PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The psychological impact of injuries sustained in motor vehicle crashes: Systematic review and meta-analysis
AUTHORS	Craig, Ashley; Tran, Yvonne; Guest, Rebecca; Gopinath, Bamini; Jagnoor, Jagnoor; Bryant, Richard; Collie, Alex; Tate, Robyn; Kenardy, Justin; Middleton, James; Cameron, Ian

VERSION 1 - REVIEW

REVIEWER	Felicity Goodyear-Smith University of Auckland, New Zealand
REVIEW RETURNED	05-Apr-2016

GENERAL COMMENTS	This is a well conducted systematic review and meta-analysis. The methods used in conducting the review and analysing the findings are robust and appropriate.

REVIEWER	Gerasimos Kolaitis Department of Child Psychiatry, Athens University Medical School, Agia Sophia Children's Hospital, Greece
REVIEW RETURNED	17-Apr-2016

GENERAL COMMENTS	This is a well-written, interesting and important systematic review and meta-analysis of studies on psychological impact following MVC injuries. Psychological impact of MVC is a relatively neglected area taking into account the serious and chronic consequences may have for MVC victims and their lives.

REVIEWER	Filip Arnberg Uppsala University, Sweden
REVIEW RETURNED	10-May-2016

GENERAL COMMENTS	This meta-analysis set out to examine the role of injury on psychological distress after motor vehicle crashes. The design of this meta-analysis gives rise to several challenges in the interpretation of the results. Attempting to summarize a heterogeneous sample of studies, the authors have pooled very different outcomes, e.g., self-reported trait anxiety or depressive symptoms and clinician-assessed generalized anxiety disorder and PTSD. However, outcomes should operate similarly after highly stressful events if they are to be pooled to reflect aspects of a latent construct of psychological distress after MVCs. If not, the

quantitative summary may not be meaningful. Unfortunately, the reader is not provided evidence for the similarity of the pooled outcomes in this regard. The authors are recommended to conduct separate pooled analyses on sets of more homogeneous outcomes.

I provide other specific points below.

PRISMA Checklist
It would be helpful if the authors submitted a PRISMA checklist that corresponded to the manuscript. For example, I did not find any information about the existence of a review protocol on pages 5-6 (Checklist #5). I did not find any definition of the outcome despite the indication in the checklist (#11) that there was a definition on pages 5-6.

Introduction
Page 6 line 3: Are the authors sure about >40% PTSD after MVCs? That is similar to PTSD after rape.

P6 line 17: The authors could clarify why they chose this particular focus of this review.

Methods
The authors are recommended to provide additional details of their search strategy—perhaps in an appendix.

The reader is not given a working definition of psychological distress and this makes it is difficult to judge whether the choice of search terms could provide good coverage.

I do not understand the section about MeSH. What did the authors search for in the databases that do not use MeSH, eg PsycINFO (please correct your spelling of this database)? I think that additional explanation is needed in addition to citing ref. 26. Also, were the authors aware of the limitations of using MeSH headings, as noted in several studies? How did the authors source relevant studies from journals?

The authors state that one inclusion criterion is that the study should have a majority of injured participants. However, they report <50% average rate of SCI—note that his is recurring throughout the manuscript, where 45% is referred to as “majority”, which is indeed confusing for the reader. Please clarify.

What is meant by ”coding reliability was established by one other author”? Please clarify, perhaps the best way is to state what this person did—did they independently code all studies, a subset, or what?

P8 line 10: what is meant by, ”R…Software was used to perform … related statistical outputs”?

The paragraph on fail-safe N could be shortened. And I think that the authors have misunderstood the file-drawer problem—who would assume that it is 5% in the literature for this meta-analysis? Please revise.

P11 Table 1: The authors fail to indicate what measure (if any) was used to make a diagnosis—e.g., the CAPS, SCID, or MINI.

