Comparative Effectiveness Review Disposition of Comments Report

Research Review Title:
Attention Deficit Hyperactivity Disorder: Diagnosis and Treatment in Children and Adolescents

Draft review available for public comment from October 17, 2016, to November 14, 2016.

Research review citation: Kemper AR, Maslow GR, Hill S, Namdari B, Allen LaPointe NM, Goode AP, Coeytaux RR, Befus D, Kosinski AS, Bowen SE, McBroom AJ, Lallinger KR, Sanders GD. Attention Deficit Hyperactivity Disorder: Diagnosis and Treatment in Children and Adolescents. Comparative Effectiveness Review No. 203. (Prepared by the Duke University Evidence-based Practice Center under Contract No. 290-2015-00004-I.) AHRQ Publication No. 18-EHC005-EF. Rockville, MD: Agency for Healthcare Research and Quality; January 2018. Posted final reports are located on the Effective Health Care Program search page.
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Comments to Research Review

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Comments on draft reviews and the authors' responses to the comments are posted for public viewing on the Web site approximately 3 months after the final research review is published. Comments are not edited for spelling, grammar, or other content errors. Each comment is listed with the name and affiliation of the commentator, if this information is provided. Commentators are not required to provide their names or affiliations in order to submit suggestions or comments.

The tables below include the responses by the authors of the review to each comment that was submitted for this draft review. The responses to comments in this disposition report are those of the authors, who are responsible for its contents, and do not necessarily represent the views of the Agency for Healthcare Research and Quality.

#	Commentator & Affiliation	Section	Comment	Response
1.	TEP Reviewer #1	Quality of Report	Good	No response needed.
2.	TEP Reviewer #2	Quality of Report	Good	No response needed.
3.	TEP Reviewer #3	Quality of Report	Superior	No response needed.
4.	TEP Reviewer #4	Quality of Report	Fair	No response needed.
5.	TEP Reviewer #5	Quality of Report	Good	No response needed.
6.	TEP Reviewer #6	Quality of Report	Fair	No response needed.
7.	Peer Reviewer #1	Quality of Report	Good	No response needed.
8.	Peer Reviewer #2	Quality of Report	Superior	No response needed.
9.	TEP Reviewer #1	General	I am pleased with the report. My only suggestions are: 1. In the analysis of the assessment measures, is it possible to include a summary of the cost of any of these measures?	Comments on the cost of the various tools was considered outside the scope of the review.
10.	TEP Reviewer #1	General	2. In addition to the analysis done in 2011, there was a rigorous lit review done on an AHRQ grant to McMasters University performed in 2001 when the initial guidelines were developed. The 2011 revisions used those results as a baseline for their review. It would be helpful if you can obtain that review because the extent of the research on ADHD has been huge and sometimes that older data gets forgotten.	The earlier review has now been referenced as part of our discussion of the 2011 review throughout the current review.
11.	TEP Reviewer #1	General	3. The results of the MTA covers a 10 year period and part of what is meaningful to the results is how they changed over that period particularly from 14 months when the interventions ended to 2 and 3 years as well as the 8-10 year interval.	The limitations of addressing larger trials within the context of this update is now discussed.

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12.	TEP Reviewer #1	General	4. On page 67 where you have a comparisons of categories, is it possible to age the age groups studied since age could be a factor? I hope my comments are helpful. The results will help us in our considerations particularly in being able to weigh the strength of the evidence.	Throughout the updated report we now include information on the age category of individuals included in each study.
13.	TEP Reviewer #2	General	My specific comments for each of the sections listed below are included in the attached pdf. The overarching comments are: 1) the AAP clinical care guidelines are misquoted as saying that medication should be the first line of treatment which is not accurate for young children or older children (please see the cmts that are embedded in the text for the specific location); 2) I think it needs to be made very clear that the conclusions are cumulative reflection of the evidence, capturing the results from the 2011 review as well as this updated review.	We thank the reviewer for their helpful review and comments. The AAP guideline discussion is now clarified under treatment strategies. We also clarify throughout the review the findings from the 2011 report and the consistency/inconsistency of these earlier findings with our current findings when possible.
14.	TEP Reviewer #2	General	[preface] Limiting the review to just a few years makes this report interesting to researchers but as helpful to practitioners	We understand that this is a limitation of this review, and we have now listed it as such in the limitation section.
15.	TEP Reviewer #2	General	[abstract] "Update" is a bit misleading. The previous review focused on preschoolers, so this is not an update but a limited expansion	The abstract was updated to clarify the scope of the review.
16.	TEP Reviewer #2	General	[abstract] Would be helpful to explain reasons for not including all available evidence to draw conclusions, it makes a significant difference in regard to the utility of this report	Our reasoning is explained in the “other factors” section of the report.
17.	TEP Reviewer #2	General	[abstract] The results show that BT and Omega-3 have some moderate findings, whereas medications have zero moderate findings. This is not reflected in this conclusion. See page 126	The strength of evidence rating is different from a discussion of the effect found in the studies. No change is needed.
18.	TEP Reviewer #2	General	[abstract] This [statement in the abstract conclusion that pharmacotherapy appears most effective in the previous review] is confusing. The previous AHRQ review focused on preschoolers and found parent training to be most effective. I am not aware of an AHRQ review that focused on ADHD across all ages and came to this conclusion	The abstract has been edited to clarify this issue. We have also restructured the review to highlight the findings in relationship to what was already known.

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19.	TEP Reviewer #2	General	[abstract] The review did not limit itself to new approaches, it included new evidence only (e.g., of some older approaches which have long since been declared well established). A conclusion based on all available evidence vs. evidence that is time limited will be very different	The abstract has been edited to clarify this issue.
20.	TEP Reviewer #2	General	[abstract] It is very important to contrast these findings with the evidence from other systematic literature reviews that found behavior therapy to be well established (see www.effectivechildtherapy.org)	We reference previous systematic EPC reviews that address behavior therapy and discuss the consistency/inconsistency of our findings.
21.	TEP Reviewer #3	General	What an impressive achievement! The time, rigor, and attention to detail that went into this report are clearly evident. I have provided more specific comments below, but my primary suggestion is to improve the readability of the report by simplifying the tables and take-away points in several places. Also, it would be helpful to include a synthesized summary that includes evidence from the 2011 report and builds on it, rather than completely separating the evidence from the last 5 years. Thank you for this incredible piece of work.	The key points from the 2011 report are now listed for the reader.
22.	TEP Reviewer #4	General	The report's clinical implications are addressed throughout the document but are best summarized on page 129 in the short paragraph titled, "Implications for Clinical and Policy Decisionmaking." The points made there could be adapted for a set of practiitioner guidelines. As for the key questions, it was not clear to me why the issue of rating scale diagnostic methods was limited to children below the age of 7, while biometric measures that might aid diagnosis were limited to ages 7-17. No rationale for these rules was provided in this huge review. The first-line recommendations for the use of FDA approved medications would mean that they could only be used for children 6 years and older, which greatly limits the applicability of the findings of KQ1 for clinicians.	We expand within the methods on the reasoning behind our scope given prioritized uncertainties balanced with limited time and resources.

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23.	TEP Reviewer #4	General	On page 11, under Scope and Key Questions, the paragraph titled "Other Factors" that there are four new medications. In fact, there other medications approved by the FDA during the time of scope of this review, including a long-term liquid medication (Quillivant XR), Aptensio XR, Evekeo, and Zenzedi.	These medications are differently formulated versions of already available therapies. This is now better clarified in the text.
24.	TEP Reviewer #5	General	The report is clinically meaningful, as many ADHD-treating physicians are concerned with the value of utilizing nutritional supplements, the new NEBA test, and behavioral interventions with their patients. Information has been limited for the former two intervention modalities and a summary is desperately needed to inform clinical care.	No response needed.
25.	TEP Reviewer #5	General	The target population, audience, and key questions are explicitly stated and are sound.	No response needed.
26.	TEP Reviewer #5	General	However, the strength of the evidence for the included research (in isolation) is inadequate to actually answer the research questions.	No response needed.
27.	TEP Reviewer #6	General	This literature review presents information published since 2009 to update 2011 report. 3 key questions were addressed. The limited time frame, tedious review process (more quantitative than enhanced quality assessment), the difficult to understand criteria (consistency?) on some assessments made it hard for me to expect an important impact of the findings for the prescribing community and even less so for consumer understanding.	We thank the reviewer for their comment and hope that revisions to the final report clarify the findings.
28.	TEP Reviewer #6	General	What will the impact of showing so little sufficient information mean? Should the goal be to continue to support some of these narrowly defined topics by calling for research proposals for public funding? Some issues seem less than critical at this time.	How best to incorporate these findings in guideline initiatives or policy is outside the scope of this review.

