Reviewer(s)' Comments to Author:  
Reviewer: 1  
  
Comments to the Author  
The expressive writing procedure is a very intriguing field given the variety of beneficial outcome findings associated with expressive that have been reported and the variety of samples that have been examined. Better understanding for whom and under what conditions expressive writing is beneficial is an important area of investigation. Despite the potential importance of the meta-analysis described in this manuscript, there are significant conceptual and methodological limitations of the manuscript.  These issues are described below.  
  
1.      The introduction indicates that the expressive writing procedure developed by Pennebaker and Beall (1986) instructs individuals to write about traumatic events. This is not an accurate description as the instructions are to write about a “stressful or traumatic” life event. The types of events that are written about widely varied (parent divorce, sexual assault, moving to college).

**Changed this.**   
  
2.      When reviewing meta-analytic studies of expressive writing in the introduction section, a description of the large (over 500 studies), meta-analysis on expressive writing conducted by Frattaroli (2006) is generally neglected. This omission is curious given the authors focus on the small sample sizes of meta-analyses that have been conducted in this area. Moreover, this meta-analysis provided interesting findings regarding moderators of expressive writing, and categorized outcome measures. Lastly, Frattaroli only included studies that used a control condition and randomization

**Added more on the Frattaroli part.**   
  
3.      Describing the Reinhold et al. study as including randomized controlled trials is somewhat misleading as this study included studies that randomized participants to conditions (expressive writing vs. control condition). Moreover, the manuscript indicates that the Reinhold study did not find evidence supporting expressive writing as an intervention, but this meta-analysis only examined depression outcome measures.

**Updated this meta’s description**   
  
4.      The introduction section places a heavy focus on framing the expressive writing procedure within an ACT-based framework. However, many theories have been suggested and it’s likely that there are multiple underlying mechanisms that account for the beneficial outcome associated with expressive writing. One recent theory proposed is using a distance perspective (see Kross and colleagues). This theory has some empirical support as well.

**Added this theory. Just need to find a citation.**   
  
5.      The focus on describing expressive writing as an “intervention” seems premature given that the vast majority of expressive writing studies include college student samples, often not selected based on measures of psychopathology.

**Fixed to tasks or writing depending on the context of the sentence**  
  
6.      The description of studies examining the effective of expressive writing reducing PTSD is incomplete. One study is cited in which individuals with a diagnosis of PTSD were recruited and showed no change relative to a control writing condition. However, there are several other studies using individual selected on the basis of at least moderate PTSD symptom severity that have shown significant reduction in PTSD symptom severity relative to a control condition (and relative to baseline score of PTSD), and one of these studies is later described in the section on PTG (but this study did not examine PTG).  Moreover, the interpretation of the greater heart rate finding for expressive writing condition in the Sloan et al.  (2011) study was not that expressive writing is not efficacious for PTSD – as the introduction indicates -but rather that 3, 20 minute sessions was not a sufficient dose.  Moreover, the study by Di Blasio et al., (2015) did not select women on the basis of PTSD or PTS symptoms. Overall, the description of the literature regarding PTSD or PTS and expressive writing is selective and what is reviewed is not accurately described.

**-added the Sloan study to the PTSD paragraph and excluded from the PTG paragraph based on their feedback.**

**-excluded the heart rate thing**

**-also fixed Di Blasio**

7.      The definition and general description of QOL is not accurate. There is clearly value in examining whether expressive writing affects QOL but a definition of QOL that is widely accepted should be presented.

**Fixed into a widely acceptable definition**  
  
8.      The goal of the study is to examine the efficacy (not effectiveness) of expressive writing following a traumatic event, and outcome measures related to PTSD, PTG, and QOL are examined. However, studies were not selected based on whether or not individuals experienced a traumatic event as defined by PTSD diagnosis. Moreover, the majority of individuals who experience a traumatic event are resilient and do not go on to develop symptoms of PTSD, including subthreshold level. Thus, selecting studies that selected individuals who experienced PTSD and have symptoms of PTSD would be critical to the meta-analysis.

12. Overall, it’s difficult to draw any meaningful conclusions from this meta-analysis given that studies were not selected based on selection of PTSD or PTS participants, and the methods used across the studies included vary widely (e.g., selection and type of participants, different number of sessions, duration of sessions, instructions for writing, where writing took place, variation in outcome measures)

1.      P. 12 states, “this particular meta-analysis examines studies of patients with different types of psychopathology and medical diagnose on PTG, QOL, and PTS.” I think this broad focus on disorders is problematic given comments above regarding potential variability of outcomes across disorders. This could result in some studies with large effects, some studies with small/negligible effects, and an overall non-effect to small effect. Also, how can you say they are PTS without a diagnosis of PTSD, or at least Criterion A exposure? PTG should likewise occur in the context of trauma exposure.