	Results The placement of the sensitivity section in the results is confusing to me. This section could go into the last parts of the Results. P17 line 30: The authors should not pool injury types in the analysis of time since injury if they found it unsuitable in the primary analyses. This analysis also is highly suspect given the variations in time between event and assessment in controls. Discussion Curiously, the authors repeatedly discuss compensation issues despite there being no meta-analytic results to discuss, see specific points below. This study does not include any meta-analytic results for compensation claims and so it seems odd that they discuss this issue in favor of other pertinent issues. P17 line 54: Not sure why the authors here choose to say anything about distress following the injury types not included in the analysis. Just state that it was not possible to conduct analyses for these types of injury. P18 line 19: Perhaps I do not understand the authors' intentions with the sentence "Further research is required to clarify the role of trauma exposure and severity…"—is there really nothing in the literature about the role of trauma exposure and severity? Is it only likely that distress is elevated by a traumatic accident in the absence of injury? P18 line 34: What findings in the meta-analysis suggest that poor coping with these stressors increase the risk of mental disorders? Also, note that compensation status was not analysed yet the authors suggest altering compensation processes to make them less distressing. P19: "For mTBI, the majority of studies found mTBI produced higher levels of distress than general trauma injuries": This conflicts with the results section: "removing this study [comparing mTBI to noninjured ctrls] from the meta-analysis resulted in a reduced and non-significant summary effect size of 0.18 (95%CI -0.002-0.37; p=0.052)" Please revise. P19 line 34 "There are no obvious reasons why psychological distress in people with a WAD should be higher than in people with SCI or mTBI." I suggest that the varying proportions of actually injured people in the three groups and the differences in outcome measures need to be addressed here. Any comparison among injury types would need to take this imbalance into consideration. Also, the authors' discussion here (line 50f) would benefit from considering the reliable finding of social support as a determinant for posttraumatic stress. Please relate this discussion to the literature on social support after traumatic events. Also note that there have been discussions about how a (visible) traumatic injury may serve as a testimony to the horrific events, leading to perhaps beneficial effects of the injury— this is related to the authors' deliberations on WAD. P20 line 34: Again, the authors discuss the role of compensation claims for distress.

	P21 line 10: The authors should clarify the limitations of performing meta-analysis when there are varying proportions of injured participants in these samples. P21 line 23: What is meant by ”This bias phenomena”? P21 line 32: There are scores of potential sources of heterogeneity. One issue I think need attention in the context of meta-analysis is the characteristics of the control groups, e.g., with regard to their status on the time from event to assessment—we know that posttraumatic stress diminishes with time for a majority and so variations here will surely lead to heterogeneity. This needs to be addressed and the authors are recommended to expand on these differences and be clear about the consequences of this variation in their discussion. Page 22 line 14: Yet again, the authors discuss compensation.

REVIEWER	Richard Meiser-Stedman University of East Anglia, UK
REVIEW RETURNED	24-May-2016

GENERAL COMMENTS	This meta-analysis, which summarises the literature concerning the psychological impact of injuries sustained in the MVCs, is an important and very welcome contribution. The topic is important and the meta-analysis is rigorous and well-conducted. I thought the paper was well-written. Methods: Could you authors state which data points were used in the meta-analyses? Presumably the first data point was used, but this needs to be clearer. Was data excluded if too close to the injury, e.g. within a month? P12: “Comparing an injury such as mTBI to a minor trauma injury does not control for the experience of injury.” The use of a trauma injury control group does control for the experience of injury; the point made in the following sentence (i.e. this is a tough comparison group, as distress might be expected in a injury-exposed, non-mTBI group) seems to assume this. This point needs to be made more clearly. Discussion: P19 “There are no obvious reasons why psychological distress in people with a WAD should be higher than in people with SCI or mTBI.” This claim is misleading and needs to be changed; the distress levels in these groups were not directly compared. They yielded different effect sizes, but this could related to multiple other factors. P22 “Involvement in compensation will likely add to this distress.” For the Conclusion sub-section, this point seems unwarranted and a distraction from the important actual findings of the meta-analyses conducted. Minor comments: P4: “Both mild to moderate TBI and MI are prevalent following a MVC.13,14,15” Adding a percentage range here would be helpful.

	p6: Psychinfo (see also Abstract)

REVIEWER	Dr Krista Lanctot Sunnybrook Research Institute, Canada
REVIEW RETURNED	16-Jun-2016