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29.	TEP Reviewer #6	General	Some questions HAVE been answered sufficiently in the past, e.g. growth suppression/weight loss with stimulants; cardiovascular parameters (bp; heart rate) with stimulants (MTA across the years) yet this review does not find data acceptable. Some clarification might be useful.	Within the introduction we do state that psychostimulants can be effective in reducing distractibility, improving sustained attention, reducing impulsive behaviors, and improving activity level (page 1). In addition when discussing the key findings from the 2011 report (page 4) we highlight how psychostimulants provide control of ADHD symptoms and are well tolerated in children 6 years and older (page 4). We chose not to comment further on older literature outside of the 2011 report because it was not in scope.
30.	TEP Reviewer #6	General	The absence of PRACTICE patterns from observational data is glaring. There is evidence of the growth of antipsychotic use in community populations (even as first treatment!) for off-label treatment of behavior. E.g. Burcu M, Zito JM, Ibe A, Safer DJ. Atypical antipsychotic use among Medicaid-insured children and adolescents: duration, safety, and monitoring implications J Child Adolesc Psychopharmacol. 2014;24(3):112-119. I believe this is ethically and clinically a most compelling reason to move into assessing outcomes in community populations. A mixed strategy (first conduct a descriptive claims analysis for a profile of practices and use it as a national sampling frame and then large national prospective cohort from regional academic settings to enhance information on adherence, satisfaction, clinical outcomes. In my assessment, post-marketing surveillance studies of effectiveness and safety are badly missing from the federal research agenda.	Although important, practice patterns goes beyond the focus of this report.

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31.	Peer Reviewer #1	General	The clinical meaningfulness of the review is quite limited. it does seem definitive in recommending that clinicians shouldn't use any of the available alternative approaches to the questionnaires, although hints that CPT and TOVA might be useful. The implications for treatment are more obscure. It seems to say that there is basically no new information from prior recommendations--parent training for young children, MPH or Atomoxetine for older children. The conclusion lumps together cognitive training, CBT, and neurofeedback, says they seem helpful but questions whether this is because of the impact on parents. My read of the evidence tables suggests, though, that CBT is actually useful and the others are not. The statement that there is no data on cardiac risk is not helpful...given the FDA recommendations and the AAP rebuttal. The impact on height of MPH and other stimulants seems consistent in the findings; this might be highlighted more rather than lumped under "no change in BMI."	We feel that the evidence supports the statements we make within the review and as described in the report we are limited by the available evidence. We included cardiac outcomes as an outcome of interest but did not find relevant evidence. Neurofeedback, cognitive training, and CBT each have separate sections describing the findings from the evidence review (e.g., tables 13, 14, and 15). The abstract now calls out the specific findings from these interventions separately. In addition, the Discussion was edited to better reflect these findings. The issue of height change is discussed as appropriate under Detailed Synthesis – Pharmacologic Versus Nonpharmacologic.
32.	Peer Reviewer #2	General	This is a comprehensive, quite valuable review. It is clinically relevant, and addresses many questions that have arisen over the last decade - re: neurofeedback, supplements, new diagnostic tools.	No response needed.

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33.	Public Reviewer #2 (Nathanial Counts, Mental Health America)	General	Dear Director Bindman: Mental Health America appreciates the Agency for Healthcare Research and Quality's (AHRQ) thoughtful update to comparative effectiveness review for diagnosis and treatment of Attention Deficit Hyperactivity Disorder (ADHD) in children and adolescents. Mental Health America (MHA) - founded in 1909 and with over 200 affiliates across the United States - is the nation's leading community-based nonprofit dedicated to helping Americans achieve wellness by living mentally healthier lives. Our work is driven by our commitment to promote mental health as a critical part of overall wellness, including prevention for all, early identification and intervention for those at risk, integrated health, behavioral health and other services for those who need them, and recovery as a goal. As part of our work, MHA strives to provide individuals and their families with the most up-to-date research on treatment options, and AHRQ's comparative effectiveness reviews are essential resources for the organization. To this end, MHA requests that AHRQ consider expanding its evidence review to studies before 2011. While AHRQ did review the literature in 2011, the key questions are sufficiently different between the two reviews that important pre-2011 research may be missed. To ensure that we have the most comprehensive examination of the evidence, AHRQ should ensure that relevant pre-2011 studies are synthesized into this current review. MHA appreciates your consideration and looks forward to continuing to work with AHRQ in providing individuals with the information they need to make informed decisions about their care. Please reach out at any time to Nathaniel Counts, J.D., Director of Policy at Mental Health America at ncounts@mentalhealthamerica.net.	We have now included an expanded description of the previous 2011 AHRQ report with reference to the 2001 report. Specifically, in the Introduction of the current report (page 4) we provide an overview of the reports and how our current report relates in terms of scope. Additionally, for KQ 2 in the Results, we have included a section after the Key Points for each comparison detailing Findings in Relation to What is Already Known, in which we place the findings of the current report in the context of the 2011 EPC Report on ADHD.
34.	TEP Reviewer #2	Introduction	Many readers will need this [MPH] spelled out.	We have verified that all abbreviations are now spelled out when initially used in the report.

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35.	TEP Reviewer #2	Introduction	This [statement that new review builds on the previous review] implies that all of the previous research is also incorporated in this review, which is not the case.	We have clarified in the revised report the role of the previous reports and how this new review differs in scope.
36.	TEP Reviewer #2	Introduction	Citations are necessary here [after statement that psychostimulants are effective], especially since this report's conclusions do not end up supporting this statement.	The introduction now better describes what is known about the role of psychostimulants and a reference has been added to support the potential for psychostimulants to be effective.
37.	TEP Reviewer #2	Introduction	Behavior therapy has also been found to be effective, see previous AHRQ report on preschoolers as well as www.effectivechildtherapy.org for older children, and is recommended by AAP.	The discussion of the role of nonpharmacologic therapies, including behavior therapy, has been expanded. Greater emphasis is placed on how nonpharmacologic therapies can be used with pharmacologic approaches.
38.	TEP Reviewer #2	Introduction	This [AAP recommendation] is not accurate. AAP recommends behavior therapy as the first line for preschoolers, and both stimulant therapy and behavior therapy for school age children, preferably in combination. See points 5 A and B on page 2 of the AAP Clinical Practice Guideline	We now include a sentence reflecting the AAP's recommendation of behavior therapy.
39.	TEP Reviewer #3	Introduction	1. Abstract: "including" should be added in the last sentence of results.	This is corrected.
40.	TEP Reviewer #3	Introduction	2. Which outcomes are of interest in this report, and why? Are we primarily interested in ADHD symptoms? It would be helpful to give a rationale as to why other outcomes (academic functioning, functional impairment, sleep, etc) are meaningful for this population and have implications for long-term health.	Per AHRQ process, the outcomes included in this review were determined through engagement with the nominating sponsor, key informants during the topic refinement stage, public review of the protocol, and then discussions with the technical expert panel. These outcomes were seen as being both patient-centered and most critical for decisionmaking across the multiple stakeholders.
41.	TEP Reviewer #3	Introduction	3. Background, pg 7, line 42. ADHD-Not Otherwise Specified is not mentioned and should be added.	We have made the suggested edit.

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42.	TEP Reviewer #3	Introduction	4. pg 7. Unanswered questions might include which outcomes are important to target, feasibility of treatments in various accessible settings.	Feasibility was not assessed within this review. We looked at all outcomes presented in the included articles as guided by the listed outcomes of interest.
43.	TEP Reviewer #3	Introduction	5. Population pg 8. What about medical populations (concussion, sleep apnea, etc)?	We listed the pre-specified populations of interest – again guided by the original 2011 report, engagement with the nominator, key informants, technical expert panel members, and public posting. Other specific populations were seen as outside of the scope for this report given existing resources.
44.	TEP Reviewer #3	Introduction	6. Pg 9. Adverse effects of diagnosis. What is the evidence that there may be adverse effects of diagnosis, or is it more of a theoretical concern?	Labeling is a theoretical concern for all diagnoses.
45.	TEP Reviewer #3	Introduction	7. Pg 9 line 40 -- should be "Conners"	We have made the needed change.
46.	TEP Reviewer #3	Introduction	8. Pg 9 line 9. Should mention that AAP recommends behavioral intervention as the first line of tx for children under 6	We have made the suggested edit.
47.	TEP Reviewer #3	Introduction	9. Regarding the information reviewed in the first 10 pages - is this background information including evidence from the current review, or from literature up to 2016 in general? It is helpful to clarify at which point in time this is considered to be the state of knowledge. And, it will be helpful to clarify the general purpose of this review section (provide context? summarize literature, including that not addressed by the review, on ADHD?).	The purpose of the background introduction section is to set the stage for the review prior to this current systematic review – we now clarify this in the text.
48.	TEP Reviewer #3	Introduction	10. I would like to see a description of the rationale for updating the 2011 review. Why is 5 years the appropriate time frame for an update? How will this report further knowledge and practices for researchers, clinicians, pediatricians, and families?	More text was added to the Introduction regarding the rationale for this update.
49.	TEP Reviewer #4	Introduction	The introduction is well written, but I have specific quesitons.	No response needed.

