Karl Stukenberg**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Conclusion - I am dismayed that my profession has lost track of it's essential mission - at least as I understand it - to understand people. There is a long history of empirical support for the qualities of the treatment relationship to have a much stronger relationship to the outcome of a treatment than the "brand" of treatment that we provide. RCTs dictate certain types of treatments - and these treatments then are considered to have been proven effective and others not so. People do not come to us for treatment of depression - they come to us because they need help. We diagnose them with depression - for our purposes - not for theirs. We need to continue to assess whether we have helped the people that we purport to help in ways that are consistent with their needs - not with artificial labels that we apply to improve our ability to create a world that we can exert artificial control over. Our patients do not live in that world - where whether they meet criteria for a diagnosis matters - what matters is whether they are able to work and to live - and for how long they can do that after they have completed treatment. The paradigm that is presented in this guideline for research is far too narrow to encompass and understanding of what actually transpires in psychotherapy that leads to change - and ironically the narrowing of this focus is not likely to lead to pinpointing the solution to the problem, but focusing light in a part of the treatment that is least likely to effect the outcome - the technical interventions of the therapist. This, in turn, has the potential to mislead the public and the third party payers to insist on less effective treatments that are tested using false premises - an outcome that will take decades or longer to recover from. Psychotherapy - at its best - is not a technical undertaking but an organic and human one - an undertaking that should be studied carefully, as this document proposes - but not with the constraints that this document includes. We need to know what helps people recover - but the path laid out here is not consistent with decades of research into psychotherapy and its effect.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*



**13.****Commenter: Emily Laumeier, on behalf of the Committee on Division/APA Relations**

Comment type: Group Comments

Group name: CODAPAR Committee on Division/APA Relations

Do you have any other comments about the draft guideline document?

General Comment - At its October 2018 meeting, the Committee on Division/APA Relations (CODAPAR) reviewed the draft guidelines. CODAPAR noted with appreciation that the movers have sought input from APA Divisions, including by distributing a Call for Comments to APA Division Presidential Trios using the divtrio@lists.apa.org listserv. CODAPAR does not have substantive comment at this time but does wish to commend the group's efforts to seek involvement from APA Divisions.

**Panel response***Thank you.***14.****Commenter: Alan Nathan**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I endorse the comments made by Nancie Senet, Jonathon Shedler, Jeffrey Axelbank, and Jack Novack and others. I am deeply concerned that these guidelines advertise that particular short-term, technical interventions, and medication as the treatments of choice. All the while the guidelines themselves stipulate the lack of evidence that one form of treatment is superior to another. The common factors research seems to be barely considered. And the limitations of RCTs while mentioned seem to not be taken seriously at all. I am greatly concerned that APA is presenting a false message to the public and to psychotherapists.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Axelbank (Commenter 22), Senet (Commenter 73), Shedler (Commenter 51) and Soldz (Commenter 10). The panel has attempted to address the topics of the common factors model and the limitations of RCTs while also recognizing the importance of RCTs for determining efficacy and effectiveness of treatments being considered.*

I am also concerned by the lack of representation of PsyD clinicians and psychoanalysts on the Clinical Practice Guideline Development Panel. There is an implied message that psychoanalytic treatments are second tier if that which ignores evidence that these treatments have long-term effectiveness (e.g., work of Jonathon Shedler and many others including research coming out of Europe).

**Panel response**

*The panel was composed of diverse clinicians, researchers and community members but members were not selected based on representation of any particular degree or theoretical orientation. Efforts were made to ensure that the major theoretical orientations, including psychodynamic if not psychoanalytic, were represented among panel members. It is correct that there is some evidence for the long-term effectiveness of both CBT and Psychodynamic Psychotherapy. In any case, there is no systematic review of this issue that we could have used for our guideline.*

I'm further concerned that conflict of interest is defined as conflict between interests of covered individual and APA and that the policy is designed to promote transparency, integrity of the initiative, protect APA and covered individual. This does not go far enough in addressing conflicts of interest that impact the general public. I am deeply concerned by the influence of the pharmaceutical industry and insurance companies in the push to endorse the cheapest treatments possible. It is hypocritical to state that there is a danger of bias against these corporate interests (see p. 95 of the guidelines). There is a deep problem of too much power in the hand of too few individuals in our culture and to say that there is bias against corporate interests is disingenuous at best.

**Panel response**

*Panel members were asked to disclose all potential COI and indeed, the panel is aware of the potential for corporate interests to influence decision making. COI was discussed regularly and reviewed, and strategies utilized to manage potential COI. Please see panel disclosure summaries for more details.*

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**15.****Commenter: Kyle Shultz**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am opposed to these guidelines. I would like to support the comments previously made by Jonathan Shedler, Nanci Senet, and Jeffrey Axelbank.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Axelbank (Commenter 22), Shedler (Commenter 51), Senet (Commenter 73), and Soldz (Commenter 10).*

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**16.****Commenter: Tamara Sofair-Fisch**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am opposed to these guidelines.

I would like to support the comments previously made by Jonathan Schedler, Nanci Senet, and Jeff Axelbank. I feel these guidelines misrepresent the options available which have clear clinical utility as well as overlook the issues related to the client's wholistic needs beyond possible short term treatment gains.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Axelbank (Commenter 22), Shedler (Commenter 51), Senet (Commenter 73), and Soldz (Commenter 10).*

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**17.**

**Commenter: Patricia A. McKenna, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am deeply dissatisfied with these “guidelines” for several reasons. The guidelines are clearly based only on a very limited type of research: the randomized controlled trial. RCTs famously fail to represent real-world conditions, both in terms of the samples studied as well as the treatment context.

Using RCTs as the only basis for evidence for these guidelines means that ipso facto certain types of treatment like psychodynamic or humanistic psychotherapy treatments will never be considered “evidence-based” treatments. By effectively barring psychodynamic and related treatments from a guideline for the treatment of depression, APA is leading consumers, practitioners, insurers and policy makers to believe that psychodynamic therapies are invalid.

Psychodynamic and humanistic psychotherapies are treatments that emphasize the complicated psychological endeavor that is real-world psychotherapy rather than the reductionistic sliver of intervention that RCTs can investigate on a narrow subset of people chosen because they represent the cleanest, simplest form of a diagnosis.

Thus, APA has not created a guideline for the treatment of real people. And yet, who will be consuming these guidelines? Practitioners who work with real people, insurers who insure real people, and consumers who are interested in what psychotherapy can offer them. Most of these guideline readers will not have the knowledge base to understand how truly limited these findings are.

If APA does not yet have the evidence to present guidelines for real-world treatment, it should not present guidelines for treatment -- the cost to consumers, practitioners, and society of misleading guidelines is great.

**Panel response**

*We appreciate the comment that RCT's apply to restricted populations and act only as suggestive evidence of real-world treatments and only apply to outcome measures used which are symptom focused and may miss other important more complicated psychological changes. There however is no evidence of more complex treatments of more complex populations with more complex outcome criteria. This is greatly needed, and we call for more research to address all these points (see Stirman et al., 2005).*

**Reference**

Stirman, S. W., DeRubeis, R. J., Crits-Christoph, P., & Rothman, A. (2005). Can the randomized controlled trial literature generalize to nonrandomized patients? *Journal of Consulting and Clinical Psychology*, 73(1), 127-135.  
<http://dx.doi.org/10.1037/0022-006X.73.1.127>

**18.**

**Commenter: Steve Hollon, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I have had a chance to look over the depression guideline and I like it a lot. I think the Depression Guideline Panel (DGP) largely got it right (meaning that I agree with most of what they recommended) but that it was beautifully written. That being said I would encourage them to consider:

**Panel response**

*Thank you.*

1) I would let the evidence for an enduring effect for cognitive therapy and possibly behavioral activation (and maybe even dynamic psychotherapy) influence the recommendation for when to choose psychotherapy vis-à-vis medications. If I were choosing between two equally efficacious treatments I would prefer something that cuts subsequent risk in half over something that works for only so long as I take it. The guideline does discuss the issue very nicely in the text (citing Cuijpers 2013) but nothing is reflected (or even mentioned) in the recommendations. This is perhaps the major advantage that psychotherapy has over medications and one of the major reasons the original Advisory Steering Committee (ASC) selected depression as a target for a guideline in the first place. This seems like an opportunity lost to educate third party payers and the public. It is worth noting that in the United Kingdom the National Health Service has invested over 700 million pounds to train and hire therapists to deliver those psychotherapies deemed to be efficacious by the NICE guidelines precisely because they are more cost-efficient than relying on medications in the long-run.

2) Make more (I am not sure that it was even mentioned) of indications that both antidepressant medication (ADM) and psychotherapy only separate from pill-placebo and other nonspecific controls (in the case of psychotherapy) among patients who are more severely depressed (see Fournier 2010 for ADM and Driessen 2010 for psychotherapy that cites classic studies like the NIMH TDCRP by Elkin 1989/1995 or the Seattle trial by Dimidjian 2006). What that means is that the majority of the patients who get better on medications are not responding to the active medication but instead are responding to the belief that they are on medication. There is an asymmetry between medication versus psychotherapy and that is that the former must show a

specific effect in order to be brought to market whereas it is perfectly acceptable for psychotherapy to work through largely nonspecific processes as many people believe (we do pay for the purchase of friendship but what a friend). Again, if I were choosing between different interventions I would be more likely to prefer something that does not require a specific mechanism beyond a good working relationship (but that often provides something more specific) over something that does require a specific mechanism to justify its use but has no specific effect for the majority of patients that it benefits.

#### **Panel response to 1 and 2**

*Thank you for the constructive comments and feedback on the guideline. The panel is constrained in that the literature included and reviewed using the IOM guidelines did not directly address these issues. The panel has attempted to note the importance of considering long term benefits and acknowledge this limitation, while staying consistent with the process that was used to develop this guideline. We do want to let you know that the issue of baseline severity and outcome is not as clear-cut as you suggest for either pharmacotherapy (compare the recent IPD meta-analysis of Furukawa et al., 2017, 2018).*

#### **References**

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3) I was bemused by how the reviews that the panel relied on differentiated between cognitive therapy versus cognitive behavioral therapy. For example, in his classic 2013 meta-analysis, Cuijpers referred to the studies he reviewed as CBT when in fact they all used Beck's version of cognitive therapy as their specific instantiation of CBT (it is one of several different variants within that larger domain). In effect, if we do draw a distinction between cognitive therapy and CBT (as the guideline tries to do) than any enduring effect belongs to cognitive therapy and not the broader domain of CBT. Given that the DGP was forced to work from other authors' reviews and did not have the benefit of categorizing the treatments used themselves there is much room for slippage, especially since the folks who are most likely to do quantitative reviews are not likely to be all that familiar with the subtle differences among the types of treatments they are describing.

#### **Panel response**

*The panel appreciates the recognition of the challenge in dealing with reviews that inconsistently differentiated between the CT and CBT. The panel has tried to clarify this challenge and acknowledge the potential lack of clarity to which this can contribute.*

4) I would hope that the final guideline would have descriptions of the major treatments covered and a brief justification for the recommendations that were made (or not). I do think that the quality of the evidence for some interventions is stronger than it is for others despite the fact that they essentially come out looking the same in meta-analyses. For example, IPT survived a very stiff comparison to ADM in a placebo-controlled trial (Elkin 1989/1995) whereas dynamic psychotherapy has never beaten anything but its absence and “earns its spurs” largely on the basis of non-inferiority comparisons to other active interventions. The FDA insists on “assay sensitivity”; it is not sufficient to do no worse than another established medication but instead it must do better than pill-placebo (and no less well than an active comparator if one is included) in at least two trials before it can be taken to market. The quality of the evidence supporting the efficacy of IPT is considerably stronger than the quality of the evidence supporting dynamic psychotherapy since the former has survived tougher comparisons even though the two look similar in meta-analyses and both are recommended.

**Panel response**

*This was a concern that was heavily debated among the panel. In particular, a number of treatments that did not have strong efficacy data were also not differentiated from other interventions that in terms of effectiveness. In determining how to guide readers in decision-making, the panel chose the current approach recognizing that while there was some variability in the strength of evidence for some interventions, the lack of the ability to differentiate between treatments based on the outcomes reviewed, considered in the context of the recommendation to incorporate patient preference and provider expertise in the decision-making process, led the panel to recommendations as stated. The panel hopes psychologists will embrace the recommendation to pursue research that will better address these issues.*

Those few concerns notwithstanding this was an absolutely terrific job. I still think we (I was chair of the ASC when the depression guideline was launched) made a mistake by not making common cause with NICE and updating their systematic review in return for access to the 40 years of extractions that they had already done (I still do not understand why APA chose not to do so) and I would strongly recommend that the APA base all future guidelines on actual systematic reviews. I very much hope that the guideline will present the forest plots that they worked from in the Appendices; it will important that the field can see the evidence base that underlies the recommendations. I also would like to see brief descriptions of the respective types of treatments added if they are not already in the guideline along with brief summaries of what the available evidence shows with respect to both short and long-term efficacy (enduring effects in particular) and acceptability and harms if not already in the draft. Regardless, the DPG did a phenomenal job of pulling this together despite (perhaps) having one hand tied behind their backs.

**Panel response**

*A table listing brief descriptions of different types of treatment by age group is included in the appendix. We are adding a more prominent note about this to the main guideline document. Brief justifications for recommendations made are included in the justification column of each recommendations table with additional details for each available in the decision tables and grids. We are making this clearer by adding the grid or decision table the interested reader can reference for further details for each recommendation. The decision tables and grids also contain information about harms/burdens and patient values and preferences. The data included in the systematic reviews did not allow the panel to make recommendations about enduring effects. We are adding in references to the original reviews for the interested reader to view forest plots.*

*Thank you for the compliment. The guideline panel has also learned a lot during the process of the development of the guideline and we would certainly recommend basing future clinical practice guidelines on systematic reviews.*

I have a number of smaller comments scattered throughout the guideline in the form of comments on highlighted sentences and sections (see sections below).

Executive Summary - ES-1 line 19: It would be quite disappointing if the guideline did not at least consider the enduring effects of at least some types of psychotherapy (cognitive therapy for sure and behavioral activation maybe) since that is a major advantage that those types of treatments have over medications since the latter have no known enduring effects (Cuijpers 2013). That combined with the likelihood that medications work for nonspecific reasons for the majority of patients with MDD (but do produce side effects) is something that patients ought to know when choosing what kind of treatment.

**Panel response**

*Addressed above in response to items 1 and 2.*

ES-7 line 2: We do need more but there is already fairly robust evidence that cognitive therapy has an enduring effect that reduces risk by more than half (Cuijpers 2013) and that the same may be true for behavioral activation (Dobson 2008) and possibly dynamic therapy (Fonagy 2015).

**Panel response**

*Addressed above in response to items 1 and 2.*

ES-7 line 21: What about specificity? It is one thing to compare an intervention to its absence and quite another to compare it to some form of pill-placebo or generic.

**Panel response**

*Specificity has been incorporated into the determination of strength of evidence, with better treatment designs being rated as having stronger evidence.*

Executive Summary - ES-12 Recommendations: I have had a long-running debate with Pim about whether the strength of the evidence is really as strong for dynamic psychotherapy as it is for IPT. So far as I know, dynamic psychotherapy has never beaten anything and the bulk of its support comes from the fact that it has not done worse than other approaches like CBT in studies that are not all that expert in the latter. I do consider Barber 2011 a stiff test against

medications but in that trial neither exceeded pill-placebo. IPT was as efficacious as ADM in a trial in which both were superior to PLA (Elkin 1989) and the same was true for CBT in a trial with patients with more severe depressions (DeRubeis 2005) and another with atypical depression (Jarrett 1999), as well as BA in yet another trial (Dimidjian 2006). Does the rigor of the comparisons count for nothing? I can understand listing all as recommended (since their respective effect sizes are comparable) but IPT (and CBT and BA) have earned their spurs in rigorous comparisons in which they tied medications and beat nonspecific controls whereas DYN has never done better than tie its comparators in designs that either did not find or did not include nonspecific controls. Is there no consideration for “strength of evidence”? Popper would not be pleased. This point is even more true for supportive therapy than it is for DYN.

**Panel response**

*Strength of evidence and quality of comparisons were included in the panel's reviews of the literature. However, the panel found that the consistent failures to meaningfully differentiate treatments meant that the recommendations, as written, were the best representation of the current knowledge, based on the literature reviewed.*

ES-13 Recommendations: I am curious why behavior therapy is privileged over medications but not cognitive therapy. Is it because the former was found to have an enduring effect in the one trial in which this was examined (Dobson 2008) and if so what do you make of the rather robust enduring effect for cognitive therapy (6-7 of 8) in Cuijpers 2013. Some justification here is warranted.

**Panel response**

*For this particular comparison, the only literature that met inclusion criteria focused on behavior therapy.*

ES-15 Recommendations: Probably nothing that you want to deal with in the guideline but our latest study suggests that the advantage for combined treatment over ADM alone may be restricted to those patients nonchronic patients who are more severe (about a third of all MDD) whereas patients who are less severe but not chronic do not need to have CBT added to ADM and patients who are chronic do not benefit from the addition (Hollon 2014 JAMA Psychiatry). There also is reason to be concerned that adding ADM during acute treatment may interfere with CBT's enduring effect.

**Panel response**

*While the panel cannot reopen the guideline development process, it strongly recommends that future efforts incorporate such information.*

ES-16 Recommendations: Given the rather robust evidence that cognitive therapy has an enduring effect (and maybe behavioral activation too) it would appear to mention merit here or back when recommendations are made regarding acute phase therapy. Given that cognitive therapy works as well as medications in terms of producing acute response I would want to choose an intervention that cuts subsequent risk for relapse by more than half (Cuijpers 2013).

**Panel response**

*Addressed above in response to items 1 and 2.*

ES-17 Recommendations: I would mention individual CBT in particular since it is unlikely that group CBT would work and individual CBT would not. Absence of evidence is not evidence of



absence but it looks a little “goofy” to recommend group CBT but not individual CBT but if it has not been tested then that should be stated. That being said is it really the case that Gallagher and Thompson did all their trials with groups?

**Panel response**

*The guideline panel has modified the older adult recommendations to better address individual treatment. However, the literature reviewed was specific to group.*

Introduction - Page 6 line 3: Folks that I talk with say that you can do CBT with adolescents but you are largely stuck with behavior therapy with pre-adolescents. Is that reflected in the empirical literature or is it unclear?

Page 17 line 10: As above, people that I talk with say that you can do CBT with adolescents but you are largely stuck with behavior therapy with pre-adolescents. Is that reflected in the empirical literature?

**Panel response to Introduction and Pg. 17**

*The guideline has been modified to clarify differences between children and adolescents*

Page 19 line 25: I think Cuijpers 2012 this makes the case that nonspecific processes play a causal role in treatment response since it was based on a decomposition of the variance between conditions in RCTs. The process literature, as robust as it may be, cannot support a causal inference. This is very important and supports that even generic treatment is causally efficacious and especially for less severe depressions (Cuijpers 2013 focused on studies that included supportive therapy and those trials often exclude more severely depressed patients).

**Panel response**

*This statement has been rewritten to be less definitive in suggesting a causal factor for nonspecific factors.*

Page 21 line 15: The key question is whether a treatment works better than its absence (efficacy) and the flip side of that is does it cause harm and that is nicely covered, but if it were me I would separate efficacy from specificity (is treatment more efficacious than the generic nonspecifics) as the ASC recommended in their overview article in the Annual Review of Clinical Psychology (Hollon 2014). This is a standard that antidepressants have to clear to be brought to market and one that several of the psychotherapies have cleared in at least some trials (IPT in Elkin 1989; CBT in DeRubeis 2005 and Jarrett 1999; BA in Dimidjian 2006).

**Panel response**

*Thank you for the recommendation. In considering this, the panel agrees in concept with the importance of differentiating these concepts. Having defined the questions prior to the ASC's publication, they do not quite align with the structure provided. However, the section has been modified to better describe the comparisons included in the key questions.*

Page 23 line 1: I think that this is a mistake since at least some types of psychotherapies have enduring effects that medications clearly do not have. That is why the NHS is investing 700 million pounds to train and deploy people trained to do the empirically supported psychotherapies. The public should be made aware of this major difference.

**Panel response**

*See response to items 1 and 2.*

Page 26 line 16: The major advantage that at least some of the psychotherapies have over medications is that they have enduring effects. If is a major shortcoming of the guideline not to point that out. If you had to choose between two treatments for cancer that would both be equally efficacious in the short-run but that differed in terms of long-term risk which would you choose? The choice that depressed patients have to make is not so stark (most do not die) but the nature of the differential outcomes is much the same.

**Panel response**

*See response to items 1 and 2.*

Page 29 line 8: I think this is one of the few things that you got wrong but it is perhaps the biggest difference in the treatments.

**Panel response**

*See response to items 1 and 2.*

Page 36 line 5: Efficacy is good but comparative effectiveness is a little “clunky”. I would go with superiority.

**Panel response**

*The panel will review the choice of terminology.*

Page 36 line 11: I would use the term “nonspecific” rather than “active” control since the latter is too easy to confuse with a fully realized alternative treatment.

**Panel response**

*The panel will review the choice of terminology.*

Page 38 line 9: Great idea. I hope you show your grid in the appendix.

**Panel response**

*The grid is included in the supplementary materials with the guideline document and appendices.*

Page 38 line 16: I wholly agree with that decision.

**Panel response**

*Thank you for the support.*

Page 41 line 11: I hope that you strongly recommend that future RCTs do so.

**Panel response**

*The panel has attempted to do so.*

Page 45 line 2: Did the individual studies not do that or was it only the reviews that lumped them together?

**Panel response**

*There was a combination of overlap within studies as well as reviews. However, that panel has attempted to separate child and adolescent data into two separate tables to address this concern.*

Page 47 line 12: The effects are similar with respect to acute response but quite dissimilar when it comes to prevention of relapse and perhaps recurrence. I would privilege cognitive therapy over medications simply because the former reduces subsequent risk.

**Panel response**

*Addressed above in response to items 1 and 2.*

Page 47 line 19: The advantage for combined treatment may be heavily moderated (only shown by non-chronic patients who are more severe) and adding ADM may interfere with CBT's enduring effect (Hollon 2014 JAMA Psychiatry).

**Panel response**

*Addressed above in response to items 1 and 2.*

Page 48 line 18: The efficacy for dynamic psychotherapy is largely based on non-inferiority trials whereas CBT and BA and IPT based on comparisons to placebo-controlled ADM trials. Barber 2011 was a superbly designed trial with a placebo-controlled comparison to medication but neither dynamic psychotherapy nor medication separated from pill-placebo in that trial. Non-inferiority trials depend on the inference that known comparators were as efficacious as they were in the trials in which they were established but the two most recent such trials were conducted by groups with no prior history in conducting CBT (Driessen 2014; Connolly 2017). The FDA does not accept non-inferiority trials to establish the specificity of medications but for some reason we seem to be willing to use such trials to accept the specificity of psychotherapies. This is very "weak tea" methodologically.

**Panel response**

*Addressed above in response to item 4.*

Page 48 line 23: The guideline could cite DeRubeis 2014 and Cohen & DeRubeis 2018 on the development of treatment selection algorithms (especially those that use machine learning to combine indices). This is a major advance over what we were able to do even a decade ago and offers the possibility that we can actually select the optimal treatment for a given patient. In DeRubeis 2014 about a quarter of the patients would have done better on ADM than CBT and another quarter of the patients would have done better on CBT than on ADM; those patients who got their optimal treatment by chance did better than those who did not by a magnitude as large as the drug-placebo difference. This suggests that we can use such treatment selection algorithms to improve the efficiency of treatment by as much as specific treatments beat non-specific controls even without improving the efficacy of the specific treatments.

**Panel response**

*The citations have been added as suggested. Thank you for this recommendation. We would like to add, however, that the treatment selection studies you refer to are post-hoc analyses of trials, and as far as we know no RCT has shown that this method of treatment selection results in better outcomes. Therefore, although the results are important and interesting, they cannot be included in recommendations now.*

Page 49 line 6: Combined treatment increments may be heavily moderated and medications may interfere with CBT's enduring effect (Hollon 2014 JAMA Psychiatry).

**Panel response**

*Thank you for this comment. This has been noted in the paragraph.*

Page 50 line 11: The enduring effect of CBT relative to ADM is not limited to second generation medications and has been a constant since the earliest trials. That being said how did they differentiate CBT from cognitive therapy since all of the trials cited in Cuijpers 2013 used Beck's cognitive therapy as their instantiation of CBT?

**Panel response**

*The section has been modified to refer to antidepressants generally.*

Page 51 line 1: This discussion is very nicely done by the panel and fully descriptive. Should this be mentioned in the recommendations? If CBT is as efficacious as ADM with respect to acute response but has an enduring effect not found for medications (and an absence of side effects) is that not something that patients ought to know when they first decide on treatment? The fact that CBT (actually cognitive therapy) has a well documented enduring effect that BA may have as well (and possibly even dynamic psychotherapy Fonagy 2015 World Psychiatry) is something that the guideline should trumpet. Medications as helpful as they can be only work for as long as they are taken and the trend among prescribing clinicians is to keep patients with chronic or recurrent depressions on medications for the rest of their lives. The fact that at least some (and perhaps all) psychotherapies have an enduring effect and that medications clearly do not is the major advantage that psychotherapy has over medications. This is big and deserves to be considered in the recommendations.

**Panel response**

*The panel has discussed in depth how to address this issue. Given the commitment of the panel to the IOM process and the fact the reviews included in the process did not consistently review the long-term outcome literature, it would be misleading to comment on this in the recommendations themselves. This paragraph was included to provide readers with this additional information while maintaining the integrity of the guideline development process.*

Page 52 line 4: It might be worth noting (if it is the case) that it is the absence of trials (and not failure in actual trials) that leads to the lack of recommendation for CBT in this instance. Given that it generally has a therapeutic effect in both acute and continuation trials there is a likely extrapolation to be made in the absence of evidence to the contrary.

**Panel response**

*This conclusion was based on a limited number of trials that did not find benefits of group plus medication over medication alone for preventing recurrence.*

Page 59 line 6: I am surprised that the panel did not at least comment on Barber 2011 that reported moderation on the basis of gender and race; black males did better in dynamic psychotherapy whereas white females did better on medications. There also are individual studies that point toward possible moderation: Fournier 2008 found ADM superior to CBT among patients with personality disorders and the opposite among patients without; Fournier 2009 found CBT superior to ADM among patients who were married or unemployed or with multiple precipitants; Leykin 2007 found CBT superior to ADM among patients with more prior medication exposures (all from DeRubeis 2005); Hollon 2014 found that adding CBT enhanced the effects of ADM only for the third of the patients who were not chronic but more severe (patients who were less severe but not chronic did as well on ADM alone and patients who were chronic did not benefit from its addition). So far as I know these indications have not been replicated (other than perhaps marital status) but they might be worth mention. It also might be worth mentioning recent work on the development of treatment selection algorithms (DeRubeis 2014; Cohen & DeRubeis 2018). I would especially mention severity as a moderator of specificity in response. ADM does not separate from PLA among patients who are less severe (Fournier 2010) and the same appears to be true for psychotherapy with respect to nonspecific controls (Driessen 2010). That is more a problem for ADM since the majority of patients who get better on medications are not responding to the pharmacologically active agent (but do get side effects) than it is for psychotherapy since it is more than acceptable to the larger society for the latter to work through purely nonspecific (relational) mechanisms (in fact many believe that such factors account for the bulk of its effects and Cuijpers 2012 appears to support that view). If an ADM had no specific effect beyond that provided by PLA it would not be allowed to go on the market, whereas psychotherapy that worked solely through relational means would be perfectly acceptable to society at large. The fact that it also appears to have an enduring effect (best established for CBT but evident for BA and maybe DYN) and a possible beneficial effect on relationships (IPT: Weissman 1981) and employment status (CBT: Fournier 2012) suggests a possible advantage for psychotherapy over ADM among patients with less severe MDD. At the least it seems reasonable to point out that about two-thirds of the patients treated in the depression literature are female and the vast majority are white.

**Panel response**

*The nature of the data available that met the inclusion criteria for the guideline did not include much of the information referenced here. The panel would support further efforts by APA to more extensively explore this area. We also want to point out that moderators identified in trials are very uncertain because these trials typically do not have sufficient statistical power to identify such moderators. Furthermore, these moderators are typically not defined in advance, and may therefore very well be chance findings (only the significant moderators are published). We also think that new sufficiently powered trials are needed to verify that moderators are indeed predicting outcome.*

Page 61 line 25: I am not sure what “directive” is intended to communicate here since I encourage patients to do homework or practice certain skills, or read certain books but can no more “direct” them to do so than I would have been able to “direct” my son not to engage in normal teenage hijinks when he was outside of my immediate supervision. In CBT I “recommend” and “encourage” patients to follow those recommendations (as part of a collaboration in which I also note that the literature suggests that those clients who follow that advice are more likely to do better than clients who do not but also noting that such a relationship is correlational only and that it could just as likely be that clients who are going to get better for reasons unrelated to treatment might be more likely to implement my suggestions) but I do not “direct”. Is there a better more descriptive term here?

**Panel response**

*The panel feels the term captures the intended concept and is consistent with a broad division between treatments in this context.*

Page 62 line 9: This is not hard to understand since it is easier to detect an effect than it is to explain it. The first question that I want to answer is whether a treatment works (efficacy) and the second is who it works for (moderation) and the third is how it works (mediation). Efficacy has pragmatic implications, moderation refines those pragmatic implications, and mediation explains those pragmatic implications. Although there is nothing so pragmatic as a good theory (Kurt Lewin) we still do not understand how ADM or ECT work but that does not preclude our using those technologies. The first goal of any treatment guideline is to say what works on average (efficacy) and the second is to say what works for whom if there is variability (moderation). Efforts to address process and mechanism are incredibly valuable and much to be desired but they are temporally tertiary with respect to the immediate goal of providing relief from suffering and much harder to realize (despite being ultimately more valuable) since they almost always involve three variable models in which it is hard to independently manipulate the mediation therapy processes or patient mechanisms. Placebo controls do provide one exception since these are essentially efforts to separate specific from nonspecific causal elements (an exercise in construct validity that speaks to causal mechanisms) but it is hard to implement such decomposition studies with respect to psychotherapy since they are harder to blind (as if placebo controls are all that easy). In most instances studies of treatment process or patient mechanism rely on nonexperimental correlational designs or three or four variable mediational analyses within the context of a randomly controlled trial. It is simply easier to establish that the treatment had an effect on a process or mechanism or outcome than that a treatment worked through a process or a mechanism to effect an outcome. There is no reason to apologize for some questions being harder to answer than others, although it does advance the field to point out why.

**Panel response**

*This paragraph reflects a consensus of the panel. It is not clear how to address this comment above, other than the acknowledge that the panel is in agreement with the points made and has attempted to meet the goals indicated in the comment.*

Page 62 line 16: I do not agree that Cuijpers 2013 is merely correlation since it was based on a decomposition of outcome variance drawn from different sets of randomized controlled trials (no treatment vs nonspecific controls vs active treatments). I consider this the strongest evidence yet that generic factors play a causal role in treatment outcome (something works better than nothing) and consider a model for what ought to be done routinely in other systematic reviews.

Page 63 line 2: I do not agree that this study is merely correlation since it was based on a decomposition of outcome variance drawn from different sets of randomized controlled trials (no treatment vs nonspecific controls vs active treatments). I consider this the strongest evidence yet that generic factors play a causal role in treatment outcome (something works better than nothing) and consider a model for what ought to be done routinely in other systematic reviews.

**Panel response to two prior comments**

*The language here has been reviewed with panel member/ methodologist Pim Cuijpers, who was most comfortable with this characterization of the findings.*

Page 64 line 7: Effect implies causality and I doubt that the literature that John reviewed will support a causal inference. I think the more accurate way to describe this is that he studied the relation between the therapeutic relationship and outcome. A pair of studies that monitored relations over time found that adherence to specific cognitive and behavioral strategies predicted early change in symptoms that in turn predicted subsequent ratings of the quality of the therapeutic relationship and not the other way around (Feely & DeRubeis 1990; DeRubeis 1999). Correlational data (as most process studies are) are causally ambiguous and care must be exercised not to use causal language when describing such results. Cuijpers 2012 tips me toward thinking that the nonspecifics do represent a causally active component of treatment although it is not clear just what aspect of those nonspecific (moving from nothing to something) is responsible for that effect.

**Panel response**

*The panel attempted to present the interpretations they were stated in Norcross (2011) and Norcross and Wampold (2011). To clarify the nature of the data, the section has been rewritten to indicate that the proposed effects were the conclusion of an interdisciplinary task force that reviewed the literature.*

Page 65 line 20: I was a junior author on DeRubeis 2008 and it is purely theoretical and does not review actual data. It does not suffice to support statements regarding actual mediation of antidepressant effects. How medications actually work is a matter of some dispute (other than that they start by inhibiting oxidation for the MAOIs and blocking reuptake for the SSRIs and SNRIs and TCAs but what happens next is a matter of some dispute. Conventional wisdom is that they reverse a functional deficit in transmission through those systems (perhaps fueled via BDNF) whereas others think they trigger an overcompensation by homeostatic regulatory mechanisms that end up reducing transmission through those systems (Andrews 2015). You either need to do considerably more or less but DeRubeis 2015 is not an adequate cite.

**Panel response**

*Thank you for your attention to this. We are reviewing and will ensure this section is appropriately referenced.*

*Concerning a comment about mechanism of action of antidepressant pharmacotherapies and his request for a reference other than the paper by Rob DeRubeis that is cited, it seems reasonable to us to state that we do not know how these agents work. The neuropsychopharmacology of antidepressants seems well outside the charge to the Panel.*

Page 71 line 2: I am not aware of controlled trials supporting the efficacy of ACT or mindfulness in the acute treatment of depression. Have I missed something?

**Panel response**

*The VA/DoD guidelines included RCTs that included ACT and mindfulness that did not meet the IOM requirements.*

Page 73 line 3: I would remind the reader just what those differences are.

**Panel response**

*The following paragraph does so.*

Page 74 line 1: It might be good to indicate here or elsewhere why fluoxetine is preferred over other antidepressants for children and adolescents. I presume that is because it is associated with lower risk of suicidal ideation (perhaps due to its longer half-life).

**Panel response**

*Thank you for the suggestion. This has been added.*

Page 76 line 1: While not exhaustive you might want to cite a few exemplar prevention studies like by Clark or Garber-Beardslee-Brent or Mufson-Pope-Garber.

**Panel response**

*Thank you we are adding some citations.*

Page 76 line 8: Did I not just see an earlier section on mechanisms above?

**Panel response**

*Thank you for the attention to this. We have rewritten to clarify that the PICOTS did not include mechanism, and that the prior discussion of mechanisms was supplemental to the guideline.*

*While the panel includes some discussion of mechanisms of change more broadly in the guideline document, it does not make formal recommendations pertaining to mechanisms as these were not included in the underlying systematic reviews.*

Page 77 line 10: I think that the guideline panel did a great job of sticking to the IOM guidelines as well as they could given that APA did not have the money to fund a systematic review but most of the issues they had to work around (outdated reviews and single rater extractions) could have been avoided if we (APA) had conducted its own systematic review or made common cause with NICE to update their existing several decades of extractions. Opportunity lost.

**Panel response**

*Thank you. Opportunities for systematic review access for future clinical practice guideline panels can be explored.*

Page 78 line 3: Good that the panel followed the IOM standards as best it could but better if the APA had conducted (or traded off with NICE) its own systematic review.

**Panel response**

*Thank you. Opportunities for systematic review access for future clinical practice guideline panels can be explored.*

Page 80 line 1: Older therapies not recommended if they had not been tested and newer therapies not recommended if they had been tested but not around long enough to be covered in other reviews.

**Panel response**

*Thank you for the feedback. These decisions reflect the panels efforts to adhere to the IOM guidelines.*



Page 80 line 13: Good discussion of other types of designs other than RCTs.

**Panel response**

*Thank you for the support.*

Page 81 line 23: I would be reluctant to draw conclusions regarding psychotherapy in general but CBT (specifically cognitive therapy) cuts risk by more than half relative to ADM and is relatively robust odds ratios exceeding 2.0 in six of eight relevant trials and just falling short in a seventh (Cuijpers 2013). How much evidence does the panel need to say something to that effect with respect to CBT? BA is one for one (Dobson 2008) and even DYN has one (Fonagy 2015).

**Panel response**

*Addressed above in response to items 1 and 2.*

Page 81 line 10: I would qualify by adding “medical”. The trend over the last few decades has been to minimize the number of psychiatric rule-outs beyond psychosis.

**Panel response**

*While it is true that there has been a reduction in exclusion criteria, the panel is comfortable leaving the language as it is, particularly given that substance use disorders are often not excluded.*

Page 85 line 16: I know that this is the term that is commonly used in the literature but patients do not fail treatments, treatments fail patients. Using a term like “treatment resistant” carries the connotation that the problem lies within the patient rather than the inadequacy of our interventions. I would suggest “patients for whom treatments fail” or something of that ilk.

**Panel response**

*The panel is appreciative of the comment and agrees with the conceptual point and have modified the language.*

Page 85 line 17: We are on the verge of a technological revolution in which treatment selection algorithms (DeRubeis 2014) especially when powered via machine learning (Cohen & DeRubeis 2018) can greatly enhance our ability to select the best treatment for a given patient (precision medicine). Barber on the guideline panel kicked it off back in 1996 using data from the NIMH TDCRP.

**Panel response**

*We have incorporated the comment on this recommendation above. The next generation of clinical practice guideline development group hopefully will have systematic data on treatment selection algorithm*

Page 85 line 20: Pity that STAR\*D did not think to include a control group or even extend the initial medication for a subset of the patients. One of the largest studies ever (4000 patients treated across 4 subsequent levels of randomization) and it could not determine whether everything worked or nothing worked. All it produced was the world’s most expensive random numbers table. A stunning indictment of what happens when you let NIMH get involved in the design of a study as opposed to simply funding those that survive the rigors of peer review.

**Panel response**

*Thank you for the comment. The panel shares the concern about the limits of the STAR\*D study.*

Page 86 line 20: That is largely because the panel relied on reviews done by other authors who likely differed in how they identified the different therapies. Had the panel conducted its own systematic review of the extant literature it likely could have been more consistent in how it labeled the different treatments.

**Panel response**

*The panel agrees with this comment.*

Page 89 line 16: It is standard practice in medication trials to report serious adverse events to IRBs and the same is done when medication is used as a comparator to psychotherapy. That should become the standard in all psychotherapy trials.