GENERAL COMMENTS	Ashley et al performed a meta-analysis to determine the psychological impact of motor vehicle crashes related to 3 specific injuries: whiplash associated disorder (WAD), spinal cord injury (SCI) and traumatic brain injury (TBI). The PRISMA guidelines were followed. The authors appropriately chose a random effects model, taking into consideration heterogeneity. Several factors could be considered to improve the analysis. For outcomes, the authors combined anxiety, depression and injury specific scales. This should be discussed and evaluated in a sensitivity analysis. The authors state that an inclusion criterion was that the majority of participants had to be injured in a MVC, and yet state that the mean percentage of participants injured with a MVC in the SCI group was less than the majority: 45%. please clarify. The authors should state how many times that disagreement on coding had to be resolved by a third author. The authors state that 34 articles that met other inclusion criteria did not meet quantititative eligibility criteria. Please reference those articles. Were all authors contacted and refused to provide the needed quatitive data? I am concerned that removing a single study with a different control group (healthy non-injured controls instead of nonTBI controls with minor injuries) from the TBI group renders the effect size non-significant. The conclusions as stated do not reflect this uncertainty and negative finding. The fact that heterogeneity is reduced suggests that studies with different control groups should not be combined. I would like to see statistics reported in the abstract to support statements made there. Quality of the studies and the impact of quality on the findings should also be discussed. The possible impact of preexisting psychological problems are appropriately discussed. The authors state that they used linear regression. They should justify the choice of Linear Regression rather than a Mixed-Effects Meta-Regression Model, which would normally be used. Interesting review.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Felicity Goodyear-Smith
Institution and Country: University of Auckland, New Zealand
Competing Interests: None declared

This is a well conducted systematic review and meta-analysis. The methods used in conducting the review and analysing the findings are robust and appropriate.
Answer: We thank the reviewer for her comments.

Reviewer: 2
Reviewer Name: Gerasimos Kolaitis
Institution and Country: Department of Child Psychiatry, Athens University Medical School, Agia Sophia Children's Hospital, Greece
Competing Interests: None Declared

This is a well-written, interesting and important systematic review and meta-analysis of studies on psychological impact following MVC injuries.

Psychological impact of MVC is a relatively neglected area taking into account the serious and chronic consequences may have for MVC victims and their lives.
Answer: We thank the reviewer for his comments.

Reviewer: 3
Reviewer Name: Filip Arnberg
Institution and Country: Uppsala University, Sweden
Competing Interests: None declared

This meta-analysis set out to examine the role of injury on psychological distress after motor vehicle crashes. The design of this meta-analysis gives rise to several challenges in the interpretation of the results. Attempting to summarize a heterogeneous sample of studies, the authors have pooled very different outcomes, e.g., self-reported trait anxiety or depressive symptoms and clinician-assessed generalized anxiety disorder and PTSD. However, outcomes should operate similarly after highly stressful events if they are to be pooled to reflect aspects of a latent construct of psychological distress after MVCs. If not, the quantitative summary may not be meaningful. Unfortunately, the reader is not provided evidence for the similarity of the pooled outcomes in this regard. The authors are recommended to conduct separate pooled analyses on sets of more homogeneous outcomes.
Answer: Homogenous analyses (sensitivity analyses) were run on the different outcomes for depressive mood, anxiety and PTSD. These generally confirmed the results of the pooled analyses. Consequently, we have added the following at the end of the Results (page 18):

“Because various psychological distress outcome measures were pooled in the meta-analyses, a series of homogenous analyses were conducted with similar measures (i.e. only depressive mood measures, anxiety measures or PTSD measures) across the three injury types. These analyses confirmed the quantitative results of the meta-analyses for the three injury types. For instance, for mTBI, the depressive mood analysis revealed an effect of 0.20 (95% CI -0.18-0.58), and for WAD, it revealed an effect of 0.99 (95% CI 0.62-1.37), both of which are similar to the overall effect for mTBI and WAD (see Fig. 3 and 4 respectively). For anxiety in WAD, the homogenous analysis resulted in an effect of 0.78 (95% CI: 0.33-1.24), which is similar to the overall effect for WAD. However, a trend existed in which the effect for PTSD was lower for WAD and mTBI.”

I provide other specific points below.