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50.	TEP Reviewer #4	Introduction	Page 7, line 18: the sentence that reads "The previous report also concluded that there was sparse evidence... but that treatment for 12 months or longer with MPH or atomoxetine.." Did the authors mean "12 months or shorter?"	We now clarify that we meant 12-24 months.
51.	TEP Reviewer #4	Introduction	Page 9, line 15: As for adverse events, the description should include low blood pressure (alpha-2 agonists) and liver problems (jaundice with atomoxetine).	These were not prespecified adverse events.
52.	TEP Reviewer #4	Introduction	Page 9, line 32. The word "costs" ends the paragraph I would expand it to say, "there may be significant time demands, opportunity and financial costs."	We thank the reviewer for the suggested edit and have made the change.
53.	TEP Reviewer #4	Introduction	Page 9, line 43: I would add "frequency of stimulant prescription renewals (often monthly)...	This description is now added.
54.	TEP Reviewer #4	Introduction	Page 9, line 52-53. Sentence two appears to describe impairments / outcomes in childhood, yet the sentence includes "lower rates of graduation from high school. Based on the format of the paragraph, the next sentence opens, "In adulthood." There needs to be a sentence for adolescent placed here, that could include a few of the items in the sentence at the bottom of page 9 such as "negative outcomes associated with risk taking behaviors such as motor vehicle collisions or other accidents, as well as substance use..." I would add sex without protection.	We have made the suggested edit.
55.	TEP Reviewer #4	Introduction	Pages 14-16 includes a list of abbreviations and their definitions. I think the list could include a few more, such as AACAP (American Academy of Child and Adolescent Psychiatry) on page 14, line 26; COGMED on page 15, line 9; DSM-5 on page 15, line 28; PATS (NIMH Preschool ADHD Treatment Study) on page 16, after line 28; PPP (the Positive Parenting Program) and PIT (Parent Interaction Training) on page 16, after line 28; and PRISMA, which is a standard for presenting flow charts for meta-analyses.	We have updated the abbreviation list to include the missing abbreviations/definitions.

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56.	TEP Reviewer #5	Introduction	The introduction requires additional citations throughout. Statements are made without support, such as on lines 14-18 on page 19. Also, where is the support for the statement on line 41-42 of page 11? The AHRQ reports are seen as valuable research documents that synthesize research from the Intro through the Results and Conclusions. At minimum, key citations should be included for these correct, yet unsupported, statements.	We have added some references to the noted sections.
57.	TEP Reviewer #5	Introduction	Also, this statement is incorrect on line 8-9 of page 12: "The American Academy of Pediatrics (AAP) recommends stimulant therapy as the first line of therapy.11" Combination therapy is recommended for children 6 and older and behavior therapy is first line for children 4-5 years of age.	We have corrected this statement.
58.	TEP Reviewer #6	Introduction	The introduction was informative of the study questions and plan.	No response needed.
59.	Peer Reviewer #1	Introduction	No concerns with the introduction.	No response needed.
60.	Peer Reviewer #2	Introduction	Adequate. However, it does seem that the authors could expand more on the potential for clinical uncertainty, and confusion about the usefulness (or lack thereof) re: neurofeedback, supplements, new diagnostic tools, etc.	We discuss these topics within the discussion.
61.	Public Reviewer #1 (Anonymous)	Introduction	I did not see citations listed in the introduction for outcomes attributed as being associated with ADHD.	We have added in references to these statements.
62.	Public Reviewer #3 (Anonymous)	Introduction	There are multiple problems with this document:	No response needed.
63.	Public Reviewer #3 (Anonymous)	Introduction	1) AAP recommendations are completely wrong. They don't say medication first. They say the opposite for preschoolers- BT first. Combination of BT and medication is best for school age.	We have corrected this text.
64.	Public Reviewer #3 (Anonymous)	Introduction	2) This report is NOT updating, it doesn't address the same questions as before (which was preschoolers). An update implies including all the evidence, new and old, this is not that. This is a review of an arbitrary slice of time for the school age children since the strong prior evidence was never reviewed.	We have clarified the scope of this review within the introduction.

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65.	Public Reviewer #3 (Anonymous)	Introduction	3) This review doesn't properly include lots of evidence from other reviews and metaanalyses. Here is but one very well done review of behavior treatment that you should include and address to fill in the gap in your studies. http://effectivechildtherapy.org/content/attention-deficithyperactivitydisorder-adhd And of course the AAP findings that informed their recommendations in the first place. Those findings should be included.	We have now expanded our discussion of the AAP guidelines in the Introduction (page 2) and Discussion (page 82). We thank the reviewer for the reference to the Evans systematic review. We have reviewed the 21 included articles in this review and have confirmed that they are all either included in our present review or else were excluded based on our inclusion/exclusion criteria.
66.	TEP Reviewer #2	Methods	Because well established therapies were studied in detail prior to 2009 that are now applied in the field, this report does not represent current standard of care	We now discuss this limitation with the discussion section.
67.	TEP Reviewer #3	Methods	1. It is mentioned that some criteria for PRISMA were followed. Please specify which criteria were followed and a rationale for those criteria that were not followed.	All elements of the PRISMA recommendations were followed; this is now clarified.
68.	TEP Reviewer #3	Methods	2. Were quality assessments made by multiple raters as well?	Quality assessments were made by one investigator and overread by a separate investigator and any disagreements were resolved through consensus.
69.	TEP Reviewer #3	Methods	3. Please describe the strategy used for qualitative synthesis.	We now include additional details about the qualitative synthesis methods.
70.	TEP Reviewer #3	Methods	4. Q1 is focused on diagnostic strategies that can be used in primary care. But, it doesn't appear that you excluded studies based on the setting in which the strategy could be implemented. It seems unnecessary to specify primary care settings in Q1 if it did not affect inclusion. This seems more relevant and interesting as topic to be explored in the discussion.	The key question states for use in primary care or by specialists. We considered a broad range of diagnostic tests. We did not restrict solely to primary care. This is now clarified in how the key questions are stated.

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71.	TEP Reviewer #3	Methods	5. Q3. No evidence was found for Q3. I suppose this makes a statement in terms of the need for research in this area, but it seems pointless to have a primary goal of the review defined in a way that cannot be addressed. It may be too late at this point, but it seems reasonable to consider modifying this question so that some information can be reviewed. Stability of measurement over time? Use of multiple reporters? Behavioral observations? Mapping trajectories for individuals with ADHD? Other outcomes, such as functional impairment? Implications for long-term outcomes for young people with ADHD?	We share the reviewer's frustration with there being no evidence found for one of the predefined key questions. Adding in key questions at this point is outside the scope of this review.
72.	TEP Reviewer #4	Methods	KQ1 Populations (PICOTS): As stated above, I urge the authors of this report to give the rationale for the selection of the inclusion/exclusion criteria that appear on pages 18-22. It is noted on page 17 that the panel of 9 key informants (could they be identified?) gave input that led to revisions to "refine the scoping of KQ1." This limites the PICOTS population for the initial diagnosis of ADHD to children 6 years old and younger. Why? This vague phrase does not flesh out why this decision was made. It does not fully describe what other scoping changes were made. I could not find the rationale for that change in the review. This search strategy was not "explicitly stated and logical." Without a rationale, I do not believe that particular inclusion criterion is justifiable.	We now expand within the methods on the reasoning behind our scope given prioritized uncertainties balanced with limited time and resources.
73.	TEP Reviewer #4	Methods	KQ2 Interventions (PICOTS): I would recommend including the FDA-approved long duration clonidine (KAPVAY) in the list of Alpha-2 agonists on page 19.	Clonidine is listed; we did not restrict by duration of action – no change made.
74.	TEP Reviewer #4	Methods	Publications (PICOTS) inclusion criteria on page 22 include the phrase, "Published January 1, 2009 to present." I disagree with the use of the word "present," as the review itself is limited to publications that end on December 31, 2015. I think dates should only be used in this inclusion criterion.	We now include the specific end date for our review in the text. Note that the revised final report now covers evidence through 11-07-2016.

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75.	TEP Reviewer #4	Methods	I am concerned about the extremely brief discussion included in the "Strength of the Body of Evidence" on page 24. The reader is directed to AHRQ's Methods Guide for clarification. However, there is no operationalization (using anchor points) for how the reviewers weighed analyzed the publications to arrive at the vague qualitative descriptors or grades ("high, moderate, low strength of evidence;" "insufficient" if the evidence were too weak). Most of the publications were graded as insufficient in the tables, but is it not clear what effect the grade assigned for bias or for SOE had in the overall interpretation of the published data. The total lack of explanation of the methods use to determine bias in the publication, and the lack of what kind of bias is intended is worrisome. In fact, without clear operationalized criteria shown for each study awarded the "bias" label, the assignment of bias described on page 23 in Table 3 can be suspected of being biased by itself! None of the tables in the review indicate the specific reasons why a study was assigned a particular SOE label or why a particular study was suspected of being biased.	We now include in the Methods Table 4, which provides additional details about the SOE ratings and how they were operationalized. Extracted quality data on specific domains and their assessment are uploaded to SRDR for individual studies.
76.	TEP Reviewer #4	Methods	I also couldn't find the rationale for having different sample size for the randomized controlled trials listed under "study design (PICOTS) criterion on page 21 in Table 2 depending on the type of intervention or treatment component involved. Randomized Controlled Trials (RCTs) with 20 or more participants were accepted for KQ 1 and 3; RCTs with 50 or more participants were required for KQ2, except when 2 or more pharmacological agents were involved, then only trials of 100 participants or more were included). The review indicates that the FDA standard of sample size was included, but the rationale for doing that was never clearly stated. That means that the different key questions had different standards of proof. While this could be justified, it wasn't clarified satisfactorily.	We have now added in additional reasoning to our exclusion criteria column for study design about the sample size requirements.