**Panel response**

*The text has been modified to emphasize the need in psychotherapy trials.*

Page 90 line 11: CBT cuts the rate of subsequent relapse in half relative to prior ADM (Cuijpers 2013).

**Panel response**

*Addressed above in responses to items 1 and 2.*

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**19.**

**Commenter: Marsha Morris, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - I endorse the comments submitted by JEFF Axelbank

**Panel response**

*The panel has described the extent of topics covered by the guideline, as well as key areas not covered, as noted by Dr. Axelbank. The panel does not agree these make the guideline worthless but does acknowledge that it is important for the limits of this guideline to be clear and has made efforts to do so. The panel also strongly supports further efforts that expand upon this guideline and address the important concerns raised by Dr Axelbank and the Psychotherapy Action Network.*

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**20.**

**Commenter: Russell Holstein, Ph.D. [EMAIL]**

I wish to endorse the comments from Dr. Nancie Senet, Dr. Jonathan Shedler and the Psychotherapy Action Network. There is no good reason for these guidelines.

Russell Holstein, Ph.D.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Senet (Commenter 73), Shedler (Commenter 51) and Soldz (Commenter 10).*

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**21.**

**Commenter: Maureen J Hudak**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I would like to support the comments previously made by Jonathan Schedler, Nanci Senet, and Jeff Axelbank. I feel these guidelines misrepresent the options available which have clear clinical utility as well as overlook the issues related to the client's wholistic needs beyond possible short term treatment gains.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**22.**

**Commenter: Jeffrey Axelbank, Psy.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment – First, I want to heartily endorse the comments of the Psychotherapy Action Network (PsiAN), Jonathan Shedler, Nancie Senet, and others who have articulated far better than I can why this Guideline is unacceptable.

I will add my own comments here. In the Scope section, the guideline states:

Due to limited resources and timing constraints this guideline does not address screening for depression, assessment of associated comorbid conditions (e.g., suicidality, medical problems), monitoring response to treatment, locus of care, prevention of depression, dose, timing or duration of treatments for depression, costs of treatment, long term benefits of treatment, mechanisms of change, bipolar disorder, or efficacy of treatments for disorders other than depression. (emphasis added) This is as far as one has to go to see that this Guidelines is fatally flawed. These limitations, freely admitted by the guideline authors, so severely limit the usefulness of this Guideline as to make it worthless. If they did not consider long-term benefits of treatment, for example, this makes the guidelines useful only for clinicians interested in the short-term benefit to the patient. Count me out of that one! And if they are not considering dose, timing, and duration of treatment, which are critical decisions and factors, how useful can these guidelines be? And finally, if they are not considering comorbid conditions, they might as well not bother. Every single person I see for depression also has co-morbid conditions, most often trauma.

I could go on and on about the uselessness of these guidelines, but others have done so in greater detail.

**Panel response**

*The panel has described the extent of topics covered by the guideline, as well as key areas not covered, as noted by Dr. Axelbank. The panel does not agree these make the guideline worthless but does acknowledge that it is important for the limits of this guideline to be clear and has made efforts to do so. The panel also strongly supports further efforts that expand upon this guideline and address the important concerns raised by Dr Axelbank and the Psychotherapy Action Network.*

But all this leads me to this:

STOP! STOP! STOP! STOP! STOP!

Stop this destructive, divisive, useless, unscientific Clinical Professional Guidelines (CPG) program! This is an effort to fit the square peg of psychology research into the round hole of medical research. The three CPGs issued thus far are so flawed that they are basically fraudulent attempts to legitimize recommendations that the research in each guideline cannot support.

I am reminded of an unfortunate parallel with the recent Kavanaugh nomination and hearings. Note that I am not asserting a parallel in content, but rather the process.

What we have with these CPGs is a program that is dividing the field, polarizing us (and also reflective of pre-existing polarization), and marginalizing an important group. And we have a group that is railroading this (the Guidelines program) through, despite fervent opposition, and legitimate doubts about a flawed program. And that opposition is being dismissed as “anti-science” (similar to “no corroborating evidence”). We have a group that is so hell-bent on getting these guidelines out there, for their own reasons, that they are willing to short-circuit any thoughtful further examination (similar to stopping a more thorough FBI investigation to get at the truth) such as the New Business Item proposed to the APA Council of Representatives at the March 2018 meeting. And the end result of all this will be a fractured APA and field of psychology, and a diminishing of and a loss of trust in the institution.

Does all this sound familiar????

So, please, STOP, the Guidelines program. Hit the pause button. I understand that at the last ASC meeting, many of the concerns were discussed and responded to. However, the volume of concerns and their legitimacy warrants stopping the program to reassess the entire endeavor. It is flawed at its root, not in the details of its implementation. Not pausing it will only lead to further division and harm to the profession.

What is needed is more communication between researchers and practitioners. We need more communication between people of various theoretical orientations. Instead what we have is a minority promulgating a divisive and useless and harmful program.

Perhaps one way to say this is that we need CRGs – Clinical Research Guidelines. Such guidelines would be written by clinicians and practitioners to guide researchers in the kinds of research that would be relevant and helpful.

In conclusion, I'd like to hope that these comments and those of others critiquing these guidelines will be taken seriously, in contrast to the way in which they were ignored entirely in

the issuing of the PTSD CPG. The final version and the draft version of that guideline were nearly identical, despite serious concerns submitted in the comments period. I'm hoping that in the case of this Depression Guideline, the effort will be stopped, instead of rammed through.

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**ASC response**

*The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.*

*Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.*

*Expecting any one document to address every consideration that goes into responding to the burden of mental illness and the complexities of the human mind and the social world in which we live and relate to others would doom any initiative to failure; in turn, the argument that because CPGs do not address every aspect of human suffering, CPGs have no value and are dehumanizing seems unfair. We would never claim that CPGs provide a 'one-stop-shop' for clinical care needs (and the introductory section of the guideline clearly encourages use of the guideline in conjunction with clinician judgement and information about clients' preferences and values), but CPGs can nonetheless be a very valuable part of clinical decision making.*

*Another important concern raised focuses on the impact of relying so heavily on randomized controlled trials (RCTs). We wish to note that evidence from sources beyond RCTs (e.g., observational data) was used when evaluating potential harms and burdens of interventions as well as patient values and preferences, and these criteria were used along with the efficacy data from systematic reviews to make the guideline recommendations. Notwithstanding, it is true that RCTs are the main source of data used in the systematic reviews. This is because RCTs are the studies most likely to meet the rigorous quality standards expected for systematic reviews and are a methodology that allows for causal inferences to be drawn. This is considered best practices in the U.S. and internationally for guideline development, which prioritize the use of systematic reviews for identifying and assessing evidence. Following best practices was considered important to create a guideline that will be viewed as credible.*

**23.****Commenter: Karen Saakvitne**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Any guidelines for the treatment of depression that do not include the role of Adverse Childhood Experiences, childhood neglect and abuse, and trauma are inadequate as the vast majority of all mental health clients are managing the sequelae of those experiences.

Any treatment of interpersonal and attachment trauma must include trauma-informed therapeutic relationships. These relationships need to include an understanding of the neurophysiology of trauma - which overlaps with the neurophysiology of depression and dissociation.

**Panel response**

*The committee appreciates the commenters' concerns about ACES in youth mental health. Unfortunately, the treatment of trauma and the role of trauma in depressive illness in youth is outside the purview of this guideline.*

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**24.****Commenter: Amanda Rickard**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - While I want to commend and appreciate the work required to compose this document, I have concerns about its lack of usefulness and misleading nature, which may actually harm the public rather than guide and help as intended. First, I am confused about how the Guideline is even necessary since it is acknowledged within that there is no basis to determine which therapies work best.

**ASC response**

*We appreciate the commenter's note about the work that went into composing this document. The commenter's note suggests we need to make the conclusions and their rationale clearer in the revised CPG, which we have worked to achieve. The panel did not intend to communicate that there is no basis upon which to determine which therapies work best. Rather, the panel endorses the use of well-designed research to identify and compare treatments, even though sometimes those meaningful comparisons will indicate comparable outcomes. This too is useful information for practitioners. Thus, even though comparisons did not differentiate between the efficacy or effectiveness for some treatments for the adult literature, we see this as important information that one can have confidence in the range of recommended treatments as being supported by the best available research. The panel did not intend to communicate that there is no basis upon which to determine which therapies work best. The panel does in fact endorse the use of well-designed research to identify and compare treatments. The panel does recognize that for the adult literature, comparisons did not differentiate between some treatments. The panel sees this as important evidence of the value of research for increasing confidence in a wider range of treatments.*

In creating this Guideline for depression, the APA will be misleading the public and continue to support the extremely narrow focus of manualized research and training programs for clinicians. Moreover, my understanding of the remission rates in the research included in the Guideline would be inadequate to reach significance in a medical meta-analysis. Thus, what is the purpose of recommending these manualized treatments over others? It seems the decision to include only randomized controlled trials (RCTs) exclusively in this Guideline is a major hindrance in providing accurate information about the optimal treatment of choice of patients. Instead of considering the advantages of insight-based therapies (e.g., humanistic, psychoanalysis, and psychodynamic) for real change and patient treatment preference, these types of treatments will no longer be included or considered because RCTs are not feasible, even though epidemiological, outcome, and other studies have shown these treatments to be effective (if not more so) in treating depression and creating lasting positive change for patients than the skills-based manualized treatments touted by the Guideline. Perhaps as Psychotherapy Action Network (PsiAN) suggests it would be best to present this document as a compendium of existing RCT studies rather than a Guideline. Lastly, not only does my experience as a practicing therapist provide anecdotal evidence of the response and effectiveness of insight-based approaches, I am shocked at how little cultural, individual, and systemic factors are addressed in the therapies recommended in the Guideline. Even Dr. John Norcross, who is well-respected for his model of stages of change and integrative patient-focused therapy approaches, notes that only patients in the action stage of change are ready for these skills-based therapies while the vast majority of patients presenting for therapy are in the pre-contemplation and contemplation stages of change, which require insight-based approaches to help them proceed toward the action stage of change. Instead of helping patients access helpful guidance about what therapies provide lasting and satisfying change, this Guideline in its current form will result in the continued dehumanization and reductionistic problems inherent in the disconnect between academic/research agendas and the actual holistic treatment of real complex humans. This Guideline is not helpful to me as a psychologist, and it will not be helpful in the current form to the public. It seems to me that the document only furthers the agenda of psychologists who do not actually practice therapy. To be honest, if APA continues to minimize or ignore the insight-based therapies, I am not sure what use it is to me to continue to be a member of the organization.

**ASC response**

*There are many important points raised here and we appreciate the time taken to share these concerns. We will try to respond to the major issues raised in this comment:*

*One important concern center on whether the impact of individual differences is adequately considered. This is a critical issue. As one step, we are adding a demographic breakdown of the samples for each study included in the systematic reviews for the depression guideline. This will at least provide readers with information about the racial and ethnic and United States/international diversity of the samples. Also, we have tried to be clear in the document about whether individual differences in treatment outcomes were examined (but there was insufficient data to draw conclusions, as typically occurred) vs. individual differences were not considered.*

*Additionally, we have included a brief standardized statement tied to individualizing treatments to the introduction of each CPG, and the Advisory Steering Committee has written a more comprehensive document on this topic that will be posted on the CPG website. With these documents, we hope to make it clearer the ways that individual differences can still play a key role in developing a treatment plan that uses CPGs to*

*synthesize the best available research evidence in conjunction with factors tied to each individual's unique values, background, and identities.*

*Another important concern raised focuses on the impact of relying so heavily on randomized controlled trials (RCTs). We wish to note that evidence from sources beyond RCTs (e.g., observational data) was used when evaluating potential harms and burdens of interventions as well as patient values and preferences, and these criteria were used along with the efficacy data from systematic reviews to make the guideline recommendations. Notwithstanding, it is true that RCTs are the main source of data used in the systematic reviews. This is because RCTs are the studies most likely to meet the rigorous quality standards expected for systematic reviews and are a methodology that allows for causal inferences to be drawn. This is considered best practices in the U.S. and internationally for guideline development, which clearly prioritize the use of systematic reviews for identifying and assessing evidence. Following best practices was considered important to create a guideline that will be viewed as credible.*

*A concern was raised about the high remission rates for the recommended treatments, which is an important issue to consider. We fully agree that the remission rates of our most effective treatments are lower than we would like. Nevertheless, many depressed patients are seeking treatment and we want to offer them currently available treatments with the strongest evidence of efficacy based on the best available research. The fact that we need to continue treatment development does not mean that we should not strive to offer the best we can to our patients who need help now.*

*Concern was expressed that the CPG is biased against insight-oriented therapies. We very much hope this is not the case as the goal is to provide as unbiased an evaluation of the evidence as possible. We understand that the concern stems in part from the heavy use of RCT data to guide recommendations. Notably, some insight-oriented therapies (e.g., psychodynamic psychotherapy) that are supported mostly by non-RCT evidence have been evaluated with some RCTs, and thus are represented in the guideline. Further, we try to make very clear that the exclusion of therapies not supported by data from RCTs does not mean these therapies are ineffective - just that insufficient RCT data are available to demonstrate their effectiveness. We hope that the conclusions of this and other CPGs can encourage funding agencies and others to support future RCTs to study those therapies that were not included due to insufficient evidence.*

*CPGs are intended to recommend those treatments that have the strongest systematic research support. Recommending treatments that have not been systematically evaluated is counter to one of the basic goals of a CPG, and a CPG would not be credible if it did not rely on a systematic review of high-quality efficacy data. Thus, it is our strong hope that more treatments will be systematically evaluated in a way that allows for causal inferences to be drawn so that even more treatments can be included in future CPGs.*

*The comments about the importance of considering stage of change are interesting and also thought provoking. We wholeheartedly agree that one needs to consider a patient's readiness for any treatment. However, we don't see this issue negating the CPG-recommended treatments. There are multiple approaches to addressing ambivalence about being in therapy or about making changes tied to a particular intervention*



*approach. It is normative to use motivational interviewing approaches or a broad range of other strategies or frameworks to build rapport and strengthen the therapeutic relationship and commitment to treatment, either in advance of or in conjunction with one of the recommended treatments. Thoughtfully working with patients at different stages of change is a routine part of clinical care and we do not see why this would negate the CPG-recommended treatments.*

*Finally, a concern was raised that the guideline will perpetuate dehumanization and a reductionistic approach. We feel this perception may stem in part from an unrealistic expectation about what a CPG is intended to offer. The depression guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guideline is designed to make a particular contribution; one that is circumscribed, but nonetheless very valuable – namely, to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults. As noted in the introduction of the CPG, treatment planning is not meant to be guided by only one source of information, but we believe that knowing what the best available research says about which treatments can be helpful, on average, is important information to help guide treatment planning.*

#### **Panel response**

*The panel appreciates the concerns shared by Dr. Rickard. The panel did not intend to communicate that there is no basis upon which to determine which therapies work best. The panel does in fact endorse the use of well-designed research to identify and compare treatments. The panel does recognize that for the adult literature, comparisons did not differentiate between some treatments. The panel sees this as important evidence of the value of research for increasing confidence in a wider range of treatments.*

## **25.**

**Commenter: Dennis Debiak, Psy.D. on behalf of APA Division 39**

Comment type: Group Comments

Group name: Division of Psychoanalysis (39) Do you have any other comments about the draft guideline document?

General Comment - First, I would like to reiterate what Division 39 President-Elect Barry Dauphin said in his general comments on these proposed Guidelines: "The Guideline is clearly the byproduct of a rigorous process and reflects a tremendous amount of dedication on the part of the panel. They are to be commended." We know that you all worked very hard on this without payment. We so much appreciate your time and effort.

#### **Panel response**

*Thank you very much.*

Also, I would like to support the comments on these proposed guidelines made by Division 39 members Norman Abeles, Barry Dauphin, Jonathan Shedler and Virginia Shiller.

In consultation with other members of Division 39, I offer the following comments:

In general, although months were given for APA member to review and provide comment on these proposed guidelines, we feel that even more time is necessary. Even in the time frame provided, we feel that we cannot do justice to critiquing these guidelines.

**Panel response**

*Thank you for your comments. Please refer to the response from the ASC in the beginning of this document.*

Also, we believe that the critique of the child/adolescent portion of the guidelines by our colleague Virginia Shiller is comprehensive and applicable to the adult portion of guidelines as well. Overall, we are pleased to see that these guidelines are supportive of psychodynamic psychotherapy.

With that said, in our view the APA depression guidelines contain the following problems:

- 1) They do not present the data to the reader in a way that they can evaluate their assessment of the studies or meta-analyses they rely on;

**Panel response**

*We are adding in additional information about how to read the associated evidence grids as we realize this might have been less clear to individuals not familiar with the grids. We are also adding in more specific information about which recommendations came from which grid or decision table. It is our hope that this will help the interested reader more easily see the data and information on which each recommendation was based.*

- 2) they eliminate crucial studies that may not be in those meta-analyses;

**Panel response**

*The panel acknowledges that the selection approach used excludes some studies. However, it was adopted to increase confidence that the studies included meet an established level of quality so as to increase confidence in the recommendations made.*

- 3) they oversimplify the notion of co-morbidity in that they do not distinguish how there may be different pathways to depression in those with personality disorders, substance abuse and/or anxiety disorders and other co-morbid conditions that might necessitate different treatment approaches;

**Panel response**

*The panel encourages future efforts that can build on this and address issues of comorbidity in greater depth.*

- 3) they do not discuss specific techniques for treating those with different types of depression and why they may be more or less effective from a clinical point of view;
- 4) they do not take into account the different intrapsychic configurations that may underlie different types of depression, notably the developmental level of object relations in those with introjective versus anaclitic depression. There is a vast research literature on the latter generated by Sidney Blatt and his colleagues.
- 5) Perhaps most important, although they distinguish the different types of depression. Overall depression is treated as one entity without taking into account all the different variants of it,

including the ways that symptoms of depression are linked to level of personality organization (e.g. neurotic, borderline or psychotic).

Thank you.

**Panel response**

*It would not have been feasible to examine “everything related to the treatment of depression.” Early on, given the breadth of the topic this guideline addresses, we needed to make a decision regarding a singular focus and decided to focus on depression as experienced by three different age groups (i.e., adolescents, adults, older adults).*

*In addition, the literature is replete with theories regarding the cause of depression and its variants. However, the goal of this committee was not to assess the validity of these theories, rather, the differential efficacy of treatment approaches. The lack of high-quality studies demonstrating a strong relationship between a particular theory and the efficacy of a related treatment approach (i.e., whether the hypothesized “causative” variable mediated treatment outcome) makes such a task extremely difficult.*

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**26.**

**Commenter: Thomas A. Caffrey, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Executive Summary - Passage: P. ES-6, line 17 (“Limitations”) “Additional limitations the panel noted across cohorts include that much research is highly dependent on federal research funding. Also, there are differences in the amount of research evidence available for different therapeutic approaches (e.g., there is more evidence available for cognitive behavioral therapy than for psychodynamic therapy and hardly any for humanistic therapies)” (p.ES-6)

Comment: At what point does the difference in research funding make comparisons moot? Or might that point have already been reached? It appears to be a big difference. If so, how valid does the Guidelines’ less frequent recommendation of psychodynamic psychotherapy become?

**Panel response**

*Dr. Caffrey raises an important question regarding differences in external funding and amount of research evidence (ES-6, line 17). We note later in this paragraph that these factors limit the broad applicability of research findings, but we do not agree that it makes comparisons moot. However, data was often not available or not sufficient for the panel to be able to make comparisons between treatments.*

Executive Summary - Passage: P. ES-6 Line 22 (“Limitations”) “Further, while much research focused on symptomatic change, additional research is needed on secondary outcomes such as patient-centered outcomes (i.e., quality of life and interpersonal relationships, social engagement, occupational functioning).” (p.ES-6)

Comment: Why are other-than-symptomatic changes characterized here as “secondary”? Given the nature of the changes cited, I would recommend calling them “other than, and possibly more important than, symptomatic changes.”

**Panel response**

*Thank you for this helpful suggestion. We are removing the “secondary” phrasing here.*

Executive Summary - Passage: P. ES-14 Line 11 (“Recommendations” for General Adults)  
 “For adult patients with sub-clinical depression, the panel suggests that clinicians offer psychotherapy (Psychotherapy in general including both cognitive behavioral therapy and non-cognitive behavioral therapy psychotherapies [e.g., interpersonal counseling, problem-solving therapy, life review therapy]).”

Comment: A clear bias appears to run through the recommendations. “Behavioral,” “Cognitive,” and “Cognitive Behavioral” treatments are each or together repeatedly cited as recommended. “Psychodynamic” or “Dynamic” is seldom cited, in spite of its wide use. And, as the cited passage indicates, the authors appear to be thinking in the on-off terms of “cognitive behavioral therapy” (ON) and “non-cognitive-behavioral therapy psychotherapies” (OFF) – with the added exclusion of “dynamic,” “psychodynamic,” or other term that includes the place of patients’ unconscious processes, among the cited examples (that is, with a clearly conscious weighting among the examples cited even among the “OFF” possibilities [counseling, problem-solving, and (conscious) life-review]). In short, the bias tips against therapies that draw on patients’ unconscious processes (their unexamined assumptions, repressed memories or thoughts, and habitual defenses).

**Panel response**

*Thank you for this feedback. The document is being revised with this point in mind.*

**27.**

**Commenter: Brook Hersey, Psy.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - As a clinical psychologist, I treat many individuals with depression. Major depression is a devastating condition. I am always deeply interested in resources that enhance my ability to provide relief, and I am an eager consumer of research. If APA created a compendium of articles on depression and what works to treat it, and were able to purchase the rights to make full-text articles available to all comers, I would be grateful. I am always eager to read, think and grow.

However, my reading of the guidelines does not offer me much as I think about each unique person I treat. The over-reliance on RCTs negates the complex comorbidities, psychosocial and cultural realities of individuals with depression. While the draft document is rife with caveats and acknowledgments of the importance of individually tailored treatments, the tables read as quite rigidly prescriptive.

I have read the Psychotherapy Action Network’s thoughtful critiques, and I hope that the drafters of these Guidelines will read them and take them to heart. I also found the “NICE under scrutiny” document, produced by the UK Council for Psychotherapy to raise meaningful concerns.

I really fail to see what we gain by adopting these guidelines. I think there’s some hope that psychology will be medicalized. But as a psychologist (non-tenure track assistant professor and

provider of psychotherapy), I find little in the guidelines that helps me understand depression better.

Further, the Guidelines begin with the statement that they are intended to be “aspirational and not intended to create a requirement for practice.” However, it seems naive to me to think that the guidelines won’t be used by decision makers (a.k.a. insurance companies looking for additional reasons to deny coverage) or by the legal system (where individual practitioners may be vulnerable in law suits if they did not follow the guidelines). I refer you to the Fall 2018 issue of the APA Practice Organization’s “Good Practice.” To quote: “For any provider, keeping abreast of the guidelines is also important for legal and reimbursement reasons... Showing that you’re aware of the latest clinical research in your field can help waylay potential arguments that you are not practicing in your area of competence.” A sidebar on page 26, attributed to Jana Martin, advises such things as documenting rationale when one is choosing a treatment outside the guidelines. In other words, when we try to think broadly about patient care, we face increased administrative burdens and increased liability risks.

My strong recommendation would be to replace the practice guidelines with easily accessible compendiums of research.

Respectfully submitted.

**Panel response**

*Thank you for addressing your concerns. Please refer to the response from the ASC in the beginning of this document.*

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**28.**

**Commenter: Jeannine Zoppi**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - It seems to me that all Practice Guidelines developed by APA, including the PTSD and Depression Clinical Practice Guidelines, are based on narrowly defined science, promote the medicalization of the psychology rather than support psychology as a distinct and essential profession, and appear to serve the needs of insurance companies rather than the needs of diverse psychologists and their diverse patients. And if narrowly defined science and research is used to develop clinical practice guidelines then any psychological treatments outside those narrow parameters are excluded. This exclusion puts many psychologists at risk for licensing board complaints, malpractice lawsuits, and denial of treatment authorization and/or reimbursement by any insurance company who adopts the narrowly focused clinical practice guidelines as part of their medical necessity criteria. APA should represent and support all psychologists and should not be developing clinical practice guidelines that disenfranchise any group of psychologists or treatment modality.

I also endorse the comments submitted by the Psychotherapy Action Network.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement on p. 5 for discussion of this topic.*

**29.****Commenter: Laurence S. Baker, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - There is I believe too little emphasis on doing something about what depresses one (when one is in fact reacting to "things"). There are many events and policies in this world that reasonably cause one to be depressed, even seriously so - the President depresses me badly, for example - and treatment is not what is needed.

ACTION is in order, but we as a profession talk way too much, and act way too little. AND we do not sufficiently urge our patients to act on what distresses them.

**Panel response**

*The panel appreciates this comment but given our purpose of reviewing and comparing specified and researched treatments, this is outside our purview.*

**30.****Commenter: Nicola Ranson, LCSW**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am deeply distressed at the limited view which seems to influence the current document on Depression treatment guidelines. From what I understand, the research cited does not make sufficient comparisons between different approaches available. Not only are cutting edge therapies such as Somatic Experiencing, Mindfulness, EMDR appear to be excluded, but also insight-oriented psychodynamic therapies. I am afraid that our profession is becoming beholden to the medical model and its so-called brief therapies, which, in a few years will be replaced by others, rather than the tried and true therapies which may pay more attention to the human relationship that evolves, as well as the Somatic based and Mindfulness approaches. I have practiced psychotherapy for over 20 years and have been an Adjunct Faculty member in a Marriage and Family Therapy program at National University for much of that time. Yours, Nicola Ranson

**Panel response**

*We were limited by the data available from systematic reviews of psychotherapy outcome studies.*

*Regarding the lack of sufficient comparisons between all the newer approaches available one of the reasons for this was that newer treatments or ones not included in meta-analyses in the last 5 to 7 years were not considered. This leaves major gaps in what was reviewed and helps explain the omission of what promising treatments may be. The difficulty in reviews of the type undertaken by the committee is the granularity of treatment naming. Give there are more than 500 brand name therapies only major name categories of major schools already used in meta-analyses could be used*

*The current clinical practice guideline development panel operated under IOM (2011) standards, to the extent possible, to ensure rigorous guideline development. By following IOM systematic review standards only meta-analyses and systematic reviews and only those that met AMSTAR standards were used. AMSTAR is (A Measurement Tool to Assess Systematic Reviews; an instrument used to evaluate systematic reviews and*

*meta-analyses for quality and consists of a checklist assessing eleven items. The use of these criteria plus the problem of so many brand name therapies prevented newer treatments from being included. This eliminated such approaches as Somatic Experiencing, Mindfulness, EMDR and EFT, which did not have meta analyses or systematic reviews that met criteria. A case in point was that EFT which has two comparative trials on the treatment of depression with good outcomes, one which showed comparable outcomes to CBT, was not able to be looked at as a treatment under its own name because it did not meet quality criteria. The committee did expand the AMSTAR criteria to some degree to include additional meta-analyses of high quality that did not quite meet all the AMSTAR criteria to expand the range to cover more studies but at no point were individual studies or comparisons looked at, only meta-analyses.*

### 31.

**Commenter: Kristin Leprich**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Executive Summary - "Due to limited resources and timing constraints this guideline does not address screening for depression, assessment of associated comorbid conditions (e.g., suicidality, medical problems), monitoring response to treatment, locus of care, prevention of depression, dose, timing or duration of treatments for depression, costs of treatment, long term benefits of treatment, mechanisms of change, bipolar disorder, or efficacy of treatments for disorders other than depression."

I believe it would be more accurate to say that these guidelines cover the above topics in minimal detail. It appears rather contradictory when the above topics are indeed briefly mentioned in scattered areas of the guidelines, such as when cyclothymia, manic episodes, and hypomanic episodes are mentioned in regards to diagnostic material for major depression after bipolar disorder is stated not to be a topic covered. Another example would be the section specifically titled "Considering Patients' Diverse Backgrounds, Identities, and Comorbidities." This section speaks of another topic that is explicitly stated to not be discussed: comorbid conditions.

#### **Panel response**

*Thank you for this feedback. The panel's intention in noting the scope was to indicate that the above items were not addressed as part of the panel's recommendation statements, which were based on systematic reviews of the literature. However, your point is well taken that this can be confusing for the reader. We are adding a line to the scope to indicate that these items are not addressed in the panel's recommendations.*

Considerations for Treatment Implementation - As I mentioned in my other comment, bipolar disorder is stated not to be a topic discussed, but there are smatterings of it discussed in the guidelines anyway. I'll clarify that I support these inclusions, and I would make a suggestion to further warn for the use of antidepressants in a patient who possibly has, or who does have, a diagnosis of bipolar disorder. Antidepressants run the risk of inducing manic episodes in those with bipolar disorder at high doses or without mood stabilizers added alongside the treatment.

**Panel response**

*We concur with Dr. Kristin Leprich's comments about the potential for adverse effects of antidepressant pharmacotherapy in persons living with bipolar disorder (i.e., switch into mania).*

Additionally, a diagnosis of bipolar disorder is often overlooked in favor of a diagnosis of major depressive disorder, especially for bipolar type 2, where depressive episodes are far more prevalent and severe than the muted form of manic episodes (hypomania). Therefore, it is the clinician's responsibility to be aware of signs of an incorrect diagnosis and alter treatment methods accordingly if these signs occur in the middle of treatment aimed for depression.

**Panel response**

*The commenter brings up an important concern that is beyond the scope of this guideline.*

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**32.**

**Commenter: Marianne Ernesto** [NOTE: SAME COMMENT AS LOIS CONDE, PHD]

Comment type: Group Comments

Group name: Staff Liaison for CPTA

Do you have any other comments about the draft guideline document?

General Comment - Consider shortening the title to: Clinical Practice Guidelines for the Treatment of Depression Across the Lifespan.

**Panel response**

*The panel has modified the title to be more concise.*

The word judgement has two official spellings:

Judgment

Judgement

Choose one and make sure the document is consistent.

**Panel response**

*Thank you, we are reviewing the document for consistency in spelling.*

Consider adding a caveat about the use of assessment measures in diagnosing depression that reminds psychologists and other practitioners that a significant score on a depression measure is not synonymous with a clinical condition of depression. Multimodal assessment and carefully considered interpretation are needed.

**Panel response**

*Although your comment is sound clinical advice, please note that this guideline focused on treatment of depression and not its assessment.*

Minimize the use of "also" and "further" in the description of the document. Those words are used excessively and it detracts from comprehension of the content.

**Panel response**

*Thank you, we are making this edit.*



"Treatment as usual" is a broad term that might have unintended consequences. "Treatment as usual" is sometimes tantamount to the use of outdated treatment procedures, or worse, engaging in activities that involve listening as a friend might listen but providing very little in the way of actual treatment. Consider changing the definition of Treatment as Usual as follows: Change "...care that is customarily provided in a particular situation" to "...empirically supported treatment that is customarily provided in a particular situation."

**Panel response**

*The panel appreciates these comments. The stylistic modifications will be taken into account in review of the draft. The use of the term treatment as usual does have limitations as CPTA and Dr. Condie indicate. However, the panel has decided to retain it given that it was often the term used by researchers to describe the control condition in studies.*

The document is well prepared, overall. Congratulations.

**Panel response**

*Thank you very much!*

Considerations for Treatment Implementation - Consider expanding the Special Population sections of the treatment recommendations. There is a section that addresses treatment of individuals with both dementia and depression, but the document is fairly silent on other special populations (e.g., those with Intellectual Disabilities or other characteristics that might compromise their comprehension of treatment).

**Panel response**

*Unfortunately, the panel did not have information to expand addressing special populations. The panel supports future efforts to provide targeted guidelines for special populations.*

The section on Taking Into Account Patient Values and Preferences offers some good suggestions for tailoring treatment to individual patients and giving considerations to their barriers to treatment. Consider tightening this section a bit so that clinicians understand that they should use empirically guided judgment (not personal judgment or proclivities) where judgment is indicated and flexibility is needed. Similarly, the section on Adapting Treatment to Fit the Individual, though important, is phrased somewhat awkwardly and thus makes the document seem internally inconsistent—as in, "here are some guidelines that you should follow," and "it won't work sometimes, so do whatever you want." It does appear the authors are trying to encourage clinicians to use sound clinical judgment but the sentence with the Webb et al. 2010 reference on p. 58 seems to unravel the entire document.

**Panel response**

*Thank you for this feedback. We have worked to tighten this section by adding information about the use of empirically guided judgment in several locations throughout the section.*

In the section in Table 1 on Complementary and Alternative Treatments, it may be worthwhile to point out that those procedures are not covered by insurance. In their current form, it seems there is room for misunderstanding. It would be a mistake for someone to believe that APA has endorsed yoga as a "therapy," and thus it is billable (to insurance companies). Psychologists

have sometimes found their way to Ethics Committees because they have billed insurance companies for those or similar non-therapy techniques and then have been charged with fraud. I cannot give you specific information for confidentiality reasons, but please be clear about the intent of the writers--that alternative therapies are meant to be adjunctive, not the main goal. If you are, indeed, endorsing these techniques as treatment techniques via the guidelines, then please do so more specifically.

**Panel response**

*The issue of reimbursement is an important one as well, but outside the scope of the panel.*

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**33.**

**Commenter: Rebecca McDermott**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Conclusion - I think you need to review the NICE guidelines. I think the issue with depression is not initial outcomes but relapse prevention thus different interventions should be recommended for first of third episode interventions.

I think it is a big mistake not to differentiate the disease pathology of depression from other disorders as it is more an issue of reoccurrence than of treating a single episode which in most cases will remit on its own.

**Panel response**

*Thank you for the constructive comments and feedback on the guideline. The panel is constrained in that the literature included and reviewed using the IOM guidelines did not directly address these issues. The panel has attempted to note the importance of considering long term benefits and acknowledge this limitation, while staying consistent with the process that was used to develop the guidelines. To the best of our knowledge, the new NICE guideline has not yet been published and have been sent out for public comments twice, which indicates that they are controversial. Nonetheless, we agree that differentiating between first and later episodes would be very useful. However, unfortunately, this is hardly done in research, and virtually all RCTs include people with a depression without differentiating between first or later episodes (except in relapse prevention). That means that this knowledge is simply not yet available, and we have to deal with the evidence that is available.*

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**34.**

**Commenter: Justin Shubert**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Conclusion - I'm very concerned that psychodynamic therapy has been left out as an effective treatment measure. There is ample evidence to substantiate the claim that psychodynamic therapy is effective for depression (see IPA and APA studies in the past 10 years). Please update this form to include psychodynamic therapy as a treatment recommendation.

**Panel response**

*Psychodynamic therapy is one of several therapies on the panel's list of recommended treatments for adults with depression. Moreover, psychodynamic therapy was included in multiple systematic reviews underlying the panel's work, including one review whose primary focus was on psychodynamic therapy*

**35.****Commenter: Rose Oosting**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Intended Use of Guidelines - The APA Guidelines Program itself is misguided and ineffectual at its very essence. It perpetuates a view of psychology as reductionistic, scientistic, essentializing and even, in its application, dehumanizing. The disappointing conclusions of each of the Guidelines put forth so far highlight the problematic consequences of attempting to reduce human distress to compendia of symptoms, sacrificing the power and stability of meaningful diagnostic distinctions and accounts of therapeutic action to the expediency of funding and publication cycles. The Guidelines make the assumption that RCT's are valid; which is questionable at its base. There are valid criticisms of the SAMHSA clearinghouse, and these are not adequately addressed by APA or the guidelines committee. It makes me wonder whom APA is serving: patients, or insurance companies?

**ASC response**

*The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.*

*Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.*

*Expecting any one document to address every consideration that goes into responding to the burden of mental illness and the complexities of the human mind and the social world in which we live and relate to others would doom any initiative to failure; in turn, the argument that because CPGs do not address every aspect of human suffering, CPGs have no value and are dehumanizing seems unfair. We would never claim that CPGs provide a 'one-stop-shop' for clinical care needs (and the introductory section of the guideline clearly encourages use of the guideline in conjunction with clinician judgement and information about clients' preferences and values), but CPGs can nonetheless be a very valuable part of clinical decision making.*

How the APA Clinical Practice Guideline Compares to Other Clinical Practice Guidelines for Treatment of Depression - It is notable that the referenced studies are very limited; and also that the same authors reappear in difference referenced sources. Eg, Cuijpers, and Xhou, in

particular. This does not indicate a wide range of research effort, and invalidates much of the support.

**Panel response**

*Some researchers have conducted multiple meta-analyses. We do not understand why that would invalidate the work. In our view it is the primary studies that are relevant, not so much the meta-analyses that have been done.*

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**36.****Commenter: Jon Rose, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am alarmed and disappointed that APA would even consider publishing these misguided guidelines. The method used, limiting evidence to a few review articles published within the past 5 years, guaranteed that the guidelines would be rubbish. Most grants for psychotherapy outcome research in the past decade have focused on manualized short-term treatments for patients with only one clear problem and no significant co-morbidities. Those samples are easy and relatively inexpensive to study, but not representative of most people seeking therapy. Excluding excellent, thorough individual studies done decades earlier results in very distorted conclusions. Ignoring important individual patient characteristics in choosing a treatment also seems irresponsible in light of decades of research. For example, my work with Dolores and Larry Thompson: Gallagher-Thompson, D. & Steffen, A. (1994). Comparative effects of cognitive-behavioral and brief psychodynamic psychotherapies for depressed family caregivers. JCCP 62(3), 543-549 showed psychodynamic therapy was more effective than CBT for depressed older adults who had been family caregivers less than 44 months, but DBT was more effective for those providing care for a family member longer. Our research on problem-solving vs. behavioral activation therapies for depressed older adult caregivers found that both were effective, but behavioral activation was more-so. That is just the opposite of these guidelines. Even in the child section, claiming that there is no evidence that play, behavioral, family, problem-solving, and psychodynamic therapies are no more effective than placebo is outrageous! We can go all the way back to Dibbs in search of self, published half a century ago to see clear scientific evidence (though not a brief placebo-controlled experiment) that play therapy is VERY effective. There are hundreds of studies that support that early work. These guidelines, if published will be harmful to the field and to the public.

Please start over. Take a more comprehensive look at the literature, including important and still-relevant studies done before computerized indexing. Then create a living online document that can be updated as new treatments for depression emerge.

Jon

Jon Rose, Ph.D.