**Dr. B – was thinking we could maybe go through the included studies, separating our effects into those with a PTSD diagnosis and those with not, then an overall (like we had). So we would have to add the ES for those with PTSD and sub-clinical in the PTS section. What do you think?** **I combined all of the comments relating to this above. Both reviewers had same comment.**  
  
9.      The decision to only examine the experimental condition is a major limitation of the study. The authors state that this decision was made because the degree of change within the experimental condition is important. That is accurate, however, to most accurately understand the effect of an experimental condition the control comparison needs to be considered. This is especially important in PTSD where symptoms change over time without any intervention.

**Well I don’t think there’s any fixing this so I’m just going to consider this one done.**   
  
10.     The typical methods for meta-analysis is that two individuals review and rate each identified article to ensure inclusion/exclusion criteria are met.  It does not appear that happened in this study and it’s unclear how studies were evaluated for inclusion. Moreover, a clear description of inclusion/exclusion criteria is not provided.

**Dr. B I added this but could you check because I’m having a hard time remembering exactly how we screened since it was so long ago.**   
  
11.     A table of studies included in the meta-analysis and characteristics of these studies is important to include in the manuscript.

2.      I understand there is a table detailing all articles used for the meta-analysis, but more information is needed in the text to help the reader conceptualize the studies. For example, of the 45 articles, how many informed the PTS, PTG, and QOL analyses each? What treatments were represented? What disorders were the focus of the treatment?  

**Do you think we could have someone on the paper do this? Do you think it’s necessary or just a preference thing? Combined these two comments together from both reviewers. I guess we could just incorporate the meta\_data.csv file into a table, adding PTSD diagnosis/no PTSD and then specifying the type of writing (Pennebaker or WET trademarked by Sloan). Maybe that’s why our results are low.**   
  
  
Reviewer: 2  
  
Comments to the Author  
This article reports a meta-analysis of the effectiveness of expressive writing on post-traumatic stress (PTS), post-traumatic growth (PTG), and quality of life (QOL). Their aim is to examine within-treatment effects and to use newer techniques that have not been implemented in priori meta-analyses (i.e. power for effects on intervention groups, p-Curve, p-Uniform). Below is a commentary on the manuscript by section.  
  
Introduction  
1.      The Introduction needs considerable reorganization and focus. As the authors aptly point out (p. 8), “it is important to focus on individual variables in order to determine the effectiveness of expressive writing for specific diagnoses and psychopathology.” Yet, the Introduction canvases multiple different disorders (e.g., somatic health symptoms, psychological well-being, general health, physical health, grades, self-efficacy, depressive symptoms, post-partum distress), which may have varying effects in terms of outcomes. Moreover, often the DV isn’t clear (e.g., Henry et al. (2010) “benefited a rural population for those individuals surviving breast cancer.” On what outcomes? Same question with Lancaster et al., and other citations throughout the Introduction). To the authors’ point, expressive writing may vary broadly across conditions and outcomes. Thus, it is critical to pinpoint the variables of interest within the meta-analysis.

**Added the outcome variables for henry and Lancaster**  
  
2.      Comment #1 is accentuated in the context of the lacking review of PTS, PTG, and QOL. Of particular concern, the review of PTS is very skewed and does not put the studies into context of treatment development. For example, the one Sloan study cited in the PTS section is from 2011, which was when the narrative writing treatment was being developed. That is, this was one of the first published RCTs that indicated that WED (Written Exposure Disclosure) was NOT significantly different from control comparison. This let to further development and refinement of the treatment, resulting in Written Exposure Therapy (WET), which is the current treatment being utilized for PTSD (Sloan et al., 2012). The inclusion of WED studies in the meta-analysis is misleading because this treatment is no longer being used, and in fairness to Sloan and colleagues, they admitted (and published in 2011) that WED didn’t work, which was why they revised it. Moreover, their most recent trial (JAMA, 2018) does not seem to be included, leaving me to wonder if the authors’ selection of studies was skewed and not within the context of treatment development.