PRISMA Checklist
It would be helpful if the authors submitted a PRISMA checklist that corresponded to the manuscript. For example, I did not find any information about the existence of a review protocol on pages 5-6 (Checklist #5). I did not find any definition of the outcome despite the indication in the checklist (#11) that there was a definition on pages 5-6.
Answer: The PRISMA checklist has been updated.
Further, psychological distress has now been defined at the end of the Introduction on page 5 as: “Psychological distress was defined as an unpleasant condition that can negatively influence daily functioning, including a range of symptoms commonly believed to be troubling and disturbing, such as

elevated anxiety and depressive mood."

Introduction
Page 6 line 3: Are the authors sure about >40% PTSD after MVCs? That is similar to PTSD after rape.
Answer: This has been changed on page 5 to " and from 20-40% ... (ref 7 and 16)

P6 line 17: The authors could clarify why they chose this particular focus of this review.
Answer: Thank you for this comment. We have added further clarification to the text on page 5 and the following was added: "This has not been done previously and psychological distress was made the focus of this study as the impacts of MVCs are potentially serious and personally traumatizing."

Methods
The authors are recommended to provide additional details of their search strategy—perhaps in an appendix.

Answer: Thank you for this comment. This has now been copied into a Supplementary file for mTBI as an example, as requested by the Editor.

The reader is not given a working definition of psychological distress and this makes it is difficult to judge whether the choice of search terms could provide good coverage.
Answer: A definition has now been provided on page 5 at the end of the Introduction.

I do not understand the section about MeSH. What did the authors search for in the databases that do not use MeSH, eg PsycINFO (please correct your spelling of this database)? I think that additional explanation is needed in addition to citing ref. 26. Also, were the authors aware of the limitations of using MeSH headings, as noted in several studies? How did the authors source relevant studies from journals?
Answer: Thank you for this comment. The text in the original paper was unclear. We did not only employ MeSH terms. We used a combination of free text (keywords) and MeSH to ensure a high sensitivity search, and to minimize the risk of missing relevant studies. The text at the beginning of the Method has therefore been changed on page 6 to:

"This consisted of free text keywords that included the following: "psychological injury", "psychopathology", "anxiety", "depression", "depressive mood", "PTSD", "adjustment disorder", "acute stress disorder", "motor vehicle accident", "motor vehicle crash", and "road traffic crash". These keywords were run primarily, and secondary searches were then conducted with these words in association with TBI, SCI, whiplash, MI, fractures, burns and back injury/ back pain. Multiple search engines were used, and search syntax and strategies tailored to the unique capabilities of each search engine. To ensure that no papers were missed, Medical Subject Headings (MeSH) vocabulary thesaurus was also used with MeSH fixed term descriptors in a hierarchical structure that permitted searching at various levels of specificity."

The authors state that one inclusion criterion is that the study should have a majority of injured participants. However, they report <50% average rate of SCI—note that his is recurring throughout the manuscript, where 45% is referred to as "majority", which is indeed confusing for the reader. Please clarify.
Answer: Thank you for this comment. This has now been explained more clearly on page14 in the paper in the Results under the section Spinal Cord Injury:

“All studies were shown to be of acceptable quality. The mean percentage of causes of SCI in the five studies was calculated, with the majority of causes resulting from trauma of which MVC was the most prevalent cause (45%), followed by falls (20.6%), sporting accidents (15.5%), and less frequently due to assaults and non-traumatic causes like disease.”

What is meant by ”coding reliability was established by one other author”? Please clarify, perhaps the best way is to state what this person did—did they independently code all studies, a subset, or what?

Answer: The following has been inserted in the Method on page 8: “One of the authors was responsible for coding studies against inclusion criteria, and coding reliability was established by another author who independently assessed a random subset of the papers. Disagreement on coding was minimal and was resolved by evaluation of a third author.”

P8 line 10: what is meant by, ”R…Software was used to perform … related statistical outputs”?
Answer: Thank you. This has been clarified in the paper on page 8 with the following text: “R Statistical Software is a freely distributed powerful statistical platform that enables the analysis of data in sophisticated ways. To perform the meta-analysis, the Metafor package from R Statistical Software was used.”