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77.	TEP Reviewer #5	Methods	Yes, the inclusion and exclusion criteria are stated clearly and generally justified. The midcourse change in the scope of the review (pertaining to the sample size of studies included in Q2) should be further justified. Was this performed simply to reduce the review burden? Or were there implications on the detectable effect sizes of interest? The outcome measures were appropriate and quite comprehensive. The statistical methods were appropriate, with meta-analyses conducted, as possible.	We have now added in additional reasoning to our exclusion criteria column for study design about the sample size requirements.
78.	TEP Reviewer #6	Methods	I am not sure why samples required 50 subjects. Maybe that could be justified.	We have now added additional reasoning to our exclusion criteria column for study design about the sample size requirements.
79.	TEP Reviewer #6	Methods	Criteria for assessing quality of studies could be briefly addressed in a brief report to announce the findings to the clinical practice community.	In addition to the details provided in the methods – we also include in the appendixes the description of the data elements used in the quality forms for rating.

#	Commentator & Affiliation	Section	Comment	Response
80.	TEP Reviewer #7	Methods	I object to the restriction of the review to RCTs/between-group studies rather than all types of controlled designs--RCTs, crossover, and single-subject studies. I said that the limitation to between-group designs was going to result in the very outcome that you obtained no evidence that behavioral interventions work. At the time, I said that would be not because behavioral treatments do not work but instead because the vast majority of the literature for ADHD is NOT RCTs or between-group studies. A review that eliminates 90% the literature for an intervention will likely fall short of the requisit level of evidence. Compare your result with Fabiano et al (ref. below), who did a large meta analysis of BT studies in 2009 and showed clear and substantial effects mostly with elementary-aged children. In contrast to the AHRQ review, Fabiano et al included all types of designs. The same for the Pelham & Fabiano review in 2008 (ref below, which was updated in 2014 by Owens and Evans in the same journal). I have attached below several examples of studies that suggest conclusions different from those drawn by the document REVIEW Fabiano, G., Pelham, W.E.,Coles, E.K., Gnagy, E.M., Chronis,A.M., & O Connor, B. (2009). A metaanalysis of behavioral treatments for attention-deficit/hyperactivity	This review included a broad range of studies that would allow inferences regarding competing therapies. We included traditional randomized trials comparing treatment results over time to individuals allocated to different treatment arms or usual clinical care. These studies typically compare aggregate individual differences over time between the different treatment arms. Cross-over studies could also be included. Unlike the referenced report by Fabiano, we excluded pre-post studies with no comparator and single subject studies (i.e., case reports). Uncontrolled pre-post studies and case reports are subject to high risks of bias. No reference is made to the review by Fabiano et al because of the high risk of bias based on the studies that are included.
81.	Peer Reviewer #1	Methods	I briefly reviewed these, but in all honesty the EPC is more expert in search strategies and statistical methods than I am!	No response needed.
82.	Peer Reviewer #2	Methods	Inclusion and exclusion criteria are appropriate. Search strategies quite clear. Definitions and ouitcomes appropriate.	No response needed.
83.	Public Reviewer #3 (Anonymous)	Methods	As stated, this is NOT an update. It's new for the 6-17 but incomplete. Even for the preschoolers a real update would a) include the previous evidence and look at all of it together and b) look at the preschool data separately because it is very clear that younger children's needs are not the same as older children's needs. Hence the differentiated AAP recommendations - different things work at different ages so the review should look at that.	We have attempted to clarify in the introduction and methods what portions of the original review this updates and where this review is new. In addition throughout the report we now highlight our findings in relation to the previous review as well as highlighting the specific ages targeted by each included study.

#	Commentator & Affiliation	Section	Comment	Response
84.	Public Reviewer #4 (Manisha Madhoo, Shire Pharmaceuticals)	Methods	To Whom It May Concern: Thank you for the opportunity to comment on AHRQ's review of the diagnosis and treatment of Attention Deficit Hyperactivity Disorder (ADHD) in children and adolescents. First, please kindly consider a complete and comprehensive search methodology because the 2016 proposed research key questions are very important and relevant and NOT duplicative of the ones in the 2011 published report (Table 1). Such a comprehensive search, which will include evidence (or published literature) before 2011, will serve many clinicians and various stakeholders, as it will provide them with full scientific and clinical picture on the diagnosis and management of ADHD in children and adolescents.	We thank the reviewer for their comment and understand the differences between the 2016 and 2011 key questions. Unfortunately, extending the existing review to cover the evidence prior to 2011 is not feasible at this time. We hope that the contemporary evidence on the identified key questions is useful to decisionmakers.

85.	Public Reviewer #4 (Manisha Madhoo, Shire Pharmaceuticals)	Methods	Second and linked to the first point, the authors should consider the scientific rigor of randomized, double blind, placebo and/ or active control studies in the pharmacotherapy and non-pharmacotherapy management of child and adolescents patients with ADHD. Please see the list of potential missing studies for your considerations (Table 2). Table 2: List of additional studies for consideration • Spencer TJ, Adler LA, Weisler RH, Youcha SH. Triple-bead mixed amphetamine salts (SPD465), a novel, enhanced extended-release amphetamine formulation for the treatment of adults with ADHD: a randomized, double-blind, multicenter, placebo-controlled study. J Clin Psychiatry. 2008 Sep;69(9):1437-48. Epub 2008 Sep 9. • Biederman J, Boellner SW, Childress A, Lopez FA, Krishnan S, Zhang Y. Lisdexamfetamine dimesylate and mixed amphetamine salts extended-release in children with ADHD: a double-blind, placebo-controlled, crossover analog classroom study. Biol Psychiatry. 2007 Nov 1;62(9):970-6. Epub 2007 Jul 12. • Biederman J, Krishnan S, Zhang Y, McGough JJ, Findling RL. Efficacy and tolerability of lisdexamfetamine dimesylate (NRP-104) in children with attention-deficit/hyperactivity disorder: a phase III, multicenter, randomized, double-blind, forced-dose, parallel-group study. Clin Ther. 2007 Mar;29(3):450-63. • Findling RL, Childress AC, Krishnan S, McGough JJ. Long-term effectiveness and safety of lisdexamfetamine dimesylate in school-aged children with attention-deficit/hyperactivity disorder. CNS Spectr. 2008 Jul;13(7):614-20. • Findling RL1, Ginsberg LD, Jain R, Gao J. Effectiveness, safety, and tolerability of lisdexamfetamine dimesylate in children with attention-deficit/hyperactivity disorder: an open-label, dose-optimization study. J Child Adolesc Psychopharmacol. 2009 Dec;19(6):649-62. doi: 10.1089/cap.2008.0165. • Wigal SB, Kollins SH, Childress AC, Squires L; 311 Study Group. A 13-hour laboratory school study of lisdexamfetamine dimesylate in school-aged children with attention-deficit/hyperactivity disorder. Child Adolesc Psychiatry Ment Health. 2009 Jun 9;3(1):17. doi: 10.1186/1753-2000-3-17.	We include below the disposition of the suggested citations: Spencer: This paper is published prior to our 2009 search limit and so is not included in our review. It was not included in the original 2011 systematic review. Biederman 2007a: This paper is published prior to our 2009 search limit and so is not included in our review. Note that in the 2011 review it was excluded for “No included comparisons of outcomes” Biederman 2007b: This paper is published prior to our 2009 search limit and so is not included in our review. It was not included in the original 2011 systematic review. Findling 2008: This paper is published prior to our 2009 search limit and so is not included in our review. It was not included in the original 2011 systematic review. Findling 2009: This paper is published prior to our 2009 search limit and so is not included in our review. It was not included in the original 2011 systematic review. Wigal: This paper was reviewed but excluded at the full-text stage for no comparator of interest.

#	Commentator & Affiliation	Section	Comment	Response
86.	Public Reviewer #4 (Manisha Madhoo, Shire Pharmaceuticals) (Previous row continued)	Methods	• Findling RL, Childress AC, Cutler AJ, Gasior M, Hamdani M, Ferreira-Cornwell MC, Squires L. Efficacy and safety of lisdexamfetamine dimesylate in adolescents with attentiondeficit/ hyperactivity disorder. J Am Acad Child Adolesc Psychiatry. 2011 Apr;50(4):395-405. doi: 10.1016/j.jaac.2011.01.007. Epub 2011 Mar 3. • Findling RL, Cutler AJ, Saylor K, Gasior M, Hamdani M, Ferreira-Cornwell MC, Childress AC. A long-term o pen-label safety and effectiveness trial of lisdexamfetamine dimesylate in adolescents with attention-deficit/hyperactivity disorder. J Child Adolesc Psychopharmacol. 2013 Feb;23(1):11-21. doi: 10.1089/cap.2011.0088. • Dittmann RW, Cardo E, Nagy P, Anderson CS, Bloomfield R, Caballero B, Higgins N, Hodgkins P, Lyne A, Civil R, Coghill D. Efficacy and safety of lisdexamfetamine dimesylate and atomoxetine in the treatment of attention-deficit/hyperactivity disorder: a head-to-head, randomized, double-blind, phase IIIb study. CNS Drugs. 2013 Dec;27(12):1081-92. • Coghill D, Banaschewski T, Lecendreux M, Soutullo C, Johnson M, Zuddas A, Anderson C, Civil R, Higgins N, Lyne A, Squires L. European, randomized, phase 3 study of lisdexamfetamine dimesylate in children and adolescents with attention-deficit/hyperactivity disorder. Eur Neuropsychopharmacol. 2013 Oct;23(10):1208-18. doi: 10.1016/j.euroneuro.2012.11.012. Epub 2013 Jan 15. • Coghill DR, Banaschewski T, Lecendreux M, Johnson M, Zuddas A, Anderson CS, Civil R, Dauphin M, Higgins N, Lyne A, Gasior M, Squires LA. Maintenance of efficacy of lisdexamfetamine dimesylate in children and adolescents with attention-deficit/hyperactivity disorder: randomized-withdrawal study design. J Am Acad Child Adolesc Psychiatry. 2014 Jun;53(6):647-657.e1. doi: 10.1016/j.jaac.2014.01.017. Epub 2014 Mar 4.	Findling 2011: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Findling 2013:This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Dittmann: This paper was reviewed but excluded at the full-text stage for no comparator of interest. Coghill 2012: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Coghill 2014:This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup".