Director, SCI/D Outpatient Psychology

Chief, VAPAHCS Geropsychology Section and Chair, Psychology Professional Standards Board

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**Panel response**

*We appreciate the reviewer's strong feelings about the effectiveness of play therapy. However, the development of this guideline followed specific methodologies as outlined in the document and used AMSTAR and IOM criteria. We do not say that play therapy isn't effective but rather there is insufficient evidence to recommend it for a specific age group of depressed youth. The work from Gallagher and Thompson is indeed very important and impressive. However, the finding in their 1994 JCCP paper was a post-hoc finding that may be true, but can also very well be a chance finding, especially considering the relatively small sample size. Before this finding could find its way to treatment guidelines, it has to be confirmed in a new well-powered trial. To the best of our knowledge such a replication has not been done. So, this finding is highly interesting, but not ready for being used in the treatment guideline.*

*We could not find your study comparing problem-solving with behavioral activation. But your finding that behavioral activation was more effective than problem-solving is not in agreement with other studies comparing these two therapies. There are large meta-analyses showing that both have comparable effects in the treatment of depression. One study is unfortunately not sufficient evidence that one is more effective than the other, especially in light of other studies suggesting comparable efficacy.*

*To address the issues in these comments would be outside the scope of this guideline and we recognize the limitations of the methodology throughout the guideline.*

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**37.****Commenter: Colleen A Redding**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Please consider including the efficacy of Exercise as a first line treatment for depression, especially mild-to-moderate depression. Here are some Meta-Analyses:

J Psychiatr Res. 2016 Jun;77:42-51. doi: 10.1016/j.jpsychires.2016.02.023. Epub 2016 Mar 4. Exercise as a treatment for depression: A meta-analysis adjusting for publication bias. Schuch FB, Vancampfort D, Richards J, Rosenbaum S, Ward PB, Stubbs B. PMID: 26978184 DOI: 10.1016/j.jpsychires.2016.02.023

Psychol Med. 2018 May;48(7):1068-1083. doi: 10.1017/S0033291717002653. Epub 2017 Oct 10.

Treating depression with physical activity in adolescents and young adults: a systematic review and meta-analysis of randomised controlled trials.

Bailey AP, Hetrick SE, Rosenbaum S, Purcell R, Parker AG.

BMJ Open. 2017 Sep 18;7(9):e014820. doi: 10.1136/bmjopen-2016-014820.

Exercise for patients with major depression: a systematic review with meta-analysis and trial sequential analysis.

Krogh J, Hjorthøj C, Speyer H, Gluud C, Nordentoft M.

PLoS One. 2017 Sep 22;12(9):e0184666. doi: 10.1371/journal.pone.0184666. eCollection 2017.

Non-pharmacological treatment for depressed older patients in primary care: A systematic review and meta-analysis.

Holvast F, Massoudi B, Oude Voshaar RC, Verhaak PFM

Efficacy of home-based non-pharmacological interventions for treating depression: a systematic review and network meta-analysis of randomised controlled trials.

Sukhato K, Lotrakul M, Dellow A, Ittasakul P, Thakkestian A, Anothaisintawee T.

BMJ Open. 2017 Jul 12;7(7):e014499. doi: 10.1136/bmjopen-2016-014499. Review. PMID: 28706086

Pharmacological and non-pharmacological treatments for major depressive disorder: review of systematic reviews.

Gartlehner G, Wagner G, Matyas N, Titscher V, Greimel J, Lux L, Gaynes BN, Viswanathan M, Patel S, Lohr KN.

BMJ Open. 2017 Jun 14;7(6):e014912. doi: 10.1136/bmjopen-2016-014912. Review. PMID: 28615268

**Panel response**

*Thank you for the feedback. The panel did examine exercise monotherapy and made a conditional recommendation for use. The panel downgraded this to a conditional recommendation because it had only efficacy data and not comparative effectiveness data in the reviews that met inclusion criteria.*

General Comment - Please consider including iCBT and other computer-delivered treatments for MDD.

Computer-based psychological treatments for depression: a systematic review and meta-analysis.

Richards D, Richardson T.

Clin Psychol Rev. 2012 Jun;32(4):329-42. doi: 10.1016/j.cpr.2012.02.004. Epub 2012 Feb 28. PMID: 22466510

Internet-based and other computerized psychological treatments for adult depression: a meta-analysis.

Andersson G, Cuijpers P.

Cogn Behav Ther. 2009;38(4):196-205. doi: 10.1080/16506070903318960. PMID: 20183695

Technology Delivered Interventions for Depression and Anxiety in Children and Adolescents: A Systematic Review and Meta-analysis.

Grist R, Croker A, Denne M, Stallard P.

Clin Child Fam Psychol Rev. 2018 Sep 18. doi: 10.1007/s10567-018-0271-8. [Epub ahead of print] Review. PMID: 30229343

Computer therapy for the anxiety and depression disorders is effective, acceptable and practical health care: An updated meta-analysis.

Andrews G, Basu A, Cuijpers P, Craske MG, McEvoy P, English CL, Newby JM.

J Anxiety Disord. 2018 Apr;55:70-78. doi: 10.1016/j.janxdis.2018.01.001. Epub 2018 Feb 1. Review. PMID: 29422409

Internet-delivered transdiagnostic and tailored cognitive behavioral therapy for anxiety and depression: a systematic review and meta-analysis of randomized controlled trials.

Păsărelu CR, Andersson G, Bergman Nordgren L, Dobrea A.  
Cogn Behav Ther. 2017 Jan;46(1):1-28. Epub 2016 Oct 7. Review.  
PMID: 27712544

**Panel response**

*The panel made a conditional recommendation for use of internet CBT for sub-threshold depression. While the panel wished to review different treatment modalities such as Internet-delivered compared to individual more broadly, the appropriate studies were not included in the identified reviews that met inclusion criteria to be able to examine this question.*

General Comment - Please consider iCBT targeting insomnia as a primary treatment for depression:

Internet-Delivered Cognitive Behavioral Therapy to Treat Insomnia: A Systematic Review and Meta-Analysis.  
Seyffert M, Lagisetty P, Landgraf J, Chopra V, Pfeiffer PN, Conte ML, Rogers MA.  
PLoS One. 2016 Feb 11;11(2):e0149139. doi: 10.1371/journal.pone.0149139. eCollection 2016.  
Review. PMID: 26867139

**Panel response**

*While the panel wished to review different treatment modalities such as Internet-delivered compared to individual more broadly, the appropriate studies were not included in the identified reviews that met inclusion criteria to be able to examine this question.*

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**38.**

**Commenter: Jack Novick, Ph.D.**

Comment type: Group Comments

Group name: Association for Child Psychoanalysis

Do you have any other comments about the draft guideline document?

General Comment -

From Jack Novick, Ph.D., President of the Association of Child Psychoanalysis (ACP). The Association of Child Psychoanalysis endorses the comments previously made to the APA Depression Guideline Committee by ACP member Nancie Senet, Ph.D. as follows:

[COPIED AND PASTED DR. NANCIE SENET'S COMMENTS]

**Panel response**

*Thank you for your comments. You are correct in pointing out that there were significant limitations when developing this guideline following IOM standards (e.g., the requirement that a meta-analysis to be included should not be older than 5 years from the date of review). In addition, other problems inherent in the literature (e.g., variability in labeling various interventions obfuscate accurate classifications; poor methodology) further represent limitations. However, it may be important to view this guideline as part of an evolutionary and hopefully transactional process. In other words, such limitations in both the process (i.e., whether IOM guidelines should continue to be used as the gold standard) and content (i.e., the quality of the studies included in a review) may be attenuated as a function of initial attempts at developing guidelines, followed by*

*independent feedback such as that provided by you, that ultimately lead to improvements. For example, it is the panel's hope that delineating such limitations can inform the field (i.e., both researchers and funding sources) to help fill the gaps and correct mistakes in order to ultimately provide for guidelines with few limitations. In the meantime, such guidelines are important to develop within the context of transparency and willingness to articulate extant limitations.*

### 39.

**Commenter: Lynette Sim**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Treatment of adult depression.

By the time most patients reach my office, and the offices of colleagues, they have tried every self help, quick fix "therapy" and to a person is looking for psychodynamic therapy. They refer to it as talk therapy.

Third party payers prefer "evidence based therapies" which are appropriate for surgical procedures but not psychotherapy because thoughts and emotions don't have clear parameters. Psychodynamic therapy guides patients to change.

Treating patients based on insurance company driven algorithms is economics oriented not patient care oriented.

#### **ASC response**

*The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.*

*Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.*

*Expecting any one document to address every consideration that goes into responding to the burden of mental illness and the complexities of the human mind and the social world in which we live and relate to others would doom any initiative to failure; in turn, the argument that because CPGs do not address every aspect of human suffering, CPGs have no value and are dehumanizing seems unfair. We would never claim that CPGs provide a 'one-stop-shop' for clinical care needs (and the introductory section of the guideline clearly encourages use of the guideline in conjunction with clinician judgement and information about clients' preferences and values), but CPGs can nonetheless be a very valuable part of clinical decision making.*



**40.****Commenter: Virginia Shiller, Ph.D.**

Comment type: Group Comments

Group name: Division of Psychoanalysis (39)

Do you have any other comments about the draft guideline document?

Comment: The draft guidelines states that, for children and adolescents: “Based on the literature reviewed that met the IOM and AMSTAR requirements, cognitive behavioral therapy and interpersonal psychotherapy were the only psychotherapy interventions with evidence of efficacy.” However, included in the Zhou et al. review, there is one solid study by Trowell et al. (2007) that looked at the efficacy of psychodynamic therapy and family therapy, and this multi-site study concluded: “The results of this study suggest both Individual Therapy (response rate 74% by End of Therapy) and Family therapy (response rate 75% by End of Therapy) may be more effective in the treatment of depression than other forms of treatment. Previous studies have found a response rate in the region of 60% to CBT and 52–56% to Fluoxetine and 71% to CBT and Fluoxetine combined.” (p. 165) Further, Trowell et al. note: “A significant number of cases in both therapy groups had co-morbid conditions. Almost a third of cases in the study had 3 or more co-morbid conditions. Following therapy, there was a decrease in comorbid conditions, particularly anxiety disorders and conduct disorders, which are often associated with depressive disorders. This occurred in both therapy groups.” (p. 165) One study supporting one of the “recommended” treatments (Wood et al., 1996) noted: “On the other hand, the significance of our results for routine clinical practice is limited by the finding that the DTP did not appear to have any specific benefits for comorbid problems such as anxiety and conduct symptoms. This is important because comorbidity is very common among depressed adolescents. It suggests that in routine practice the DTP should be combined with other forms of treatment.” Further, Wood et al. state: “In interpreting the follow-up findings, we should first emphasize that our study was not designed to detect large differences between the groups at follow-up, but rather to test hypotheses about short-term treatment effects. Some of the differences between the groups at the three-month follow-up would probably have reached significance with a larger sample. However, by the 6-month follow-up these differences were trivial in a statistical sense, and therefore it seems reasonable to conclude that specific treatment effects, as measured by case-control differences, genuinely did not persist.” (p. 744.)

Reviewing details of the studies included in the Zhou et al. meta-analytic article, I am concerned that these recommendations should not ignore the issue of co-morbid conditions, and that longevity of improvements in a very important aspect to assess. And, there is no basis to say there is no evidence that psychodynamic therapy or family therapy is not effective for children and adolescents. References: Trowell, J., Joffe, I., Campbell, J., Clemente, C., Almqvist, F., Soininen, M., Koskenranta-Aalto, U., Weintraub, S., Kolaitis, G., Tomaras, V., Anastasopoulos, D., Grayson, K., Barnes, J., & Tsiantis, J. (2007). Childhood depression: a place for psychotherapy An outcome study comparing individual psychodynamic psychotherapy and family therapy, *Eur Child Adolesc Psychiatry* 16:157–167 DOI 10.1007/s00787-006-0584-x) Wood A, Harrington R, Moore A (1996) Controlled trial of brief cognitive behavioural intervention in adolescent patients with depressive disorders. *J Clin Psychol Psychiatry* 37:737–746), it is not clear why CBT is considered more effective. F

**Panel response**

*We understand that it is desirable to see the Trowell et al. study as demonstrative of the effects of these treatment approaches. However, it is important to acknowledge research methodology experts who state that inclusion of a control condition is the only way to know whether these youth are getting better because of these specific treatments or whether they would have gotten better with time alone or any other treatment. The panel appreciates the commenter's perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

Given the paucity of studies of psychodynamic therapy focusing solely on depression, it seems reasonable to look at studies that treat other diagnoses, or individuals with multiple diagnoses which include depression, to gain relevant information regarding the efficacy of psychodynamic psychotherapy. A study by Abbass et al. (Abbass, A., Rabung, S., Leichsenring, F., Refseth, J. S., & Misgley, N. (2013). Psychodynamic Psychotherapy for Children and Adolescents: A Meta-Analysis of Short-Term Psychodynamic Models. *Journal of the American Academy of Child & Adolescent Psychiatry*, 52, 863-875.) looked at 11 studies (9 were RCT's) that compared Short Term Psychodynamic Therapy with either other active treatments or minimal contact controls and standard care. Diagnoses ranged from borderline personality disorder, depression, anxiety, eating disorder, internalizing disorders, and mixed disorders. This review found "preliminary data indicating that STPP may be effective for a range of conditions in children. Moderate to large sustained within group gains were seen across all dimensions examined (i.e., general psychopathology, anxiety, mood disorders, somatic complaints, interpersonal functioning, and personality/behavioral problems) except interpersonal problems, which showed small gains only in follow-up. These changes were also reflected in high remission rates in serious mental disorders where these rates were provided, and where treatment was not 'restricted'.... The effects of STPP were similar overall to those of what were generally robust treatment comparators (p. 871). Importantly, the finding that all within-group effects increased in size in follow-up matches what has been found in studies of TPP with adults; the effect of this intervention appears to be not only sustained over time, but increased in what some have referred to as a 'sleeping effect.' This supports the hypothesis that changes in this brief therapy are persistent and that certain blocks to personal and psychological development are positively affected by these interventions." (p. 873.)

**Panel response**

*We appreciate the reviewer's strong feelings about the effectiveness of psychodynamic therapy. However, we are constrained by the scope and methods for this guideline which is to focus on those studies of patients with depression diagnosis specifically. Finally, we refer you to Pg. vii of the guideline, where the panel indicates that a lack of evidence about a treatment does not imply that that treatment is not efficacious.*

Discussion of Clinical Recommendations - As noted earlier, there is really little or no guidance for children younger than 9, who in the "real world" are often seen either in play therapy or dyadic therapy with parent/caregiver. We would argue that to gain some information, one should look at meta-analyses of play therapy or dyadic therapy with younger children, who may have a variety of diagnoses.

E.g., the Bratton et al. study does this. (Bratton, S., Ray, D., & Rhine, T. (2005). The efficacy of play therapy with children: A meta-analytic review of treatment outcomes. *Professional Psychology: Research and Practice*. 36, 376-391). From abstract, p. 376: "A meta-analysis of 93 controlled outcome studies (published 1953-2000) was conducted to assess the overall

efficacy of play therapy and to determine factors that might impact its effectiveness. The overall treatment effect for play therapy interventions was 0.80 standard deviations. Further analysis revealed that effects were more positive for humanistic than for non-humanistic treatments and that using parents in play therapy produced the largest effects. Play therapy appeared equally effective across age, gender, and presenting issue.”

**Panel response**

*We agree that there are limited studies of interventions for youth under 9 years of age. However, the methodology of the guideline precludes us from including data from meta-analyses that are not specific to depression. We acknowledge the limitations to the guideline.*

Discussion of Clinical Recommendations - We are concerned that there are no guidelines for infants and young children. These children display a variety of symptoms that may not be well described by the DSM depression criteria; very young children’s adjustment is often assessed by measures of attachment security. Given that children with insecure attachments are at greater risk, compared with children with secure attachments, for later receiving diagnoses of internalizing disorders (Groh et al., (Groh, A. M., Roisman, G. I., van IJzendoorn, M. H., Bakermans-Kranenburg, M. J., & Fearon, R. (2012). The Significance of Insecure and Disorganized Attachment for Children’s Internalizing Symptoms: A Meta-Analytic Study. *Child Development*, 83, 591–610.), a preventative approach to depression would potentially emphasize early interventions. There are a number of evidenced-based treatments (some of these are RCT’s) that provide attachment-focused interventions for infants and toddlers that show treatment can help children move from insecurity to a secure attachment; these attachment-focused treatments might be included in the guidelines. Evidence-based interventions are reviewed in Steele & Steele (2018) (Steele, H. & Steele, M. (2018). *The Handbook of Attachment-Based Interventions*, New York: Guilford Press.)

**Panel response**

*The reviewers make good points however, the guideline is not focused on preventive treatments but rather focused on interventions for youth who already have a diagnosis of depression. The guideline followed the parameters outlined for the review in the section on scope of review, which do not include studies on infants nor studies not specific to depression. We recognize the limitations of the guideline throughout the document.*

Discussion of Clinical Recommendations - In Summary, in Table 1 of the Draft Guidelines, regarding Recommendations for the Child and Adolescent Population, we have two significant concerns.

- A. Most importantly, we are concerned that information about the complexities of the research questions still to be addressed are not reflected in the Table 1 Guidelines, and that those interested in a simple “take-away” will be misled. Indeed, in the text the authors spend considerable time speaking to the weaknesses of, limitations of and caveats to their claims, nearly as much as they spend presenting methods and conclusions. So, the simplistic guidelines in Table 1 do not reflect the issues raised in the text. The risk that insurance companies and other seeking to limit treatment will ignore the nuances of the text in favor of the “headlines” is quite worrisome.

**Panel response**

*We recognize that there are limitations to how we can present the information in the tables and the risks identified by the commenter. We will try to make the footnotes and table explanations more visible to prevent the reader from being misled. We have also revised Table 1 to provide separate recommendations for children versus adolescents in separate tables.*

Currently, Table 1 states that for “Initial Treatment,” two psychotherapies, Cognitive-behavioral therapy and Interpersonal therapy, earn the term “Recommend Use” (pp. ES 12-13). We recommend that the Table 1 headings be changed to better reflect the data. We suggest that the guidelines might replace “Recommend Use” with “Treatments which have been the focus of the greatest number of studies regarding their effectiveness, and which show efficacy.” Then, for “Additional psychotherapy recommendations of initial treatment,” instead of “Insufficient evidence for recommendation” we believe that the current state of affairs would be better reflected by a statement such as “Therapies that have been less intensively studied but which show evidence of effectiveness.” This recommendation is based on the fact that there is definitely some evidence that psychodynamic as well as family therapy are effective; there are simply significantly fewer studies which have been conducted using these modalities.

**Panel response**

*We can understand the reviewer’s viewpoints. We believe that the footnotes accompanying the tables makes the same points as the reviewer does regarding the limitations of the data. We have edited the table to better highlight the limitations of the data in a manner making these footnotes easier to find by readers*

- B. Secondly, we feel that Recommendations for the Child and Adolescent Population should be clarified, that these pertain to older children (i.e. age 9 and up) and not to younger children. Whether there should be another section providing preliminary guidance regarding treatment for children younger than 9 (where there aren’t RCT’s specifically for treatment of depression) is a question.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10). As stated in response to other reviewers who have made a similar comment, we will clarify that the recommendations pertain to largely to youth 9 years and older. We do not have review data at this time to make recommendations regarding youth under 9 years of age.*

Process and Method - 1) We question the notion that the only useful data comes from RCT’s:  
 A) Authors of a 2013 study argued “There is evidence that quasi-experimental and observational studies do not yield effect sizes that systematically differ from those of RCT’s” (p. 864 Abbass et al.); and also: “RCTs and non-RCTs did not differ with regard to between-group effect sizes” (also see p. 870.) (Abbass, A., Rabung, S., Leichenring, F., Refseth, J. S., & Misgley, N. (2013). Psychodynamic Psychotherapy for Children and Adolescents: A Meta-Analysis of Short-Term Psychodynamic Models. Journal of the American Academy of Child & Adolescent Psychiatry, 52, 863-875.) [Regarding this point, Abbass et al. also cite: Leichenring, D. Randomized controlled vs. naturalistic studies. A new research agenda. Bulletin of the Menninger Clinic. 2004; 68, 115-129; Shadish, Q., Matt, G., Navarro, A., Phillips, G. The effects of psychological therapies under clinically representative conditions: A meta-analysis. Journal of Consulting and Clinical Psychology, 2000, 126, 512-529.]

B) Further, the methodologies behind RCT's do not take into account the vast array of individual differences which arguably are even more pronounced among young people. There is no consideration of the "What Works for Whom and Why" paradigm. None of these studies fully take into account underlying characteristics of the individual for which we have ample research evidence suggesting their importance (e.g. temperament, attachment style, personality style, locus of control, etc.)

#### **Panel Response**

*Among the central features of RCTs are inclusion of comparison conditions, randomization of participant assignment to conditions, and masking of assignments. As a result, RCTs reduce various forms of experimental bias and provide the clearest evidence for the efficacy or comparative effectiveness of interventions within a population. Also, RCTs often have large samples, which enhances the ability of the study to detect effects of interventions.*

*RCTs have been criticized for including only certain types of participants (e.g., those of particular demographic backgrounds or without comorbid conditions). However, over the last 20 years, many RCTs have included a wider range of participants in an effort to make the results more applicable to real world clinical practice.*

*Despite having more diverse samples, RCTs still may not be able to pick up differential effects across subgroups of participants. To answer questions about differential effects, either much larger RCTs or RCTs focused on particular subgroups are required.*

*Also, although RCTs can indicate whether an intervention works or not, they generally will not be informative about the mechanism by which the intervention works. And RCTs will provide limited information about factors that interact with the intervention to produce specific outcomes in individual patients.*

*Of course, not all RCTs are of high quality. As described above, RCTs are assessed in systematic reviews using a variety of criteria (risk of bias, consistency, directness, precision). Lower ratings on these criteria may be due to issues related to the design, conduct, or reporting of an RCT. In developing APA's guidelines, only high-quality RCTs were used to determine the strength of evidence for the efficacy or comparative effectiveness of interventions.*

*Researchers are exploring other designs, in addition to RCTs, for assessing the efficacy or comparative effectiveness of interventions. These include historical control, case control and single case designs. However, studies using such designs often still carry a high risk of bias and lack generalizability and are not broadly accepted as providing strong support for efficacy or comparative effectiveness.*

Process and Method - The draft guidelines include only one meta-analysis of treatment of children and adolescents (Zhou, X., Hetrick, S. E., Cuijpers, P., Qin, B., Barth, J., Whittington, C. J., ... Xie, P. (2015). Comparative efficacy and acceptability of psychotherapies for depression in children and adolescents: A systematic review and network meta-analysis, *World Psychiatry*, 14, 207-222.) This review combines studies of both children and adolescents. The average age of participants in the studies was 14.7 years. Only 5 of the 52 studies included children as young as 7 or 8 years; some of these studies including 7- and 8-year-olds had small N's overall, so the actual number of children at the younger end of the age range in the

individual studies isn't clear. It therefore seems important to clarify that the Zhou et al. meta-analytic results cannot be presumed to apply to children under approximately 9 years. Also, replication of the results of this meta-analysis would be important before it was used to reach major conclusions about recommendations for therapy.

**Panel response**

*The reviewer makes an important point. We will clarify that although the studies included children as young as 6 years old, most of the children included in the clinical trials were 9 years or older, thus the findings may not be applicable to youth under the age of 9 years of age.*

Process and Method - While the Zhou et al. study concluded that "at post-treatment, only interpersonal therapy (IPT) and cognitive-behavioral therapy (CBT) were significantly more effective than most control conditions " (p. 207,) the authors of this meta-analysis failed to note that one solid study (Trowell et al., (2007), see below) that included children aged 9-15 years showed that both psychodynamic psychotherapy and Family Therapy resulted in significant decreases in depression (to the point that youngsters were no longer clinically depressed.) This study had the benefit of also showing that treatment resulted in a decrease in co-morbid conditions. We feel it is arbitrary to require that studies include a control condition to be considered as evidence of treatment effectiveness.

Trowell, J., Joffe, I., Campbell, J., Clemente, C., Almqvist, F., Soininen, M., Koskenranta-Aalto, U., Weintraub, S., Kolaitis, G., Tomaras, V., Anastasopoulos, D., Grayson, K., Barnes J., & Tsiantis, J. (2007). Childhood depression: a place for psychotherapy An outcome study comparing individual psychodynamic psychotherapy and family therapy. *Eur Child Adolesc Psychiatry*, 16, 157–167 DOI 10.1007/s00787-006-0584-x.

Important conclusions reached by Trowell et al.: "The results of this study suggest both Individual Therapy (response rate 74% by End of Therapy) and Family therapy (response rate 75% by End of Therapy) may be more effective in the treatment of depression than other forms of treatment. Previous studies have found a response rate in the region of 60% to CBT [5] and 52–56% to Fluoxetine [13, 14] and 71% to CBT and Fluoxetine combined [29]." (p. 166)

Also: "A significant number of cases in both therapy groups had co-morbid conditions. Almost a third of cases in the study had 3 or more co-morbid conditions. Following therapy, there was a decrease in comorbid conditions, particularly anxiety disorders and conduct disorders, which are often associated with depressive disorders. This occurred in both therapy groups." (p. 166)

**Panel response**

*We understand that it is desirable to see the Trowell et al. study as demonstrative of the effects of these treatment approaches, however, it is important acknowledge research methodology experts who state that inclusion of a control condition is the only way to know whether these youth are getting better because of these specific treatments or whether they would have gotten better with time alone or any other treatment. The panel appreciates the commenter's perspective that the conclusion is not consistent with current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

Process and Method - One important study was not included in Zhou et al meta-analysis but is relevant to effectiveness of psychodynamic therapy. This study was a methodologically rigorous trial, had a non-inferiority/equivalence result, and included long-term follow-up:

Goodyer, I. M., Reynolds, S., Barrett, B., Byford, S., Dubicka, B., Hill, J., Holland, F., Kelvin, R., Midgley, N., Roberts, C., Senior, R., Target, M., Widmer, B., Wilkinson, P., & Fonagy, P. (2016). Cognitive behavioral therapy and short-term psychoanalytical psychotherapy vs a brief psychosocial intervention in adolescents with unipolar major depressive disorder (IMPACT): A multicentre, pragmatic, observer-blind, randomized controlled superiority trial, *The Lancet Psychiatry*, 4, 1-11.

This study randomly allocated 470 children and adolescents aged 11-17 years with a DSM-IV diagnosis of major depressive disorder to either CBT or short-term psychoanalytical psychotherapy (as well as a brief psychosocial intervention group.) CBT and short-term psychoanalytical psychotherapy were equally effective in decreasing self-reported depression symptoms at end of treatment, and continued to prove equally effective at a follow-up 86 weeks after initiating treatment.

**Panel response**

*We agree that this is an important study, however, it was not published in the timeframe of our review process. The panel appreciates the commenter's perspective that the conclusion is not consistent with some current evidence. While the panel would prefer to comprehensively include all research, it had to set a timeline for ending searches and the panel cannot select more recently published studies with which they are familiar without systematically searching for all relevant studies; otherwise it has a risk of a "cherry-picked" set of data.*

Process and Method - 5) In considering limitations of the Zhou et al. study, we are concerned that current research has not emphasized long-term follow-up. In the Zhou et al study, only 10 of the 52 studies followed patients at least 12 months, and no studies followed children more than 24 months. As the draft guidelines note "The impairments associated with adolescent depression also have found to persist into adulthood and reflect the significant morbidity and lifelong impairment associated with the disorder" and "children with subsyndromal depression are at risk for developing depression diagnoses later in childhood and adolescence." (p. 3 draft guidelines). Should the goal of treatment be simply to work to remit current symptoms, or might it be more beneficial to offer a longer intervention that not only results in remission of symptoms but addresses underlying factors that may result in new episodes of depression later in childhood, adolescence, or adulthood? While we do not have the data to address the question as to which treatments are most likely to limit or prevent further episodes of depression beyond two years, we should be careful to acknowledge that this is an important issue and that current knowledge should not discourage provision of longer-term or more in-depth treatments that may prove beneficial for long-term mental health.

**Panel response**

*The reviewer brings up important points about the course of depression in youth and the limited knowledge that we have about long-term effects of the different treatment approaches. The question regarding the benefits of offering a longer-term intervention to prevent new episodes in the future remains an empirical question. We agree that we can acknowledge that understanding which treatments are most effective in preventing further episodes is still in need of investigation. Reviewing this literature was outside the scope of this guideline.*

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**41.****Commenter: Nancy Burke**

Comment type: Group Comments

Group name: Psychotherapy Action Network Do you have any other comments about the draft guideline document?

General Comment - A Case of Mistaken Identity: A Plea for the Funding of Useful Psychotherapy Research Masquerading as a Treatment Guideline

[COPIED AND PASTED PSIAN LETTER]

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

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**42.****Commenter: Ronald B. Miller, PhD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am writing in strong support of the extensive response to the APA Clinical Practice Guideline on Depression submitted by The Psychotherapy Action Network (PsiAN). I have read their response carefully, and it calls for APA to radically re-think the prevailing research paradigm that has grown out of the APA Boulder-model research minded doctoral programs. This model is mired in a view of scientific research that is still overly influenced by the logical positivism of the early 1900's, and fails to consider the fundamental difference between testing a theory and testing a professional practice or any human action in the real world. The Guidelines also fails to take into consideration the APA's policy statement of the early 2000's endorsing "evidence based practice" rather than "empirically based practice."

Ronald B. Miller, PhD  
Professor of Psychology  
Saint Michael's College  
Director, MA Program in Clinical Psychology APA Fellow  
Chair, Vermont Board of Psychological Examiners

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*



**43.****Commenter: Helen Wu**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - This guideline and the PTSD one do not address the actual challenges clinicians have in treatment. A high functioning and motivated patient with little or no maladaptive personality traits would likely do well in any of the main therapy options available. When I see the word "guideline," I expect advice and help in what is actually challenging in treatment. I want help in working with the non-motivated patients who have maladaptive personality traits in combination with whatever other diagnoses that they may have. This is what we should be spending our efforts on and this is what would be of actual practical use.

**Panel response**

*Thank you for the feedback. APA produces two types of guidelines: clinical practice guidelines and professional practice guidelines. Clinical practice guidelines focus on specific treatments for disorders/conditions and are based on a systematic review of the literature. Professional practice guidelines focus more on issues of professional practice that are relevant to psychologists and are informed by the literature. The depression guideline is a clinical practice guideline and while it includes some discussion of challenges clinicians have in treatment, its recommendations focus on specific treatments. Currently a professional practice guideline is being developed to address professional practice issues in the treatment of PTSD and another professional practice guideline is in development that addresses the foundations of intervention. These will ideally complement guidance in CPGs.*

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**44.****Commenter: Nell Logan, Ph.D., ABPP**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I want to strongly support the comments by Jonathan Shedler and Nancie Senet!!!! I cannot express the issues nearly as well as they can!!!!

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

**45.****Commenter: Dr. Teresa Rose**

Please use my comments below, the website didn't allow me to correct some mistakes. Thank you.

Begin forwarded message:

From: XX

Date: October 13, 2018 at 3:16:40 PM CDT

To: XX

Cc: XX

Subject: Comment re: Seeking Comments on Draft APA Clinical Practice Guideline for the Treatment of Depression in Children, Adolescents, and Young, Middle-aged, and Older Adults

New comment from Dr. Teresa Rose

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Intended Use of Guidelines - Here is the very first paragraph in this section.

"This guideline is intended to be aspirational and is not intended to create a requirement for practice. It is not intended to limit scope of practice in licensing laws for psychologists or for other independently licensed professionals, nor limit coverage for reimbursement by third party payers."

The audience for this document, based on conclusions found within, is likely ONLY third party payers, licensing bodies and the government. How else do you suppose this document will be used? Very directly in the rest of the document it is clear these guidelines provide virtually NO guidelines for real therapists with real patients in the real world. Reliance on studies of poor quality, with the mythical patient that has exactly one issue is ridiculous. And given APAs history with the DOD and the CIA torture issue, is it not logical to suppose an official APA "guideline" document such as the one here will provide a tiny bit of cover for denial of coverage or benefits, governmental abuse and other problematic uses?

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

Just with this huge disclaimer alone I would suggest starting over to try and create a document of guidelines that really does address treatment in the field, hospitals, clinics, agencies, private practice, the VA and others. Real treatments for depression should include humanistic, psychodynamic, psychoanalytic and other human centric treatments. Not manualized mechanical so called therapy as the so called treatment of choice.

**Panel response**

*Psychodynamic therapy is among those treatments recommended by the panel for treatment of depression in adults. The panel agrees that additional research is needed in other types of therapies such as humanistic and specifically comments on this and issues a call for future research to address this.*

The word “aspirational” alone is a farce. Why would guidelines be released that aspire to something. If they are only aspirational it would seem clinically imperative to NOT publish them until they were actually useful to the real and vital treatment of depression.

**Panel response**

*This clinical practice guideline as well as APA’s other clinical practice guidelines are all aspirational because they are not intended to create a standard or requirement of practice in the treatment of the particular disorder or condition.*

Intended Use of Guidelines - “Due to limited resources and timing constraints this guideline does not address screening for depression, assessment of associated comorbid conditions (e.g., suicidality, medical problems), monitoring response to treatment, locus of care, prevention of depression, dose, timing or duration of treatments for depression, costs of treatment, long term benefits of treatment, mechanisms of change, bipolar disorder, or efficacy of treatments for disorders other than depression.”

Another direct quote from your document. In what scientific universe is this document even close to being a guideline for anything???? Again, with real people in the real world, with multiple life challenges, concerns, physical problems, substance use, family issues and a whole myriad of other issues as a human being, this document purports to help exactly not one real person on the planet. It’s hard to see how guidelines for that unicorn person helps anyone but insurers and government with a vested interest in cost cutting, not treatment.

**Panel response**

*The panel is in agreement that treatment must be individualized to the particular patient and the patient’s circumstances. Please refer to the sections on “Individualizing Treatment” at the beginning of the guideline document as well as the section on “Adapting Treatment to Fit the Individual” in the discussion for further information on this topic.*

Executive Summary - Recommendations are then made for CBT or interpersonal treatment. Exactly how can you do so based on your own comments in the intended use section, let alone in the rest of the document? There was nothing in the entire document that specifically references the RELATIONSHIP between therapist and patient as one truly robust finding in most research as a “curative” factor. As a practicing psychologist, I find these guidelines absolutely worthless as they stand with one exception. The call for more research and particularly NOT RCT research. Qualitative, longitudinal, in the field, more anthropological and other human-centric based forms of research are vital if these so called guidelines are to have any use to real practitioners and the culture at large. Medical RCT research is worse than useless at determining the real process of human psychological treatment of human beings. Until APA and corporations come to grip with this fact, millions of people will continue to suffer. Of course APA and corporations and the government have an inherently different stake than patients and practitioners in these guidelines.

These guidelines likely will continue the reprehensible handing control of real treatment to those with a vested interest in limiting or denying help where it is needed most. These guidelines should not be published until real data and information useful in the real world is available. What a disappointing document that will likely do much more harm to patients than provide any help.

**Panel response**

*The panel made recommendations for a variety of treatments in addition to CBT and interpersonal treatment, please refer to all recommendations tables to view the full range of recommended treatments. The panel agrees that the relationship between the therapist and patient is important. Please refer to the panel's discussion of the importance of the therapeutic relationship in the section, "Enhancing Therapeutic Alliance and Other Principles/Processes of Change. The panel also plans to add additional discussion of the therapeutic relationship in the revised guideline document.*

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**46.****Commenter: Betsy Nettleton**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am attaching comments drafted by the Psychotherapy Action Network (PsiAn) as I find them a compelling argument against the adoption of the Clinical Practice Guidelines.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**47.****Commenter: Marguerite Stewart**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I support in entirety the comments of PsiAN (Psychotherapy Action Network) regarding the proposed APA guidelines for treatment of depression.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**48.****Commenter: Linda Michaels**

Comment type: Group Comments

Group name: Psychotherapy Action Network

Do you have any other comments about the draft guideline document?

General Comment - A Case of Mistaken Identity: A Plea for the Funding of Useful Psychotherapy Research Masquerading as a Treatment Guideline

[COPIED AND PASTED PsiAN LETTER]

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

**49.****Commenter: Luciene Sant'Anna Takagi, PsyD, LLC**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - American Psychological Association (APA) Guideline Development Panel deserves congratulations for the significant amount of work involved in addressing depression in children, adolescents, young, middle age and older adults. Sections on the adaptability and generalizability of the recommended treatments to different settings and providers, the limitations of the recommendations, and the implications of following IOM standards are praise worthy. However, one fourth of the pages on these guidelines are on its limitations. Lack of studies that cover a wider breath of psychotherapeutic modalities was noted. Many of the studies included were of low or limited quality. Studies used for child and adolescent depression seemed to suggest that CBT and interpersonal psychotherapy were better than no treatment, but nevertheless, the panel still recommended these two treatment modalities! These guidelines recommendations may influence the public's opinion and the opinion of third party payors (insurance companies) to seek or to cover treatments that are not recommended by these guidelines.

Although these guidelines are an improvement from the PTSD Guidelines, they are still documenting that only certain treatments are more effective than others for a specific disorder. Unlike in medicine, the necessary ingredients for a psychotherapy treatment to be effective, relies less on the results of Randomized Control Trials and more on the non-specific factors. Additionally, for a National Association representing all psychologists, to issue two separate sets of guidelines (clinical practice and professional) with different sets of recommendations, seems not only confusing, but also inefficient.

I am hereby endorsing the PsiAN and other colleagues' statements, opposing the PTSD and Depression Guidelines. Further, the two separate guidelines initiative needs to be urgently revised. If Guidelines for Clinical Disorders are needed, there should be ONE set of Guidelines. It should incorporate different methodologies and modalities, in order to be inclusive of the diversity of public needs, psychologists orientations, and evidentiary sources available.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

**50.****Commenter: Dan Livney**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am substantively in agreement with the comprehensive statement previously sent out by Jonathan Shedler and PsiAN:

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

**51.*****Commenter: Dr. Jonathan Shedler***

The guidelines document offers a careful and detailed critique of the research literature considered for the guidelines, and in fact provide a compelling argument for why APA should NOT be issuing guidelines. There are two major concerns: 1) the guidelines do NOT offer practical clinical guidance, and 2) The guidelines will be misinterpreted by the public, policy maker, and many psychologists. If the guidelines fuel public misunderstanding, this would be a serious public disservice.