The paragraph on fail-safe N could be shortened. And I think that the authors have misunderstood the file-drawer problem—who would assume that it is 5% in the literature for this meta-analysis? Please revise.
Answer: The paragraph has now been shortened. Thank you for spotting the file-drawer error. The following text has been placed in the Method on pages 9-10:
“The “file-drawer” problem assumes meta-analyses studies have included in their selected studies only 5% (0.05) of papers that show Type I errors, while the remaining 95% did not attain a significant finding and remain unpublished.33”

P11 Table 1: The authors fail to indicate what measure (if any) was used to make a diagnosis—e.g., the CAPS, SCID, or MINI.
Answer: The diagnostic interviews used for the mTBI studies have been placed in Table 1. The SCI and WAD studies only used psychometric tests.

Results
The placement of the sensitivity section in the results is confusing to me. This section could go into the last parts of the Results.
Answer : Thank you. We agree and have placed it towards the end of the Results.

P17 line 30: The authors should not pool injury types in the analysis of time since injury if they found it unsuitable in the primary analyses. This analysis also is highly suspect given the variations in time between event and assessment in controls.
Answer: Thank you for this suggestion. See also our answer above on this issue. While such an approach has problems, we still believe it provides interesting findings and has some validity. However, we accept that this analysis is potentially problematic, and so we have added this as a limitation to the time since injury section in the Results. See also our response to Reviewer 5 on why we used a linear regression (the last response for Reviewer 5). A linear regression was used to determine the simple relationship between the dependent variable (effect sizes) and time. We pooled injury types as the combined numbers over the three injuries would provide improved validity for the linear regression.

Discussion
Curiously, the authors repeatedly discuss compensation issues despite there being no meta-analytic results to discuss, see specific points below. This study does not include any meta-analytic results for compensation claims and so it seems odd that they discuss this issue in favor of other pertinent issues.
Answer: Reference to compensation has been reduced as suggested. However, we feel that some brief mention of it is important since it does impact the psychological status of people involved in a MVC and entering compensation.

P17 line 54: Not sure why the authors here choose to say anything about distress following the injury types not included in the analysis. Just state that it was not possible to conduct analyses for these types of injury.
Answer: We have removed discussion linked to the other injuries and have stated as suggested: that it was not possible to conduct analyses for these.

P18 line 19: Perhaps I do not understand the authors' intentions with the sentence "Further research is required to clarify the role of trauma exposure and severity…"—is there really nothing in the literature about the role of trauma exposure and severity? Is it only likely that distress is elevated by a traumatic accident in the absence of injury?
Answer: Thank you for this comment. We agree. On page 20 we have removed the phrase "Further research is required to clarify the role of trauma exposure and severity." The next sentence begins with : "However, it is likely that psychological distress is elevated when people experience a traumatic accident even when sustaining no injury."

P18 line 34: What findings in the meta-analysis suggest that poor coping with these stressors increase the risk of mental disorders? Also, note that compensation status was not analysed yet the authors suggest altering compensation processes to make them less distressing.
Answer: On page 20 we have replaced "will not cope well with such stressors" with "..some people injured in a MVC will develop elevated psychological distress when faced by such multiple stressors,.."

P19: "For mTBI, the majority of studies found mTBI produced higher levels of distress than general trauma injuries": This conflicts with the results section: "removing this study [comparing mTBI to noninjured ctrls] from the meta-analysis resulted in a reduced and non-significant summary effect size of 0.18 (95%CI -0.002-0.37; p=0.052)" Please revise.
Answer: We agree, and have clarified this in the Discussion on page 21.

P19 line 34 "There are no obvious reasons why psychological distress in people with a WAD should be higher than in people with SCI or mTBI." I suggest that the varying proportions of actually injured people in the three groups and the differences in outcome measures need to be addressed here. Any comparison among injury types would need to take this imbalance into consideration.
Answer: This point is accepted. We have therefore inserted this into the Discussion text on page 22: "However, any differences in psychological distress between the WAD, MTBI and SCI groups may also be explained by differences in proportions of injured people in the groups and differences in outcome measures used."