#	Commentator & Affiliation	Section	Comment	Response
87.	Public Reviewer #4 (Manisha Madhoo, Shire Pharmaceuticals)	Methods	Third, the authors may wish to consider some medical/professional associations' guidelines and research papers published in peer review journals. Some examples include ADHD diagnosis and management guidelines by American Academy of Pediatrics and Academy of Child and Adolescent Psychiatrists (references attached). Recently, American Academy of Neurology experts have published guidance for clinicians on EEG and ADHD (Table 3). These are just a few examples and not all inclusive. Table 3: Medical/ Professional Guidelines •http://pediatrics.aappublications.org/content/pediatrics/early/2011/10/14/peds.2011-2654.full.pdf • http://www.jaacap.com/article/S0890-8567(09)62182-1/pdf •https://www.aan.com/Guidelines/home/GetGuidelineContent/822	We include below the disposition of the suggested citations: • The Pediatrics 2011 paper was excluded at the abstract stage because it is a guideline. As part of our methods, we reviewed the relevant component references for inclusion • JAACAP paper is published prior to our 2009 search limit and so is not included in our review. Note it was included in the 2011 systematic review. • The AAN paper was not included in our search and so we have added in it as a manual include. From screening it was excluded for being a guideline. Per our methods, we screened the component refs for possible inclusion. Note that we also include a brief discussion of these guidelines and their relevance to our review in the revised discussion section.

88.	Public Reviewer #4 (Manisha Madhoo, Shire Pharmaceuticals)	Methods	Lastly, the authors may want to reconsider the exclusion of placebo controlled studies or provide more details and clarity on the rationale for exclusion of these studies (Table 4). Once again, we thank you for the opportunity to be part of the robust discussions. Table 4: List of excluded PLB studies for reconsideration • Waxmonsky JG, Waschbusch DA, Glatt SJ, et al. Prediction of placebo response in 2 clinical trials of lisdexamfetamine dimesylate for the treatment of ADHD. J Clin Psychiatry 2011;72(10):1366-75. PMID: 21367347. • Childress AC, Arnold V, Adeyi B, et al. The effects of lisdexamfetamine dimesylate on emotional lability in children 6 to 12 years of age with ADHD in a double-blind placebo-controlled trial. J Atten Disord 2014;18(2):123-32. PMID: 22740112. • Escobar R, Montoya A, Polavieja P, et al. Evaluation of patients' and parents' quality of life in a randomized placebo-controlled atomoxetine study in attention-deficit/hyperactivity disorder. J Child Adolesc Psychopharmacol 2009;19(3):253-63. PMID: 19519260. • Kratochvil CJ, Vaughan BS, Stoner JA, et al. A double-blind, placebo-controlled study of atomoxetine in young children with ADHD. Pediatrics 2011;127(4):e862-8. PMID: 21422081. • Martenyi F, Zavadenko NN, Jarkova NB, et al. Atomoxetine in children and adolescents with attention-deficit/hyperactivity disorder: a 6-week, randomized, placebo-controlled, double-blind trial in Russia. Eur Child Adolesc Psychiatry 2010;19(1):57-66. PMID:19568826. • Svanborg P, Thernlund G, Gustafsson PA, et al. Atomoxetine improves patient and family coping in attention deficit/hyperactivity disorder: a randomized, double-blind, placebo-controlled study in Swedish children and adolescents. Eur Child Adolesc Psychiatry 2009;18(12):725-35. PMID: 19466476. • Svanborg P, Thernlund G, Gustafsson PA, et al. Efficacy and safety of atomoxetine as add-on to psychoeducation in the treatment of attention deficit/hyperactivity disorder: a randomized, doubleblind, placebo-controlled study in stimulant-naive Swedish children and adolescents. Eur Child Adolesc Psychiatry 2009;18(4):240-9. PMID: 19156355.	We include below the disposition of the suggested citations: Waxmonsky: Excluded at Full Text Level for not original data or publication date prior to Jan, 2009. Childress: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Escobar: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Kratochvil: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Martenyi: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Svanborg 2009a: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Svanborg 2009b: This paper was reviewed but excluded at the full-text

#	Commentator & Affiliation	Section	Comment	Response
			• Takahashi M, Takita Y, Yamazaki K, et al. A randomized, double-blind, placebo-controlled study of atomoxetine in Japanese children and adolescents with attention-deficit/hyperactivity disorder. J Child Adolesc Psychopharmacol 2009;19(4):341-50. PMID: 19702486. • Wehmeier PM, Schacht A, Ulberstad F, et al. Does atomoxetine improve executive function, inhibitory control, and hyperactivity? Results from a placebo-controlled trial using quantitative measurement technology. J Clin Psychopharmacol 2012;32(5):653-60. PMID: 22926599. • Wehmeier PM, Schacht A, Wolff C, et al. Neuropsychological outcomes across the day in children with attention-deficit/hyperactivity disorder treated with atomoxetine: results from a placebocontrolled study using a computer-based continuous performance test combined with an infra-red motion-tracking device. J Child Adolesc Psychopharmacol 2011;21(5):433-44. PMID: 22040189. • Durell TM, Adler LA, Williams DW, et al. Atomoxetine treatment of attention deficit/hyperactivity disorder in young adults with assessment of functional outcomes: a randomized, double-blind, placebo-controlled clinical trial. J Clin Psychopharmacol 2013;33(1):45-54. PMID: 23277268.	stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Takahashi: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Wehmeier 2012: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Wehmeier 2011: This paper was reviewed but excluded at the full-text stage because it "Evaluates only FDA indicated drug(s) and has less than 100 patients and/or less than 6 months followup". Durell: Excluded at full text level for not having population of interest.
89.	TEP Reviewer #2	Results	This [statement from KQ2 results: This updated systematic review—although focused on assessing the comparative efficacy and safety of FDA approved ADHD medications versus placebo—was likewise unable to make definitive conclusions given the small number of studies during the current time period and the limited quality of those studies.] is an example that this current report really doesn't build on the previous report, it is separate, limited, and has a different focus	We now clarify the scope and limitations of the report.

#	Commentator & Affiliation	Section	Comment	Response
90.	TEP Reviewer #2	Results	Please clarify the age inclusion criteria in the previous report focusing on preschoolers and current report focusing on all children [From Key Points "Findings in Relationship to What is Already Known" for Pharmacologic vs Nonpharmacologic referring to 2011 AHRQ report]	This difference in age categories is now clarified.
91.	TEP Reviewer #3	Results	This section was the hardest to read due to repetitiveness and excessive detail in multiple places.	We hope that the reviewer finds the revised final report more clear
92.	TEP Reviewer #3	Results	1. One thought is to include a summary 1-2 sentence "take away" for each type of intervention described.	The summary key points at the start of each section are designed for this type of summary section.
93.	TEP Reviewer #3	Results	2. The strength of evidence by outcome tables were the most helpful to me. But, the narrative of the findings is already given in the detailed synthesis and the paragraphs in the far right column of the tables were clunky and redundant. The table description should be synthesized and shortened for readability.	We understand that there is some repetition in the report but hope that the textual and table formats are of use to diverse readers.
94.	TEP Reviewer #3	Results	3. ADHD labeling / stigma - what was the take away of these two studies?	We have now included an additional summary statement.
95.	TEP Reviewer #3	Results	4. Pg 70 Academic Performance - missing word after "improvement" -- "in"	We have corrected the typo.
96.	TEP Reviewer #3	Results	5. "Findings" Tables. I wonder about the utility of providing means and SD for each outcome in each study when we don't know what the range of the scale is and sometimes a comparison was not done. The listing of measures and comparison of means is very helpful, but detailed info such as this may not be very useful to the reader and makes the tables clunkier.	We keep this information in the findings tables since we feel it could be helpful to some readers. We do however now include Table 4, which describes the various diagnostic tools and their scoring systems and so this Table will help inform the readers' interpretation.
97.	TEP Reviewer #3	Results	6. It would help to know the range and mean ages for studies in the "Findings" tables, rather than only in the table at the end of the document. This would assist with interpretation of for whom the treatment is appropriate or effective. For example, pg. 80 on CBT -- it's hard to discern here which ages this targeted.	We now include throughout the report in the tables information on which age range categories the included studies represent.

#	Commentator & Affiliation	Section	Comment	Response
98.	TEP Reviewer #3	Results	7. Parent training, behavioral training. A good definition is not provided, though heterogeneity across the intervention content is mentioned. It would be helpful to describe the types of strategies, goals, or structures of such programs. Also, targeted age ranges. Same with CBT interventions.	A brief description of parent behavior training and CBT is now provided.
99.	TEP Reviewer #3	Results	8. Other approaches. A list of these interventions would improve readibility, rather than embedding the types within the paragraph.	We now list these specific other approaches upfront in the section when we detail the categorization.
100.	TEP Reviewer #3	Results	9. Table 29, first row, grammatical error in far right column.	We have corrected the typo.
101.	TEP Reviewer #3	Results	10. I'm wondering why CHP is considered an "other approach" rather than a behavioral training invention. If the Abikoff organizational skills program is considered behavioral training, it seems that the CHP would also qualify.	We agree with the reviewer that CHP could be considered a behavioral training intervention. We classified it as an “other approach” because it is multimodal, including academic skills support and recreation time.