1) the guidelines do NOT offer practical clinical guidance According to the information provided by the guidelines document itself: -There is no basis for recommending any bona fide form of therapy for adult depression over any other (even supportive therapy, which I imagine was not designed to treat the causes of depression per se). (p. 47 -Approximately 70% of patients who receive the "recommended" therapies remain depressed or relapse quickly (p. 90) -The measured outcomes do not necessarily dovetail with what patients regard as treatment success (p.57). -It is not at all clear that research on therapy "brands" is a sound investment of research dollars (or that a focus on therapy brands is a sound basis for developing guidelines), since only 17% of improvement is attributable to specific brands/technique, versus 50% for other factors that the studies essentially treat as error variance (p. 62).

2) The guidelines will lead to widespread misunderstanding The major issue is that when APA formally designates a treatment as "recommended for use," most people (who will never read the original source) will take this to mean that the recommended treatments work-- that is, that patients who receive them can expect to get better. And those in our profession who have a partisan agenda will encourage that misunderstanding. This has already happened with the PTSD guidelines. In the PTSD guidelines, prolonged exposure therapy (pet) and cognitive processing therapy (cpt) are designated "highly recommended." However, the overwhelming majority of patients who receive these treatments (approximately two-thirds) still have PTSD after treatment. But this is not how APA has been presenting the findings to the public or to psychologists. A recent article in the APA monitor, for example, stated: "For providers, [the guidelines] offer recommendations that... quickly summarize which treatments have been shown to work for hundreds or even thousands of patients." Given that two-thirds of patients still have PTSD after treatment, it would be at least as accurate to say that the recommend treatments have been shown NOT to work for hundreds or even thousands of patients. the majority of patients. In short, the PTSD guidelines are already being used in ways that mislead and misinform. Now APA plans to officially "recommend for use" therapies that also fail most patient most of the time (given that ~ 70% of patients in the included studies either don't improve or relapse quickly). The likelihood is extraordinarily high that the "recommend for use" designation will be misunderstood by the public. A "reasonable person" (including the general public, the media, policy makers, and many psychologists) will take this to mean that patients who get these treatments can expect to get better. If terms like "recommend" inadvertently fuel such public misunderstanding, that would be a significant public disservice. In fact, the literature offers no basis to recommend \*forms of therapy.\* What the guidelines panel was really evaluating was the methodology/quantity of research studies. That crucial distinction will quickly be lost when the findings and recommendations are disseminated. Instead of issuing recommendations about \*therapies,\* the guidelines document is primarily evaluating methodological strength of research, not strength of treatments. That suggests that the document should not be issues as a "guidelines" document at all, and should more accurately be presented as a "Methodological Review of RCT research on depression." Such a title and

designation would dramatically reduce the risk of widespread public misunderstanding, and the risk of misuse of the document.

**Panel response**

*Thank you for the thoughtful comments. The panel recognizes the deep concern expressed, particularly that the guideline both does not provide meaningful guidance, and that it could be promoted or misused in ways that could cause harm. In relation to the concern that the panel was “evaluating the methodology quantity of research studies” the panel adopted the IOM approach to guidelines, with some modifications as detailed in the guideline, because the members unanimously believed it was the most valid and defensible approach to providing guidance to readers. The fact that the panel ended up recommending a number of treatments in the adult section and fewer in the other two cohorts is a reflection of the growing body of literature across models of treatment in that area, and we hope it will encourage individuals to study treatments in the cohorts that had less available research. The panel also agrees about the importance of shared factors and advocates for future guideline processes to develop methodologies to incorporate the research in this area in an effective manner. The panel discussed this issue extensively throughout the process, leading to the recommendations for future directions.*

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**52.**

**Commenter: Psychotherapy Action Network (PsiAN)**



**A Case of Mistaken Identity: A Plea for the Funding of Useful  
Psychotherapy Research Masquerading as a Treatment Guideline**

The Psychotherapy Action Network (PsiAN) advocates for the availability of psychological treatments that work. Among the manifestations of human distress, the experience of depression is certainly widespread and significant, calling out for effective remediation. For this reason, our organization has had an acute interest in the conclusions drawn by the creators of the Guideline for the Treatment of Depression that was recently issued for comment by the American Psychological Association. PsiAN had offered critique regarding the scientific limitations and usefulness of the APA’s previously-issued Guideline for the Treatment of PTSD, and had hoped that our concerns would be addressed in the Guidelines to follow.

We recognize and appreciate the tremendous amount of work that went into creating this document, as well as the genuine concern of all involved for the welfare of people who suffer with depression. However, as a map for clinicians and their patients as to how to participate in effective psychotherapeutic treatment, this document, like its predecessors, offers virtually no additional insight to the working clinician, and may offer hinderance due to its misleading presentation of its findings. On the other hand, as an overview of the state of mental health research in this country and beyond, it offers a remarkably comprehensive and invaluable snapshot of the biases, inequities and inadequacies in basic research support for the treatment of a distress syndrome that is a major cause of human suffering across race, class and culture.

### **The Guideline as a Clinical Resource**

First off, it is important to note that the resources used to create the evidence-base for the Guideline overview aimed to address, for the most part, not which therapies worked better than other therapies, but which therapies worked better than non-therapies. Were a sub-title for the Guideline to be created, “Which therapies are better than nothing?” would be far more accurate than “Which therapies work best?” The authors of the Guideline themselves acknowledge that their evidence does not support the latter question, but only the former. In fact, at various places in the Guideline text, they explicitly state that they have no basis upon which to discriminate from among various identified “brands” of treatment, and simply cannot say which ones are most (or least) effective. Yet with its Guidelines project, the APA has created the expectation that consumers, researchers and practitioners will learn which, from among the menu of available therapies, they should choose. In highlighting certain treatments to the general public, and to the insurance companies and policy-makers who will likely use the Guidelines to justify funding decisions no matter what the APA claims it intends as their optimal use, the Depression Guideline, as the others, will reinforce the virtual monopoly of a very narrow range of treatment perspectives in terms of research, training and practice upon patients, to their detriment.

Meanwhile, even if readers are sufficiently sophisticated to realize that the Guideline does not speak to the question of most relevance to them, i.e., “Which treatment among those that are out there should I choose?,” but only offers a recommendation as to which treatments have been studied in particular ways and found effective, they will likely not be aware of the Guideline’s principle finding, which is that none of the treatments included in its analysis are sufficiently effective for a sufficient number of people to merit sustained investment, at least according to the measures it values. In fact, the remission rates yielded by the treatments whose findings are included, were they summarized in medical meta-analyses, would be considered woefully inadequate to justify recommendation. To tell patients, “of 8-to-10 of you entering treatment, one will improve more than they would have had they remained on a wait list,” could appear, to a savvy mental health commissioner interested in cost-benefit outcomes, as a stronger recommendation for waiting list therapy than for any of the “brands” whose level of documentation rendered them worthy of the stamp of approval granted to them by the Guideline.

This is not to say that all depression treatments are not effective, but only that this Guideline, like its predecessors, construes the definition of evidence, upon which its conclusions are based, so narrowly as to render documentation of the effectiveness of some commonly-practiced and potentially more helpful treatments virtually impossible. The decision to restrict its data to include randomized controlled trials (RCTs) exclusively, while less directly celebrated in this document than in the PTSD Guideline, nonetheless still shapes the data considered and the recommendations made. Noting that it is artificially restrictive does not alter the bias it introduces into the treatment recommendations. As before, it functions as a guarantee that certain treatments, including humanistic psychotherapy, psychodynamic psychotherapy and psychoanalysis, for instance, will never be considered evidence-based treatments, no matter their capacity to change lives and transform symptoms. Not only would it be impracticable to incorporate a control group into a study of potentially several years’ duration, but it would be unethical to mandate that subjects remain on waiting lists or be restricted to



“treatment as usual” (whatever that means – there is much slippage there) for that duration, even were research funding to be available, which it is not. Epidemiological, outcome and other naturalistic studies are far more appropriate as measures of the effectiveness of many if not most forms of psychotherapeutic treatment, but are ruled out on the basis of their complexity, difficulty to operationalize, and inconsistency with the aspirations of equivalence between psychological and medical research, as dictated by the IOM, that inform the APA’s Guidelines Program. Yet these very studies are the ones which most accurately reflect the actual experiences of service users. When lifestyle factors, relationship and work success and productivity are explicitly ruled out as relevant variables in evaluating treatment, a yawning disconnect opens between the purpose of the Guideline and the concerns of those who are intended to use it. In addition, likely the most relevant question, both from a public health standpoint and from the standpoint of individual experience, is, more accurately, “Which treatments are most helpful *over time*?” What studies show is that a large proportion of even that small percentage of those whose specified treatments offered them more benefit than did “treatment as usual” tend to experience a recurrence of their symptoms within a short time, necessitating a revolving door of intervention that grows increasingly heavy for many discouraged individuals to push open after multiple treatment episodes. The lack of follow-up post-treatment allows such widespread clinical phenomena to be disregarded, giving the illusion that so-called short-term treatments “applied” episodically are actually less time-consuming than are long-term treatments, without any data to substantiate this assumption.

Even within the narrow confines of the Guideline’s format, the scientific validity and integrity of the conclusions it arrives at are of major concern, not only to PsiAN and various others voicing their hesitations on the APA site, but to the authors of the Guideline itself. Indeed, the authors spend nearly as much time speaking to the weaknesses of, limitations of and caveats to their claims (43% of the text, as measured by one PsiAN member) as they do to their methods and conclusions, giving the impression that they are deeply troubled by the lack of robustness of the results put forward in this document. They often highlight the low quality of many of the studies they include, the very limited patient pool to which the findings likely apply, and the multiple factors that prevent their conclusions from being generalizable to most of the people who will likely walk through a therapist’s door. In every population studied, they stress repeatedly those idiographic and idiosyncratic features of treatment that are to be determined by consensus between therapist and patient, but that fall outside the narrow confines of the recommendations provided in the Guideline. They speak to the failure of the studies consulted to offer any understanding of the mechanisms of therapeutic action. They cite the therapist-patient relationship as perhaps the most important factor in therapeutic success while disregarding all of the parameters, including time frame, which could support the growth of trust between practitioner and patient. While it is true that many of these concerns are endemic to research in general (including the difficulty in standardizing treatment protocols, dropouts, exclusion criteria, settings, populations and the like), their unusual magnitude threatens to overwhelm the meager conclusions of the Guideline. In tone, the document alternates between presenting an air of cautionary inconclusiveness on the one hand and a mandate that good clinical practice *requires* consultation of the Guideline on the other. ***In fact, we believe this document can only be useful to clinicians if no specific treatments are recommended.***

In summary, this latest addition to the APA Guideline family only increases our conviction that the Guidelines Program itself is misguided and ineffectual at its very essence, and even further, that it perpetuates a view of psychology as reductionistic, scientific, essentializing and even, in its application, dehumanizing. The disappointing conclusions of each of the Guidelines put forth so far highlight the problematic consequences of attempting to reduce human distress to compendia of symptoms, sacrificing the power and stability of meaningful diagnostic distinctions and accounts of therapeutic action to the expediency of funding and publication cycles.

### **What the Guideline Shows**

For the most part, the Depression Guideline is most useful as it highlights which of the commonly practiced treatment modalities have been most frequently studied and which have been underrepresented on the investigational grid. In the Guideline, endorsements are made solely on the fact that given treatments have been researched, rather than upon evidence that they are better than those that have not been. Thus, the Guideline offers both a demographics of research support and an overview of the modal characteristics of the studies that receive funding and publication. Along with its sibling Guidelines, it throws into stark relief the disparity between clinical psychology as it is researched and clinical psychology as it is practiced, between symptom-focused and person-focused interventions, between interventional and developmental treatments, and between short- and long-term investigational horizons. The true takeaway from the Guidelines project is that the inequity in funding for basic inquiry in clinical psychology, along with the use of IOM guidelines to restrict what research is considered, narrows the pool of endorsable treatments to the point where only a few disappointing performers can be put forward as options to the public, and the insights offered by psychology's premier professional organization contain almost more caveats than assertions. In noting that the research evaluated has discovered very little in the way of successful treatment of depression and that it hasn't adequately understood why this is, the Guideline can be interpreted as evidence that RCT studies of groups sharing a diagnosis based on a symptom checklist, treated by manualized protocols, are a dead end.

Just as in other, yes, scientific fields, results-driven, short-horizon parameters are impoverishing our ability to speak to basic questions such as the way psychic suffering is metabolized, and how it can best be described, theorized and treated. These parameters are nowhere manifested more clearly than in the academy, where pressures upon students and faculty alike to generate research data quickly has skewed both the research and clinical aspects of clinical psychology training, creating an echo chamber that leaves many psychologists simply unaware of generational blind spots and their legacies of amplification. The Guidelines point us towards a recognition that current funding models, in which RCTs are institutionally and federally supported while more intricate and long-term clinical research models are left to be self-funded, parallels a suggestion made in the Guideline that inequities in the availability of psychotherapy to underserved groups be remedied by clinical psychologists (whose real income has dropped precipitously over the past three decades and who already do much pro bono work) agreeing to see more patients at a low fee. We must not allow this message to be the dominant one coming from the mouth of an organization that claims to speak for psychology as a discipline. In short, the questions the Guidelines Project poses cannot be answered without a re-visioning of psychological treatment and thus of the mission of psychology *as a whole*. May the

APA's quest for answers to the important questions the Guidelines Project raises lead us to make clinical psychology truly *psychological* again, and more vital, diverse, authoritative and transformative in the process.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

**53.**

**Commenter: Evan Mayo-Wilson**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Process and Method - Congratulations on completing this major undertaking! My comments focus on (1) the quality of the evidence used in this guideline and (2) the relationship between the recommendations and the evidence.

Foremost, I was glad to see the guideline is based on systematic reviews. However, I noticed that the reviews came from a small number of authors. Was there a systematic search for systematic reviews? For example, the Cochrane Collaboration produces systematic reviews that I would have expected to be relevant. Additionally, the presentation of results from the included reviews was inconsistent. For example, Zhou 2015 and Cipriani 2016 are both NWMAs, yet the results from Zhou are summarized qualitatively (e.g., "No significant differences", a meaningless statement) while results from Cipriani are presented as effect sizes with precision. The methodology for identifying, selecting, and synthesizing evidence from existing reviews is unclear and appears to be limited.

**Panel response**

*For the older adult population, a systematic search for systematic reviews was conducted by an evidence-based practice center. For the general adult population, the panel started with a systematic review conducted by an evidence-based practice center. The panel identified gaps in the systematic review and completed a prioritization survey to prioritize gaps. After identifying the highest priority gaps, panel members with expertise in this subject recommended existing systematic review to address those gap areas, a systematic search was not conducted. All recommended reviews were subjected to an AMSTAR quality review before the panel determined whether or not to use that review. Likewise, for the child/adolescent population, reviews recommended by panel members were subjected to an AMSTAR quality review. The feedback you raise though is an excellent idea to consider for future guidelines. Advisory Steering Committee members are also considering whether to solicit additional public feedback on existing reviews that could be used by panel members.*

*Effect sizes with precision were presented in the grids or decision tables for nine out of the ten reviews. For the remaining review, the Zhou et al. (2015) review, the panel decided to work directly from the figure presented on page 216 of the review. The panel made this decision for two main reasons: to enhance the efficiency of the process due to the large volume of effect sizes (over 130 effect sizes) in this review and also for enhanced ease of viewing and making comparisons among this large volume of effect sizes given the clear graphical presentation of these effect sizes and their precision in the figure on page 216 of Zhou et al. (2015). We will add a note to explain this to*

*readers. As the panel's process evolved over several years working on this guideline, consideration was given to ways to enhance the process, including efficiency as the process evolved. Lessons learned over the course of developing this and previous guidelines will help to inform APA's future guideline development efforts.*

Process and Method - Because the guideline seeks to answer the question “which intervention is best” rather than “is intervention X better than nothing”, the ideal method would be to conduct NWMAs including all the interventions one might consider for each problem/population. Eyeballing the overall evidence from a bunch of systematic reviews is suboptimal for reasons that must have been obvious to the panel. An overarching NWMA could help answer the question of whether CBT or fluoxetine leads to greater reductions in symptoms. It is important to know which of the recommended psychotherapies is most effective, yet the recommendations say little about the size of their comparative effects. The next guideline could consider identifying or sponsoring an overarching NWMA for adults and one for children/adolescents. I hope APA will embrace scientific methods to determine which psychotherapies are most effective.

**Panel response**

*Thank you for your comment and suggestion. We agree that NMAs can better answer the question which treatment is best. We did not, however, conduct a systematic review or meta-analysis ourselves, but used existing meta-analyses. We followed the IOM guidelines for developing this guideline and searched the literature for existing meta-analyses that could help us answering our questions. At that time, no NMA met our quality criterion (including the Barth et al., Plos Med 2013 NMA). There is no doubt that future updates of the guideline will certainly include NMAs.*

Recommendations - Some recommendations go beyond the data or are based on shoddy evidence. Notably, the interpretation of evidence about subgroups is sometimes unjustified. For example, the recommendation for people who are “also experiencing relationship distress” is strange because the guideline does not identify any evidence that Problem-Focused couples therapy is better than other therapies for this subpopulation; evidence that Problem-Focused couples therapy is better than wait-list in a particular subgroup does not warrant recommending it over other possible interventions. The guideline recommends group therapy for older adults, yet the guideline recommends a broad array of psychotherapies for working-age adults without specifying a preference for individual or group treatment. The fact that somebody did a trial of group therapy doesn't mean that individual therapy is less effective and/or acceptable, so the specificity of the recommendation seems unjustified. Also, it would be reasonable to generalize from trials that included younger adults: Why wouldn't we expect the same psychotherapies to work for people 60 years old and people 70 years old? In the absence of evidence that “temporarily homebound African-American adults” are different from other people, there is no need to make specific recommendations about this subgroup. There's little evidence to recommend “life review therapy” for subthreshold depression; it would be better to recommend an intervention backed by a substantial body of evidence or to say nothing (note, the guideline excludes “prevention” and one could argue that interventions for “subthreshold” depression are preventive). It would also be reasonable to say nothing about acupuncture, homeopathy, and similar “complementary” interventions with no plausible mechanisms of action. Several recommendations are too specific (e.g., modality, subgroups) in light of limited evidence.

**Panel response**

*Specific guidance met the parameters set by panel. They are reflective of the variability of the literature reviewed, with some studies focusing on very targeted populations (e.g., depressed individuals in a distressed couple's relationship).*

Recommendations - The guideline goes beyond the data about harms, which wasn't reviewed properly. For example, the guideline should clarify what sources of evidence were used, and the quality of evidence, to support the claim that second generation anti-depressants cause fewer AEs compared with older drugs.

**Response**

*The IOM (2011) standards for guideline development acknowledge that data about harms is not always readily available in RCTs and thus allows for lower quality evidence such as observational studies and clinician report to be used to evaluate harms in clinical practice guideline development. Following this IOM (2011) guidance, the panel used data on harms from observational studies as well as clinician and patient report to gather information on harms in addition to the generally limited available information on harms from the systematic reviews. Due to the nature of this additional evidence it was considered no higher than low/very low quality. We will add a note about this to the guideline. Additional information about the panel's evaluation of harms/burdens is detailed in the guideline document section, "Assessing magnitude of harms/burdens." This section also provides information about the sources of evidence used to evaluate harms/burdens as do the decision tables and grids.*

Also, please define the term "second generation", which is jargon.

**Response**

*We will add a definition to the document.*

I am surprised that the panel considered the evidence "insufficient" to determine whether psychological or pharmacological interventions should be used for the initial treatment of children and young people. There is a substantial body of unpublished evidence (e.g., clinical study reports) and many observational studies about the AEs associated with medications in this and other populations. While the panel focused on published trials and systematic reviews of trials, those sources contain too little information about harms to be used for guideline development. If the panel didn't look at appropriate sources of information about harms, then the guideline shouldn't make claims about comparative effects.

**Panel response**

*The lack of comparative effectiveness data comparing psychological versus pharmacological interventions in the systematic reviews used by the panel for children/adolescents resulted in insufficient evidence for the panel to be able to make comparative effectiveness recommendations for children/adolescents.*

Also, the guideline should recommend caution regarding complementary interventions like St. John's Wort (for which the Cochrane review includes important qualifications).

**Panel response**

*Thank you for this excellent suggestion. We are adding a note recommending caution regarding complementary interventions.*

Recommendations - Some of the “recommendations” don’t read like recommendations. For example, “Problem-solving therapy(group), which was superior to reminiscence therapy (group)” states a “result”, not a “recommendation”. Why recommended problem-solving therapy based on this comparison? Very limited evidence that X is better than Y doesn’t prove that either is superior to Z or to no-treatment.

**Panel response**

*Thank you for the comments. Specific guidance met the parameters set by panel. They are reflective of the variability of the literature reviewed, with some studies focusing on very targeted populations (e.g., depressed individuals in a distressed couple's relationship). The panel has reviewed and edited recommendations for clarity.*

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**54.****Commenter: Alieta-Marie Lynch**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Recommendations – I am a 54 year old white woman with a 29 year old married daughter, a 15 year second marriage and a 21 year career at Yale Law School. I have experienced multiple traumas in my lifetime, some being a physically and mentally abusive first marriage, family of origin issues and the unanticipated though expected death of my mother actually in my arms in November 2013. That latest event sent me into a spin that my tenacity and grit along with unhealthy coping mechanisms could not overcome. I could not even go into a grocery store without major panic attacks at the thought of all the decisions that required. I didn’t know that was the cause at the time. I am healthy enough with amazing medical benefits to realize in May, 2017, that I was in trouble and reached out to therapy. Yoga, meditation, essential oils, diet, rest -nothing was helping me to stabilize. My interpersonal relationships were in distress. I was struggling in my fast pace work environment requiring independent thought. I was overwhelmed, on the brink, the edge, to the point where occasional suicidal thoughts entered my mind. I have turned to therapy throughout my life as needed and experienced excellent and not so helpful therapists. I was in so much pain, resistant to sharing my story again. Lucky for me I found an excellent Licensed Clinical Psychologist who was covered under the insurance plan with unlimited sessions. My continued long term therapy has worked wonders! Under her care, I have been able to clearly see behavior patterns, draw healthy boundaries, and reduce (and hopefully with continued treatment eliminate) my responsive triggered emotions. These results and more would not have been possible in short term therapy for a host of reasons: the development of a trusting, comfortable relationship with psychologist; the change in my ability to receive and process the provided information and guidance; and simply the time to see results combined with the faith and hope that generates. Separate marriage therapy was recommended and started in November, 2017. I see the benefits of long term therapy in that dynamic as well.

**Panel response**

*Thank you for taking the time to share your story. We are very sorry to hear of the difficulties as well as multiple traumas you have endured. We appreciate your feedback on the benefits of long-term therapy and wish you the very best as you continue your journey.*

**55.****Commenter: Dick Kessler**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Intended Use of Guidelines - My comment about the 'draft' depression guidelines applies to all guidelines published by APA for 'guidance' in dealing with specific populations and diagnoses. Based on my experience with such guidelines published for a different profession, such guidelines easily and quickly become standards of practice once the legal profession gets hold of them.

For example, the first time a depressed client commits suicide and the family wants to hold the Psychologist accountable. The attorney for the family will use the "aspirational" guideline as a basis for a measure of 'good practice' and once the family wins, a precedent is established. This precedent is not necessarily a legal one but such aspirational guidelines, like those published by APA, are used by insurance companies as a basis for settlement since most liability insurance companies do not want to go to court due to the expense of litigation, resulting in the aspirational guideline becoming a practical standard of care. And then who actual pays? The insureds through raised premiums and of course the defendant.

I have seen this happen in several cases from another profession and the publishing agency, like APA, is no where to be found when push comes to shove. They just sit back and claim that it is only an aspiration guideline, not a standard of care, resulting in its membership taking it on the chin.

General Comment - My comment about the 'draft' depression guidelines applies to all guidelines published by APA for 'guidance' in dealing with specific populations and diagnoses. Based on my experience with such guidelines published for a different profession, such guidelines easily and quickly become standards of practice once the legal profession gets hold of them.

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I have seen this happen in several cases from another profession and the publishing agency, like APA, is no where to be found when push comes to shove. They just sit back and claim that it is only an aspiration guideline, not a standard of care, resulting in its membership taking it on the chin.

**Panel response**

*Thank you for sharing your concerns. Please refer to p. 9 of the document for the ASC's discussion on this topic.*

**56.**

**Commenter:** *Ellie Muska, MSW, LCSW*

**Comment type:** Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - The panel recommends CBT and Interpersonal therapy for the treatment of depression but does not offer sufficient scientific evidence as to why and excludes any mention of any other type of treatment. This is misleading and lacks any accuracy.

**Panel response**

*The panel recommends numerous treatments in addition to CBT and Interpersonal therapy. For example, for the general adult population the panel notes that the following therapies appear to have comparable effects: behavioral therapy, psychodynamic therapy, and supportive therapy in addition to the aforementioned CBT and interpersonal therapy. Please refer to tables one, two, and three in the guideline document for the full list of all recommendations. Evidence for recommendations is listed in both the justification column of each table as well as in the associated decision table or grid. In order to help the reader better find this information in the decision tables/grid we are adding information to the recommendation tables on which grid or decision table corresponds to the recommendation.*

**57.**

**Commenter:** *Charles M Lepowsky*

Comment: The proposed guidelines do not include any reference to the role of Information Technology (IT) media in patient communications or access to care (e.g., with insurers or care providers). Research demonstrates that there are significant differences in IT fluency and usage between groups for independent variables including ethnicity, annual household income, educational experience, and especially, age. It is recommended that the advisory panel review the literature indicating that the default use of IT media for communicating with all patient populations (e.g., by Medicare) creates a barrier to access to care for specific patient populations (e.g., older adults, especially over age 70). Based on the research mentioned above, it is suggested that assessment of IT fluency/frequency of IT utilization in specific functional domains (e.g., use of health insurance websites) be incorporated into recommended standardized intake assessment, especially for at risk populations including older adults. Such assessment allows individualized treatment planning using communication methods accessible to the patient. Such assessment also makes it possible to identify what IT might be potential resources to assist in the patient's treatment. Finally, such assessment makes it possible to identify areas in which IT is a barrier to access to care for the patient, and whether the patient desires assistance in developing greater IT fluency, or other methods would better serve the patient for communication and treatment purposes.

**Panel response**

*The commenter raises an important concern, the level at which IT use is a barrier to access to care for specific populations. However, the topic was not a focus of the guideline panel (nor was assessment in general). This is an area the panel would support future efforts to address.*

Comment: The proposed guidelines do not currently include specific references to person-centered practices. It is recommended that the advisory panel review the literature on person-



centered practices and their documented benefit in improving treatment outcomes, and ratings of satisfaction by patients and health care providers.

**Panel response**

*Gaps identified by the panel included an examination of supportive psychotherapy, psychodynamic psychotherapy, subclinical depression, and efficacy of psychological treatments. The panel followed best practices of using reviews current within the past five years and an independent search was not conducted outside of these reviews for additional studies that may not have met inclusion criteria for the reviews.*

*The panel however used an additional AMSTAR evaluated meta-analyses focused on nondirective supportive psychotherapy (Cuijpers et al., 2012) to supplement the initial reviews in order to ensure that person centered, and other supportive treatments were covered. Therapies need to be grouped together and person Centered therapy was included in the supportive treatment grouping.*

*The conclusion was that effectiveness studies demonstrated similar effects across psychotherapy and so person-Centered therapy is included in this. Thus, the panel is not able to recommend specific monotherapies for initial treatment.*

- *General models that appear to have comparable effects include*
  1. Behavioral Therapy
  2. Cognitive, Cognitive-Behavioral, and Mindfulness-based Cognitive Therapy
  3. Interpersonal Psychotherapy
  4. Psychodynamic Therapies
  5. Supportive Therapy

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**58.**

**Commenter: meyer a rothberg, phd**

Comment: Entire Guideline: Much too short.

**Panel response**

*Thank you for the feedback. We are making some edits which will likely increase the length of the document somewhat though we are more commonly told the document is too long!*

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**59.**

**Commenter: NORA MARKS**

Comment: This looks good. I found the information on the alternative treatments interesting.

**Panel response**

*Thank you very much.*

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**60.**

**Commenter: Julie A. Parsons, Ph.D.**

Comment: I am reading these guidelines with an eye to how helpful they will be for practitioners and whether anything in the guidelines can be misconstrued and used to put at risk an individual practitioner who is providing sound treatment albeit outside the recommended

treatments. I was pleased with the easy-to-read format, and by the scope of the discussion sections which addressed many of the salient treatment issues with regard to treating depression, as well as the acknowledgment of the limitations of the reviewed research. These discussion sections do allow individual practitioners considerable leeway in providing a range of treatments without being contra-indicated by the Guidelines. This leads me to a discussion of several issues which I think are important, but were only briefly or indirectly touched upon, i.e. 1. the body of outcome research which the Guidelines describe as outdated or not meeting IOM standards but which nevertheless contribute to a collective body of knowledge that informs many of us; and 2. the research on common factors which may account for more of the outcome variance than specific treatment modalities.

With respect to the outdated research I wonder if a historical section in the Guidelines could summarize some of this research...which continues to contribute to our understanding of the overall efficacy of therapy. It has been nearly 40 years since I was a graduate student, but I still recall the findings of specific outcome studies, including research that suggested the level of experience of the therapist was related to outcome as was the degree of commitment and enthusiasm the therapist had regarding his/her chosen modality. Many of these findings apply to the treatment of depression. Secondly, my ideas about common factors were developed in response to a discussion on the COR listserve (March 9, 2018) and specifically to the comments made by Gregory Jurenec. I strongly support Dr. Jurenec's suggestion that all guidelines contain a discussion of common factors (factors common to credible therapy techniques). The lack of differentiation in outcomes between treatment modalities described in the Depression Guidelines certainly is relevant to this discussion. In summary, I am suggesting an additional section which discusses the historical relevant outcome research, especially that which addresses common factors. This general research (not specifically targeting the treatment of depression) is important when evaluating treatment efficacy today and will put the research on specific modalities and specific patient populations in context.

#### **Panel response**

*Thank you for your compliment. We are pleased that you found the guideline to be helpful. Interesting point, but this was not our goal to determine characteristics of therapists who have better outcome*

*Regarding the suggestion of a historical section describing previous research on psychotherapy, we think that it is a good idea to bring together and summarize all the knowledge on treatments of depression that has been collected over the past four decades. However, to include that in the guideline is not in line with the goal of the guideline, because we not only collected evidence, but we also weighted its' scientific strength. To do that with all previous research is an enormous enterprise, which is not feasible within the context of this guideline. Furthermore, there are now more than 600 randomized trials on psychological treatments of depression, and more than 500 on antidepressants. To summarize that is a huge enterprise. Your example of level of experience of the therapist is not about randomized trials but about moderators of outcome. This is another huge area of research than goes beyond randomized trials and summarizing all that (as well as other relevant research areas) is such a large enterprise that this is clearly not feasible within the context of the development of a treatment guideline.*

*Your second point is about common factors. It is true that most credible treatments result in comparable effect sizes. That cannot, however, be considered as evidence that these effects are caused by common factors. Although trials can show that a therapy works,*

*they cannot show how they work. Showing how a therapy works is scientifically a complicated process that requires much research on different levels (Kazdin, 2007). Furthermore, comparative outcome trials have also several methodological problems, such as the considerable lack of statistical power to find differential effects and the low quality of many comparative trials (Cuijpers et al., 2019). In the case of common factors, it can be doubted whether there is enough evidence that these are indeed the driving mechanisms of therapy (Cuijpers et al., 2019). In depression, for example, there are also other mechanisms that can explain why all therapies have comparable effects. One explanation is for example that depression is a multifactorial problem and that changing one factor may change the whole system but changing another factor may change the system as well. Then the process is different, but the outcome is the same.*

#### References

- Cuijpers, P., Reijnders, M., & Huibers, M.J.H. (2019). The role of common factors in psychotherapy outcomes. *Annual Review of Clinical Psychology*, 15, 207-231. <https://doi.org/10.1146/annurev-clinpsy-050718-095424>
- Kazdin, A.E. (2007). Mediators and mechanisms of change in psychotherapy research. *Annual Review of Clinical Psychology*, 3, 1-27. <https://doi.org/10.1146/annurev.clinpsy.3.022806.091432>

## 61.

**Commenter: John Markowitz, M.D.**

Comment: Thank you for the opportunity to review these guidelines. The more objective they are, the greater credibility they will bear. I'm impressed that you have taken an evenhanded approach, putting aside the ideological conflicts that have too often beset the field. A few very minor points: 1. Page vi, line 6: should be "The term "guidelines" refers...." 2. Page vi, line 21: "arrived at" 3. Page ES-1, lines 7-10: Perhaps you should also consider yet another relevant Cuijpers et al. review: Cuijpers P, Geraedts AS, van Oppen P, Andersson G, Markowitz JC, van Straten A: Interpersonal psychotherapy of depression: a meta-analysis. *American Journal of Psychiatry* 2011;168:581-592 4. Page ES-2, line 4: "there were not enough data"

#### Panel response

*Thanks for your edits. We have referenced a more recent meta-analysis of IPT in the guideline from 2016 (Cuijpers et al., 2016, AJP), which is inclusive of the studies included in the 2011 meta-analysis.*

## 62.

**Commenter: Anthony Quintiliani**

Comment: I have read the APA guidelines on depression. Great job - better than the PTSD guidelines re. scope and depth as well as variety of clinical findings.

My one suggestion is to do a deeper dive into the mindfulness-based research, especially two meta-analyses on how meditation (mainly) improved depression, anxiety, chronic pain, and emotion regulation problems (think trauma and addictions). The findings noted a consistent moderate effect, but they were effects!

I am a career clinical health psychologists and an addictions counselor. I have over 35 years of clinical experience in community clinics, schools, and higher education (Graduate Counseling at UVM, etc.). I also have over 35 years of clinical training (doing workshops and graduate courses) in core co-occurring disorders and clinical interventions. I am past President of the VPA. In my many years of practice I have found that perfectly effective Psychologists tend to NOT read about and know clinical intervention and outcome research outside their personal clinical preferences - mainly CBT and psychodynamics.

Please take another look at the mindfulness area. Thanks for considering this request.

**Panel response**

*The panel thanks Dr. Quintiliani for the constructive and thoughtful comment. The panel will not be able to expand the scope of literature reviewed for the current guideline. The panel had explicitly included mindfulness-based interventions in the search criteria, and mindfulness-based interventions are included among recommended treatments. We will review and edit the text to assure that it is clear that these interventions were included.*

*We thank Dr. Anthony Quintiliani for his comments and suggestion to elaborate on mindfulness meditation as a treatment for depression. The panel will not be able to expand the scope of the literature reviewed for the current guideline, because to do so would undermine the process upon which it was based. However, the panel had explicitly included mindfulness-based interventions in the search criteria, and mindfulness-based interventions are included among recommended treatments. To make this explicit the panel is revising page 14, and page 41; table 2, item 2.b.i.2 to read "Cognitive, cognitive-behavioral, and mindfulness-based cognitive therapy".*

*We do want to acknowledge Dr. Quintiliani's important point that Mindfulness-Based Cognitive Therapy (MBCT) is increasingly being used to prevent depression relapse and to reduce depressive symptoms. Since our original review, two meta-analyses of randomized controlled trials have examined the methodologic rigor and depression outcomes of Mindfulness-Based Meditation Interventions, namely, Agency for Health Care Research and Quality (2014), and Khusid and Vythilingam (in Military Medicine, 2016). The AHRQ publication showed moderate improvement for depression among adults 18 years of age and older. The Khusid and Vythilingam meta-analysis showed that MBCT monotherapy was as effective as maintenance antidepressants for preventing depressive relapses, decreasing residual depressive symptoms, and improving quality of life. Adjunctive MBCT is cost-effective and significantly decreases psychiatric comorbidity, and antidepressant use. The latter review included RCT's up to August 2015, (many of included RCT's were published prior to 2012). Because of the timing of our process, some of these materials could not be incorporated into our recommendations, but the panel does advocate for inclusion of mindfulness-based practices in further guideline development.*

**63.****Commenter: Jeffrey Lyness, MD**

Comment: The guideline says that it does not cover psychotic depression, which is fine. However, I believe the guideline either should explicitly exclude melancholic depression ('major depression, with melancholia'), or should establish separate guidelines for that important subgroup. Melancholic patients do not respond well to psychosocial treatments alone, and thus require medication, ECT (which may be a first choice for the most severe/dysfunctional), or other 'somatic' therapies.

**Panel response**

*Thank you for this comment. Melancholic depression is indeed an important issue. Because of the broad focus of the guideline, we did not focus specifically on melancholic depression. However, if there would have been a systematic review meeting our inclusion criteria and specifically aimed at melancholic depression, it would have come up in our searches.*

*Unrelated to the guideline we would like to point at the review of Baumeister and Parker (2012). This review concludes that there is currently insufficient evidence for specific treatment working better for any subtype, including melancholic depression. We also would like to point at the "individual patient data" meta-analysis comparing CBT and antidepressants, in which no evidence was found that pharmacotherapy is more effective than CBT in melancholic depression (Cuijpers et al., 2017).*

**References**

Baumeister, H., & Parker, G. (2012). Meta-review of depressive subtyping models. *Journal of Affective Disorders*, 139(2), 126-140.  
<https://doi.org/10.1016/j.jad.2011.07.015>

Cuijpers, P., Weitz, E., Lamers, F., Penninx, B. W., Twisk, J., DeRubeis, R. J., Dimidjian, S., Dunlop, B. W., Jarrett, R. B., Segal, Z. V., & Hollon, S.D. (2017). Melancholic and atypical depression as predictor and moderator of outcome in cognitive behavior therapy and pharmacotherapy for adult depression. *Depression & Anxiety*, 34(3), 246-256. <https://doi.org/10.1002/da.22580>

**64.****Commenter: Philip C. Kendall, PhD, ABPP**From: **Philip C. Kendall**

Date: XXX

Subject: Re: Seeking comments on the APA Clinical Practice Guideline for the Treatment of Depression

To: XXX

Hey Beth:

Sorry to be a tad picky, but I was told that there is an incorrect citation to our work in the document. Wanted to provide the correction.

The correct citation is below.

I wasn't sure how best to handle the matter...so I tossed it your way.

I hope that's OK, but I guess you can bounce it back (grin).