Also, the authors' discussion here (line 50f) would benefit from considering the reliable finding of social support as a determinant for posttraumatic stress. Please relate this discussion to the literature on social support after traumatic events. Also note that there have been discussions about how a (visible) traumatic injury may serve as a testimony to the horrific events, leading to perhaps beneficial effects of the injury— this is related to the authors' deliberations on WAD.
Answer: Thank you for these comments. We have briefly discussed the benefits of social support in

dealing with distress in the Discussion on page 22: “The psychological benefits of enhancing social support and participation are well known for post-traumatic adjustment,71,72 and interventions should integrate strategies for strengthening social support networks in people suffering a MVC.”

We have added 2 supporting papers for this.
71: Arnberg FK, Hultman CM, Michel PO, Lundin T. Fifteen years after a ferry disaster: Clinical interviews and survivors’ self-assessment of their experience. Eur J Psychotraumatology 2013; 3:1-9.
72: Craig A, Nicholson Perry K, Guest R, Tran Y, Middleton J. Adjustment following chronic spinal cord injury: determining factors that contribute to social participation. Brit J Health Psychol 2015; 20:807–823.

P21 line 10: The authors should clarify the limitations of performing meta-analysis when there are varying proportions of injured participants in these samples.
Answer: This has been addressed in the limitations on page 23.

P21 line 23: What is meant by ”This bias phenomena”?
Answer: The term “Bias phenomena” is not needed, so we have removed this term from the text.

P21 line 32: There are scores of potential sources of heterogeneity. One issue I think need attention in the context of meta-analysis is the characteristics of the control groups, e.g., with regard to their status on the time from event to assessment—we know that posttraumatic stress diminishes with time for a majority and so variations here will surely lead to heterogeneity. This needs to be addressed and the authors are recommended to expand on these differences and be clear about the consequences of this variation in their discussion.
Answer: Thank you for this pertinent point. We have addressed this further in the Limitations on page 23 with the following statement:
“Performing meta-analyses when there are varying proportions of injured participants in the samples is also a limitation.”
“Related to this source of variance is the concern about the qualities of the injury groups. In the prospective studies, for instance, their status over the course of the research will vary, given that post-traumatic stress can often diminish for a majority and so add additional heterogeneity.”

Reviewer: 4
Reviewer Name: Richard Meiser-Stedman
Institution and Country: University of East Anglia, UK
Competing Interests: None declared.

This meta-analysis, which summarises the literature concerning the psychological impact of injuries sustained in the MVCs, is an important and very welcome contribution. The topic is important and the meta-analysis is rigorous and well-conducted. I thought the paper was well-written.
Answer: Thank you
Methods: Could you authors state which data points were used in the meta-analyses? Presumably the first data point was used, but this needs to be clearer. Was data excluded if too close to the injury, e.g. within a month?
Answer: Yes the first data point was used. We have clarified this on page 8.” The data points used for the analysis included the first assessment reported in the studies.”
No data was excluded as all assessment began at least one month after the injury.

P12: “Comparing an injury such as mTBI to a minor trauma injury does not control for the experience of injury.” The use of a trauma injury control group does control for the experience of injury; the point made in the following sentence (i.e. this is a tough comparison group, as distress might be expected in a injury-exposed, non-mTBI group) seems to assume this. This point needs to be made more clearly.
Answer: Thank you. We have addressed this in the mTBI Results section.

Discussion:
P19 “There are no obvious reasons why psychological distress in people with a WAD should be higher than in people with SCI or mTBI.” This claim is misleading and needs to be changed; the distress levels in these groups were not directly compared. They yielded different effect sizes, but this could related to multiple other factors.
Answer: We agree. This has now been addressed in the Discussion on page 22. See also our response to Reviewer 3 on this issue.

P22 “Involvement in compensation will likely add to this distress.” For the Conclusion sub-section, this point seems unwarranted and a distraction from the important actual findings of the meta-analyses conducted.
Answer: See response to reviewer 3. We have reduced discussion of compensation in the paper.

Minor comments:
P4: “Both mild to moderate TBI and MI are prevalent following a MVC.13,14,15” Adding a percentage range here would be helpful.
Answer: This has been addressed in the Introduction on page 4.
p6: Psychinfo (see also Abstract)
Answer: These have been corrected.