#	Commentator & Affiliation	Section	Comment	Response
102.	TEP Reviewer #4	Results	Figure 2 is an important flow chart that depicts the careful search process used to identify the important and acceptable 95 articles from a original pile of 12,281 articles. It would be helpful if the reviewers could use the full PRISMA format that is now employed for many meta-analyses. Other than the removal of 3216 duplicates, there was no indication what exclusion criteria were met that reduced the size of the search from 9,081 to 7,956 articles. It does not explain how 95 articles were removed from the 1125 abstracts that passed to arrive at the 1030 where the articles were then read. Specific exclusion criteria are listed only for the 1030 articles that were removed from the abstracts that passed the earlier triage. Finally, it would be helpful to know which exclusion criteria reduced the size of the approved articles from 95 to 82 articles.	We agree that the flow diagram gives a key depiction of the flow of articles through the literature search and screening process. We have employed PRISMA elements in presenting the number of articles: (1) identified through database searching, (2) removed from the search results due to duplication across databases, (3) identified through other sources (gray literature/ manual searching or referral by investigators), (4) screened at the abstract level, (5) excluded at the abstract level, (6) that passed abstract screening and moved forward to full-text screening, (7) that were excluded at the full-text level (with reasons for exclusion), and (8) that passed full-text screening and were included in the review. We then present the total number of studies that the included articles represent, as in many cases results for one study are published in more than one article; we also present a further breakdown of the number of studies applicable to each KQ. We acknowledge that we do not provide reasons for exclusion at the title/abstract screening level beyond documenting the number of citations excluded at that stage; to provide reasons for exclusion of all citations excluded at that stage (7,956 abstracts for the Draft report) is not currently an element of the standard EPC process and is not part of the PRISMA format for the title/citation level of screening.

#	Commentator & Affiliation	Section	Comment	Response
103.	TEP Reviewer #4 (previous row continued)	Results		In the diagram for the Draft report, we depicted 1,125 articles that passed abstract screening and moved forward to be reviewed in full-text. 95 articles met inclusion criteria at the full-text screening level to be included in the review. We further depicted the exclusion of 1,030 articles that did not pass full-text review and listed the reasons for their exclusion with corresponding Ns for each reason. The final number of included articles for the Draft report was 95; those 95 articles represented 82 studies. The information in the flow diagram has since been updated to reflect additional screening performed for the Final report.
104.	TEP Reviewer #4	Results	For KQ1, the review carries out careful ROC analyses to determine the accuracy of the diagnostic approach based on what the review identified as the "gold standard", mainly the application of the DSM-IV criteria using a rating scale or confirmation by a specialist. While rating scales can identify symptoms and grade their severity, the identification of a diagnostic condition needs to be determined using specific questions with anchor points that are carefully defined. For example, the DSM-IV asks the interviewee if the symptom occurs "often" without defining what constitutes "often." These do not constitute the gold standard for diagnosing ADHD. Rather than rating scales which vary in source and do not use anchor points, carefully done research studies used standardized structured interview forms like the DISC-IV from specifically identified sources such as parents or teachers. A more careful review would have indicated the source of the diagnostic information being recorded by the rating scale. Similarly, the review could require the use of a structured interview with known psychometric properties, and also take into account if the interviewers have had their inter-rater reliability estimated using the kappa statistic.	We now include the gold standard used for the different diagnostic studies in Table 8.

#	Commentator & Affiliation	Section	Comment	Response
105.	TEP Reviewer #4	Results	The results tables for the pharmacologic studies failed to identify the pharmacological agents involved or indicate the dosage ranges used. No attempt was made to develop a range of dose-by-weight measures so the clinician could compare his or her own treatment approaches. No description of the length of the study was made and no indication of the study design was included. Furthermore, no attempt was made to list the reported effect size from the study. For those reasons, I do not believe that the tables are up to the current standards of meta-analytic review.	With the Study Characteristics tables included in Appendix F we include several of these characteristics (agents, dose, study design, length of follow up) which might be helpful to the reader. Abstracting the specific dose per weight was often not possible because this information was not always provided in the articles. Furthermore, many of the studies use dose escalation over time in an attempt to titrate to an effect. Therefore, a decision was made not to abstract specific dosing data during the review process. This limitation is now highlighted in the report.
106.	TEP Reviewer #5	Results	I did not find any studies missing from the review based on my knowledge of the field. However, given the quality of the studies, which were almost all poor, I question whether conclusions may be drawn about the research questions, and Q2 in particular. I appreciated the sections that place the results in the context of previous work. This methodological step should be mentioned in the abstract and a conclusion should be made about what this report adds, given the low quality of evidence overall in this review, relative to the stronger evidence of the previous review.	We have now clarified additional limitations of our review.
107.	TEP Reviewer #6	Results	It is hard to imagine how this report will be incorporated into the clinical practice literature. There was no executive summary although I can see that the discussion might provide that.	We envision that stakeholders will use this report in various mechanisms.
108.	TEP Reviewer #6	Results	Some review questions ignore important caveats. I am not sure the data are persuasive on the effectiveness of atomoxetine vs stimulants. A finding of no difference may just reflect use in different populations. Atomoxetine is viewed by many as inferior to stimulants. ADE comparisons may not be comparing similar populations.	The purpose of the review is to present the findings from the evidence and allow decisionmakers to incorporate this evidence in to their decisions as appropriate. We have highlighted evidence for specific subgroups where available. Unfortunately the evidence is sparse for defined subgroups.

109.	TEP Reviewer #7	Results	The papers below are not in your review. The first has random assignment to medication and to behavioral treatment and combination treatment in a within subject crossover design with a fairly large N (about 50). It compares the three modalities for which you called in your summary of the treatment research gap, but it is not an RCT or a between group study, so I assume that is why it was not included, despite being the kind of study that the document said would be particularly useful It goes along with a study reported by Fabiano et al, 2007, which were the classroom outcomes for the same children. Pelham, W.E., Burrows-MacLean, L., Gnagy, E.M., Fabiano, G.A., Coles, E.L., Wymbs, B.T., Chacko, A., Walker, K.S., Wymbs, F., Garefino, A., Hoffman, M.T., Waxmonsky, J.G., & Waschbusch, D.A. (2014). A dose-ranging study of behavioral and pharmacological treatment in social settings for children with ADHD. Journal of Abnormal Child Psychology, 42, 1019-1031. doi:10.1007/s10802-013-9843-8 I think all three of refs below are in your review but are said not to have outcomes of interest and are therefore not included in your tables. One focuses on peer-relationship interventions, the other on father s parenting, and the third on classroom behavior and academic functioning. I am astonished that those outcomes are not of interest. All three of these are between-group designs and therefore should have been included in your review but were excluded. OConnor, B.C., Fabiano, G.A,, Waschbusch, D.A., Belin, P.J., Gnagy, E.M., Pelham, W.E., Greiner, A.R., & Roemmich, J.N. (2014). Effects of a summer treatment program on functional sports outcomes in young children with ADHD. Journal of Abnormal Child Psychology, 42(6), 1005-1017. doi:10.1007/s10802-013-9830-0 Fabiano, G.A., Pelham, W.E., Cunningham, C.E., Yu, J., Gangloff, B., Buck, M., Linke, S., Gormley, M. Gera, S. (2012). A controlled trial of behavioral parent training for fathers of children with ADHD. Journal of Clinical Child and Adolescent Psychology, 41, 337-345. doi:10.1080/15374416.2012.654464	We thank the reviewer for the additional citations. The paper by Pelham et all was reviewed at the full-text stage but was excluded because it evaluated only an FDA-indicated drug, had less than 100 patients, and had a followup of less than 6 months. The reviewer is correct that the outcomes identified within the first two cited references – although important -- were not identified as outcomes of interest for this review. The third citation was excluded because it had less than 50 patients. We agree with the reviewer that the two final citations (Pelham 2016a, Pelham 2016b) would be excluded from our review, although we highlight in the discussion the importance of behavioral interventions and the need to study them further.