Kendall, P.C., Gosch, E., Furr, J., & Sood, E. (2008). Flexibility within fidelity. Journal of the American Academy of Child and Adolescent Psychiatry, 47, 987-993.

phil k

Philip C. Kendall, Ph.D., ABPP  
Distinguished University Professor  
Laura H. Carnell Professor of Psychology  
Temple University

Board Certified in Child and Adolescent Clinical Psychology  
Board Certified in Cognitive and Behavioral Therapy

**Panel response**

*Thank you for catching this. We have made this correction.*

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**65.**

**Commenter: June Feder**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - First, I would like to thank the depression guideline panel members for the extensive work that they did in reviewing the targeted literature related to depression treatment and providing an extensive discussion including summary of data highlighting the seriousness and prevalence of depressive disorder nationwide and beyond, analysis of criteria for selection of foundational studies, and explication of how that analysis led to designated recommendations. The committee is also to be commended for its careful attention to the feedback following the introduction of the initial guideline for the treatment of PTSD which brought into the open widespread concerns including the methodological basis for the selection of foundational studies that limited recommendations to a narrow range of evidence-based approaches which many felt did not provide a sufficient and valid picture about what is actually needed in clinical practice to address complicated psychological and emotional processes most often requiring nuanced, complex and individualized consideration.

The document reflects attention to many of these concerns and they are incorporated into the body of the guideline in numerous places as the authors consistently and repeatedly point out and highlight limitations, qualifications and cautions about using the very treatments they have proposed and headlined. These comments (sampled in selections below) reflect author concerns in fundamental areas of guideline consideration:

On efficacy (ES7,8): "Note that the recommendations pertaining to efficacy do not imply that these treatments are superior to other active treatments."

On systematic reviews (ES3): "While this (utilizing systematic reviews/metanalyses) is consistent with rigorous guideline development, the panel noted this approach can be limiting in that studies exploring the efficacy of psychotherapy are not conducted equally across modalities and are not regularly updated every five years due, in part, to psychotherapy research receiving support from government funds (rather than private companies)."

On the crucial issue of research funding across modalities (ES5): 1. “Much research is highly dependent on federal research funding.....while a large number of studies focused on the efficacy of one approach, Cognitive Behavioral Therapy (CBT), fewer studies have examined other widely utilized treatments including psychodynamic therapy, interpersonal psychotherapy, behavioral activation, problem-solving therapy, and emotion-focused therapy, among others.

On treatment factors beyond immediate symptom relief: (p. 19) " there is a growing body of literature that demonstrates that shared aspects (common factors [e.g., hope, expectancy, therapeutic alliance]) of interventions (Norcross, 2011) relative to model-specific components are germane to optimal treatment outcomes (regardless of the therapeutic orientation implemented). This particularly has been demonstrated for the treatment of major depression."

So herein lie both the strengths and the weaknesses. Along with the panel's laudatory efforts to address research limitations and bring to light cautionary concerns, by doing so the writers raise fundamental questions about the benefits of moving forward on guideline development via a restricted methodological approach which constrains consideration of a broader range of evidence-based modalities, and that serves to limit construction of a more inclusive, integrated treatment paradigm far more suitable and useful as a guide for psychotherapy practice.

In the light of questions about the applicability of these recommendations to actual clinical practice, it is interesting that, from the beginning , the authors turned our attention to consideration of the 2006 report from the APA Presidential Task Force on Evidence-Based Practice (p. vii) “which emphasizes the integration of research, patient values and preferences, and clinician judgement for making treatment decisions” an equation widely viewed as fundamental to best practices for clinical work. I believe it is also relevant to this discussion that when the APA Council of Representatives passed the proposal in 2010 to develop Clinical Practice Guidelines, it was this document that constituted foundational policy for APA practice guidelines. The movement toward the systematic review as the sole methodological underpinning for selection of studies to generate treatment recommendations came out of Advisory Steering Committee discussions in the aftermath of Council approval which, one could argue, might conceivably have affected that outcome if voting members had greater understanding at the time about what form the research basis would take and how the resulting guideline would be shaped. There's no way to know at this point although we do know that when the initial guideline was issued there was a strong and widespread response reflecting member concerns. It was gratifying that in this guideline, the panel itself offered the recommendation of “the development of a workgroup to review guideline development processes to determine if specific modification may be indicated for guidelines related to mental and behavioral health” (p. 80). This is actually in line with the NBI on healthcare practice guideline policy that was submitted in February 2018 by Fred Wertz, PhD and June Feder, PhD currently in the pipeline (NBI 13B/Feb 2018) which has proposed a Council-guided task representative Task Force to review a range of methodologies and approaches and provide recommendations to Council for steps going forward.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

**66.****Commenter: Dr. James Wilk**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

How the APA Clinical Practice Guideline Compares to Other Clinical Practice Guidelines for Treatment of Depression - The APA Draft Guideline makes reference to the UK's NICE Guidelines in respect of a number of areas addressed. However, it is not clear whether Panel members are aware that the NICE Guidelines cited are themselves highly controversial here in the UK, to put it mildly.

Far from being in any sense definitive or respected, the relevant NICE Guidelines (including also the Guideline on the treatment of depression which was due to be published this year but which has been withdrawn in consequence of the serious flaws identified), all remain bitterly contentious, and are widely regarded by clinical psychologists, psychotherapists, medical practitioners and academic researchers in Britain as being clinically of little or no value, in places seriously misleading and, in the view of many distinguished investigators, often risible on scientific methodological grounds.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

On a positive note, and importantly for the Panel's work at this stage of the process, it is these scientific critiques of the NICE Guidelines which would appear to have a direct bearing on the APA Draft Guideline and are of more interest and constructive assistance than the NICE Guidelines themselves. While many references could be cited, since the Panel's time is limited the three most important, concise and directly relevant critiques to date have been the following:

(i) most important of all in terms of its direct bearing on a number of the Draft APA Guideline's specific recommendations, are the findings of Dr Pieter Nel, a leading UK Clinical Psychologist, in respect of the evidence for the use of CBT in the treatment of depression in children and adolescents.

The 2005 NICE Guideline on the treatment of Child and Adolescent Depression is even more controversial than the recent draft NICE Guideline on Adult Depression and has since been thoroughly discredited in a number of areas from a scientific point of view, not least because its recommendation of CBT appears to be based on no evidence whatsoever.

The serious flaws in the 2005 NICE Guideline have been concisely but comprehensively brought to light in Dr Pieter W. Nel's 2014 paper, "The NICE guideline on the treatment of child and adolescent depression: a meta-review of the evidence for individual CBT," published in the European Journal of Psychotherapy & Counselling, 16:3, 267-287, DOI: 10.1080/13642537.2014.929595 The Full Text Manuscript of the published article is also freely available on a Creative Commons license at the University's website: [https://uhra.herts.ac.uk/bitstream/handle/2299/15849/Nel\\_Accepted\\_Manuscript.pdf?sequence=9&isAllowed=y](https://uhra.herts.ac.uk/bitstream/handle/2299/15849/Nel_Accepted_Manuscript.pdf?sequence=9&isAllowed=y)

Dr Nel, an outstanding UK researcher, is Director of Clinical Psychology doctoral training (the DClinPsy degree, the principal Clinical Psychology qualification in the UK)—in the Dept. of



Clinical Psychology, School of Life and Medical Sciences, University of Hertfordshire, one of the leading doctoral clinical psychology programmes in the UK providing Clinical Psychologists to the National Health Service (the Health Service funds the doctoral candidates' training).

Dr Nel's article is rigorous, well argued and highly regarded by professionals and academics in the UK and in Continental Europe, and in my view it merits the Panel's close attention as it concisely and authoritatively addresses many key points in the Draft APA Guideline and, by the same token, could prove potentially embarrassing for the APA to ignore.

While providing compelling argument and data to back up the Panel's assessment of there being insufficient evidence to recommend CBT for children and adolescents, Dr Nel's analysis casts serious doubt on the Draft APA Guideline's assertion on p. 46, lines 13-14, that "IPT-A and CBT still present the best evidence that the field has to date on positive outcomes for the treatment of depression in youth." This remarkable statement needs to be reconsidered, as it could be used illegitimately to provide backing for unproven and potentially harmful treatments for one of the most vulnerable populations.

(In discussion with my University of Oxford colleagues at a meeting here (also in 2014), the 2005 Guideline was cited as a classic example of "policy-based evidence" as opposed to "evidence-based policy," with the NICE Guideline's disgraceful distortion of research findings plainly motivated by the imperative to back cost-based government policy decisions which had already been taken, by citing an evidence-base which, on closer inspection of the source documents, simply did not exist. Dr Nel's paper also refers.)

**Panel response**

*We appreciate the commenter's comments and reference to Dr. Nel's article. However, the methodology of the guideline precludes us from including this article in the empirical basis of this guideline. We acknowledge the limitations to the guideline and we also point to the limitations of the literature within which not enough diversity in treatment modalities has been studied using the AMSTAR and IOM criteria within the time frame set as the parameter for this guideline.*

The other two essential critiques of the NICE Guidelines for our Panel to consider as a matter of some urgency are:

(ii) the damning criticisms made by the UKCP in respect of the current Draft NICE Guideline on depression in adults;

As the Panel may know, the United Kingdom Council on Psychotherapy (or "UKCP") is the principal body for the education, training, accreditation and regulation of psychotherapists and psychotherapeutic counsellors in the UK, irrespective of treatment modality.

UKCP has produced a definitive response to the NICE Guidelines you cite, and on close inspection, their critique would appear to cast serious scientific doubt on the validity of many the APA Draft Guideline's conclusions and recommendations.

As a result of the seriousness of the flaws uncovered, as recently as 18th June 2018 the UKCP were forced publicly to urge NICE not to publish its draft Guideline for Depression in Adults: <https://www.psychotherapy.org.uk/news/nice-urged-not-publish-draft-guideline-depression-adults/>

In April 2018 our medical practitioner colleagues had been equally damning in their critique of the NICE Guideline's flawed methodology, insisting it was dangerous to patient health and that "not only does the guidance ignore the 'severity' of depression at the start of the treatment, but it focuses on short-term outcomes, despite depression often presenting as a long-term condition."

You may or not be aware that the Guideline has accordingly been ditched and that on 2nd October 2018, NICE announced that a replacement Guideline was being prepared and would be submitted to consultation in due course.

I believe our APA Guideline Development Panel will find the UKCP's response to be sobering reading, if they have not yet seen it. The full text of the UKCP's objections submitted in response to the consultation are found in the document, "UKCP Critique of NICE Guideline on Depression," which I have emailed under separate cover, for your convenience, as I could not find a way to attach it here.

(iii) finally, more broadly, the criticism in respect of the overall NICE Guidelines methodology as assessed by UKCP researchers:

NICE's approach to all of their Guidelines, specifically in respect of psychological therapies, has been the subject of a further, well-known critique by the UKCP: Guy, A., Thomas, R., Stephenson, S., & Loewenthal, D. (2011). NICE under scrutiny: The impact of the National Institute for Health and Clinical Excellence guidelines on the provision of psychotherapy in the UK. London: UKCP Research Unit, Roehampton University, Again, I think you will find this makes relevant and sobering reading where our own Draft Guideline is concerned. All of these damning methodological concerns have been echoed, repeatedly, in subsequent responses to NICE consultations not only by psychologists but by psychiatrists and general practitioners.

Again, I have emailed a copy of the UKCP publication to the Panel under separate cover, to save the Panel's time, as these are too important to ignore.

I hope that these comments will prove helpful and constructive in the completion of the Panel's task.

Dr James Wilk, University of Oxford  
Fellow of the New York Academy of Medicine (Psychiatry Section)  
Clinical Fellow AAMFT  
Member of the American Psychological Association  
drjameswilk@icloud.com  
[james.wilk@seh.ox.ac.uk](mailto:james.wilk@seh.ox.ac.uk)

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC statement beginning on p. 5 for discussion of this topic.*

**67.****Commenter: Barry Dauphin, PhD, ABPP**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - The Guideline is clearly the byproduct of a rigorous process and reflects a tremendous amount of dedication on the part of the panel. They are to be commended. Nevertheless, I have numerous concerns, in fact more than I can articulate without devoting an enormous amount of time undertaking. So, I'll address some general concerns about the Guideline process.

In the Executive Summary (ES) it is noted, "Due to limited resources and timing constraints this guideline does not address..." (pg. ES-1), and the ES lists of host of things the Guideline does not address. The list is not exhaustive, of course. But that is part of the point of my comment. Decisions are made at various levels to limit what is considered for the Guideline (exclusion criteria), making it of limited utility to practicing clinicians who are in a position to consider a wider range of variables in each case than any given research study is capable of. I am concerned that the Guideline will be oversold to patients and professionals as more definitive than it can possibly be because all of us have limitations on our resources and time. In the guise of being "helpful", I am concerned that APA will market a highly condensed version of the "bottom line" which will make it appear that truly complex and painful difficulties in living can be satisfactorily addressed briefly, and attempt to persuade patients to want only what is being marketed. I am concerned that the marketing will make it appear that the recommended treatments are more successful than the empirical evidence reveals. I am also concerned that treatment brands will be front and center as opposed to the working alliance.

**Panel response:**

*The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.*

*Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.*

*Expecting any one document to address every consideration that goes into responding to the burden of mental illness and the complexities of the human mind and the social world in which we live and relate to others would doom any initiative to failure; in turn, the argument that because CPGs do not address every aspect of human suffering, CPGs have no value and are dehumanizing seems unfair. We would never claim that CPGs provide a 'one-stop-shop' for clinical care needs (and the introductory section of the guideline clearly encourages use of the guideline in conjunction with clinician judgement and information about clients' preferences and values), but CPGs can nonetheless be a very valuable part of clinical decision making.*

The over emphasis on RCTs is a shortcoming for the scientific development of guidelines and for practicing clinicians. There is a history within the development of Guidelines in medicine for RCT only or RCT –centric research to lead the field astray. For example, Stradling & Davies (1997) [Stradling, J. R. and Davies, R. J. (1997), The unacceptable face of evidence-based medicine. *Journal of Evaluation in Clinical Practice*, 3: 99-103. doi:10.1046/j.1365-2753.1997.00105.x] documented the fact that a highly effective treatment for sleep apnea (Nasal Continuous Positive Airway Pressure: NCPAP) was omitted from treatment guidelines, because it lacked RCT research, while the RCT research that had been conducted did not examine a multitude of problematic outcomes associated with sleep apnea. And yet medicine has much more clear-cut outcomes than mental health will ever have.

RCTs can provide information about very broad outcomes that can be measured in a fairly superficial manner by patients and/or mental health professionals completing the same instruments. That illustrates what is similar about the “outcomes” but does not address what individual patients desire for an outcome. So, yes, RCTs must be part of the development of guidelines, but each case is unique and each therapy dyad (in individual therapy) is unique. In fact, the confluence of variables in each case is enormous. Each therapy can also be considered a “rare” event, more suitable for case study approaches (see: Hoffman, I. Z. (2009). Doublethinking our way to “Scientific” legitimacy: The desiccation of human experience. *J Am Psychoanal Assoc*, 57(5), 1043-1069. doi:10.1177/0003065109343925]. Multiple forms of evidence should be utilized in the development of all guidelines. The impressively thorough document created here will have almost no practical use for clinical psychologists, but they are supposed to take the guidelines fully into account when doing treatment. How are they supposed to take this into “fully” account, by reading hundreds of pages that says nothing about actually working with people? The RCT approach overemphasizes what a group of patients has in common and minimizes to the point of obliteration how they are different.

**Panel response**

*One of the problems when examining the effects of treatments for depression is that many patients with depression recover spontaneously, without any active treatment. One systematic review estimate that 23% of prevalent cases of untreated depression will remit within 3 months, 32% within 6 months and 53% within 12 months (Whiteford et al., 2013). That means that any study without a control group cannot be used to assess the effects of a treatment, because such a study cannot estimate whether participants recovered because of the treatment or because of spontaneous recovery. And when a control group is used, the randomized trial is clearly the best design (compared to no randomization, which introduces all kinds of bias). Although it is clear that RCTs have weaknesses, we still think there is no other design that comes even close when we want to know if a treatment works. Of course, other designs are very useful and needed in examining psychotherapies and depression, but if we want to know if a therapy works, there is no good alternative for the RCT.*

**Reference**

Whiteford, H.A., Harris, M.G., McKeon, G., Baxter, A., Pennell, C., Barendregt, J.J., & Wang, J. (2013). Estimating remission from untreated major depression: a systematic review and meta-analysis. *Psychological Medicine*, 43(8), 1569-1585. <https://doi.org/10.1017/S0033291712001717>

Although the Guideline does not address cost (pg. 23), I propose that cost is a hidden factor in the development of all of the APA Guidelines. “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, Are´an, Craske, Crawford, Kivlahan, et al, pg. 214: Hollon, S. D., Aréan, P. A., Craske, M. G., Crawford, K. A., Kivlahan, D. R., Magnavita, J. J., ... Kurtzman, H. (2014). Development of clinical practice guidelines. Annual Review of Clinical Psychology, 10, 213-241. DOI: 10.1146/annurev-clinpsy-050212-185529). Hollon et al (2014) articulated a detailed rationale for the creation of Clinical Practice Guidelines (CPG) in general, and I would regard that article as seminal in the APA’s Guideline development. In the first paragraph, they noted the rise of healthcare costs as an important consideration for creating guidelines. Costs were not overtly included in the Depression Guideline (or PTSD) process, but they played an implicit role in the development of the APA’s rationale for Guideline development, as most of the authors in Hollon et al (2014) were on the APA Advisory Steering Committee. At least one noteworthy thing is missing from the concern expressed about the “explosion” in healthcare costs, namely the word mental, as in mental healthcare. There is no explosion in mental health costs over the last few decades in contrast to general healthcare costs.

Mental Health and Substance Abuse Treatment have shown downward trends in funding relative to total healthcare spending (both accounting for 9.3% of total healthcare spending in 1986 to 7.3% of total healthcare spending in 2005: National Expenditures for Mental Health Services & Substance Abuse Treatment 1986 – 2005, SAMSA, 2010). Furthermore, prescription drugs only accounted for 7% of mental health spending in 1986, but accounted for 27% of mental health treatment in 2005. On top of that, hospital treatment accounted for another 27% of mental health spending in 2005 (SAMSA, 2010). Thus over half of the costs for mental health services (in 2005) were not associated with outpatient psychosocial interventions and outpatient psychotherapy. Those who work in private practice, group practices, community mental health centers, and other social agencies providing psychological services are not responsible for cost increases. But I am concerned that patients and providers will pay the price of misguided starting assumptions for the development of treatment guidelines.

RCTs are focused on brief treatments. We learn what can happen at a group level in a brief treatment, which is good to know and important to consider, but not nearly sufficient. The hidden role of cost worries affects what gets researched in the first place. Undertaking research for longer term interventions aimed at more complex outcomes is both very expensive and time consuming. It is hard to envision an early career professional being able to become 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.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

I am not convinced that the caveats included in the Guideline are sufficient to prevent them from being used as a standard of care, which could be used against psychologists accused of malpractice (and there are no RCTs for malpractice; that proceeds according to case law). Furthermore, in appraising practice guidelines in medicine, Sox (2014) notes that “...health

insurance companies use guidelines to help decide their coverage policies. (pg. 200).” [Sox, H. C. (2014). Do Clinical Guidelines Still Make Sense? Yes. *Annals of Family Medicine*, 12(3), 200–201. <http://doi.org/10.1370/afm.1657>]. The long term effects of the Guideline could have significant consequences for practicing clinicians and their patients even if it will be hard to discern the link. Folks will wring their hands in the limitations coming from those darned insurance companies without realizing that our own professional organization contributed to the handwringing.

**Panel response**

*The Legal and Regulatory Affairs department at APA routinely works on behalf of psychologist members when it becomes aware that third party payers, health care systems or others are potentially misusing APA policy or not appropriately administering their own policies. In that regard, those with indication of misuse should bring that to the attention of the appropriate offices at APA in order to determine a course of action.*

*In addition, language has been added to this document to specify appropriate legal use and other efforts are underway to promote appropriate use of the guideline across settings and practices.*

The Guideline does not address “7. mechanisms of change” (pg. 23). 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 deceiving 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 scepticism as results from ‘lower’ designs.” (pg. 148). [Sleigh J.W. (1997) Logical limits of randomized controlled trials. *Journal of Evaluation in Clinical Practice* 3, 145-148].

Krauss (2018) analyzed the ten 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! What RCTs for psychotherapy ever approach such “small” Ns? How well are RCTs assessing confounding variables? [Krauss, A. (2018). Why all randomised controlled trials produce biased results. *Annals of Medicine*, 50:4, 312-322.].

As noted by the late Sydney Blatt, “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, pg. 639). [Blatt SJ. The Effort to Identify Empirically Supported Psychological Treatments and Its Implications for Clinical Research, Practice, and Training: Commentary on Papers by Lester Luborsky and Hans H. Strupp *Psychoanalytic Dialogues*. 11: 635-646.]

**Panel response**

*Thank you for your comment. The guideline is indeed about the effects of treatments and not about mechanisms of change. The point you raise about the paper of Sleigh (1997) is, however, also not about mechanisms of change, but about knowing the mechanism of the underlying pathology. You may know the underlying pathology, but that does not automatically say how the treatment works. Unfortunately, in depression there is not much evidence for the mechanisms of the underlying pathology or the mechanism of change. Kazdin (2007) has clearly described how complicated it is to show how a therapy works and there is to the best of our knowledge, no therapy for depression for which the mechanisms of change are clear.*

*Regarding the personal view of Sleigh that the RCT is not a gold standard, we think, as indicated previously, that especially in depression there is no other design that comes even close to RCTs in showing if a treatment works. This is especially true in depression because of the high spontaneous recovery rates.*

*We agree that RCTs on psychotherapies for depression have mostly focused on the effects on depressive symptomatology and less on other outcomes. However, in the development of the guideline, we have also looked at other outcomes, such as quality of life and adverse events. It should also be noted that these decisions have been made by the whole panel, including the patient representatives.*

*Apart from the guideline we want to add that there are quite some RCTs that have examined the effects of psychotherapies on all kinds of other outcomes, such as quality of life (Kolovos et al., 2016), social functioning (Renner et al., 2013), social support (Park et al., 2013), and many other outcomes.*

*We fully agree that RCTs have many limitations, including low statistical power, as is indicated in the Krauss article you refer to. The problem is that other designs also have many limitations. Combined with the problem that other designs do not have control conditions, the evidence from other designs is considerably weaker than the evidence from RCTs.*

*We do not understand the issue of confounding variables, because RCTs are exactly designed to exclude the influence of such confounding variables (through the randomization process) and RCTs are not designed, nor well-suited to examine the influence of confounding variables on outcomes.*

*We agree with Sydney Blatt that it is important to do research on the processes of therapeutic change. However, we are afraid that this underestimates the complexity of research on this process of therapeutic change. Kazdin (2007) has shown that this is a very complex research question, requiring multiple studies with different designs, with does not result in any definite answer about this process. The resources available for the treatment guideline would in no way be sufficient to make even a first start in examining the processes of therapeutic change.*

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- Renner, F., Cuijpers, P., & Huibers, M.J.H. (2014). The effect of psychotherapy for depression on improvements in social functioning: A meta-analysis. *Psychological Medicine*, 44(14), 2913-2926. <https://doi.org/10.1017/S0033291713003152>

It is a tough slog to try to get through the hundreds of pages of the Guideline plus appendices, so what if I conclude by saying, due to limited resources and timing constraints this comment does not address all of my concerns about the Guideline and Guideline process, but it is a good start.

**Panel response**

*Thank you, we appreciate the sense of humor!*

**68.**

**Commenter: Mark Siegert**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - These guidelines are harmful for psychologists, as they, like prior guidelines, continue to imply that only RCT researched psychotherapy is recommended, even when the data from those studies are weak. Importantly, it minimizes all other types of research, which are used by the majority of psychologists, and while caveats are listed, the headlines that APA has guidelines and these are the recommended treatments will harm all patients and clinicians who are very successfully treating these patients, but not based on the weak research from RCT's.

Following through with this is potentially asking psychologists to swallow a poison pill, unless we use only those methods. I support the position taken by the Psychotherapy Action Network.

Mark Siegert, PhD

**Panel response**

*Thank you for your comment. Please also see our answer to the comments from Barry Dauphin, PhD (Commenter 67).*

*One of the problems when examining the effects of treatments for depression is that many patients with depression recover spontaneously, without any active treatment. One systematic review estimates that 23% of prevalent cases of untreated depression will remit within 3 months, 32% within 6 months and 53% within 12 months (Whiteford et al., 2013). That means that any study without a control group cannot be used to assess the*



*effects of a treatment, because such a study cannot estimate whether participants recovered because of the treatment or because of spontaneous recovery. And when a control group is used, the randomized trial is clearly the best design (compared to no randomization, which introduces all kinds of bias). Although RCTs have weaknesses, we still think there is no other design that comes even close when we want to know if a treatment works. Of course, other designs are very useful and needed in examining psychotherapies and depression, but if we want to know if a therapy works, there is no good alternative for the RCT.*

#### Reference

Whiteford, H.A., Harris, M.G., McKeon, G., Baxter, A., Pennell, C., Barendregt, J.J., & Wang, J. (2013). Estimating remission from untreated major depression: a systematic review and meta-analysis. *Psychological Medicine*, 43(8), 1569-1585. <https://doi.org/10.1017/S0033291712001717>

## 69.

**Commenter: Thomas E. Allen MD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Most RCT studies tend to be short term because it is difficult to carry out long term (5, 10, 15, 20 years) RCT studies so we miss the value which has been shown in psychoanalytic studies of the improvement that continues for years afterward but does not happen in other therapies. The limited RCT are used to give false legitimacy to guidelines which are then used to limit benefit coverage. No one asks the question "Are you better off today than you were before these type of guidelines?" because they might have to address the number of mentally ill people in the prison system, the escalating suicide rate, the teenage rage killings, the substance abuse epidemic, etc. etc. Depression is often not a disease in itself but the symptom of a more major "soul sickness" that we ignore at our peril.

#### Panel response

*Thank you for your comment. We agree that the vast majority of RCT's do not have long-term outcome data. As you know, such studies would be very costly to conduct. Plus, such designs need to be careful about controlling for intervening factors that occur posttreatment during a 5-, 10- etc. plus follow-up. We agree that long-term follow-up information across therapies are an important missing piece.*

## 70.

**Commenter: David Antonuccio**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I notice there are some recommendations for augmenting ineffective antidepressant treatment with an additional antidepressant treatment. The few augmentation reviews meta-analyses that exist don't seem to support increased efficacy of augmentation. In addition, it is clear that side effects and risks are enhanced by combining medication. For these reasons, the recommendation of antidepressant augmentation with another antidepressant would seem to be ill-advised and unsupported by the available data.

**Panel response**

*We thank Dr. David Antonuccio for his comment on antidepressant augmentation for the treatment of patients not responding adequately to first-line treatment. While we concur with the spirit of his comment underscoring the need for more controlled RCT's, we would like to highlight a double-blind, randomized, placebo-controlled trial of aripiprazole augmentation of venlafaxine in the case of older adults with major depression who did not respond or responded only partially to venlafaxine. This large multisite clinical trial, sponsored by the NIMH and published by Eric Lenze and colleagues in the Lancet in 2015, demonstrated a significantly higher remission rate (44%) for aripiprazole augmentation compared to placebo augmentation (29%) of venlafaxine. The study also provided detailed medical and neurologic data on the safety of using aripiprazole, with respect to cardiometabolic and neurologic side effects. Other studies have shown the value of augmentation with lithium carbonate. A still open question has to do with the relative efficacy and safety of pharmacotherapy switching versus augmentation strategies in the context of treatment resistant depression. PCORI is sponsoring several ongoing studies of this issue, so the jury is still out, as Dr. Antonuccio suggests. It seems particularly important to examine age as a moderator of relative efficacy versus tolerability and safety of switching and augmentation pharmacotherapy strategies, since the relative balance of positive and negative effects may shift or evolve with age.*

*Dr. David Antonuccio comments on the limited data about the comparative effectiveness of augmentation versus other strategies such as switching classes of medication. He is correct on this point. In the text we indicate the need for further comparative effectiveness research, especially in patients with difficult to treat depression.*

**71.****Commenter: Herbert S. Gross MD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - I am commenting on the guidelines. Psychology still seems trapped in behaviorism. I came of age when psychology was opening up to cognitive models and construing the mind as situated in a brain which is situated in a body and a culture. CBT simplifies by constraining dialogue much as behaviorism simplified the view of the mind/brain/body/culture. Unconstrained dialogue as in generic talk therapies opens itself up to a more complex set of influences- influences associated with unstraightforward coping. These influences are kept implicit- in the background- only to surface later after the short-term benefit from CBT wanes. Thus the recommendation for CBT to the exclusion of more open-ended therapies is seriously misguided.

Herbert S. Gross M.D.

Chair, committee on Government Relations and Insurance

American Psychoanalytic Association

**Panel response**

*Thank you for the comment. The panel made efforts to be broadly inclusive across psychotherapies. While CBT was commonly recommended, the panel does note that a number of other models were also recommended. In addition, the panel does recognize the need for further review of long-term outcomes and supports future efforts to do so.*

**72.****Commenter: Robin Doran, PhD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - The results show that there are no significant differences which means that every treatment

**Panel response**

*We do not understand this comment; it appears to be incomplete.*

**73.****Commenter: Nancie Senet, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - APA's panel that developed this clinical guideline for depression did an excellent job of presenting evidence that disputes the credibility of its own recommendation for the treatment of depression. Additionally, it presents excellent arguments for disputing the usefulness, in general, of developing clinical guidelines using IOM standards that tend to limit the systematic reviews upon which those guidelines are based to ones that include only RCT studies.

There are 199 pages to this depression guideline, 43% of those pages is a discussion of its limitations, limitations that seriously impacted the panel's ability to make treatment recommendations based on good scientific evidence. On page 14 there is a discussion of how the panel attempted to be as broadly inclusive as possible, however, the systematic reviews on which their work was based did not include many important populations. Additionally, the guideline did not include adequate coverage of pertinent research literature. Also, many of the studies included in the systematic reviews, that were the basis of this guideline, were of low quality. Further, accurately labeling the treatment psychotherapies used in the studies was a challenge in that there was a large variability in how systematic review teams clustered particular forms of psychotherapies. Importantly the panel also drew attention to the fact that there were differences in the amount of research evidence available for different therapeutic approaches "(e.g., there is more evidence available for cognitive behavioral therapy than for psychodynamic therapy and hardly any for humanistic therapies)."

**Panel response**

*Thank you for your comments. You are correct in pointing out that there were significant limitations when developing guidelines using IOM standards and best practices for guideline development (e.g., an included meta-analysis should not be older than 5 years from the date of review). In addition, other problems inherent in the literature (e.g., variability in labeling various interventions obfuscate accurate classifications; poor methodology) further represent limitations. However, it may be important to view this guideline as part of an evolutionary and hopefully transactional process. In other words, such limitations in both the process (i.e., whether IOM guidelines should continue to be used as the gold standard) and content (i.e., the quality of the studies included in a review) may be attenuated as a function of initial attempts at developing guidelines, followed by independent feedback such as that provided by you, that ultimately lead to improvements. For example, it is the panel's hope that delineating such limitations can*

*inform the field (i.e., both researchers and funding sources) to help fill the gaps and correct mistakes in order to ultimately provide for guidelines with few limitations. In the meantime, such guidelines are important to develop within the context of transparency and willingness to articulate extant limitations.*

Yet on page 77, referring to recommendations for the treatment of depression in children and adolescents, the panel writes “The best evidence is for interpersonal psychotherapy...and cognitive behavioral therapy (CBT) enabling a recommendation for both of these psychosocial interventions versus no treatment or waitlist, and versus treatment as usual or psychological placebo conditions.” At the same time and in the same paragraph the panel writes that “When the aforementioned treatments (CBT, interpersonal psychotherapy (adapted for adolescents), behavioral therapy, cognitive therapy, family therapy, play therapy, problem-solving therapy, psychodynamic, and supportive psychotherapy) were compared to each other, there was insufficient evidence for the panel to recommend for or against clinicians offering any of the psychotherapies over any of the other psychotherapies listed.”

For the panel to recommend on page 17 two treatments for childhood depression (cognitive behavioral and interpersonal psychotherapy) is misleading. The research studies actually showed that they were better than no treatment while there was no evidence to support that they were better than other therapies. On page 18, the panel states that if neither recommended psychotherapy is available or neither is acceptable to the patient, an alternate model could be considered. The panel warns, though, that there is insufficient evidence to recommend one over another and then lists the psychotherapies appearing in the preceding paragraph for which no evidence had been found for the superiority of one over the other. That list had included CBT and Interpersonal Therapy. Yet the panel now excluded CBT and interpersonal therapy from it, (the same list in which they had previously been listed) and, instead, placed those two therapies into a preferred (recommended status) in relation to the others.

#### **Panel response**

*The committee appreciates the reviewer's concerns. The literature that the committee was asked to review met AMSTAR and IOM criteria (with parameters included as to types of studies and time frame within which studies were conducted). Further, the committee utilized categories consistent with AMSTAR guidelines. Based on these categories, studies were either recommended (strong or conditional) or listed as having insufficient evidence to be recommended. One of the limitations of the research literature in general (including that based on the parameters the committee was asked to consider) is that few psychotherapeutic modalities have been compared in head to head trials. As a result, the committee felt there was insufficient evidence to recommend any treatments as superior to treatments against which they were not compared. Instead the committee elected to add the designation of recommended treatment to only those treatments that demonstrated superiority over no treatment. The committee has noted this important limitation of the literature (lack of equivalence and superiority trials of differing psychotherapeutic treatments for depression) in the document.*

The panel did the exact same thing for the treatment recommendations for older adults: recommending two treatments over other treatments without any research evidence on which to base such a comparative conclusion. Here again, the two recommended treatments happened to have been ones included in the admittedly sparse number of studies from this age group in the systematic review, in which the RCT included only treatment versus no treatment, NOT

comparative effectiveness research. The panel was mixing apples with oranges and coming out with an orange pie that lost the apples.

**Panel response**

*We concur with Dr. Nancy Senet's comment about the need for more comparative effectiveness trials in older adults with major depression. And we would highlight also the implication of her comment calling for subgroup analyses that can address with adequate statistical power the question of which treatment for which patient. Such moderator analyses are rare in clinical trials of older adults but do exist. Absent data on comparative effectiveness, the panel was careful to note the limitation of available evidence in treatment recommendations and generally used caution in highlighting the strength of its recommendations.*

The presentation of the research results in this way misleads the reader into thinking that actual evidence was found to recommend, in the case of child and adolescent depression, CBT and Interpersonal Therapy over the other therapies and in the case of depression in older adults, Group Life Review Therapy and Group CBT over other psychotherapies while at the same time stating that there is no comparative effectiveness research showing sufficient evidence to recommend these therapies over others.

So, yet again, this guideline will fuel misinformation to media, schools, insurance companies, government, etc. that one or two forms of treatment are superior to others and therefore should be considered the recommended treatments. In this case, CBT (as is usual since it is easily fit into RCT studies) and the two other listed therapies for the treatment of depression. What is not highlighted to these users is that these recommendations are not based on valid scientific evidence for their superiority over other therapeutic interventions. It should not be our own professional organization that is promulgating misinformation to clinicians and to the wider public about scientific research findings in psychology.

Moreover, this guideline is putting clinicians in the very difficult position of needing to take seriously this scientifically unsubstantiated clinical guidance. Although the panel states on page 6 that "this guideline is intended to be aspirational and is not intended to create a requirement for practice," it also states on the same page, contrarily, that in considering this guideline's recommendations, APA endorses the statement from the British National Institute for Health and Clinical Excellence (NICE): "...When exercising their judgement, professionals are expected to take this guideline fully into account..."

Challenges in Developing the Guideline and Recommendations for Future Efforts - There is another question to be raised considering the panel's honest presentation of the enormous range of limitations that the authors faced in using the research evidence available to them to produce a guideline for treatment of depression. It is a question generated by the concerns voiced by this panel about the lack of good methodology and reporting in treatment studies. "overall these limitations require caution and conservative recommendations given the gaps in the depression intervention literature. Thus, while the panel makes recommendations based on following the IOM (2011a) criteria for rigorous guideline development, the panel is aware of the limits of its assessment and scope of its recommendations." Page 122. The question is whether the clinical guideline program is, in and of itself, a disingenuous program given that it purports to provide guidance for scientifically based recommendations for treatment to clinicians, yet does not have the science to adequately do so at this time. Why press on with a flawed guideline program when so much more time, money, and energy needs to be devoted to developing

better comparative research methods for psychotherapy? Why press on when the bulk of research about psychotherapy results is considered unusable for developing treatment guidelines because it is not RCT research? The IOM standards were developed to judge the value of medical interventions and are best suited to that goal rather than to the goal of deciding on the comparative value of psychological interventions. Yet APA and other organizations continue to cling to the IOM model of research standards. Those standards don't fit and many of the reasons that they don't fit are clearly articulated by this depression guideline panel. I applaud them for their honesty in assessing and articulating the serious limitations under which they were required to work in order to produce this guideline. Why spend our precious resources on a program of such dubious value?

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

**74.**

**Commenter: Sue Cutler**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - The panel drew attention to the fact that there were differences in the amount of research evidence available for different therapeutic approaches "(e.g., there is more evidence available for cognitive behavioral therapy than for psychodynamic therapy and hardly any for humanistic therapies)." Yet on page 77, referring to recommendations for the treatment of depression in children and adolescents, the panel writes "The best evidence is for interpersonal psychotherapy...and cognitive behavioral therapy (CBT)..." THEN, in the same paragraph the panel writes that "When the aforementioned treatments (CBT, interpersonal psychotherapy (adapted for adolescents), behavioral therapy, cognitive therapy, family therapy, play therapy, problem-solving therapy, psychodynamic, and supportive psychotherapy) were compared to each other, there was insufficient evidence for the panel to recommend for or against clinicians offering any of the psychotherapies over any of the other psychotherapies listed." INSUFFICIENT EVIDENCE

For the panel to recommend on page 17 two treatments for childhood depression (cognitive behavioral and interpersonal psychotherapy) is misleading. The research studies actually showed that they were better than no treatment while there was no evidence to support that they were better than other therapies.

**Panel response**

*We appreciate the reviewer's concern and note that the goal of the guideline is to make recommendations based upon the existing evidence. Given the AMSTAR and IOM guidelines set as the parameters for the committee, the committee elected to describe recommendations using the statement, "The best evidence is for....". The literature that the committee was asked to review met AMSTAR and IOM criteria (with parameters included as to types of studies and time frame within which studies were conducted). Further, the committee utilized categories consistent with GRADE guidelines. Based on these categories, treatments were either recommended (strong or conditional) or, listed as having insufficient evidence to be recommended or in a few cases were recommended against. One of the limitations of the research literature in general (including that based on the parameters the committee was asked to consider) is that*

*few psychotherapeutic modalities have been compared in head to head trials. As a result, the committee felt there was insufficient evidence to recommend any treatments as superior to treatments against which they were not compared. Instead, the committee elected to add the designation of recommended treatment to only those treatments that demonstrated superiority over no treatment. The committee has noted this important limitation of the literature (lack of equivalence and superiority trials of differing psychotherapeutic treatments for depression) in the document.*

The panel did the exact same thing for the treatment recommendations for older adults: recommending two treatments over other treatments without any research evidence on which to base such a comparative conclusion. Here again, the two recommended treatments happened to have been ones included in the admittedly sparse number of studies from this age group in the systematic review, in which the RCT included only treatment versus no treatment, NOT comparative effectiveness research. The panel was mixing apples with oranges and coming out with an orange pie that lost the apples.