Reviewer: 5
Reviewer Name: Dr Krista Lanctot
Institution and Country: Sunnybrook Research Institute, Canada
Competing Interests: None declared

Ashley et al performed a meta-analysis to determine the psychological impact of motor vehicle crashes related to 3 specific injuries: whiplash associated disorder (WAD), spinal cord injury (SCI) and traumatic brain injury (TBI).
The PRISMA guidelines were followed.
The authors appropriately chose a random effects model, taking into consideration heterogeneity. Several factors could be considered to improve the analysis.
For outcomes, the authors combined anxiety, depression and injury specific scales. This should be discussed and evaluated in a sensitivity analysis.
Answer: Thank you for this excellent suggestion. A sensitivity analysis has now been performed towards the end of the Results on page 18. See reply to Reviewer 3.

The authors state that an inclusion criterion was that the majority of participants had to be injured in a MVC, and yet state that the mean percentage of participants injured with a MVC in the SCI group was less than the majority: 45%. please clarify.
Answer: Thank you for this comment. We have addressed this in the Results in the SCI section on page14. Please see response to Reviewer 3.

The authors should state how many times that disagreement on coding had to be resolved by a third author.
Answer: This has now been addressed in the Method on page 8 in the “Meta-analysis coding and

computations" section.
.
The authors state that 34 articles that met other inclusion criteria did not meet quantititative eligibility criteria. Please reference those articles.
Answer: Thank you. These 34 references have been placed in a supplementary file and uploaded.

I am concerned that removing a single study with a different control group (healthy non-injured controls instead of nonTBI controls with minor injuries) from the TBI group renders the effect size non-significant. The conclusions as stated do not reflect this uncertainty and negative finding. The fact that heterogeneity is reduced suggests that studies with different control groups should not be combined.
Answer: Thanks for this comment. This issue has been addressed in the Limitations

I would like to see statistics reported in the abstract to support statements made there.
Answer: Effect sizes for the three injuries have been placed in the Abstract.

Quality of the studies and the impact of quality on the findings should also be discussed.
Answer: A quality analysis has been performed for all 24 studies and results included in the tables. The following section explaining our quality analysis has also been placed in the Method on page 11:

" Quality analysis of the selected studies
The following criteria were used to determine the quality of the papers selected for the meta-analysis: (i) the study involved two groups, a majority MVC group and a control non-MVC group; (ii) reliable and validated psychological distress outcome measures were used; (iii) a statistical power analysis was conducted to determine optimal sample size to find differences between groups; (iv) appropriate statistical analyses were conducted to determine differences between groups, and (v) the study utilized a prospective design rather than a cross sectional design. Each criterion was awarded one point, for a maximum score of 5 points, with higher scores indicative of better quality. All studies were examined and the quality analysis results for all studies are shown in Tables 1-3. Quality ratings of 3 or above were considered acceptable. It should also be mentioned that no studies utilized a randomized design, while all studies provided a satisfactory literature review and made appropriate conclusions based on their findings."

The possible impact of preexisting psychological problems are appropriately discussed.
The authors state that they used linear regression. They should justify the choice of Linear Regression rather than a Mixed-Effects Meta-Regression Model, which would normally be used.

Answer: Thank you for this excellent suggestion. A fixed effects meta-regression requires prospective data with a reasonable sample size, and its benefit is that it enables the determination of within study variation and indeed would be a superior method if a majority of studies contained prospective data after the injury. However, as only 11 of the 24 studies included in the meta-analysis contained prospective data, we used a linear regression to test the simple association between time since injury and the psychological distress effects. The limitations of such an analysis have been discussed in the Results on page 19:
"It is accepted that pooling all injury types together is a limitation of this analysis, especially given variations in time that will occur for the MVC participants as well as for the controls. The linear regression, however, does provide an estimate of the association between time since injury and the psychological distress effects."

VERSION 2 – REVIEW

REVIEWER	Richard Meiser-Stedman University of East Anglia
REVIEW RETURNED	07-Jul-2016

GENERAL COMMENTS	The manuscript has been substantially improved and I have satisfied that my earlier comments have been addressed.

REVIEWER	Dr Krista Lanctot Sunnybrook Research Institute, Toronto, Canada
REVIEW RETURNED	21-Jul-2016

GENERAL COMMENTS	The concerns have been addressed.