			Fabiano, G.A., Vujnovic, R., Pelham, W.E., Waschbusch, D.A., Massetti, G.M., Yu, J., Pariseau, M.E., Naylor, J., Robins, M.L., Carnefix, T., Greiner, A.R., & Volker, M. (2010). Enhancing the effectiveness of special education programming for children with ADHD using a daily report card. School Psychology Review, 39, 219-239. I believe that the following are the papers of mine to which I think Drs. Greenhill and Wolraich were referring on the call today. They were published in February of this year online. They caused quite a stir in both the media and the academic community. Pelham, W.E., Fabiano, G.A., Waxmonsky, J.G., Greiner, A.R., Gnagy, E.M., Pelham III, W.E., Coxe, S., Verley, J., Bhatia, I., Hart, K., Karch, K., Konijnendijk, E., Tresco, K., Nahum-Shani, I. (2016). Treatment sequencing for childhood ADHD: A multiple-randomization study of adptive medication and behavioral interventions. Journal of Clinical Child and Adolescent Psychology, 1-20 Page, T.F., Pelham III, W.E., Fabiano, G.A, Greiner, A.R., Gnagy, E.M., Hart, K.C., Coxe, S, Waxmonsky, J.G., Foster, E.M., & Pelham, W.E. (2016). Comparative cost analysis of sequential, adaptive, behavioral, pharmacological, and combined treatments of childhood ADHD. Journal of Clinical Child and Adolescent Psychology, 1-12. I think you would probably throw this study out of your review because it is not a traditional RCT and does not have large groups for each outcome but it appears to be the kind of pragmatic randomized trial to which you refer in your section on treatment research gaps. The results are highly relevant to the treatment of ADHD especially in primary care because they show that low doses of treatment are effective, that behavioral treatment first produces the best outcome, and that the cost of intervention that begins with a low dose of BMOD is substantially lower than beginning treatment with medication. Such an approach that would save the healthcare system nearly 4 billion dollars per year if it were implemented with all elementaryaged ADHD children in the U.S.	

#	Commentator & Affiliation	Section	Comment	Response
110.	Peer Reviewer #1	Results	May of my concerns about the results section are noted in my overall comments. Specific areas of note: p. 35--CPT and exec Function and Exec Function tests--these are listed as direct tests, consistent and at least one precise, but the SOE is insufficient; rationale for insufficiency would be helpful.	We have modified the strength of evidence rating to be low for executive function tests.
111.	Peer Reviewer #1	Results	p42..the lack of long term impact from the MTA study, which was the basis for many of the recommendations for medication management, merits greater attention. Whether this simply means they reverted to usual care after the study, or whether the short term visible behavioral impacts don't translate to long term impacts on outcomes is worth noting.	On page 36, the text states “Insufficient evidence is available to know whether this is due to a lack of long-term treatment benefit or reflects the need for more intensive care for the subjects after completion of the MTA study.”
112.	Peer Reviewer #2	Results	In all of the Strength of Evidence tables, in the Limitations columns, the definition (and direction) of the terms high, medium, and low are unclear. Does "High" that a study has "high" limitations? I assume that it is just the opposite, and but this should be made more clear, perhaps in the original methods or as a footnote in the Strength of Evidence tables.	We now provide additional details in the methods section to guide the reader on the meaning of low, medium, or high study limitations.
113.	Public Reviewer #1 (Anonymous)	Results	For tables where quality is mentioned, would be useful to indicate how quality was determined - poor, fair, good in what regard - - this clearly is better stated in the text, but in such a lengthy document people may quickly glance at a table	We now include a footnote in the relevant quality tables that point the reader to the relevant Methods section.
114.	Public Reviewer #3 (Anonymous)	Results	The results basically say that behavior treatment and omega-3 are the only parts that have moderate evidence, but they aren't in the abstract. Those findings should be	We feel that the abstract highlights the main findings of the systematic review. No change made.
115.	TEP Reviewer #2	Discussion/ Conclusion	Since very little new evidence was added since 2011, most likely due to the poor quality of the studies, it would be helpful to encourage funding agencies consider new metrics for improving quality of grants.	We agree that there are many areas of needed future research and in the discussion we point to areas of greatest uncertainty.
116.	TEP Reviewer #2	Discussion/ Conclusion	This [findings suggest that FDA-approved ADHD meds should be primary treatment approach] is not supported by the results. There is no evidence provided that medications should be the first line since the moderate levels are behavior therapy and omega 3	We have modified this statement: and now indicate that “Our systematic review was not able to provide further guidance regarding the comparative effectiveness of FDA-approved medications.” (page 82)

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117.	TEP Reviewer #2	Discussion/ Conclusion	this [FDA-approved ADHD meds for children under 6] is against the AAP clinical guidelines	We have modified this statement and now state that: "Overall, pharmacotherapy has been more studied than other treatment approaches and is generally considered the first approach to treatment for children and adolescents over 7 years of age. Insufficient data were available to determine whether they should be the first line of therapy for children under 7 years of age." (page 84)
118.	TEP Reviewer #2	Discussion/ Conclusion	[conclusion that for ADHD treatment, FDA-approved drugs most likely to be effective and assosciated w/low risk of adverse event] Not supported by current results.	This text in the Discussion has been rewritten.
119.	TEP Reviewer #3	Discussion/ Conclusion	1. I am wondering to what extent the overall conclusions only describe the 2010-2015 literature, completely separating this evidence from literature pre-2010. It seems that the most helpful implications from the paper would come by combining 2011 report conclusions with the current conclusions to give a definitive state of the evidence, especially for treatments that have limited studies available.	We now include a summary table which compares the findings from the 2011 systematic review and our present review.
120.	TEP Reviewer #3	Discussion/ Conclusion	2. There are many compelling intervention strategies described in the paper with small samples, methodological limitations, or simply too few RCTs to be considered strong evidence or to be mentioned in the conclusions. Which treatments do the authors view as promising or in need of further RCTs? This would be useful due to ambiguity in particular about the effectiveness of behavioral treatments.	We agree about the gaps around the effectiveness of behavioral treatments. This has been expanded in the future needs section. We are not prescriptive about study design and outcomes, but agree that it is important to emphasize this need.
121.	TEP Reviewer #3	Discussion/ Conclusion	3. My take-away is that the current report does not add or change any of the current recommendations or best practices related to diagnosis, treatment, or follow-up of ADHD. For this reason, I believe it would be helpful for the authors to do further qualitative synthesis of promising methods/treatments that will be "watched" in the coming years for emergence of further evidence. To me, that would be an important function of this review beyond what we already know and relevant to both research and clinical care.	We agree that prioritization of future needed research is an important step -- although the suggested level of detail and direction is outside the scope of this review.

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122.	TEP Reviewer #3	Discussion/ Conclusion	4. What can be concluded about treatments across different age groups? Though children under the age of 6 are a focus of the review, it is unclear whether the conclusions should be applied universally to this age group, versus school aged children, adolescents, and adults.	Our findings within this report are those supported by the evidence for the key questions (including populations, interventions, and outcomes) of interest. Extrapolating these findings to other populations is outside our scope.
123.	TEP Reviewer #3	Discussion/ Conclusion	5. Individuals with ADHD have other needs to be addressed beyond the outcomes reviewed. For example, health risk behaviors such as substance abuse, binge eating, etc. What are the implications of current research for bettering health outcomes more broadly for individuals with ADHD? How might future research address it?	Although a topic of importance, this is outside of the scope of our review.
124.	TEP Reviewer #3	Discussion/ Conclusion	6. Regarding limitations, can the authors speak to the national/international demographics of ADHD versus the demographics of most samples? Is race/ethnicity, gender, SES in most studies reflective of the population?	Unfortunately this is beyond the scope of our review.
125.	TEP Reviewer #4	Discussion/ Conclusion	I agree that the implication of the major findings of this review are cleary stated, and the future research se5.5)ction clearly translates the findings into new research.	No response needed.
126.	TEP Reviewer #4	Discussion/ Conclusion	Most of the relevant literature has been cited. Some research articles that appeared during the period between 1/1/2009 and 12/31/15 were not included, even follow-up studies of cohorts with ADHD that had been included in the previous AHRQ review of 2011. For example, the NIMH PATS study 6 year followup, that involved over 100 children who had been diagnosed with ADHD when they were in the preschool period (ages 3.5 to 5.5 years) that was published in a peer-reviewed journal in English was missing.	The PATS study was excluded at full-text level for no intervention of interest.

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127.	TEP Reviewer #5	Discussion/ Conclusion	A major concern that I have is drawing any conclusions about the studies in this review, due to the very low strength of evidence for Q1 and Q2 and the complete lack of evidence for Q3. It may be more reasonable to make a conclusion about the lack of quality studies related to diagnosis and treatment and indicate that there is an insufficient quantity of high quality evidence to even summarize the results. My concern is that this review will be used to debunk conclusions of the reviews of previous high quality studies indicating the value of behavioral interventions. In fact, those studies, while older, where of greater strength than anything published in the current review period and can therefore not be compared directly to the previous body of research. The new research recommendations focus on the need for better studies and the recommendations are clear and should be very useful. However states such as these: "Based on the previous evidence review, pharmacotherapy appears to provide the most effective short-term improvement in symptoms. This evidence review did not find additional benefit in adding neurofeedback or behavioral therapy to pharmacotherapy." are not based on sufficient evidence, particularly when taken out of the broader context of research.	We have modified the strength of our statements. Additionally, the quoted statement has been revised as follows: "The 2011 AHRQ systematic review highlighted the benefit of psychostimulants for children 6-12 years of age with ADHD for up to 24 months and found that adding psychosocial/behavioral interventions to psychostimulants is more effective than psychosocial/behavioral interventions alone for children with ADHD and oppositional defiant disorder"
128.	TEP Reviewer #6	Discussion/ Conclusion	K1: Harms of labeling: not sure using parent or teacher opinions is more than opinion. So, not clear what longitudinal cohort would bring to the situation.	We agree. Labeling should be assessed as part of an overall assessment of treatment effectiveness, as described in the text. No change.
129.	TEP Reviewer #6	Discussion/ Conclusion	K2: Much more attention to adverse drug monitoring beyond industry-sponsored trial approach. Tools e.g. PAERS are quite unfriendly and haven't been adapted for community care. Funding for user friendly rating scales for parent monitoring is needed with dosing information and total drug regimen known.	Although a topic of importance, this is outside of the scope of our review.
130.	TEP Reviewer #6	Discussion/ Conclusion	K3: Is the only acceptable study design to assess ADHD monitoring a pragmatic trial? Can data from large claims datasets not be useful in preliminary analysis to establish strategies common in the community. Encouraging cohort study with EMR protocols could be inexpensive and reveal more about the practices that should be prioritized for future research.	Claims data are unreliable for diagnosis. The role of "big data" and electronic medical records is now discussed.