The presentation of the research results in this way misleads the reader into thinking that actual evidence was found to recommend, in the case of child and adolescent depression, CBT and Interpersonal Therapy over the other therapies and in the case of depression in older adults, Group Life Review Therapy and Group CBT over other psychotherapies while at the same time stating that there is no comparative effectiveness research showing sufficient evidence to recommend these therapies over others.

**Panel response**

*The committee appreciates the commenter's concerns and proposes the following statement to help reduce confusion:*

*"CBT and IPT are two psychotherapies which have multiple RCTs demonstrating their efficacy in decreasing depression symptoms in adolescents. The comparison conditions in these studies include waitlist control group, supportive psychotherapy/treatment as usual, however they do not include behavior therapy, play therapy, problem-solving therapy, psychodynamic psychotherapy, manualized supportive psychotherapy thus there is insufficient information regarding their comparative effectiveness to a greater number of therapeutic approaches".*

Therefore, this guideline would misinform the media, schools, insurance companies, government, etc. that one or two forms of treatment are superior to others and therefore should be considered the recommended treatments. In this case, CBT (as is usual since it is easily fit into RCT studies) and the two other listed therapies for the treatment of depression. What is not highlighted to these users is that these recommendations are not based on valid scientific evidence for their superiority over other therapeutic interventions. It should not be our own professional organization that is promulgating misinformation to clinicians and to the wider public about scientific research findings in psychology.

**Panel response**

*To address the issues in these comments would be outside the scope of these guideline and we recognize the limitations of the methodology throughout the guideline.*

Moreover, this guideline is putting clinicians in the very difficult position of needing to take seriously this scientifically unsubstantiated clinical guidance. Although the panel states on page

6 that "this guideline is intended to be aspirational and is not intended to create a requirement for practice," it also states on the same page, contrarily, that in considering this guideline's recommendations, APA endorses the statement from the British National Institute for Health and Clinical Excellence (NICE): "...When exercising their judgement, professionals are expected to take this guideline fully into account..."

There is another question to be raised considering the panel's honest presentation of the enormous range of limitations that the authors faced in using the research evidence available to them to produce a guideline for treatment of depression. It is a question generated by the concerns voiced by this panel about the lack of good methodology and reporting in treatment studies. "overall these limitations require caution and conservative recommendations given the gaps in the depression intervention literature. Thus, while the panel makes recommendations based on following the IOM (2011a) criteria for rigorous guideline development, the panel is aware of the limits of its assessment and scope of its recommendations." Page 122. The question is whether the clinical guideline program is, in and of itself, a disingenuous program given that it purports to provide guidance for scientifically based recommendations for treatment to clinicians, yet does not have the science to adequately do so at this time. Why press on with a flawed guideline program when so much more time, money, and energy needs to be devoted to developing better comparative research methods for psychotherapy? Why press on when the bulk of research about psychotherapy results is considered unusable for developing treatment guidelines because it is not RCT research? The IOM standards were developed to judge the value of medical interventions and are best suited to that goal rather than to the goal of deciding on the comparative value of psychological interventions. Yet APA and other organizations continue to cling to the IOM model of research standards. Those standards don't fit and many of the reasons that they don't fit are clearly articulated by this depression guideline panel.

**Panel response**

*Thank you for sharing your concerns. Please see the full response to PsiAN (Commenter 52) and Drs. Shedler (Commenter 51) and Soldz (Commenter 10).*

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**75.**

**Commenter: Gilbert Todd Vance**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Recommendations - I am a certified provider of CBASP for chronic depression. I am surprised to see CBASP categorized as having insufficient evidence to recommend, given that CBASP has been used in multi-site clinical trials since the 1990s. There is a wide variety of types of research indicating CBASP's benefits, ranging from multi-site RCTs to N of 1 studies.

**Panel response**

*Thank you for your comment. Please see the full response to Dr. Penberthy's analysis (Commenter 77).*



**76.****Commenter: Massimo Tarsia, PhD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Recommendations – Dear Panel Members,

I refer to the Table on page ES-13 stating that “there is insufficient evidence to recommend CBASP” for the treatment of adult patients with depression.

I believe this recommendation is at odds with current evidence and in complete divergence from the latest clinical guidelines being developed in Europe.

The European Psychiatric Association recommends CBASP as the psychotherapy treatment of choice for chronic depression.

Jobst A, Brakemeier E.-L., Buchheim A., Caspar F., Cuijpers P., Ebmeier K.P., Falkai P., van der Gaag JR., Gaebel W., Herpertz S., Kurimay T., Sabaß L., Schnell K., Schramm E., Torrent C., Wasserman D., Wiersma J., Padberg F. (2016). European Psychiatric Association Guidance on psychotherapy in chronic depression across Europe. *European Psychiatry*, 33, 18-36.

In the United Kingdom, the revision of the National Institute for Health and Care Excellence (NICE) guidelines for the treatment of depression in adults, due to be published next year, is also set to include CBASP as a recommended psychological therapy for the treatment of chronic depression (Persistent Depression).

<https://www.nice.org.uk/guidance/gid-cgwave0725/documents/html-content>

I trust you will review these guidelines and the clinical research evidence upon which they are based as you continue to work towards producing the final version of the APA guidelines.

**Panel response**

*Thank you for your comments. Please see the full response to Dr. Penberthy's analysis (Commenter 77).*

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**77.****Commenter: Jennifer Kim Penberthy, Ph.D., ABPP**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - Dear esteemed colleagues, thank you for your efforts in putting this draft together and for the work you have done. I would like to point out one the significant omission of CBASP literature supporting its effectiveness in treating persistent depression. Please indulge me in summarizing results from extensive CBASP research and support. CBASP hit the scene in a big way with a large randomized controlled multi-center study of 681 chronically depressed patients at twelve sites across the United States that used CBASP for its psychotherapy arm (Keller et al., 2000). This trial compared the effectiveness of 12 weeks of nefazodone alone, CBASP individual therapy alone, and the combination of both. Patients in the CBASP-alone and combined-treatment groups received 12-16 sessions of psychotherapy. Remission was defined a priori as an HRSD score of no more than 8 at both week 10 and week

12 for those who completed the 12-week protocol and at the time of withdrawal for those who did not complete the study. A satisfactory therapeutic response was defined as a reduction in the HRSD score by at least 50 percent from base line to week 10 and week 12, with a total score of 15 or less at these times but of more than 8 at week 10, week 12, or both for those who completed the study and at the time of departure for those who did not complete the study. The patients with these favorable outcomes were combined to form a single response group. All other patients were considered to have had no response. The results at the end of the 12-week acute phase favored the combination approach. Among the 519 subjects who completed the study, the rates of response were 55% in the nefazodone group and 52% in the psychotherapy group, as compared with 85% in the combined-treatment group ( $P < 0.001$  for both comparisons).

The authors (Keller et al., 2000) conclude that both of the monotherapies yielded similarly efficacious results, with no differences found between them ( $g = 0.04$ ,  $SE = 0.10$ ,  $CI95 [-0.15$  to  $0.23]$ ,  $P = 0.68$ ). The combination of CBASP and nefazadone was more effective in reducing symptoms than either CBASP alone ( $g = 0.54$ ,  $SE = 0.10$ ,  $CI95 [0.35 - 0.73]$ ,  $P < 0.001$ ) or nefazadone alone ( $g = 0.49$ ,  $SE = 0.10$ ,  $CI95 [0.31 - 0.68]$ ,  $P < 0.001$ ). The average effect size comparing the combined treatment to the monotherapies was  $g = 0.52$  ( $SE = 0.10$ ,  $CI95 [0.43 - 0.82]$ ,  $P < 0.001$ ). The effects calculated from mean change scores produced an effect size of  $g = 0.55$  ( $SE = 0.10$ ,  $CI95 [0.37 - 0.74]$ ,  $P < 0.001$ ) for the comparison of combined treatment to nefazadone alone, and for the comparison of combined treatment to CBASP alone produced an effect size of  $g = 0.64$  ( $SE = 0.10$ ,  $CI95 [0.45 - 0.83]$ ,  $P < 0.001$ ). A secondary analysis of the temporal sequence of symptom change showed that the overall advantage of the combined group was attributable to sharing both the earlier onset of benefit seen in the nefazodone-alone condition and the later-emerging benefit seen in the CBASP-alone condition (Keller et al., 2000).

The Keller et al. (2000) trial also implemented a crossover phase for non-responders to monotherapies (61 patients in CBASP; 79 patients in nefazodone). Patients in both arms showed clinical benefits by switching so that, at 24 weeks, their outcomes matched those of the combined group at 12 weeks (Schatzberg et al., 2005). The study had a continuation phase, with twelve monthly sessions added to the acute treatment phase. In this continuation phase, 82 patients who had responded to CBASP in the acute treatment phase were randomly assigned to either once-monthly CBASP sessions or assessment appointments. In the intent-to-treat sample, overall response rates were significantly higher for patients who crossed over to CBASP from nefazodone (57% [35/61]) than for patients who crossed over to nefazodone from CBASP (42% [33/79];  $\chi^2 = 5.03$ ,  $P = .03$ ). There was no significant difference in rates of remission or response without remission between the two groups. In the completer sample, there were no significant between-group differences in the rates of response without remission, remission, or overall response. In the CBASP condition, significantly fewer patients experienced recurrence than in the assessment only condition (Klein et al., 2004).

Another CBASP trial (Kocsis et al., 2009) evaluated a total of 808 patients with chronic depression across eight academic sites. In the first phase of this study, participants received twelve weeks of open-label antidepressant medication according to a pharmacotherapy algorithm similar to the STAR\*D study. In the second phase of the study, patients who had not responded or only partially responded to medication received all next-step pharmacotherapy options with or without adjunctive psychotherapy and were assigned to one of three treatment conditions for another 12 weeks: a medication switch or augmentation, supplementary CBASP, or supplementary supportive therapy (SPT) as active control condition. About 40% of the non-responders in the first 12-week phase later remitted within the second 12-week phase. Remission rates at week 24 were defined by an HAM-D score  $< 8$ , HAM-D score reduction 50% from baseline, and a Clinical Global Improvement score of 1 or 2. Remission rates were 39.5% in the medication augmentation or switch group, 38.5% in the medication plus CBASP group and 31.0% in the medication plus SPT group, and these were not statistically different. This

study failed to find clear advantages of CBASP supplemented with medication over SPT ( $g = 0.18$ ,  $SE = 0.11$ ,  $CI95 [-0.04 \text{ to } 0.39]$ ,  $P = 0.10$ ) or antidepressant monotherapy ( $g = 0.12$ ,  $SE = 0.14$ ,  $CI95 [-0.15 \text{ to } 0.39]$ ,  $P = 0.39$ ). Comparing the efficacy of CBASP with the average effect observed in the control groups (SPT, medication), a nonsignificant and small effect size resulted ( $g = 0.15$ ,  $SE = 0.13$ ,  $CI95 [-0.10 \text{ to } 0.41]$ ,  $P = 0.24$ ). The effect sizes that were calculated from mean change scores of CBASP and SPT ( $g = 0.16$ ,  $SE = 0.08$ ,  $CI95 [0.01\text{--}0.31]$ ,  $P = 0.04$ ) as well as CBASP and medication only ( $g = 0.28$ ,  $SE = 0.10$ ,  $CI95 [0.09\text{--}0.48]$ ,  $P = 0.004$ ) turned out to be small, but significant, respectively. These results should be interpreted with caution, however because the study may have selected for patients with a preference for drug treatment and the number of therapy sessions was low.

A randomized controlled trial compared CBASP with IPT and included thirty non-medicated patients with early-onset chronic depression who were randomly assigned to 22 sessions of individual IPT or CBASP over 16 weeks (Schramm et al., 2011). Observer-rated blinded measurements on the HDRS comparing CBASP to IPT were only marginally significant (HAM-D;  $g = 0.66$ ,  $SE = 0.37$ ,  $CI95 [-0.07 \text{ to } 1.39]$ ,  $P = 0.08$ ). However, CBASP revealed clear benefits in self-reported depression when compared to IPT post treatment (BDI;  $g = 0.85$ ,  $SE = 0.38$ ,  $CI95 [0.11\text{--}1.59]$ ,  $P = 0.03$ ). In the ITT sample, both post-treatment response rates and remission rates were significantly higher in the CBASP arm (64.3% responders, 57.1% remitters) than in the IPT arm (26.7% responders, 20.0% remitters). The effect size derived from the comparison of mean change scores was  $g = 0.62$  ( $SE = 0.34$ ,  $CI95 [-0.05 \text{ to } 1.28]$ ,  $P = 0.07$ ). This was the only study, which provided 1-year follow-up data for a considerable number of patients from the original sample (76%). The depression outcomes were administered 12 months after the last therapy session had been conducted. From pretreatment to 1-year follow-up, a small sized, but insignificant effect size was observed ( $g = 0.41$ ,  $SE = 0.43$ ,  $CI95 [-0.44 \text{ to } 1.25]$ ,  $P = 0.35$ ). The authors concluded that CBASP showed significant advantages over IPT in the group of early-onset and mostly early-traumatized patients and assumed that the specific strategies tailored to approach the therapeutic relationship explained most of the difference arm.

Wiersma et al. (2014) conducted a one-year effectiveness randomized controlled trial of CBASP versus care as usual (CAU) with 139 chronically depressed patients in three clinics in the Netherlands. CAU consisted of psychotherapy treatments generally offered to chronically depressed patients at these sites (CBT: 53%,  $n = 38$ ; IPT 25%,  $n = 18$ ; short psychoanalytic SP: 10%,  $n = 7$ ; supportive/structured therapy: 7%,  $n = 5$ ; pharmacotherapy only: 5%,  $n = 4$ ). Patients attended a mean of 24 CBASP sessions or 23 sessions of CAU and more than 60% of the patients received supplementary pharmacotherapy. Participants were assessed with the self-report version of the Inventory of Depressive Symptomatology (IDS-SR) at baseline and weeks 8, 16, 32 and 52. Response was defined as a 50% symptom reduction in the IDS-SR and remission as an IDS-SR score below 13. CBASP and CAU did not differ from each other on the IDS after 8 weeks ( $t = 0.49$ ,  $p = 0.63$ ), 16 weeks ( $t = -0.03$ ,  $p = 0.98$ ) and 32 weeks ( $t = -0.17$ ,  $p = 0.86$ ) of treatment ( $g = 0.21$ ,  $SE = 0.17$ ,  $P = 0.22$ ;  $g = 0.21$ ,  $SE = 0.17$ ,  $P = 0.22$ ;  $g = 0.22$ ,  $SE = 0.17$ ,  $P = 0.19$ ). At week 52, patients assigned to CBASP had a greater reduction of depressive symptoms compared to patients assigned to CAU (week 52;  $g = 0.55$ ,  $SE = 0.17$ ,  $CI95 [0.21\text{--}0.89]$ ,  $P = 0.001$ ).

In the completer sample, rates of responders (CBASP: 41.2%, CAU: 18.9%) and remitters (CBASP: 26.0%, CAU: 9.4%) differed between the groups, but in the ITT sample, no differences were found for either response (CBASP: 31.3%, CAU: 21.1%) or remission (CBASP: 19.4%, CAU: 9.9%). Comparing the mean change scores of CBASP and CAU, an effect size of  $g = 0.34$  ( $SE = 0.17$ ,  $CI95 [0.01\text{--}0.67]$ ,  $P = 0.04$ ) emerged. Dropout rates were high but the same in both groups, and dropouts did not differ on baseline demographic and clinical variables. CBASP completers were less likely to fulfill DSM-IV criteria for major depression than CAU completers (CBASP: 26.1% vs. CAU: 65.3%) at week 52. This result remained significant in the ITT analysis (CBASP: 49.3% vs. CAU: 76.4%).

A recent multi-site study (Schramm et al., 2015) compared CBASP with escitalopram. Sixty patients with chronic MDD were randomized to CBASP (22 sessions) or escitalopram plus clinical management (ESC/CM). The primary outcome measures were the Montgomery-Asberg Depression Rating Scale (MADRS) score, assessed by blinded raters, and the self-rated Inventory of Depressive Symptoms (IDS-SR) after 8 weeks of treatment. In case of non-improvement (< 20% reduction in MADRS score), the other treatment condition was added for the subsequent 20 weeks of extended treatment. The ITT analysis revealed that clinician- and self-rated depression scores decreased significantly after 8 weeks and found no significant differences between the two rating methods ( $g = -0.29$ ,  $SE = 0.24$ ,  $CI95 [-0.76 \text{ to } 0.18]$ ,  $P = 0.23$ ). The effect size based on mean change scores was  $g = -0.16$  ( $SE = 0.24$ ,  $CI95 [-0.63 \text{ to } 0.31]$ ,  $P = 0.50$ ). Response rates after 28 weeks were high (CBASP: 86.2%, ESC/CM: 93.3%), remission rates moderate (CBASP: 31.0%, ESC/CM: 46.7%) and improvement in global functioning and quality of life significant; none of the differences between the two groups was significant. Comparing the mean changes from 8-week post treatment to 32-week post treatment in the combined group (CBASP + ESC/CM) to the mean changes in the ECS/CM group yielded an effect size of  $g = 0.59$  ( $SE = 0.29$ ,  $CI95 [0.01-1.16]$ ,  $P = 0.04$ ). Accordingly, the comparison of mean changes in the combined group and the CBASP group produced an effect size of  $g = 0.49$  ( $SE = 0.29$ ,  $CI95 [-0.09 \text{ to } 1.07]$ ,  $P = 0.09$ ). After being augmented with the respective other condition, non-improvers to the initial treatment caught up with initial improvers in terms of depression scores and response and remission rates by the end of treatment.

An 8-week randomized controlled trial conducted in Germany (Michalak, Schultze, Heidenreich, & Schramm, 2015) examined the effects of group mindfulness based cognitive therapy (MBCT) and treatment as usual (TAU), group CBASP and TAU, and TAU alone in 106 chronically depressed patients. All participants enrolled met the criteria for PDD (either chronic MDD and/or DD). The primary outcome measure was the HRSD score at the end of treatment. Secondary outcome measures were the BDI and measures of social functioning and quality of life. Remission was defined as a HRSD score of 8 or less at post treatment. MBCT produced an effect size of  $g = 0.42$  ( $SE = 0.05$ ,  $CI95 [0.33-0.52]$ ,  $P < 0.001$ ) compared to TAU. CBASP led to a significantly greater decrease in HRSD scores than TAU, with the average change in CBASP participants being roughly .82 SDs larger than that of the TAU group ( $p < .001$ ). The effect size comparing CBASP to the combined control condition (MBCT, TAU) was of small size and achieved marginal significance ( $g = 0.41$ ,  $SE = 0.24$ ,  $CI95 [-0.06 \text{ to } 0.88]$ ,  $P = 0.09$ ). The mean change effect size comparing CBASP to TAU was  $g = 0.71$  ( $SE = 0.22$ ,  $CI95 [0.27-1.15]$ ,  $P = 0.001$ ). The mean change effect of CBASP and MBCT yielded an effect size of  $g = 0.23$  ( $SE = 0.22$ ,  $CI95 [-0.15 \text{ to } 0.70]$ ,  $P = 0.20$ ) (Michalak et al., 2015).

Recent reviews and meta-analysis of RCTs of CBASP for PDD concluded that there is supporting evidence that CBASP is effective (Jobst et al., 2016; Negt et al., 2016). Specifically, Negt and colleagues (2016) evaluated the six randomized controlled trials reviewed above and assessed the efficacy of CBASP in PDD. A combined overall effect size of  $g = 0.34$  ( $SE = 0.13$ ,  $CI95 [0.09-0.59]$ ,  $P = 0.007$ ) was obtained. Compared to the control conditions, CBASP produced a significant combined effect size of small magnitude. To investigate more general efficacy of CBASP in PDD, a combined effect size was calculated ( $g = 0.34$  (0.13;  $CI95$ : 0.09-0.59;  $P = 0.007$ ). The overall combined effect size obtained from mean change scores of CBASP and the comparison conditions was  $g = 0.44$  ( $SE = 0.07$ ,  $CI95 [0.31 - 0.57]$ ,  $P < 0.001$ ;  $Q = 5.11$ ,  $I^2 = 2.14$   $P = 0.40$ ).

Negt et al. (2016) concluded that the combined overall effect sizes of CBASP versus other treatments or treatment as usual (TAU) demonstrated a significant effect of small magnitude ( $g = 0.34-0.44$ ,  $P < 0.01$ ). The researchers further offer that CBASP demonstrated moderate-to-high effect sizes when compared to TAU and IPT ( $g = 0.64-0.75$ ,  $P < 0.05$ ), and showed trends towards similar effects when compared to antidepressant medication alone ( $g = -0.29 \text{ to } 0.02$ , ns). The combination of CBASP and antidepressant medication demonstrated benefits when

compared to antidepressants only ( $g = 0.49-0.59$ ,  $P < 0.05$ ). Thus, CBASP in combination with pharmacotherapy was found to be more effective than CBASP alone. Based on a network meta-analysis comparing the efficacy and acceptability of several treatment approaches for chronic depression, Kriston, von Wolff, Westphal, Hölzel, and Härter (2014) reported that IPT was less effective than medication (OR 0.48) and CBASP (OR 0.45). Compared to the combined effect size from Cuijpers et al. (2010) review of psychotherapy treatments for DD and chronic MDD ( $d = 0.23$ ), the increased and significant combined effect size of CBASP studies in this same population ( $g = 0.34 - 0.44$ ) is very encouraging.

#### **Panel response**

*Thank you to all the individuals who have raised concerns about the finding of CBASP having “insufficient evidence” to recommend. The panel members were frankly surprised to come to this conclusion, based on the number of well-known studies. In reviewing the evidence that was included in the guideline process, unfortunately several of the studies cited by the commenter above were excluded because they did not meet the minimum threshold necessary (i.e., they were rated as very low quality or insufficient quality based on the guideline process).*

### **78.**

#### **Commenter: Stephanie Launer**

Comment: For pages 8-9 describing the scope of the problem for C&A depression, this comment relates to the paragraph on suicide. I think it is very important here to comment on the increased rates of suicide in youth that have been noted in the past ~10 years. (I don't have the citation for this.)

#### **Panel response**

*We appreciate the commenter's concern to highlight the cost of depression for children and adolescents. Given the media coverage, it seems as though there are increased rates, however the studies consistently find that suicide rates for children and adolescents have actually generally stabilized over the past 20 years or declined (Bridge et al., 2015; McLoughlin et al., 2015; Shain & Committee on Adolescence, 2016). While Bridge and colleagues 2015 reported an increase in suicide among black children, the overall suicide rate for children was stable. According to Shain and colleagues (2016), the suicide rate in teens has decreased by 28% from 1990-2013. We can certainly add this information to the section on depression and suicide.*

#### **References**

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<http://jamanetwork.com/article.aspx?doi=10.1001/jamapediatrics.2015.0465>
- McLoughlin, A. B., Gould, M. S., & Malone, K. M. (2015). Global trends in teenage suicide: 2003-2014. *QJM: An International Journal of Medicine*, 108(10), 765-780. <https://doi.org/10.1093/qjmed/hcv026>

Shain, B., & Committee on Adolescence. (2016). Suicide and suicide attempts in adolescents. *Pediatrics*, 138(1), Article e20161420.  
<https://doi.org/10.1542/peds.2016-1420>

Comment: In comparing the recommendations for C& A populations versus the recs for the adult population, I'm concerned that in the adult section (ES 13) the recommendation for selecting between treatments lists the following recommendations: o Suggest behavioral therapy rather than antidepressant medication alone. o If considering combined treatment, the panel suggests cognitive therapy plus antidepressant medication to improve likelihood of full recovery in treatment. Whereas in the C& A section (ES-9), the statement is made that o Cognitive-behavioral therapy over Interpersonal psychotherapy o When considering medication options and research between different medications, the panel recommends fluoxetine as a first line medication compared to other medications for child/adolescent patients with major depressive disorder. o There was insufficient evidence to recommend either treatment (psychotherapy or fluoxetine) over the other. I am concerned that the underlined statement on ES-9 gives the impression that the risks of fluoxetine and the risks of therapy are similarly negative. I think it would be important to include a comment here to clarify that medications generally have higher risks and potential for negative effects over the mentioned therapies. This could be clarified with something like the following: o "There was insufficient evidence to recommend either treatment (psychotherapy or fluoxetine) over the other; however, it is notable that medications present unique risk factors (side effects, negative reactions, psychological dependence, long term neurodegenerative effects, etc.) that are not associated with CBT or Interpersonal therapy."

#### **Panel response**

*Dr. Stephanie Launer's comments on the relative efficacy and safety of fluoxetine and psychotherapy (CBT) for child and adolescent depression underscores the importance of vigilance for untoward side effects of medication. We concur that this vigilance is a necessary part of ethical clinical practice (especially concerning the possible activation of suicidal ideation by medication in patients under the age of 25). We disagree with her comment that medication use may be associated long term with neurodegeneration. Rather, it is depression per se, not medication, that may be a risk factor for dementing illnesses later in life.*

## **79.**

**Commenter: Nathan D. Tomcik, PhD**

Comment: I see that the committee made the decision not to address screening/intervention for suicidal thoughts and behaviors. It is unclear to me why there is more attention given to recommendations regarding informed consent than there is to suicide risk evaluation. While I understand the rationale for this due to the scope of the document (i.e. all age groups/special populations) in light of the rise of suicides in the country (30% increase in rates since 2005) I would strongly recommend the committee reconsider this and include at least an overview of recommendations for suicide risk evaluation and safety planning.

#### **Panel response**

*The panel recognizes that review and recommendations of best practices for screening, evaluation, and safety planning is a critical area of need. However, the panel did not take this on for two reasons. First, the area of suicide assessment is pan-diagnostic, and the panel thought best addressed as a distinct set of recommendations (as done by the VA/DoD guidelines). Second, the panel, in choosing its scope, focused on intervention*

*rather than assessment (page 33, lines 22-24). While the panel considered suicidality as a critical target and reduction of suicidality an important outcome (page 28, line 19; page 33, line 18), it recognized the need for further guidance in the assessment and management of suicide.*

*To address Dr. Tomick's concerns, the panel will add the following text at page 135, end of line 2:*

*"In particular, there is need for expanded guidance on the assessment and management of suicidal behavior. While beyond the scope of this guideline, recent evidence of increasing rates of suicide across a number of cohorts indicates this is a high priority domain.*

*The committee appreciates the commenter's focus on the importance of understanding the link between depression and suicide. However, the scope of the guideline was to review the depression treatment literature (in the area of psychotherapy) and not to address best practices for evaluating suicide risk and safety planning.*

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## 80.

**Commenter: Deborah Weisinger, PsyD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment – Please see the following document which does not support any evidence that CBT treatment of depression is effective for children and adolescents: Dr. Peter Nel's 2014 paper, "The NICE guideline on the treatment of child and adolescent depression – a meta-review of the evidence for individual CBT", published in the European Journal of Psychotherapy and Counseling, 16:3, 267-287. Although the APA drafts document does say that there is an absence of evidence, nonetheless this document recommends CBT. This seems to me to be an untenable recommendation given that there is not only not evidence to support this position, but evidence that refutes its efficacy. I am concerned that APA continues to pursue a line of recommendations and guidelines for treatments without conducting clear studies and thorough scholarship as to what does help all of our patients. Thank you for the opportunity to comment.

### **Panel response**

*We appreciate the commenter's comments and reference to Dr. Nel's article. However, the methodology of the guideline precludes us from including this article in the empirical basis of the guideline.*

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## 81.

**Commenter: Joan Lavender, PsyD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

How the APA Clinical Practice Guideline Compares to Other Clinical Practice Guidelines for Treatment of Depression - Dr Nel demonstrates that there is no evidence whatsoever for the effectiveness of CBT in the treatment of depression in children and adolescents. While the APA's Draft Guideline correctly refers to the absence of evidence, it nonetheless makes a recommendation for (IPT-A and) CBT which Dr Nel's article, in the case of CBT, can refute.

**Panel response**

*We appreciate the commenter's comments and reference to Dr. Nel's article. However, the methodology of the guideline precludes us from including this article in the empirical basis of the guideline.*

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**82.****Commenter: Susan C. Warshaw, Ed.D, ABPP**

Comment type: Personal Comments Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - Children and adolescents: The entire section is dangerously misleading regarding the treatment of children and adolescents. Neither of the recommended treatments are usable to any substantive degree for children under the age of 9. I am unaware of ANY use of IPT for ages younger than adolescence. Given the acknowledged insufficient evidence based on the criteria used by the Guidelines committee, it is erroneous to suggest that either of the treatments CBT or IPT belong as recommendations for a combined Childhood/Adolescence category. I suggest acknowledging that at the present time, there is insufficient evidence to privilege any one treatment over any other for children below adolescence. There is also a growing evidence base for the use of Short Term Psychodynamic Treatment for Depressed Adolescents, but I see no mention of this significant approach to treatment of depression. It is essential to provide MUCH more acknowledgment (more than simply as a limitation) of the on going process of development which precludes combining children and adolescents into one overall category for statistical purposes. The guidelines reflect a complete lack of awareness that there is a process of development and that depression not only presents differently at different ages, has a substantial degree of co-morbidity, is not a singular process, and needs to be worked with differently depending upon the age of the child or adolescent. Essentially there is no real indication of any awareness that treatment of youngsters, with any diagnosis, varies according to developmental levels...for example the reality that treating a five year old is different from treating an eight year old, or an eleven, or 16 year old etc. There is also a complete neglect of parents as central in the treatment of children as well as adolescents. I think the committee would benefit from including a significant number of individuals who work clinically with children, in addition to those who work with adolescents such that really relevant variables could be delineated. Were there more child/adolescent persons on the committee, the ridiculous notion of combining categories could have been avoided. I find the guidelines naive with respect to any awareness of the processes involved with infant, pre school, child or adolescent psychotherapy, which includes a substantial amount of work with parents. The separation of family therapy, from parent/ child treatment, or parent treatment, again reflects a naive lack of awareness of what is really involved in treatment of children and adolescents.

**Panel response**

*Thank you for this feedback. The panel discussed and agreed that recommendations for children and adolescents should be separated and distinct recommendations made for each population. You will note this significant revision in the table of recommendations. Of particular note the panel decided to no longer include IPT among the recommendations for children and retain this only for the adolescent population.*



General Comment - Honestly, I find the entire process of guideline development premature, given the acknowledgment throughout the document of the insufficient evidence, as well as significant limitations of the available data.

To proceed with the publication of these guidelines, particularly in the areas of Child/Adolescence, as well as Older Adults, would be terribly misleading to the public, and present the profession as lacking a sophisticated awareness of psychotherapy process as relates to developmental stages, life tasks etc. These guidelines seem trapped by the imitations of the methodology to which you subscribe.

**Panel response**

*The panel appreciates the commenter's perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

**[SUBMITTED NEW COMMENT ON 10/23]**

Comment type: Group Comments

Group name: as Editor of the Journal of Infant, Child and Adolescent Psychotherapy

Do you have any other comments about the draft guideline document?

Recommendations - ES9 and ES10

The presentation of the recommendations in chart form, which combines treatment for Children and Adolescents is seriously misleading. There is scant if any evidence for the use of IPT below the age of adolescence. There is little evidence, that CBT is useful in children below the age of 8 or 9, though some cognitive behaviorally oriented treatments do exist for young children, and some variants of CBT may be used, there is scant literature in these areas. All treatments include play as an aspect of the treatment, though the use of play varies depending on the theoretical orientation of the clinician. Thus the use of the term "play therapy" needs to be operationally defined.

There is at least one earlier meta-analysis of which I am aware(if that is your criteria) that completely disagrees with the only meta-analysis (Zhou, X., et al, 2015) which you cite. Dr Pieter W. Nel's 2014 , "The NICE guideline on the treatment of child and adolescent depression: a meta-review of the evidence for individual CBT," published in the European Journal of Psychotherapy & Counselling, 16:3, 267-287, presents substantive critique of the NICE Guidelines for children and adolescents, and calls into serious question the efficacy of CBT for children and adolescents at all.

**Panel response**

*We appreciate the commenter's comments and reference to Dr. Nel's article. However, the methodology of the guideline precludes us from including this article in the empirical basis of the guideline.*

Recommendations - ES9 and ES10

The comments of Dr. Nick Midgely(8/29/18 this list), University College London, need to be taken extremely seriously in their critique of these current APA Draft guidelines. Dr. Midgely and his colleagues are among the most innovative researchers in the area of Child and Adolescent Treatment, having done exhaustive study of the extant treatment research literature. Midgely states " ... these recommendations are inconsistent with the recommendations of the National Institute of Clinical and Health Excellence (NICE) in the UK, which takes a comparably

thorough approach to evaluating the evidence in making its recommendations, but concludes that a broader range of psychotherapies should be considered in the case of child and adolescent depression. Given the significance of the findings of the IMPACT study( referenced below), and the paucity of research on which the Zhou et al. network analysis was based, I would strongly urge that the APA guidelines review and take account of the findings of the Goodyer et al. (2017) study, before making their recommendations regarding the most effective psychotherapies for depression in children and adolescents. If not, we risk limiting the range of therapies offered to children and families, despite evidence that a broader spectrum of approaches may serve the needs of young people more effectively". "REFERENCES: Abbass AA, Rabung S, Leichsenring F, Refseth JS, Midgley N. Psychodynamic psychotherapy for children and adolescents: a meta-analysis of short-term psychodynamic models. *J Am Acad Child Adolesc Psychiatry*. 2013;52(8):863-75. Goodyer, I., Reynolds, S., Barrett, B., Byford, S., Dubicka, B., Hill, J., Holland, F., Kelvin, R., Midgley, N., Roberts, C., Senior, R., Target, M., Widmer, B., Wilkinson, P., Fonagy, P. Effectiveness And Cost-Effectiveness Of Cognitive Behaviour Therapy And Short-Term Psychoanalytic Psychotherapy Compared With Brief Psychosocial Intervention In Maintaining Reduced Depressive Symptoms 12 months after end of treatment in Adolescents with Unipolar Major Depression (IMPACT): A Pragmatic Superiority Randomised Controlled Trial. *Lancet Psychiatry*, 2017; [https://doi.org/10.1016/S2215-0366\(16\)30378-9](https://doi.org/10.1016/S2215-0366(16)30378-9) Trowell J, Joffe I, Campbell J, Clemente C, Almqvist F, Soininen M, et al. Childhood depression: a place for psychotherapy. An outcome study comparing individual psychodynamic psychotherapy and family therapy. *Eur Child Adolesc Psychiatry*. 2007;16(3):157-67 Zhou X, Hetrick SE, Cuijpers P, Qin B, Barth J, Whittington CJ, et al. Comparative efficacy and acceptability of psychotherapies for depression in children and adolescents: A systematic review and network meta-analysis. *World Psychiatry*. 2015;14(2):207-22.

Recommendations - The guidelines summaries minimize the role of parents, a role which is critical to all child and adolescent treatment, and in contemporary thinking ( rooted in largely relational and attachment perspectives) the sine qua non of good practice. One cannot relegate the role of the parent to solely a person who provides permission. The models being promoted, child or adolescent focused, without contextual factors including parental involvement are antiquated treatment models and certainly do not represent best practice in current training programs. I say this as one who has trained doctoral (Ferkau Graduate School, Yeshiva University 1982-2009, and postdoctoral students, William Alanson White, Child and Adolescent Treatment Training Program (1998-2018), Adelphi, Derner Institute Postgraduate program in Child and Adolescent Treatment., among other programs at a pos graduate level. I am also Editor of the *Journal of Infant, Child and Adolescent psychotherapy*, and am privileged to receive and publish peer reviewed articles on all aspects of Child and Adolescent Psychotherapy. There is little in these guidelines that reflects the kind of solid treatment that is occurring internationally, the current excitement about Attachment based interventions ( with a substantive body of research attendant), parent child, including parent infant interventions, and the vast literature on treatment of trauma and traumatic loss in childhood and its relationship to childhood depression and its sequelae.

Thus I am heartened to support some of the recommendations of Dr. Jessica Andrade, Board of Educational Affairs. She notes "there were fewer reviews and meta analyses related to the treatment of depression with children/adolescent populations and the conclusions from these reviews are more tentative than the recommendations for treatment in adult populations". I support her conclusion and recommendation "(1) commission a de novo review with a specific emphasis on children/adolescents developmental periods". I suggest a revision of her second recommendation to suggest not simply a white paper instead of a guideline, but a white paper

which truly represents the reality that there is insufficient data to suggest any one treatment modality as superior to any other with respect to treatment of depression in children and adolescents, and calls for an honest discussion of the state of research in child and adolescent treatment and I support number “(3) broaden the criteria for reviews such that a larger number of sources might be used.” I might also add a look at additional sources of evidence, inclusion of research which looks at change in processes, as opposed to continuous reviews of labeled methodologies. APA should take the lead in considering the problems involved in developing high quality methodology to assess evidence for treatment effectiveness at varying developmental levels. It is time to move beyond the “same old same old”, and to note the reality that there is a trend towards integrative treatments that should be accounted for as well.

**Panel response**

*The panel appreciates the commenter’s perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

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**83.****Commenter: Larry M Rosenberg**

Group Name: Section for Childhood and Adolescence, Division 39

Having reviewed the APA recommendations for treating children with depression there are several areas of concern. As you will read, we are in full agreement with comments submitted by Virginia Shiller on behalf of Division 39 and add the following

1) The guidelines appear to assume that depression in children is a homogeneous category when, in fact, it is not. For example, no distinction is made between appropriate treatments for major depression, persistent depression, or situational depression, nor is any mention made of the substantial evidence of heterogeneity within each of these separate diagnostic categories.

a. It is not clear that the APA treatment guidelines considered research involving childhood depression that occurred comorbidly with other diagnoses. This is problematic because childhood depression often presents in combination with other symptom constellations. This is frequently seen in clinical practice.

b. Second, there is no distinction made for the treatment of depression that results from different causes. For example, omitted from consideration is childhood depression that is related to trauma or aversive events, particularly those that are cumulative and relational in nature.