**I added this into the intro but we need to decide if we want to separate the WED (Pennebaker) from WET (Sloan 2012 and apparently now the mainstream writing therapy for PTSD). Add JAMA 2018, sloan 2012. Do another lit review to ensure we didn’t miss anything.**   
  
3.      The review of the literature on PTS and QOL is likewise sparse, and would provide a useful context for understanding and contrasting the meta-analytic results.

**Need to do this.**   
  
4.      It is hard to tell from the description, which types of expressive writing treatments were included across investigational groups.

**Specified this in the ‘current meta-analysis’ section**  
  
5.      The authors discuss in numerous places about the importance of value-congruent behavior, values, and ACT. This relates to point #3 in terms of the treatments being used because not all expressive writing treatments focus on values, rather on expression of thoughts and emotions and writing about the details of the trauma. In the introduction, it is not clear to me how ACT and values driven behavior are relevant to the meta-analysis, and the authors do not come back to this point in their Discussion.

**Fixed I tried to cut down a lot of the ACT focus in the intro.**   
  
6.      The authors define PTSD according to DSM-5 criteria, but then acknowledge later on that most studies are based on DSM-IV criteria. The reader should be informed of DSM-IV criteria and the discussion should include this as a limitation. This would also help the reader to understand the Figures better that focus on PTSD symptom clusters.

**Fixed added the DSM-IV criteria within the introduction (3 symptom domains instead of 4 I think)**

7.      There are several places in which the organization and flow of the manuscript need revision. For example:  
a.      P. 2. The authors begin to talk about how expressive writing can increase mental and physical health. They then shift to discussing how disclosure of harmful thoughts and emotions can lead to negative outcomes, but then quickly revert back to problems with repressing emotional experiences.

**Tried to make this more clear in the first paragraph.**

b.      P. 7. The authors separate Frattaroli (2006) from the other meta-analyses, likely because it is somewhat different in focus but is a bit awkward. Within that paragraph they introduce Frattaroli, talk about differences with Smyth (1998), talk about the current study, and then go back to describing the outcomes of Frattaroli. Suggest talking about each study, including the number of studies included in the meta-analysis and outcomes, before moving to the next so the reader has a comprehensive overview of each study.

**Described these more sequentially.**

c.      The authors begin to discuss theory on p. 5. They mention cognitive-processing and inhibition theories, but do not describe or tie them into the context of expressive writing, which is curious because of Pennebaker’s findings regarding processing. They then describe “the first theory” of the social integration model very briefly, but the structure is not followed by second and third theories. They do discuss exposure models, which feels most on target for expressive writing intervention, and conclude with ACT, which is not a theory and an entirely different treatment model.

**Rephrased and made that section more general.**

d.      P. 6 mentions the authors’ definitions of effect size using Cohen. This better fits within the Methods.

**Moved to methods**

e.      The authors describe a good rationale for their analyses and focus on PTS, PTG, and QOL on p. 8, but this should come earlier.

**Moved to earlier**

f.      There are studies in the PTG section that are better suited for PTS (e.g., Sloan et al., 2007, noting comment above re: WED).

**Cut Sloan from this section will just have to expand the PTG with other, more suitable references.**   
Current Meta-Analysis

Method  
1.      P. 13, search terms do not list “narrative writing”, which seems to be a notable omission given Pennebaker’s narrative therapy.

**Added this.**

Discussion  
  
1.      The findings are not well conceptualized in terms of the existing literature or theory, which is in part related to comments above regarding the summary of the literature and theory in the Introduction.

**Need to do this – save until we update other stuff**

2.      Four figures are included regarding PTS and its symptom clusters, yet symptom clusters are not discussed in the Discussion.

**Need to do this – save until we update other stuff**

3.      PTG is a very challenging construct to study, mainly because one doesn’t know where the person started in order to determine whether growth occurred and there is a problem with ceiling effects. For example, on the PTGI (presumably the measure used in most studies), one could respond 0 to the item, “I have a greater appreciation for the value in my own life” because they already had a high level of appreciation for the value in their life (i.e., ceiling effect). This conceptual issue could account for the non-effect for PTG and warrants discussion.

**Need to do this – save until we update other stuff**

4.      The reference to the Sloan et al. (2011) paper in the discussion (“results are in contrast to….”) does not seem fitting given comments regarding WED and WET above.

**Need to do this – save until we update other stuff**   
  
Minor issues  
1.      Inconsistent use of acronyms or defining of acronyms.

**Fixed**