131.	TEP Reviewer #7	Discussion/ Conclusion	FIve years ago, I was asked to comment on the previous AHRQ review of behavioral treatment for ADHD (the 2011 review restricted to preschoolers), and I made the same comment that I have made in this email--your exclusion of withinsubject studies--one of the most well excepted and efficient experimental designs in the mental health field-- has produced erroneous conclusions that border on the ridiculous. I was not asked to review the 1999 AHRQ report, but my criticism is the same. All three documents (1999, 2011, 2016) review a highly selected literature and draw the same erroneous conclusion that there is not sufficient evidence to say that behavioral treatments are effective. During the 16 years since the first of these reviews appeared, the use of medication for ADHD has skyrocketed, while the use of behavioral treatments has not, and they remain far less frequently used than medication. It is my understanding that Medicaid in most states does not cover group behavioral parent training, child social skills training, or teacher consultation visits for ADHD children (I know that is the case in Florida). However, Medicaid covers stimulant medications in all states. Is this outcome a result of the conclusions drawn in the three AHRQ reviews since 1999--that there is insufficient evidence that behavioral treatments work? If so, that would be an unfortunate and iatrogenic result of the massive but misdirected efforts that each of the AHRQ teams has put into these three comprehensive, albeit flawed, reviews. After 50 years, there is still no evidence that medication for ADHD has any benefits on outcomes in adolescence and adulthood, and those outcomes are not good. To conclude that a treatment should be recommended when the long term studies show zero benefits is scandalous and casts a cloud on all of AHRQ's activities. Finally, the final section regarding implications for clinical and policy decision-making appears to be based on the entire literature over the past 17 years but is based only on the last 5. It states that insufficient evidence is available to support behavioral therapies either alone or in combination with medication therapy. However, as I understand it, the current document reviews only the literature that was NOT reviewed in the immediately proceeding AHRQ document, which reviewed the literature between 1998 and 2011. But that document reviewed treatment studies only for children under the age of 5	We thank the reviewer for his review. The rationale for the search strategy are now better clarified. As described above, we did not include study designs that have a high risk of bias. These risk of bias assessments focus on the design and conduct of the studies and through our focus on low-risk of bias studies, attempt to reduce the potential for a systematic error or deviation from the truth. The conclusions have been re-written to address not only this review but the prior review. We agree with the great need to understand the effectiveness of behavioral interventions and we now highlight this need more. We hope that the field can act on this identified gap in the evidence. Also note that the EPC program does not make recommendations, nor fund primary studies, but rather synthesizes and summarizes the state of the evidence. We agree that better evidence is needed to support decisionmaking.

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			not for older children. During that time period, a large number of behavioral treatment studies were published on elementary-aged children with ADHD (cf Fabiano et al, 2009), but they were not incuded either in the prior AHRQ review or in the current review. The impression is given in this review that the entire literature on behavioral treatments has been covered in the prior review and in the current one put together, but that does not appear to be the case there is a 12 year gap of review for behavioral treatments for ADHD children older than 5. This omission clearly warrants correction in the current document. I am very surprised that no one in AHRQ noticed this glaring omission in your reviews.	
132.	Peer Reviewer #1	Discussion/ Conclusion	As noted above, there is a discordance between some of the evidence (e.g., CBT) and the conclusions (page 125).	The text is now more specific around the benefit of CBT and the SOE.
133.	Peer Reviewer #1	Discussion/ Conclusion	page 126--there is a double negative in the table on academic performance. I think it should say neither A nor B were found to improve performance.	We have corrected the typo.
134.	Peer Reviewer #1	Discussion/ Conclusion	The implications for clinical decision doesn't seem to follow the data. For children under 6, seems that the parent training remains preferred (no good evidence for meds, OK evidence for parent training). For children over 6, medication controls symptoms short term. Still no good data that they improve long term outcomes. Negative small impact on height. No other identified risks found (would be good to put a statistical estimate for how high the risk, e.g., of suicide or sudden death, could be and still not be detected). My read (from the review, not outside) is that CBT has promise).	This is now clarified.
135.	Peer Reviewer #1	Discussion/ Conclusion	The research recommendations are clear and follow from the findings. Given prevalence, the lack of data is remarkable.	No response needed.
136.	Peer Reviewer #2	Discussion/ Conclusion	Yes.	No response needed.
137.	Public Reviewer #1 (Anonymous)	Discussion/Conclusion	are there para-ADHD outcomes that these assessment methods or interventions might be useful for? some of these might not be expected to have improvements that map to the same kinds of improvements seen on an ADHD rating scale.	Not relevant; not a topic of this review.

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138.	Public Reviewer #3 (Anonymous)	Discussion/Conclusion	The conclusions are completely wrong. To say that the evidence supports medication first is to ignore the previous AHRQ report which said Parent Behavior Treatment is the best option for preschoolers. And the evidence from other reviews shows that there is WELL ESTABLISHED evidence for a variety of behavior treatments for school age and up. See above.	We have modified the tone of the conclusions.
139.	TEP Reviewer #3	Clarity/ Usability	The paper is well organized and thorough. And, as you know, extremely lengthy and cumbersome to read. As I have already mentioned, I would recommend simplifying tables for readability by reducing paragraph to bullet points and eliminating some specific data that do not enhance understanding. I would also recommend a 1-2 sentence "take away" from each section, formatted in a way that makes it easier for those who would only skim the report to digest	To help the reader we now include summary sections throughout the report that put our findings in relation to what was previously known. We have also modified the key points to only highlight the areas where the strength of evidence suggested sufficient evidence for findings.
140.	TEP Reviewer #3	Clarity/ Usability	The authors are naturally extremely conservative in their conclusions, which is appropriate for this review. However, I was hoping for more on future directions or qualitative findings to simulate further thinking and inquiry. Given you are not changing the standing recommendations about ADHD diagnosis/treatment, how can the review stimulate or improve the reader's knowledge and conceptualization?	We require evidence to support any conclusions that we make within the report. However, we do believe that there is an opportunity to better evaluate behavioral interventions. This is now highlighted in the text. Note also that the EPC program does not make clinical recommendations but rather speak clearly to the evidence and provide discussion of the implications for decisionmaking in the discussion.
141.	TEP Reviewer #4	Clarity/ Usability	The report was well structured and organized, and the main points are presented clearly.	No response needed.
142.	TEP Reviewer #4	Clarity/ Usability	The conclusions are difficult to interpret as the majority of studies have a strength of evidence grade that is "low" or "insufficient." Yet the conclusions make clear recommendation that "For ADHD treatment, FDA-approved drugs are most likely to be effective and appear to be associated with a low risk of adverse events." Yet these same studies are rated "low" or "insufficient" in their strength of evidence. How can those two ideas be reconciled?	We have modified the strength of our statements. Note also that the EPC program does not make clinical recommendations but rather speak clearly to the evidence and provide discussion of the implications for decisionmaking in the discussion.

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143.	TEP Reviewer #4	Clarity/ Usability	One minor correction is on page 130, line 20. The sentence beginning "The criteria was less stringent" should be "The criteria were less stringent.	This is corrected.
144.	TEP Reviewer #5	Clarity/ Usability	Yes, the report is well-structured and organized. The main summary point that I would make from this review is that there is a large volume of poorly done ADHD research that lacks the quality to inform the field. I think this conclusion should be made more strongly, including in the abstract to orient the reader.	No response needed.
145.	TEP Reviewer #6	Clarity/ Usability	Clarity is reasonable but usability is not clear. I appreciate how labor intensive this study was but can't see much light from the detailed but still narrow often opaque assessment.	No response needed.
146.	Peer Reviewer #1	Clarity/ Usability	The structured abstract seems to exclude important findings (e.g., related to treatment). The general structure is OK. I was confused when so many different things were lumped under non-pharmacologic treatments. Some were herbal supplements, some are formal manualized treatments, some appear more casual, etc. I would differentiate and separate.	A sentence was added in the introduction to help readers understand the broad range of nonpharmacologic approaches ("Understanding the role of nonpharmacologic therapies can be challenging because they encompass a broad range of approaches to care, ranging from highly structured behavioral interventions to complementary medicines."). In addition, we have split up these approaches in the relevant sections to help with readability.
147.	Peer Reviewer #2	Clarity/ Usability	Yes.	No response needed.
148.	TEP Reviewer #7	References	I suggest that AHRQ conduct a review of the behavioral treatment literature for ADHD from the early 1990s to date WITHOUT excluding crossover studies. There are a large number that show very large benefits of behavioral treatment. That exercise would demonstrate to the administrative hierarchy that you have a serious problem in your last three reviews of treatment for ADHD that begs for correction. Please see the citations in my comments above for key reviews of the literature over the past 20 years.	We thank the reviewer for their comment. Topics for new systematic reviews can be nominated to AHRQ through the Effective Healthcare website.