**Panel response**

*To address the issues in these comments would be outside the scope of the guideline and we recognize the limitations of the methodology throughout the guideline.*

2) Conspicuously absent from the recommendations is any discussion of parent involvement in the treatment of childhood depression. Conspicuous, too, is the failure to distinguish between treatments for depression as it pertains to the developmental stage of the child. This is particularly curious given that the committee has bothered to distinguish childhood from adult recommendations. The committee is aware that depression in infancy, early childhood, and adolescence presents with differing symptoms and treatment approaches need to differ in accordance with the developmental stage of the child. We believe the guidelines should include these considerations.

- a. The committee has omitted reference to working with infants despite their vulnerability to negative environmental influences. In fact, there are evidence-based attachment-oriented treatments that have demonstrated effectiveness in decreasing the likelihood of internalizing symptoms being diagnosed at later ages. Again, these treatment approaches invariably involve parent work, a point that is also not referenced in the guidelines.

**Panel response**

*We agree with the commenter that infant mental health and associated parent work is an important area. However, this area was beyond the scope of the guideline and included studies did not include children below the age of seven years old.*

- b. It does not seem useful or clinically accurate to combine children and adolescents into a single category, particularly when the committee has seen the wisdom of having separate guidelines for adults and children.

3) Indeed, all symptoms and diagnoses considered in the guidelines are described without mention of etiology, as though there is a presumption that all depressive symptoms are either genetically or randomly determined, or as though consideration of causality beyond biology would perhaps be taken as only specific to psychoanalytic thinking.

4) While we agree that suggesting race and ethnicity are considerations in taking an individualized perspective on treatment, specifying them as such in the context of "evidenced based" approaches carries the potential for inadvertent stereotyping. The guidelines do not make clear how cultural, racial or demographic differences might lead to alterations in assessment of depression or treatment approach. Without elaboration on the point, it appears more as a matter of lip service than a constructive direction for the practitioner.

**Panel response**

*To address the issues in these comments would be outside the scope of the guideline and we recognize the limitations of the methodology throughout the guideline.*

- 5) The guidelines appear to hold to the premise that the only useful data come from RCT's. Appreciating the value of RCT's, it remains unclear as to why data derived from other methodologies are less probative.

a. The draft guidelines include only one meta-analysis of treatment of children and adolescents (Zhou, X., et al, 2015). Though this was a meta-analytic study, the scope of what the committee utilized appears to be rather narrow.

b. The committee has determined that only IPT and CBT have demonstrated efficacy in the treatment of childhood depression in children and adolescents, but also clearly states that there are many limitations to the data supporting this conclusion and indeed that the existing data is insufficient to prioritize one treatment over another. The committee's final determination is, therefore, confusing.

c. Recognizing that there are far fewer published findings regarding psychodynamic treatment or family therapy for depressed children, the committee is certainly aware that empirical investigations of these modalities do exist; a limited number of studies do indicate that these modalities to be as effective as CBT or IPT. The committee's failure to acknowledge these

findings seems potentially misleading to the clinical community and the lay public. The committee seems to have only considered short-term treatment models. This would appear to be among the limitations of the Zhou et al study. Because of the singular focus on RCT's, the current guidelines may serve to discourage or diminish the value of longer-term treatment models that may alleviate the underlying causes of childhood depression and hence reduce relapse rates. They may serve to do so not only in the mind of the public, but also influence the thinking of third-party payers and mental health agency executives.

**Panel response**

*Thank you for sharing your concerns. Please refer to the ASC response on p. 5 for discussion of this topic.*

7) The guidelines omit any reference to the use of play in the treatment of childhood depression. Given that play is very often a part of work done with children and that there is empirical evidence, however scant, concerning its effectiveness, it is unfortunate that play is absent from the guidelines.

8) It is clear why some medications are recommended above others, but it is not clear why it seems that only the efficacy of medication alone was looked at vs medication combined with psychotherapy. There is empirical evidence to suggest that talk therapy combined with medication is a superior treatment to medication alone.

a. While medications can undoubtedly prove helpful, if not lifesaving in some cases, there was no apparent consideration given to their continued effectiveness post treatment. This has obvious relevance for psychopharmacological treatment provided without psychotherapy.

**Panel response**

*Reviewer makes a good point that psychotherapeutic treatments may have better continued effectiveness than medications. Using the parameters of the IOM in developing the depression treatment guideline, special attention was given to considerations regarding antidepressants. The committee followed the IOM and AMSTAR guidelines regarding the evidence base including the scope of the literature, the types of acceptable findings for use in deriving conclusions and the methodology used in each study included in the evidence review. Based on these factors, the committee (comprised of researchers, consumers, and clinicians) utilized the research data to make determinations about recommendations. Given the literature available to the committee and the associated parameters under which the committee functioned, a consensus was reached regarding the cautious recommendation of the anti-depressant Prozac for adolescents. Noting that subsequent literature, outside the scope of the committee's review has generated different findings, the committee can reiterate in the guidelines that caution is always of import when providers prescribe antidepressants to adolescents in the treatment of depression.*

b. While SSRI's prove superior to placebos in children and adolescents, placebo effects for SSRI's was not given mention even though there is evidence to suggest that placebo rates in children are significantly high and higher than they are for the adult population.

c. The guidelines make no mention of potential risk factors, or short or long-term effects of psychotropic drugs. Finally, regarding medication research, the public remains largely naïve to the percentage of drug research funded by pharmaceutical companies and the number of

psychopharmaceutical research studies that go unpublished due to lack of significant positive findings. The failure to highlight any of the unfavorable issues surrounding psychopharmacological treatment of depression in treatment in children would seem to be a shortcoming of the guidelines. This omission might inadvertently be construed as naïve endorsement of these findings. It is our view that the implications of negative findings are noteworthy and deserve mention.

) We think it important to reconsider the column headings in Table 1, which currently divide treatments into ones which fall under “Recommend Use” (pp. ES 12-13) and ones listed under “Insufficient evidence for recommendation.” These labels have the risk of encouraging insurance companies to limit coverage for treatments which have been less intensively studied but for which there is promising evidence of efficacy. In our experience, in both public and private settings, many consumers seek out therapies which encourage understanding of underlying intrapsychic issues.

**Panel response**

*The panel discussed and decided to retain the table headings. ASC members discussed whether there was a need to revise the disclaimer language in the CPG document template to enhance its clarity, but believed the current language included clear and prominent statements that the CPG was not intended to limit scope of practice. We also did a comparison of language used in the APA disclaimer and that used by other guideline developers (e.g., American Psychiatric Association, NICE) and found our language provided similar protections. Further, language has been added to the depression CPG document to specify appropriate legal use, and we continue to monitor this issue and seek ways to promote appropriate use of guidelines across settings and practices.*

*Further, the Legal and Regulatory Affairs department at APA routinely works on behalf of psychologist members when it becomes aware that third party payers, health care systems, or others are potentially misusing APA policy or not appropriately administering their own policies. In that regard, those with indication of misuse should bring this information to the attention of the appropriate offices at APA to determine a course of action.*

*We would also like to respectfully point out that an advantage of CPGs is that they inform psychologists who are eager to adopt the most current and evidence-based approaches to treatment about what those treatments are.*

**84.****Commenter: Sandra Rafman**

Comment: Child and adolescent guidelines. Is the youngest age included 7. There appear to be no pre-schoolers. This should be made clear. It may influence results for play therapy and for therapy for children in general. There is only one review for children whereas the suggestions is that there are 10 or at least that there are more reviews than one on which the guidelines are based.

**Panel response**

*We agree that there are limited studies of interventions for youth under 9 years of age. However, the methodology of the guideline precludes us from including data from meta-analyses that are not specific to depression. We acknowledge the limitations to the guideline. We believe that the footnotes accompanying the tables makes the same points as the reviewer does regarding the limitations of the data. We have edited the table to better highlight the limitations of the data in a manner making these footnotes easier to find by readers. We have also revised Table 1 to provide separate recommendations for children versus adolescents in separate tables.*

**85.****Commenter: Karin Hodges**

Comment: Thank you for doing the work to review the research. As a private practitioner, I find these literature reviews and guidelines to be extremely helpful. The guidelines for child treatment of depression seems to be in keeping with my clinical impressions about what is helpful to children and what interventions lead to reduction of depressive symptoms. One thing for providers to remember is that children are rarely only depressed. Often there is more going on (eg undiagnosed LD or other disorder, parent conflict, sleep issues, behavioral concerns, etc.) Thus, the way that I would let this guideline guide me is to treat depressive symptoms with the best treatment available (ie CBT) and then supplement that treatment with other evidence-based interventions geared at other problems.

**Panel response**

*Thank you for your feedback.*

**86.****Commenter: Nick Midgley**

Comment: These comments relate to the section on recommendations for child and adolescent depression: I note that the section of the APA guidelines relating to therapeutic interventions for children and adolescents is based entirely on a single meta-analysis, carried out by Zhou et al. (2015). At the time of its publications, a number of leading researchers in the field of psychotherapy research expressed concern that this network meta-analysis perpetuates an idea that only a limited range of therapies are effective, at a time when there is increasing awareness that offering a broad range of therapies is an important feature of mental health provision. Zhou et al. concluded their network meta-analysis by suggesting that cognitive-behavioral therapy (CBT) and interpersonal therapy are the best available psychotherapies for depression in children and adolescents, and argued that psychodynamic therapy (and play therapy) are not significantly superior to waitlist. Many psychotherapy researchers would argue that such a conclusion is not consistent with the current evidence. Firstly, this conclusion was based on only one, relatively small study of psychodynamic therapy (PDT) by Trowell et al. (2007). A closer look at the study by Trowell et al., on which these conclusions were based, is instructive.

Trowell et al. used absence /presence of depression as the primary outcome. At the end of treatment, 74% of cases were no longer clinically depressed in PDT and 76% in the family therapy condition. At the 6-month-follow-up this was true for 100% in PDT and 81% in family therapy. These results - which most clinicians would be extremely pleased to see - are hardly compatible with the result of the network meta-analysis by Zhou et al., which appears to have used one of the secondary outcome measures from the Trowell et al. study as the basis for its conclusions. It would be more than surprising to find response rates of 74% and 100% in a waiting list condition - indeed, if such a waiting list could be found, we should like to put all our patients on it. For depressive disorders (in adults), another meta-analysis found waiting list conditions to be associated with response rates of 20%. A difference in remission rates of 74% vs. 20% or 100% vs. 20% clearly corresponds to a large effect size in favor of PDT and is hardly compatible with equivalence to waiting list. Basing far-reaching conclusions about the efficacy of a treatment on a single study using a secondary outcome measure of the original study and including only indirect comparisons is premature, especially if the results are not consistent with those of other studies. Unfortunately there is no reference in the APA guidelines to Abbass et al.'s (2013) meta-analysis of short-term psychodynamic psychotherapies for children and adolescents, but this demonstrates that, when directly compared to other bona-fide therapies, psychodynamic therapies for children and young people generally demonstrate comparable outcomes across a range of clinical disorders, when using a range of validated outcome measures. As a second and perhaps more important point, I note that a large-scale RCT evaluating both psychodynamic psychotherapy and CBT as treatments for moderate to severe depression in adolescents was published in 2017 (and was therefore not included in the Zhou et al. network meta-analysis). This was the largest and best-designed RCT to include both psychodynamic psychotherapy and CBT for adolescents, and examined both clinical- and cost-effectiveness of interventions. Given the paucity of research in this field, and the size and quality of this study, it is imperative that any up-to-date treatment guidelines should take into account the the IMPACT study. In brief, its findings were that there were no significant differences in either clinical- or cost-effectiveness between these two treatments, or a third treatment, Brief Psychosocial Intervention (BPI). The conclusion by Zhou et al. that PDT is not superior to waiting list would have massive clinical implications for clinical practice in terms of potentially limiting the number and types of treatments for children/adolescents, if it is used as the only evidence on which to base these guidelines. Furthermore, these recommendations are inconsistent with the recommendations of the National Institute of Clinical and Health Excellence (NICE) in the UK, which takes a comparably thorough approach to evaluating the evidence in making its recommendations, but concludes that a broader range of psychotherapies should be considered in the case of child and adolescent depression. Given the significance of the findings of the IMPACT study, and the paucity of research on which the Zhou et al. network analysis was based, I would strongly urge that the APA guidelines review and take account of the findings of the Goodyer et al. (2017) study, before making their recommendations regarding the most effective psychotherapies for depression in children and adolescents. If not, we risk limiting the range of therapies offered to children and families, despite evidence that a broader spectrum of approaches may serve the needs of young people more effectively.

REFERENCES: Abbass AA, Rabung S, Leichsenring F, Refseth JS, Midgley N. Psychodynamic psychotherapy for children and adolescents: a meta-analysis of short-term psychodynamic models. *J Am Acad Child Adolesc Psychiatry*. 2013;52(8):863-75. Goodyer, I., Reynolds, S., Barrett, B., Byford, S., Dubicka, B., Hill, J., Holland, F., Kelvin, R., Midgley, N., Roberts, C., Senior, R., Target, M., Widmer, B., Wilkinson, P., Fonagy, P. Effectiveness And Cost-Effectiveness Of Cognitive Behaviour Therapy And Short-Term Psychoanalytic Psychotherapy Compared With Brief Psychosocial Intervention In Maintaining Reduced Depressive Symptoms 12 months after end of treatment in Adolescents with Unipolar Major Depression (IMPACT): A Pragmatic Superiority Randomised Controlled Trial. *Lancet*



Psychiatry, 2017; [https://doi.org/10.1016/S2215-0366\(16\)30378-9](https://doi.org/10.1016/S2215-0366(16)30378-9) Trowell J, Joffe I, Campbell J, Clemente C, Almqvist F, Soininen M, et al. Childhood depression: a place for psychotherapy. An outcome study comparing individual psychodynamic psychotherapy and family therapy. Eur Child Adolesc Psychiatry. 2007;16(3):157-67 Zhou X, Hetrick SE, Cuijpers P, Qin B, Barth J, Whittington CJ, et al. Comparative efficacy and acceptability of psychotherapies for depression in children and adolescents: A systematic review and network meta-analysis. World Psychiatry. 2015;14(2):207-22

**Panel response**

*We understand that it is desirable to see the Trowell et al. study as demonstrative of the effects of these treatment approaches. However, it is important to acknowledge research methodology experts who state that inclusion of a control condition is the only way to know whether these youth are getting better because of these specific treatments or whether they would have gotten better with time alone or any other treatment. The panel appreciates the commenter's perspective that the conclusion is not consistent with some current evidence. However, the guideline followed the parameters outlined for the review in the section on scope of review.*

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**87.****Commenter: Michael J Silverstein**

Comment: ES-9 Table 1 IPT-A is recommended for adolescents who have mild-moderate depression. It is not recommended for those with severe depression or those with suicidality. If an adolescent has severe depression, CBT is recommended. This is important to note for clinician may think that they can use IPT-A for adolescents with severe depression or suicidality, which is incorrect. See Dr. Laura Mufson's website for this information: <https://childadolescentpsych.cumc.columbia.edu/articles/interpersonal-therapy-adolescents-ipta>

**Panel response**

*Thank you for this feedback. The panel discussed and agreed that recommendations for children and adolescents should be separated and distinct recommendations made for each population. You will note this significant revision in the table of recommendations. Of particular note the panel decided to no longer include IPT among the recommendations for children and retain this only for the adolescent population.*

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**88.****Commenter: Robert Foltz**

Comment: In the discussion of treatment of depression in children and youth, they should urge extreme caution in any recommendations/utilization of antidepressants. While the TADS study is often touted as solid support for an evidence-based approach, further analysis would suggest that antidepressants are not effective. The TADS study examined the use of Prozac (and compared it to CBT and combined treatment). In the initial phase of the study, combined treatments seemed to outperform CBT alone. But this advantage was short-lived. Moreover, it would be inaccurate to assume this short-term benefit was attributed to Prozac. Here's why: The TADS study incorporated just over 100 into each treatment arm. Since then, additional antidepressants have been tested in hopes of obtaining FDA approval. A great example is Cymbalta. Hundreds of youth were enrolled in studies trying to demonstrate the effectiveness of Cymbalta. Their design was to compare Cymbalta to placebo to Prozac. In studies totaling over 800 youth, Cymbalta AND Prozac "failed to separate from placebo." As a result, if we are using an evidence-base to inform our recommendations, we should use the preponderance of

the evidence to inform our positions. In the end, SSRI antidepressants (or others) have not sufficiently demonstrated their effectiveness and come with a host of troubling side-effects that can further complicate the ability to obtain an optimal outcome. Available meta-analyses of antidepressants in youth further confirm these findings (one recently cautiously suggested Prozac may be an option, but I do not believe it incorporated the above comparisons).

**Panel response**

*The committee appreciates the commenter's concerns and notes that TADS was an important, though singular study with some noted safety concerns related to the specific medication used. As a result, the committee felt that it was prudent to consider all of the evidence related to the TADS trial within the context of the parameters of the committee's charge (i.e. a focus on psychotherapeutic interventions for the treatment of depression).*

**89.**

**Commenter: Allan Abbass**

Comment: Short-term Dynamic Therapy for Children and Adolescents. We have conducted a review of all the studies of short-term dynamic psychotherapy in children and adolescents. Some of these studies were of depression. Here is a link to download: [https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=3&ved=2ahUKEwjag-qbiK\\_dAhUENd8KHwn\\_C5QQFjACegQIARAC&url=http%3A%2F%2Fericastiftelsen.se%2Fdoc%2FEPU\\_13.16%2Fuppsats\\_EPU1316%2FAbbass\\_et\\_al\\_2013.pdf&usq=AOvVaw2JLalm\\_VX\\_MJR857WCfOUnw](https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=3&ved=2ahUKEwjag-qbiK_dAhUENd8KHwn_C5QQFjACegQIARAC&url=http%3A%2F%2Fericastiftelsen.se%2Fdoc%2FEPU_13.16%2Fuppsats_EPU1316%2FAbbass_et_al_2013.pdf&usq=AOvVaw2JLalm_VX_MJR857WCfOUnw) Here is the abstract: Objective: Psychodynamically based brief psychotherapy is frequently used in clinical practice for a range of common mental disorders in children and adolescents. To our knowledge, there have been no meta-analyses to evaluate the effectiveness of these therapies. Method: After a broad search, we meta-analyzed controlled outcome studies of short-term psychodynamic psychotherapies (STPP, 40 or fewer sessions). We also performed sensitivity analyses and evaluated the risk of bias in this body of studies. Results: We found 11 studies with a total of 655 patients covering a broad range of conditions including depression, anxiety disorders, anorexia nervosa, and borderline personality disorder. STPP did not separate from what were mostly robust treatment comparators, but there were some subgroup differences. Robust ( $g = 1.07$ , 95% CI  $0.80-1.34$ ) within group effect sizes were observed suggesting the treatment may be effective. These effects increased in follow up compared to post treatment (overall,  $g = 0.24$ , 95% CI  $0.00-0.48$ ), suggesting a tendency toward increased gains. Heterogeneity was high across most analyses, suggesting that these data need be interpreted with caution. Conclusion: This review suggests that STPP may be effective in children and adolescents across a range of common mental disorders. J. Am. Acad. Child Adolesc. Psychiatry, 2013;52(8):863–875. Key Words: anxiety, child, depression, psychodynamic, psychotherapy

**Panel response**

*The reviewers make good points however, the guideline is not focused on preventive treatments but rather focused on interventions for youth who already have a diagnosis of depression. The guideline followed the parameters outlined for the review in the section on scope of review, which do not include studies on infants nor studies not specific to depression. We recognize the limitations of the guideline throughout the document.*

Comment: Attention to treatment resistant depression Considering that the primary outcome in majority of patients treated with psychotherapy and pharmacotherapy is lack of remission the

APA should've a special section in this document for psychotherapy for treatment resistant depression. There is a relatively new Cochrane Review on treatment resistant depression and psychotherapy. There's notable bits on the best outcomes of treatment resistant depression are from psychotherapies including some Advanced models of cognitive behavioral methods and intensive short-term dynamic psychotherapy. Here is the link for this new review <https://www.cochranelibrary.com/cdsr/doi/10.1002/14651858.CD010558.pub2/full>

Comment: Depression and Personality Disorders Depression in the setting of personality disorders can be difficult to treat. It is a risk factor for refractoriness. We have conducted a review of Studies of short-term dynamic therapy in the setting of personality disorder and depression. Here is the link to download this: [https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=3&ved=2ahUKEwi71J3fh6\\_dAhVOU98KHWxvAI0QFjACegQIBxAC&url=https%3A%2F%2Fwww.vvpt.be%2Fimages%2FPublicaties%2Fpsychiatry-abbass-town-driessen.pdf&usg=AOvVaw3IBKpJhqRPgt-APEv8GCWw](https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=3&ved=2ahUKEwi71J3fh6_dAhVOU98KHWxvAI0QFjACegQIBxAC&url=https%3A%2F%2Fwww.vvpt.be%2Fimages%2FPublicaties%2Fpsychiatry-abbass-town-driessen.pdf&usg=AOvVaw3IBKpJhqRPgt-APEv8GCWw) Here is the abstract: The presence of comorbid personality disorder (PD) is one of the factors that can make the treatment of depression unsuccessful. Short-term Psychodynamic Psychotherapy (STPP) has been shown efficacious in the treatment of personality and depressive disorders (DD). However, the efficacy of STPP for comorbid DD and PD has not been systematically evaluated. In this study, data from patients meeting criteria for both DD and PD participating from randomized controlled trials of STPP was collected, systematically reviewed, and meta-analyzed where possible. Eight studies were included, 6 with major depression and 2 with minor depressive disorders. Pre- to post-treatment effects sizes were large ( $d = 1.00-1.27$ ), suggesting symptom improvement during STPP, and these gains were maintained in follow-ups averaging over 1.5 years. For major depression, no differences were found comparing STPP to other psychotherapies, and STPP was found superior to a wait-list condition in one study. STPP may have had an advantage over other therapy controls in treating minor depression as noted in ratings of general psychopathology. Patients with Cluster A/B and C PD were responsive to STPP, with the majority of all studied patients showing clinically significant change on self-report measures. Within the limits of this study, these findings suggest that STPP warrants consideration as a first line treatment for combined personality disorder and depression. Future research directions are proposed.

Comment: CORRECTED VERSION HERE: Attention to treatment resistant depression Considering that the primary outcome in majority of patients treated with psychotherapy and pharmacotherapy is lack of remission the APA should've a special section in this document for psychotherapy for treatment resistant depression. There is a relatively new Cochrane Review on treatment resistant depression and psychotherapy. It is notable that the best outcomes of treatment resistant depression are from psychotherapies including some advanced models of cognitive behavioral therapy and intensive short-term dynamic psychotherapy. Here is the link for this new review <https://www.cochranelibrary.com/cdsr/doi/10.1002/14651858.CD010558.pub2/full>

**Panel response**

*Thank you for the constructive comment. The panel does recognize that the current literature review did not address treatment resistant depression in detail and encourages future efforts to do so.*

**90.****Commenter: John Curry, Ph.D.**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Recommendations - The Guidelines were very thoughtfully developed and represent an enormous amount of scholarship. I have two comments, one relatively minor and the other potentially more significant. Both have to do with treatment of child and adolescent depression.

First, the relatively minor point: In discussing the fact that CBT and IPT are the two psychotherapies with evidence of efficacy, it seems the authors should point out that the evidence base for CBT is much more voluminous than that for IPT.

Second, the document does not seem to address combined psychotherapy plus medication for adolescent major depression. In the Treatment for Adolescents with Depression Study (TADS) this was the treatment with the greatest efficacy and the best combination of efficacy and safety. Also, in the Treatment for SSRI-Resistant Depression (TORDIA), adolescents who had not responded to a trial of an SSRI did better when they were switched to a different medication plus CBT than when switched to a different medication alone. The British ADAPT study did not support combined treatment over SSRI alone, but the sample was probably more severely impaired than in TADS or TORDIA.

**Panel response**

*We concur with this comment: both the TADS and TORDIA studies demonstrated the efficacy of combined treatment in patients not initially responding satisfactorily to monotherapy with psychotherapy or medication. However, the panel did not examine combined treatment for children and adolescents.*

I believe that the psychotherapy literature review (Zhou et al., 2015) that the panel used as a basis for psychotherapy recommendations did not include combined treatments (medication plus psychotherapy), and this may be why combined treatment for adolescents is not addressed in the guidelines. However, I would urge the panel to review the limited literature on this topic and to address it in the document.

**Panel response**

*The panel is not able to extend the scope of its review at this time.*

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**91.****Commenter: Patricia McMahon, PsyD**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - pp66-74 Comparison of APA Guidelines to Other Practice Guidelines  
pp74-83 Challenges in Developing the Guideline...

Many psychologists are not formally trained and licensed to prescribe medications - and will remain unlicensed throughout their careers. Within this context, it seems odd that APA devoted much time to discussing specific medication-based treatments for depression. If APA needs to discuss medication, it should spend equal time discussing medical (eg endocrine imbalances) and neuropsychological conditions that cause or are worsened with depression. However, the specifics of these comorbid discussions belong in the AMA, IOM, or apa guidelines -- or in a

guideline co-authored across fields of study. Additionally, any discussion of medication-based treatment citing specific drugs may change quickly with research advances.

Instead of listing (sometimes proprietary) models of psychotherapy as the "gold standard" or "state of the art", it will be better to create a list of psychotherapy theories, contexts, and foci that APA supports as useful in treating depression for adults, adolescents, and children. Because APA guidelines as written can be used by insurers to deny reimbursement for unstructured treatments, it will be more clinically accurate to describe an approach and its corresponding usefulness to ameliorating depressive state and symptoms. This will avoid the appearance of (and actual) conflict of interest when specific models of treatment and research are promoted while other models and research are not cited in the current Depression Guideline draft.

Research must include long-term studies of lasting benefits and improved life satisfaction for all depressed populations, ie assess more than symptomatic change over a tightly circumscribed period of time. Comorbidity and ethnic/racial diversity should be more specifically addressed in terms of long-term gains.

**Panel response**

*Please refer to discussion of this topic earlier in the document.*

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**92.****Commenter: Bob Hill**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - I regret to see the inclusion of antidepressant medication in the Guidelines for treating depression. The literature available on the effectiveness of medication is fraught with challenges, including the frequent funding support from pharmaceutical companies that encourage the selective publication of more optimistic findings, and hide findings that are not supportive. A more careful consideration of the medication treatment literature also would need to include the placebo effect, and also the challenges faced by discontinuation syndrome.

In reality the strength of interventions offered by psychologists are in the realm of psychotherapy, and we should be skeptical of our ability to recommend specific medications as the scientific data on medications is too problematic to summarize in this Guidelines document. Why include medications?

**Panel response**

*Dr. Bob Hill asks why include a consideration of antidepressant medication in the guideline, citing issues of publication bias with respect to negative studies and threats to internal validity/bias wrought by industry sponsorship. While we agree that these are legitimate concerns, our charge embraced comparative effectiveness trials that included pharmacotherapy. The meta-analyses that we referenced dealt with issues of publication bias and other threats to internal and external validity.*

**93.****Commenter: Dr Benjamin A Bensadon**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

General Comment - Worth directly citing 3 out of 4 patients, when asked their preferences directly, prefer psychological as opposed to pharmacologic therapy to manage mood (McHugh, Whitton, Peckham, Welge, & Otto, 2013, Journal of Clinical Psychiatry, 74(6), 595-602).

Yet in U.S. health care system they nevertheless are prescribed anti-depressants, even without diagnoses (Motjabai & Olson, 2011, Health Affairs, 30(8), 1434-1442).

Workforce, not science, is guiding treatment. Serotonin hypothesis of depression etiology is largely debunked but SSRI medication remains among leaders in sales.

This is unlikely to change as primary care "behavioral" health integration/psychiatric collaborative care reimbursement codes position nurse practitioners to decide if/when referral to a psychologist for non pharmacologic intervention is appropriate.

**Panel response**

*We agree with Dr. Benjamin Bensadon's comment that older adults tend to prefer psychological to pharmacological treatments for depression, but that such preferences usually are not enacted in clinical practice, in large part because of the shortage of expertise in evidence-based psychological and behavioral therapies available in the general medical sector. We feel it is important to note, however, the existence of real-world, pragmatic studies in primary care, such as IMPACT (Unützer et al., JAMA, 2002) and PROSPECT (Bruce et al, JAMA 2004) that support the utility and effectiveness of problem solving therapy and of interpersonal psychotherapy, respectively, usually in combination with antidepressant medication, for the treatment of major depression in older, primary care adults. A further recently published study (Dias et al, JAMA Psychiatry) supports the efficacy of problem-solving therapy for preventing major depression in older primary care adults with subsyndromal depressive symptoms (an example of "indicated" depression prevention).*

*We concur with Dr. Benjamin Bensadon's comments that many patients with mood disorders indicate a preference for psychological over drug therapies; and that antidepressants are often prescribed in primary care settings in the absence of a medical record diagnosis of depression. As noted elsewhere, patient preferences are determined by many factors which need to be understood as part of the process of shared decision making.*

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**94.****Commenter: Mary Marshall**

Comment: Psychology should never refer patients for medicine. That is the patient's choice not our recommendation. We can discuss the option, the pros and cons, including Side or unwanted effects, withdrawal and interaction with the other medicine and food. We should encourage less invasive techniques and remedies. We do not tell patients to stop taking they're already prescribed medicine.

**Panel response**

*Dr. Mary Marshall comments that psychologists should “never refer patients for medicine.” While we agree with her that psychologists can and do play an important role in educating patients about antidepressant medication (pros and cons), within the context of shared decision making, we also feel it is appropriate for psychologists to refer patients for consultation with physicians about the indications and contra-indications of antidepressant medication use. Thus, her use of “never” seems inappropriate to us. Further, in some states, some psychologists have obtained additional training and supervision and are recognized as prescribers.*

*Further, concerning Dr. Marshall’s comment (“psychologists should never refer patients for medication”), our intention was simply to indicate that it may be appropriate and helpful for psychologists to seek the opinion of a psychiatrist, or other qualified prescriber, concerning the indications and contra-indications for either starting and/or discontinuing antidepressant medication.*

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**95.****Commenter: Jeffrey Rubin**

Comment: Throughout the recommendation sections wherever antidepressants are recommended there is a dramatic under reporting of the well known side effects of the drugs and a discussion of these side effects. Whereas the recommendations do make mention of side effects, the difficulty of coming off the drugs when the patient wants to is not mentioned at all. The fact that teenage women, when prescribed the drugs and then become pregnant, the difficulty of coming off the drugs leads to many remaining on the drug throughout pregnancy and a number of serious effects on the fetus and new born is well documented and not mentioned as a serious problem. The sexual side effects and its effect on marriage quality is ignored. I find the balancing of the negative side effects of the drugs versus reported benefits of the drugs completely misrepresented in these guidelines and thus unjustly leads to recommending the drugs as a "first line" treatment is ethically wrong.

**Panel response**

*Dr. Jeffrey Rubin writes that the guideline gives inadequate attention to the potential side effects and difficulties with antidepressant medication withdrawal. However, similar to our response to Dr. Mary Marshall above, we note that the APA depression guideline panel emphasized the importance of collaborative care for major depression, with appropriate referral to physicians for review of the indications and contra-indications of antidepressant medication (usually in combination with psychotherapy), especially in the context of more severely ill patients. The panel was also careful to note when studies did not adequately address the adverse effects of interventions, both psychological and pharmacological.*

*Dr. Jeffrey Rubin notes the challenges faced in discontinuation of antidepressant medication. The degree of difficulty varies, in part, as a function of elimination half-life. Agents with shorter elimination half-lives (such as venlafaxine or paroxetine) may be associated with more difficult discontinuation (withdrawal) symptoms unless tapered slowly. By contrast, agents with long half-lives, such as fluoxetine, can simply be stopped with few withdrawal symptoms. Anti-depressant withdrawal symptoms need to be distinguished from re-appearance of depressive symptoms.*

**96.****Commenter: Nell Restak, PhD**

Comment: Hello,

In briefly scanning APA's new recommendations for depression interventions, I see unequivocal inclusion of psychoactive medications and polypharmacy, yet APA is composed mostly of psychologists and therapists, not doctors of medicine. Recommendations seem to be borrowed from the literature "out there," despite many drugs not being scientifically proven beneficial (most conspicuously, antidepressants, which can cause grave long-term compromise of cognitive functioning).

So my question is, which members of your panel are biologists, immunologists, neurologists, chemists, pharmacists (not psychiatrists and their pharmaceutical company partners), or multidisciplinary researcher-scientists of human beings? Will you be providing more about the negative effects of psychotropic drugging? To be a legitimate organization, APA must be prepared to evaluate all of the literature, not just one side of it.

Knowing which panel members are grounded in science will provide perspective I need to write some input for you, which I would be pleased to do.

Thank you for a reply.

Nell Restak, PhD, clinical psychology, psychoneuroimmunology

**Panel response**

*Dr. Nell Restak writes that the panel erred by "unequivocal inclusion of psychoactive medication and polypharmacy" in its recommendations. We respectfully but strongly disagree with this statement, since we drew primarily upon RCTs, systematic reviews, and meta-analyses to support our recommendations for medication use (typically in combination with psychotherapy); since we often characterized these recommendations as provisional, given the limitations of the data available, and since we emphasized the need for shared decision making in the implementation of recommendations.*

*We respectfully disagree with Dr. Nell Restak's comment that the guideline contains unqualified endorsement of psychotropic agents and polypharmacy. We also note in the text and elsewhere the potential for serious adverse effects, including activation of suicidal ideation in patients younger than 25, and falls in older adults. Adverse cognitive effects of antidepressants can be seen with older agents having anticholinergic side effects, such as tertiary tricyclic agents like amitriptyline.*

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**97.****Commenter: Gayle L. Blakely**

Comment: Comorbidity was not directly reviewed and is not reflected in the recommendations. However, given the ubiquity of comorbidity, all stakeholders need to take this limitation into consideration when reviewing the present guideline recommendations (pp. 59-60). My reaction as a post-bacc research assistant studying youth in foster care: (1) Comorbidity is prevalent among youth who have other disorders or other symptoms that do not meet disorders outside the range of clinical depression. For instance, childhood trauma, including maltreatment and non-maltreatment forms of trauma, is linked to not only depression, but also anxiety, post-



traumatic symptoms (regardless of whether or not diagnostic criteria are met for a particular trauma-related disorder), and the onset of other disorders due to negative environments. (2) Comorbidity should be addressed in recommendations for any and all disorders since there is prevalence and/or incidence rates that could be prevented. (3) What is missing from these recommendations include not only comorbidity issues, but also the complexity of psychological comorbid disorder treatments such as (a) conflicting treatment recommendations for the presence of two or more disorders and/or symptoms, which are not addressed because comorbidity was not well-studied for such recommendations, (b) harmonious treatment recommendations that work well for the presence of two or more disorders, and (c) potential iatrogenic effects of treatments for two or more disorders that require different sets of treatments that may interact with one another. Physiological and psychological comorbidity is also not addressed, such as in the cases or studies that involve ACEs and both physiological and psychological outcomes. The treatments for both physiological and psychological comorbid problems are therefore not addressed specifically, nor acknowledged, and there may be serious implications when medication recommendations for psychological problems may conflict with medication recommendations for physiological problems. Comorbidity should be included in future recommendations. For now, there needs to be a mention about both physiological and psychological comorbidity issues - regardless of whether physiological problems are related to or not related to psychological problems.

**Panel response**

*Thank you for your comments regarding the issue of comorbidity. You are quite correct in pointing to the importance of understanding how comorbidity impacts both assessment and treatment of individuals experiencing depression. Although your comments initially focus on children, the presence of comorbid disorders, whether they are psychological (e.g., anxiety, suicide) or medical (e.g., heart disease, cancer) in nature, as you know, can significantly impact the efficacy of any intervention. Unfortunately, as expressed throughout our guideline, we were only able to address certain aspects of the treatment of depression. For example, in the section entitled "Guideline Purpose and Scope: What the Guideline Addresses and What it Doesn't," we specifically mention that we were unable, due to limited time and resources, to focus on various important issues, including comorbidity (as well as issues involving diversity, prevention, dosage, etc.). To address the notion of comorbidity in any meaningful manner would require substantial effort. As you point out, there are several concerns to consider if one is both depressed and experiencing additional pathology. Unfortunately, little research has been conducted to date that can help make recommendations within a truly empirical context. Much of what occurs in real-life clinical settings are usually based on clinical acumen and not hard-core research findings (e.g., what is the "best" treatment for a woman who is both depressed and suicidal and recently experienced sexual assault?). It is our hope that research addressing comorbidity is prioritized in the future and that subsequent guidelines can address such issues.*

**98.****Commenter: Douglas Lane, on behalf of APA Division 12/Section 2: Society for Clinical Geropsychology**

Comment type: Group Comments

Group name: Society for Clinical Geropsychology (APA 12/2)

Do you have any other comments about the draft guideline document?

General Comment - These comments represent generally unanimous input from 12/2 membership, as well as the Board, and as such we hope will be considered with corresponding "weight".

Comment 1: General Comments

Members of APA Division 12 Section II (Clinical Geropsychology) appreciate APA's decision to develop guidelines for depression and acknowledge the difficulty and complexity of the task.

We applaud the coverage across the lifespan and the specific section devoted to older adults within the Executive Summary and Discussion of Clinical Recommendations. We also value the panel's decision to include a subsection within the Clinical Recommendations for "MDD or Minor Depression + Cognitive Impairment/Dementia" given the importance of this area for a subpopulation of older adults.

Within this draft, the guidelines are pitched at an overly granular level at times (i.e., content and language used appear to be based on a single study) and too abstract/theoretical at other times. The result is that the guidelines are based on an inaccurate analysis of the state of the science of treatment of depression in older adults. The guidelines as currently written are less helpful than they could be and have the potential for harm. Our comments are aimed at improving the utility of these guidelines for practitioners who are engaged in treating depression in older adults.

Executive Summary - Comment 2: Section: Executive Summary

ES-17 to ES-23 (and linked to the discussion within pp. 51-53) -We are both confused and concerned about the decision to recommend group life-review and group CBT as the 2 strongest recommendations for the psychotherapeutic treatment of MDD in older adults (p. ES-17). This recommendation is at odds with the empirical literature.

**Panel response**

*The panel appreciates the thoughtful and thorough comments offered by Dr. Lane. In this particular comment, the concern is that the panel's strongest recommendation is for group life-review and group CBT and that the literature on individual CBT is overlooked. To understand the panel's decision making please refer to the decision tables in Appendix C, especially 14, 18, 28, 6, 8, 10, 11, 15, 16, 23, 35, and 36. These decision tables detail the information available to the panel and ratings that followed from extensive discussion of these data. This decision-making process follows procedures provided by the IOM for guideline development. These procedures are described in the body of the document (pages 36-44). Group life-review and group CBT received Strength of Recommendation ratings "Recommend for use" and individual CBT received "Conditional recommendation for use." This does not mean that group treatments were found to be superior to individual treatments in comparative outcome. Some of the recommendations in the Executive Summary are indeed granular because the literature*

*on treatment of older adult depression is rather limited and some comparisons entailed only one or two studies. As the literature on this topic expands the ability to make more robust recommendations will also increase.*

-Cuijpers et al. (2014) did not find a group vs individual effect and did not make recommendations regarding treatment format. Of the 44 studies that met criteria for inclusion in that systematic review, only one study examined group CBT with MDD older adults (Ekkers, 2011). Instead, Cuijpers et al (2014) recommendations were for CBT and Problem-Solving therapy (PST) without specifying format. Although group formats may be feasible in some settings and preferred by some subgroups of older adults, very real considerations of access (i.e. transportation) mean that solely offering therapy in a group format creates a formidable barrier to treatment for many older adults.

-Of the 44 studies that met criteria for inclusion in Cuijpers et al. (2014) strategic review, only 4 included group Life Review. Of these 4, only 1 was conducted with MDD older adults in the US (Arean, 1993), and in that study, the effects of Problem Solving therapy were superior to Reminiscence Therapy (Life-Review). This point is even noted in the Executive Summary (p. ES-18), stating that Problem Solving Therapy (group) was superior to Reminiscence (group); reminiscence in the Arean et al (1993) study is generally the same as Life Review (group). The other 3 studies of Life-Review (group) consist of a dissertation study using a single therapist for all conditions and <10 per group (Watt & Cappeliez; 2000), a secondary prevention trial in which having a depressive disorder was an exclusionary criteria (Pott, 2010) and another student thesis project using a cut-off of > 10 on CESD for study eligibility and excluding participants with severe MDD (Korte et al., 2012). These studies are not sufficient on which to base a recommendation for Life Review (group) for the treatment of MDD in older adults. The data supporting Life Review are much stronger for secondary prevention of depression in at risk older adults, and in the treatment of minor depression/depressive symptomatology.

- We are concerned that available studies pointing to the efficacy of individual CBT for major depressive disorder in persons over age 65 have been overlooked and/or not given the attention they deserve.

#### **Panel response**

*The panel appreciates the thoughtful and thorough comments offered by Dr. Lane. In this particular comment, the concern is that the panel's strongest recommendation is for group life-review and group CBT and that the literature on individual CBT is overlooked. To understand the panel's decision making please refer to the decision tables in Appendix C, especially 14, 18, 28, 6, 8, 10, 11, 15, 16, 23, 35, and 36. These decision tables detail the information available to the panel and ratings that followed from extensive discussion of these data. This decision-making process follows procedures provided by the IOM for guideline development. These procedures are described in the body of the document (pages 36-44). Group life-review and group CBT received Strength of Recommendation ratings "Recommend for use" and individual CBT received "Conditional recommendation for use." This does not mean that group treatments were found to be superior to individual treatments in comparative outcome. Some of the recommendations in the Executive Summary are indeed granular because the literature on treatment of older adult depression is rather limited and some comparisons entailed only one or two studies. As the literature on this topic expands the ability to make more robust recommendations will also increase*

-Given the systematic reviews chosen for use in constructing the guideline (p. 30), and in light of patient preferences and concerns over side effects, we were struck by the extent of recommendations for pharmacotherapy. This is especially remarkable in the sections “Major Depressive Disorder” (p. ES-17) and “Subthreshold/minor depression” (p. ES-19) because the Cuijpers et al (2014) review did not cover pharmacotherapy. What are the sources for these recommendations? Recommendations for pharmacological treatments originating from studies identified within the Wilkinson & Izmeth (2012) Cochrane review are appropriate solely within the subsections “Persistent Depressive Disorder” and “Prevention of Recurrence.”

-Within the Process and Method section, there is this statement: “Also, while the panel wished to review different treatment modalities, including self-help, internet and group compared to individual, the appropriate studies were not included in the identified reviews to be able to examine this question. The panel did not evaluate evidence reviews of long-term intervention to target relapse and recurrence and evidence reviews of prevention intervention consistent with its scoping decision” (p. 29). That statement is also supported by Cuijpers et al (2014) not finding effects by treatment format (i.e., group vs individual). However, this is contradicted within ES17-ES23; recommendations are being made for group and internet-based interventions, and there is a subsection for “Prevention of Recurrence.” If the panel decided to not review treatment modalities, then these should be removed from the specific recommendations.

**Panel response**

*A question is also raised about pharmacotherapy and the source of these studies. The Cuijpers et al. (2014) did include pharmacotherapy studies and the panel was interested in pharmacotherapy + psychotherapy trials. A final note is that the IOM procedure is dependent on current systematic reviews of the literature and it will be important for the field of mental health and aging to encourage such reviews to make guidelines as up-to-date as possible.*

-In the Subsection for MDD or Minor Depression with Cognitive Impairment, separating Problem-solving therapy (bottom of p. ES-20) from problem solving behavioral therapy (top of p. ES-21) does not make sense and will confuse clinicians. Problem-solving therapy is a specific form of therapy (and also a specific principle of change) that can be placed under a very large umbrella of Behavioral and Cognitive Therapies. We do support PST being listed as its own treatment approach, but it is not applicable across all levels of cognitive impairment/stages of dementia. For that reason, it does not fit as the first/front line recommendation, and is better placed after a recommendation for Behavioral Activation. “Pleasant events behavioral therapy” (p. ES-21b) should be relabeled under its more commonly recognized label, which is “Behavioral Activation”. We see that this is also listed as “Insufficient” under the name of behavioral activation therapy. This appears to be due to the incorrect strategy of framing recommendations based on single studies, in the specific language used by that study. Overall, the data for behavioral activation as a treatment strategy and principle of change show support for its use as a treatment for depression in older adults, and has the advantage of it being applicable across levels of cognitive functioning. We would like to see stronger language supporting the use of Behavioral Activation in this section.

**Panel response**

*We agree with many of the comments offered on the Subsection for MDD or Minor Depression with Cognitive Impairment. However, as noted elsewhere, we are constrained by the conclusions rendered in the decision tables and refer you to those relating to this subsection that are included in the Appendix. As for treatment labels, we elected to use those provided by study authors and we agree that this could lead to confusion but hope that the detailed information provided in the discussion and appendices will mitigate such. We also agree that the larger literature on behavioral activation show support for it as an intervention but some of these studies were not included in the systematic reviews used by the panel and thus were not subjected to decision tables. Hopefully, more up-to-date systematic reviews will be included in the next iterations of the guideline.*

- What are the sources of data for the recommendations “MDD with medical or other complications” (p. ES-22)? We agree that this is an important area to be addressed, but other sections of the draft guidelines indicate that depression co-occurring with medical conditions was not covered (p. ES-6; p. 59-60).

**Panel response**

*These recommendations came from Decision Tables 25, 37, and 51 in Appendix C. The panel chose not to include reviews that focused exclusively on populations with a given medical comorbidity (e.g., a review on individuals with diabetes who also had depression), however if in a given review there were some data on individuals who also had medical comorbidities then that was included.*

Within the Discussion of Clinical Recommendations (pp. 51-53)

-The discussion (pp. 51-53) of recommendations for older adults should explicitly state that meta-analytic review has not found any differences in effectiveness of psychotherapy for younger and older adults and cite the following:

Cuijpers, P., van Straten, A., Smit, F., & Andersson, G. (2009). Is psychotherapy for depression equally effective in younger and older adults? A meta-regression analysis. *International Psychogeriatrics*, 21(1), 16-24.

**Panel response**

*This is an excellent recommendation and the following sentence can be inserted on page 52 at line 22. “It is also worth noting that older adults and younger adults appear to demonstrate similar improvements in response to psychotherapy (Cuijpers et al, 2009).”*

-This section of Discussion of Clinical Recommendations specific to older adults (pp. 51-53) should also explicitly remind readers that the versions of CBT, PST, Behavioral Activation and Life Review treatments that were reviewed and are being recommended for use with older adults all included use of evidence-based modifications for clinical work with older adults. This section needs to include a statement to this effect, along with a citation and link to the APA Guidelines for Psychological Practice with Older adults:

<https://www.apa.org/pubs/journals/features/older-adults.pdf>

**Panel response**

*This is an excellent idea. On page 51, line 6 the following can be inserted: "We note that these treatments included modifications that made them more appropriate for use with older adults. More detail on these issues can be found in the Guidelines for Psychological Practice with Older Adults: <https://www.apa.org/pubs/journals/features/older-adults.pdf>.*

## Introduction - Comment 3    Section: Introduction

There are some areas of the Introduction Section, specific to older adults that merit additional attention and revision.

p. 13 Reference for prevalence data provided for depression in older adults is old (1993) and does not consider potential for cohort trends. We would suggest an updated citation.

**Panel response**

*Need newer reference for prevalence, suggest we use Kessler et al. (2003).*

p. 13. The sentence "Depression in older adults is not often treatment-resistant, that is, patients may improve partially but do not remit symptomatically nor regain full functional status." is confusing and difficult to interpret. No citations are presented for either this statement, or for the statement that depression presents differently in older adults compared to younger adults. Relapsing and reoccurrence are common across adulthood and not unique to older adults.

**Panel response**

*Thank you for noting this. We believe there was a typo, so we deleted the word "not" and inserted a reference to support the assertion.*

p. 13. "Learning-based interventions and those that are behaviorally activating (such as problem-solving therapy) are a promising method of depression prevention." Suggested revision is: "Learning-based interventions and those that are behaviorally activating (e.g., behavioral activation, cognitive-behavioral therapy, problem-solving therapy) are promising methods of depression prevention."

**Panel response**

*Thank you for your comment we are currently revising this in the document.*

p. 14. "Models employing lay health counselors of similar ethnic and racial backgrounds to the patient increasingly seem to be a rational and cost-effective use of resources to reach underserved and disadvantaged older adults (Patel et al., 2010)" Comment: This is a problematic placement of this recommendation. Use of peer-assisted treatments is presented appropriately in a later section (p. 16). Here, this implies that there are health disparities and that we should not try to address these with evidence-based treatments for ethnic minority older adults within health care systems, but instead arrange for peer support. This could be seen by some as dismissive and/or an approach that could increase health disparities.

**Panel response**

*Thank you for this feedback, we are re-writing the sentence per your suggestion*

p 14. The subsection on suicidality is inadequate and should include more specific references. This is critically important given the strong association between increased age and completed suicides in some subpopulations (older white men):

Sachs-Ericsson, N., Van Orden, K., & Zarit, S. (2016). Suicide and aging: Special issue of Aging & Mental Health. *Aging & Mental Health*, 20(2), 110–112. <https://doi-org.ezproxy.umsl.edu/10.1080/13607863.2015.1099037>

It would be appropriate to discuss and cite both the PROSPECT study and findings, along with the Joint Commission (JCHO) alert and recommendation:

Alexopoulos, G. S., Reynolds, C. F., III, Bruce, M. L., Katz, I. R., Raue, P. J., Mulsant, B. H., ... Ten Have, T. (2009). Reducing suicidal ideation and depression in older primary care patients: 24-month outcomes of the PROSPECT Study. *The American Journal of Psychiatry*, 166(8), 882–890. <https://doi-org.ezproxy.umsl.edu/10.1176/appi.ajp.2009.08121779>

Sentinel Event New Alert Focuses on Suicide Ideation. (2016). Joint Commission Perspectives. Joint Commission On Accreditation Of Healthcare Organizations, 36(4), 12–17. Retrieved from <https://ezproxy.umsl.edu/login?url=http://search.ebscohost.com/login.aspx?direct=true&db=cmedm&AN=29714847&site=ehost-live&scope=site>

It is especially important to note that throughout the guidelines, there is a missed opportunity to reiterate the JCHO emphasis that suicidality should be targeted and treated explicitly.

Throughout the guideline, there is the suggestion that suicidality can be effectively treated by treating depression. Treating depression, and expecting that to effectively treat suicidality, is not in keeping with the research literature on best practices.

**Panel response**

*Suggests additional references to section on suicide (pg. 14); we can add the two references he suggests. Suggests last sentence on page 16 expand reasons for access to evidence-based practice, so include “a focused application of dissemination and implementation science” to this sentence.*

p. 16. There are a range of reasons for poor access to evidence-based practice. The last sentence on this page needs revision to state that guidelines are one means, along with a focused application of dissemination and implementation science, to improve access to quality care.

**Panel response**

*Suggests last sentence on page 16 expand reasons for access to evidence-based practice, so include “a focused application of dissemination and implementation science” to this sentence.*

**Process and Method - Comment 4 Section: Process and Method**

-The rationale for defining older adults as “50 or older” provided on ES1 is problematic and appears to be unnecessary. Although some of the studies included within the Cuijpers et al (2014) systematic review may have included adults between the ages of 50-64, a reading of the full guidelines and appendices indicates that data from the studies included in the Cuijpers et al (2014) review were individually examined and evaluated by the RTI-UNC Evidence Based Practice Center. In light of this process, it is possible to draw conclusions from the studies that included only older adults.

**Panel response**

*Raises the issue of including “50 and older”; suggest we change recommendation to 60 and older and insert footnote that some studies in systematic review included younger but panel was focusing on 60+ in making recommendations.*

-Related to the list of issues that the guideline does not address (pp. 22-23), it is a serious limitation for the guideline specific to older adults exclude a revision of treatment for depression in long-term care settings. This is a population of older adults with demonstrated high rates of depression, and an area where productive research has been conducted. We respectfully ask the review panel to consider a delay in these guidelines to allow inclusion of recommendations for this vulnerable population.

Bharucha, A. J., Dew, M. A., Miller, M. D., Borson, S., & Reynolds III, C. (2006). Psychotherapy in long-term care: a review. *Journal of the American Medical Directors Association*, 7(9), 568-580.

Snowden, M., Sato, K., & Roy-Byrne, P. (2003). Assessment and treatment of nursing home residents with depression or behavioral symptoms associated with dementia: a review of the literature. *Journal of the American Geriatrics Society*, 51(9), 1305-1317.

Cody, R. A., & Drysdale, K. (2013). The effects of psychotherapy on reducing depression in residential aged care: A meta-analytic review. *Clinical Gerontologist*, 36(1), 46-69.

Meeks, S., Van Haitsma, K., Schoenbachler, B., & Looney, S. W. (2015). BE-ACTIV for depression in nursing homes: Primary outcomes of a randomized clinical trial. *The Journals of Gerontology: Series B: Psychological Sciences and Social Sciences*, 70(1), 13–23. <https://doi-org.ezproxy.umsl.edu/10.1093/geronb/gbu026>

**Panel response**

*We agree that this is a vitally important domain but unfortunately, there were not enough studies in the systematic reviews to address this independently. We will add a sentence stressing the importance of this area of practice and the need for more research to contribute to systematic reviews of long-term depression treatment.*

-Similarly, we are very surprised that the panel has made the decision to not review the literature on treatment depression in the context of medical conditions (p. ES-6). We find it hard to believe that there are no good data available on treating depression in comorbid illnesses, and this is a major lack in these guidelines. This is especially important in light of the growth of behavioral health services integrated into primary care, and APA's efforts in this area. And, this is contradicted by the appearance of recommendations for depression in the context of medical conditions (p. ES-22).

**Panel response**

*It was determined early in the panel's work that this literature would take us beyond our resources; this does not mean however, that persons with medical co-morbidities were excluded from studies. Instead, studies addressing a particular co-morbidity (e.g., depression and diabetes) were not reviewed.*



pp. 57-58 Adapting Treatments to fit the individual: The need for implementation research is a critical reason for poor treatment outcomes -- this section does a poor job of highlighting the importance of developing treatments for implementation, including identifying the core components from a theoretical model and studying how those components can be implemented in various settings. The tone of this section, and those following, promotes the notion that providers should adapt evidence-based treatments according to their own particular practices and patients without regard to theoretical models or consideration of how fidelity might affect outcomes.

**Panel response**

*Thank you for the feedback, we are re-working this section.*

The section on "Considerations for Treatment Implementation" needs a much stronger call for dissemination and implementation research and practice, aimed at our learning how to develop and adapt treatments to promote effectiveness in real world practice settings. We would very much like to see this message more clearly articulated in this work.

Onken, L. S., Carroll, K. M., Shoham, V., Cuthbert, B. N., & Riddle, M. (2014). Reenvisioning clinical science: unifying the discipline to improve the public health. *Clinical Psychological Science*, 2(1), 22-34.

**Panel response**

*We agree with Dr. Lane that the need for implementation research is important with respect to older adult depression treatment, but this domain of intervention science is beyond the scope of this panel's work.*

*We also thank Dr. Lane for his comments and citations to the literature, made on behalf of the geropsychology section of Division 12. We endorse these comments also, while noting that our pharmacotherapy recommendations were carefully qualified by safety concerns surrounding the use of tricyclic antidepressants.*

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**99.**

**Commenter: Norman Abeles**

Comment type: Personal Comments

Do you have any other comments about the draft guideline document?

Discussion of Clinical Recommendations - Treatment for older adults--Depression Note that Nathan and Gorman(1997) in a chapter by George Niederehe (p276) notes that for comparative treatments brief psychodynamic therapy is as effective as CBT. The Guideline document only compares treatment vs no treatment and this seems to be a problem, in my opinion.

**Panel response**

*We agree with Dr. Abeles that the comparative outcomes of treatment suggest little difference in efficacy. We have tried to be careful to convey this message in our discussion of the panel's recommendations.*

**100.****Commenter: Gregory Simon**

Comment: I do question the justification for distinct recommendations for treatment of major depression in patients aged 50 and older. Those recommendations seem to be based on a relatively small evidence base of studies LIMITED to older adults while ignoring a much larger evidence base from adult studies INCLUDING older adults. There is a presumption of an interaction or effect modification (comparisons of effectiveness yield different results in younger and older adults) in the absence of evidence to that effect (What I refer to as "trying to answer a four-cell question with a two-cell design"). This leads to some very odd recommendations, such as singling out tricyclic antidepressants (such as desipramine) as preferred in older adults. I am certainly not aware of clear evidence that tricyclic drugs are superior to newer antidepressants in people over aged 50. Or evidence that group life review treatment is superior to BA, CBT, or IPT in people over aged 50. The evidence base cited regarding antidepressants and psychotherapies in the general adult population likely included far larger numbers of adults aged 50 and older than the "older adult specific" trials. It may be appropriate or even necessary to provide specific treatment selection recommendations for specific subgroups of older adults (people with cognitive impairment, people with co-occurring chronic medical conditions) - but I don't believe that distinct recommendations for all adults aged 50 and older are appropriate.

**Panel response**

*The panel thanks Dr. Greg Simon for his comments.*

*We note that the hallmark of major depressive disorder (MDD) in older adults is its co-occurrence with medical polymorbidity and varying degrees of cognitive impairment. MDD in later life is also associated with elevated risk for completed suicide and may be a risk factor for dementing illness or a prodromal expression of vascular or Alzheimer's dementia. The psychosocial context of late-life depression is also important for treatment planning, including changes in major social roles and status. These changes are consequent upon life events characteristic of later years, including retirement, bereavement, social isolation, loneliness, and increasing disability and dependence upon others as a result of medical polymorbidity and/or cognitive decline. Although treatment studies of major depression frequently include mixed-age participants, older adults (especially those aged 75 and above) are often under-represented. It is in this cohort that risk for suicide and for conversion to dementia is highest. With increasing age, moreover, a shift in the ratio of efficacy to tolerability and safety occurs, with issues of safety looming large in the oldest old; this shift presents a challenge for optimizing pharmacotherapy. In addition, because many older adults may be reluctant to acknowledge depression or to seek treatment, alliance with family caregivers is critical for successful engagement in treatment long enough to achieve remission and to prevent relapse and recurrence. Models of patient-focused and family-centered care have assumed increasing prominence in the literature. Because depression in older adults is often a relapsing, recurrent illness, it can become chronic in the absence of effective maintenance treatment. For all of these reasons, the APA depression treatment guideline panel felt that it is not justifiable scientifically or clinically to extrapolate treatment evidence from trials in middle-aged adults to the management of older adults. Rather, distinct treatment recommendations for older adults, taking into account dimensions of medical, CNS, and psychosocial determinants of treatment response variability in older adults, are appropriate scientifically, clinically, and ethically.*

*The systematic reviews of depression treatment for older adults used by the Guideline Panel did include studies with participants as young as 50 years of age but the mean age of participants in these reviews was 60 and older (Cuijpers et al., 2014; Wilkinson & Izmeth, 2012, respectively). Thus, these systematic reviews speak primarily to the effects of psychological and pharmacological treatment for conventionally defined older adults. We will add this information to make it clear that the data we are relying upon is relevant to older adults.*

*With respect to pharmacotherapy of late-life depression (and in particular, to the use of tricyclic antidepressants; TCAs), the APA guideline indicates that TCAs are third-line agents, to be used after selective serotonin re-uptake inhibitors (SSRIs) and selective serotonin-norepinephrine re-uptake inhibitors (SNRIs), or other atypical antidepressants (e.g. bupropion) have not led to remission of depressive symptoms. TCAs need to be used with caution in older adults who may have electrocardiographic conduction disturbances, are at risk for falls related to orthostasis, or who are suicidal. Their optimal use can be guided by steady-state blood level data, which bear a lawful and predictable relationship to the probability of remission. Finally, the potential for toxic drug-drug interactions needs to be considered with antidepressant pharmacotherapy in all older adults, many of whom are co-prescribed 4-6 other agents for systemic medical conditions*

*Concerning depression-specific psychotherapies with proven efficacy in late-life depression, the panel found that evidence supporting life review therapy, CBT, IPT, PST, and BA is reasonably strong but did not find that life review therapy could be recommended as superior to any of these. Rather, the panel endorses a process of shared decision making with older adults and their family care givers. It is also noteworthy that short-term, depression-specific psychotherapies and behavioral therapy appear to be efficacious in preventing subsyndromal depression from evolving into major depression (an example termed “indicated” prevention by the Institute of Medicine/National Academy of Medicine).*

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## 101.

**Commenter:** *daniel n klein*

Comment: Congratulations to the panel on a balanced and comprehensive review and set of recommendations for treating depression across the lifespan. However, I have one concern. The section on older adults provides explicit recommendations for treating patients with Persistent Depressive Disorder (PDD). However, this group of highly challenging and chronically impaired patients is entirely ignored in the review and recommendations for the general adult population, despite there being a much larger literature on PDD in this age group (e.g., Klein & Black, 2017), including several systematic reviews and meta-analyses (Cuijpers et al., 2010; Jobst et al., 2016; Levkovitz et al., 2011; Negt et al., 2016; von Wolff et al., 2012). There is evidence suggesting the value of combined psychotherapy and pharmacotherapy for some forms of PDD (Cuijpers et al., 2010; Keller et al., 2000), that the duration of treatment may need to be longer than for non-chronic forms of depression (Schramm et al., 2017), and that some forms of psychotherapy (e.g., Cognitive Behavioral Analysis System of Psychotherapy) are superior to other forms of evidence-based psychotherapy (e.g., Interpersonal Therapy) for PDD (Negt et al., 2016; Schramm et al., 2017). Given the striking and robust differences between PDD and non-chronic major depression with respect to comorbidity, early maltreatment, personality and cognitive biases, social functioning, prognosis, and risk for suicidal behavior

(Klein & Black, 2017), the lack of discussion of the treatment of PDD in the general adult population leaves a major gap in this otherwise outstanding document.

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#### **Panel response**

*The panel thanks Dr. Dan Klein for his comments about persistent depressive disorder (PDD) and his recommendation for greater attention to PDD in guidelines formulated for depression in (non-elderly) adults. Although Dr. Klein's comments do not refer specifically to older adults, it is nonetheless important to note that with increasing age, intervals between episodes of major depression shorten; and that with each successive episode, the incidence of treatment resistance and chronicity increases. The literature on chronic and treatment-resistant depression in older adults is inconclusive regarding the relative efficacy pharmacotherapy strategies such as augmentation with a second agent (e.g., the atypical antipsychotic medication, aripiprazole) versus switching classes of medication. The comparative effectiveness and safety of augmentation and switching strategies is a subject of active investigation in older adults with major depression, in a large, ongoing multi-site trial ("Optimum") sponsored by the Patient Centered Outcomes Research Institute (PCORI). Finally, as Dr. Klein notes, there is some evidence supporting the efficacy of combined pharmacotherapy and depression-specific psychotherapy in older adults living with persistent depressive disorder.*

*We thank Dr. Daniel Klein for his comments about the need for greater emphasis on treatment recommendations for adults with chronic and persistent major depressive disorder. We concur with his view of the evidence that it is often a combination of*

*antidepressant medication and psychotherapy (e.g., CBT, CBASP) that is most helpful to these severely ill and complex patients.*

**102.**

**Commenter:** *Dana Charatan, Psy.D.*

Generally speaking, I find these guidelines to be a well crafted academic documentation of just how limited the research is for using any specific orientation for a general diagnosis such as depression (the same was true for the PTSD guidelines). Since out of all treatments included only two are considered worthy of recommendation, I would hope that the evidence stating that only two forms of therapy are worth endorsing for one of the most commonly seen forms of mental illness would be close to infallible. This research is nothing of the sort, and by its own admission, since 43% of the document speaks to its limitations. The fact that the researchers are unable to specify what is effective 50% of the time (non specific factors) as opposed to something that is supposedly 17% effective (recommended treatments) underlies how unreliable the "evidence" here actually is. We do have decades' worth of consistent evidence stating that the working alliance is the most important indicator of treatment success -- yet these guidelines dismiss this factor as subsumed by "non-specific factors" (pp. 62-63).

How can psychologists use these guidelines as intended when they do not remotely resemble the complexity of real-world therapy patients that we work with every day, as opposed to research participants in highly controlled trials, who must meet selection criteria that almost any typical psychotherapy patient would quickly fail? In the very beginning of this document, it is stated that "professionals are expected to take this guideline fully into account, alongside the individual needs, preferences, and values of their patients..." How can we account for individual needs with a one-sized fits all mentality? I am in full support of the well crafted critique offered by PsiAN and ask that these guidelines be retracted and revised until a more valid and clinically useful document can be put in its place or until APA is willing to consider that RCT's are not the gold standard of efficacy when it comes to psychotherapy research.

**ASC/Panel response**

*The panel recommends more than two treatments. Please refer to Tables 1 through 4 for the full list of recommendations. We have removed the statement from NICE that is quoted above from the guideline document.*

*To address the concern about nonspecific factors, the ASC has added a section on the role of common (non-specific) factors into the CPG template to make this a standard component of guidelines moving forward. Currently, the extent of research evaluating the variance accounted for by common factors varies widely across disorders/problem areas so there is no way to include this information in similar ways across all CPGs. For the depression guideline, the panel included discussion on the importance of common factors (see sections on "Shared versus unique contributions of different psychotherapy models" and "Contributions from Shared and Specific Factors to Treatment Outcome"), and also provided some additional discussion and references on the topic of common factors in other comments about the draft guideline.*

*The ASC has proposed that a working group be formed to identify opportunities to more fully address change processes in future CPGs or related supporting material.*

*The guideline is not intended to provide a comprehensive overview of human suffering or ever suggest that a person is the sum of their symptoms. The guidelines are designed to make a particular contribution; one that is circumscribed, but nonetheless valuable. In the case of the depression CPG, it is to provide consumers, family members, and practitioners with information about which treatments have been shown to have strong research support for alleviating symptoms of depression in children, adolescents, and young, middle-aged, and older adults.*

*Treatment planning is not meant to be guided by only one source of information (whatever that source) – we believe that knowing what the best available research says about what treatments can be helpful, on average, is important information to help guide treatment planning.*

*The panel agrees that greater diversity amongst individuals participating in clinical trials would be very helpful, but the panel also notes that Appendix K does provide some detail about the patients included in every clinical trial considered in all the reviews. The inclusion exclusion criteria of each trial are not listed but it is evident that studies did include individuals with multiple diagnoses, including physical health conditions. Earlier RCTs did tend to have strict inclusion criteria but more recent studies tend to include individuals with more complex presentations. In fact, for some research trials, the problem severity of individuals being treated is greater than that of individuals treated in the community.*

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**103.**

**Commenter: Marta Miranda, Psy.D.**

While the document references the fact that depression manifests in many different ways in individual patients, there is very little discussion of the contextual factors that may underlie an individual's depressive symptoms, which need to be acknowledged and understood for successful treatment. Specifically, depressive symptoms may be the result of bullying, domestic violence, racism, homophobia, workplace harassment, being separated from one's child at the U.S. border, etc. In this context, treatment of only depressive symptoms without attending to the sociopolitical context in which these symptoms occur will not be particularly successful. Interestingly, it seems that other guidelines have addressed these issues. On page 72, the panel notes that the NICE guidelines included "consideration of language and culture" and "assessments of social network, bullying" in their depression guidelines. I think this needs to be addressed in these guidelines, possibly in the section "Adapting treatment to fit the individual" on pages 57-58.

**ASC response**

*Thank you for this comment. The ASC agrees that clinical depression does not occur in isolation but, rather, in differing psychosocial, medical, and sociocultural contexts. All of these need to be taken into account in treatment planning to optimize engagement in treatment and outcomes.*

*Unfortunately, as you point out, there are multiple areas that were not covered in detail in this guideline. Given the prevalence and ubiquity of major depressive disorder, along with the large number of studies aimed at evaluating various means to address this problem, it is extremely difficult to do justice to all the issues that appear to be related to this topic, including sex differences, comorbidity (e.g., suicide), different mood*

*diagnoses/disorders, and assessment issues. Addressing everything we thought important was not possible, so we focused on a comprehensive review of the efficacy and comparative effectiveness literature and utilized IOM guidance to structure our decision making. This allowed the panel to highlight gaps in the literature and what needs to be done in the future to be able to provide for more meaningful conclusions that can inform clinical practice (e.g., what treatments are more effective for a specific subgroup, such as women, ethnic minorities, individuals with comorbid mental or physical disorders).*

The addition of a discussion on common factors and patient preferences was an improvement on previous guidelines. I also appreciated that the panel acknowledged the downsides of using the IOM guidance, especially that they might not be the best fit for guidelines based primarily on psychotherapy research (pages 78-80). I was surprised then, to read the statement on the bottom of page 79: "adopting the IOM guidelines is considered to be the best approach." By whom? The panel makes important points about the limitations of IOM Guidance and specifically the limitations of the guidance on treatment of depression derived from the research that was IOM-sanctioned. Why is this the best approach? This seems to contradict the panel's recommendation on page 80: "The panel does recommend the development of a workgroup to review guideline development processes to determine if specific modification may be indicated for guidelines related to mental and behavioral health." I wholeheartedly agree.

**Panel response**

*Thank you. The IOM standards for guideline development are widely used across various healthcare organizations across the United States in developing clinical practice guidelines, including guidelines related to mental and behavioral health conditions, and are consistent with best practices used internationally.*

The panel notes that the studies do not adequately address comorbidities, referencing specifically medical comorbidities. However, there isn't enough discussion on comorbidities with other mental health disorders. On page 59-60, the panel states that clinicians should take into account comorbidities, but then says nothing further about how to do this. At a minimum, I think there needs to be mention of the fact that depression may be secondary to other diagnoses, such as trauma, substance abuse, and personality disorders and that it is important to accurately identify which disorder is primary and adjust treatment accordingly. That is, as a clinician, my approach to treating someone who has "only" a Major Depressive Disorder is very different than with someone whose depressive symptoms are linked to Borderline Personality Disorder.

**Panel response**

*Persons with medical co-morbidities were not excluded from included studies but the panel did not utilize reviews specific to a particular co-morbidity (e.g., depression and diabetes) and therefore did not make recommendations specific to any populations with comorbidities. Comorbidities, including comorbidities with other mental health disorders, is discussed in further detail in the sub-sections of the introduction pertaining to each age population (children/adolescents, general adults, older adults). We have also added details about comorbidities included in each review in the methods section.*

While the guidelines do discuss ethnic and racial diversity as it relates to patients who experience depression, there was no or very little consideration of other populations, in spite of the fact that there is ample discussion in the literature for some of these groups. Notably, I was

surprised to find no discussion at all of how depression is highly prevalent in LGBTQ populations. A global search of “LGBTQ” in the document found one reference to this population, on page 11 of the executive summary, and this was only to mention the lack of research on interventions with this population. Consideration of LGBTQ populations is completely absent in this document, even when other underrepresented populations are mentioned (e.g., page 20 “Guideline recommendations for underrepresented populations”). Yet, there is ample literature on the prevalence of depression and high suicide rates in both LGBTQ youth and adults. Please include this in your discussion on prevalence at least, citing relevant articles from Division 44’s journal (there are many articles on this topic in this journal alone).

**Panel response**

*Thank you for this feedback. We have added additional information to the document about the prevalence of depression in LGBTQ populations.*

Second, there is no discussion on how depression presents in a different way in non-Western populations. While I recognize that the guidelines are not meant to address diagnosis and assessment, there is ample literature to suggest that non-Western populations may not manifest depressive symptoms in the same way as Euroamericans (i.e., there is a higher prevalence of somatization versus cognitive/affective symptoms). See, for example: Ryder, A.G. & Denton, A.E. (2012). Depression in cultural context: “Chinese somatization” revisited. *Psychiatric Clinics of N. America*, 35, 15-36.

Third, and related to the above, degree of acculturation may be a more important factor than ethnicity when it comes to how depression manifests in Asian/Latino populations. This is certainly the case for assessment and, in my experience as a clinician, generally true when treating individuals from these ethnic groups. There is a sentence on page 11 about differences in depression in Chinese populations based on place of birth (i.e., those born and living in China versus Chinese Americans), but nothing specifically about more recent and less acculturated immigrants to the U.S. This should be added to the section on prevalence, as the effects of immigration/acculturation on mental health, including prevalence of depression in this population is well documented.

**Panel response**

*The panel agrees that depression can present differently in non-Western populations. However, the participants in the studies were from a range of countries outside the United States and to explore the role of culture on the presentation of depression for each represented country is beyond the scope of this guideline. The panel also agrees that acculturation is an important factor but was not able to fully address that in the guideline document. Please refer to the newly added table of demographic factors by study in the appendix for further details.*

Finally, the note on the APA Multicultural Guidelines on page 59 is wholly inadequate. It basically tells clinicians to “be multiculturally competent and read those other guidelines.” There is no indication for what this means in the context of the depression guidelines. It is not enough to tell clinicians to “be competent”; you need to explain how the depression guidelines would be applied in a multiculturally competent way.



**Panel response**

*We added culture when discussing the three legged stool of evidence based practice (consideration of patient culture). We are also adding explicit mention of culture to the Individualizing Treatment section of the guideline. We hope this addresses the commenter's request. Given the multiple cultures represented by participants in the included studies and lack of sub-group analyses, it is beyond the scope of this guideline to detail appropriate cultural application to each. It is an interesting question to consider what it means to be multiculturally competent in the treatment of depression but beyond the scope of the panel. The panel continues to encourage readers of the guideline to use this guideline in conjunction with all of APA's professional practice guidelines that provide much more guidance on important topics such as culture, LGBTQIA+ issues, gender, and so on that can inform all treatment.*

It is not clear who was included in the research studies that were used to develop the guidelines. The discussion on pages 33 and 34 ("Diversity of samples included in reviews") is quite confusing. Some sentences don't make much sense (possibly due to poor English): "Overall, of the studies with available data, 46 studies for children and adolescents had 46.67% of non-White participants." (lines 2-3 of page 34) and "Overall in the studies with available data, general adult population had 45% of non-White populations" (lines 13-14 of page 34). The same is true for the last sentence of the following paragraph. Reading this section, it seems to me that 60-70% of the adult samples in the studies were female. Is this correct? If so, the guideline for adults is very skewed toward females with depression and may not be generalizable for male populations. In addition, the document points out in several places that the samples were mostly White (e.g., page 82-83 specifically notes that people of color were not well represented in the studies used to develop these guidelines.) On page 58-59 the panel notes that "if a given set of studies that support the efficacy of a given approach included primarily White males as subjects between the ages of 25-50 years, it becomes less scientifically rigorous to suggest that this same intervention would automatically be effective for others who are depressed but are outside the pool of these specific subject demographics." I wholeheartedly agree with this. I think that this needs to be stated explicitly and incorporated into the guidelines by identifying for which populations the guidelines have been shown to be effective and the populations for which there is insufficient evidence to recommend specific interventions or treatments. That is, I'd like to see something to the effect of: "The following interventions have been shown to be effective with predominantly White, female patients born in the United States who are heterosexual and have depression with no comorbidities. There is insufficient evidence to suggest these interventions are effective with individuals outside of this group." Similarly, on page 45 (lines 14-15), the panel addresses the issue of generalizability of evidence for youth, given the low inclusion of youth of color in the samples, stating that the recommendations must be viewed as "limited" in applicability. I would go further and state that the recommended guidelines are applicable to a predominantly White youth population, but may not be applicable to youth of color. I think these caveats on applicability should be upfront in the table of guideline recommendations in the executive summary. That table may be the only thing that busy clinicians read, and the applicability issue needs to be included there.

**Panel response**

*While it is important to consider within group differences related to racial diversity and depressive illness, the current literature base precludes our ability to disaggregate racial/ethnic groups to study differences both within and between racial identifiers. Instead, current literature struggles to adequately address the racial/ethnic diversity that currently exists in the U.S, and abroad such that we can derive generalizable findings about depression treatment (psychotherapeutic and otherwise) relevant for aggregated racial groups. As it stands, the current literature fails to allow for a deep dive into across-groups differences which speaks to the need for a far greater focus on increasing the generalizability of study participants and the relevance of depression treatments to more adequately reflect the full population of persons affected by depressive illness. However, the panel added a table to the appendix detailing demographic data for each study included in the ten reviews used by the panel. The panel disagrees that the reviewed research is appropriately characterized as “The following interventions have been shown to be effective with predominantly White, female patients born in the United States who are heterosexual and have depression with no comorbidities. There is insufficient evidence to suggest these interventions are effective with individuals outside of this group.” and notes that Appendix K provides details about gender, study country and race/ ethnicity. More demographic information was in some studies but not all so the panel opted to present information that could generally be identified in the majority of included studies in this appendix.*