PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Impact of daylight saving time on road traffic collision risk: a systematic review
AUTHORS	Carey, Rachel; Sarma, Kiran

VERSION 1 - REVIEW

REVIEWER	Willlam Frith Opus Research New Zealand
REVIEW RETURNED	07-Oct-2016

GENERAL COMMENTS	No further comments

REVIEWER	Catherine Staton Assistant Professor Surgery and Global Health Duke University Medical Center, Duke Global Health Institute Duke University North Carolina, USA
REVIEW RETURNED	18-Nov-2016

GENERAL COMMENTS	This is a very interesting and well written and performed systematic review. There are only a very few minor revisions which I can suggest to improve the paper and methods. First, I didn't see any limitations section in the manuscript. While I can see your second discussion paragraph is mostly limitations, I would say that you have to further elucidate the limitations of the review. (Ie, why not double abstract and manuscript review rather than 15% testing, all US based data, language limits, variety of methods in reviews and therefor inability to do meta-analysis, the difficulty identifying specially just light impacted crashes rather than multi-cause situations) Can you state how many manuscripts you excluded for language? You state this change of timezone has happened around the world and your initial searches were without language limitations but you didn't say how many you excluded when you only analyzed the English ones. I would consider including: TRID, Lilacs, Scielo also in your database search. Finally- your discussion is quite short. There is minimal summarizing discussion in there with almost all of the discussion in the introduction. Even the limitations in the discussion section is missing some key limitations of the review. I would try to include more of these limitations and increase the discussion (possibly taking some

	information from the introduction) to further discuss your results.

REVIEWER	Keeble, Claire University of Leeds, United Kingdom
REVIEW RETURNED	06-Dec-2016

GENERAL COMMENTS	This is an interesting and well explained article, which clearly states the objectives and conclusions of the study. Where is article could be stronger is in the justification for not conducting a meta-analysis. The search strategy specifically selects quantitative analyses with primary data, yet these values are not reported in detail in the results section. Further reasons as to why a meta-analysis could not be performed would strengthen the paper considerably. The section stating inconsistent findings in the impact of DST on road traffic collisions, injuries and fatalities may benefit from additional details, such as the size of the studies involved and how conclusive the results were. Where long-term impact is discussed, it would be useful to report both the number and percentage for reductions in crashes. Numbers as well as percentages would also be beneficial in the section describing different types of road users. Seasonal effects and the time of year (such as data collected around holiday periods) could also be discussed further. The biases present in the individual studies and any bias in the data collection for this review could also be included in greater detail. This research could be key to informing time-zone decisions, but would benefit from further details being added.

REVIEWER	Martin, JL IFSTTAR / Univ Lyon France
REVIEW RETURNED	07-Dec-2016

GENERAL COMMENTS	Estimating the influence of daylight-saving time on road traffic collision risk is an interesting topic. This systematic review would help policymakers if it gave a clear result. Unfortunately this is not the case. It is still worthwhile however to have reviewed all papers concerning this question. The introduction of the paper is well written, explaining the possible road safety effects of switching over to DST. The numbering of the research questions looks strange (only 1, with 1.a and 1.b). Perhaps there is something missing from the list? Otherwise the research strategy seems comprehensive and appropriate. The double check for the selection and the classification of papers needs to be considered. Highlighting papers in which there is a breakdown by road-user type and time-of-day is also useful. I agree with the authors' decision not to attempt a meta-analysis, as data and applicable methodologies are too widely different. Concerning the Tables, I suggest specifying the sample size for each study (total or average number of subjects per year, for example). This is particularly important since other numbers are given in the "findings" column.

	The figure should be presented in the other direction to be more readable. The results are clearly presented. The reason why DST could have a short term effect on crash risk - due to sleep deprivation - is explained in the results. However it may be better to move that to the methods section (to better understand the choice of structure for Tables 1 and 2). Finally the discussion could be more comprehensive. I suggest to enrich it by discussing what is missing from current studies for them to have more meaningful results (whether positive, or not)? For example, what data and what study design are needed to efficiently adjust for variations in weather and lighting conditions? Also, there is no mention of cyclists' collisions. Is such information missing from every reviewed paper, as the consequences of DST on cyclists should be close to that for pedestrians. If the authors take account of these minor remarks, I think this paper will be worthy of publishing in the BMJ Open journal

VERSION 1 – AUTHOR RESPONSE

Reviewer 1
No further comments

Reviewer 2
Comment 1) First, I didn't see any limitations section in the manuscript. While I can see your second discussion paragraph is mostly limitations, I would say that you have to further elucidate the limitations of the review. (Ie, why not double abstract and manuscript review rather than 15% testing, all US based data, language limits, variety of methods in reviews and therefor inability to do meta-analysis, the difficulty identifying specially just light impacted crashes rather than multi-cause situations)
Response 1) Thank you for this suggestion. We have now added a limitations sub-section that deals with five limitations of the review: 1) Validity of the DST literature in the context of the time-zone shift; 2) Our decision to exclude the literature on the British Standard Time experiment; 3) The heterogeneous nature of the DST literature-base in terms of method, analytic strategy and day/week/month comparisons, and the impact of this on any meta-analytic review; 4) Statistical limitations of the studies included in the review, and; 5) double-screening of 15% of papers (see p. 26-28).

Comment 2) Can you state how many manuscripts you excluded for language? You state this change of timezone has happened around the world and your initial searches were without language limitations but you didn't say how many you excluded when you only analyzed the English ones.
Response 2) We have added this detail to the Results section: 'There were eight papers published in a language other than English, on which abstract and/or full-text review could not be performed.' (p.8-9)

Comment 3) I would consider including: TRID, Lilacs, Scielo also in your database search.
Response 3) Thank you for these suggestions. We have conducted full searches on these databases and have included details in our methods section (p. 7). All figures have been updated in the results section (p. 10) and PRISMA flowchart.

Comment 4) Finally- your discussion is quite short. There is minimal summarizing discussion in there with almost all of the discussion in the introduction. Even the limitations in the discussion section is missing some key limitations of the review. I would try to include more of these limitations and increase the discussion (possibly taking some information from the introduction) to further discuss

your results.
Response 4) Thank you for this feedback. We have now provided a more comprehensive discussion section, with an extended consideration of the limitations. We feel the paper is stronger following this feedback and revision: ‘The DST literature, taken as a body of research, should not be used to support or refute the assertion that shifts in time-zones can have a road safety benefit. Inconsistent findings and conclusions across studies, combined with the heterogeneous nature of the studies, mean that DST could possibly have a positive or negative impact on collisions, but may also have no effect. For the UK and Republic of Ireland, where a move to CET is being debated, arguments may be on a stronger foundation where they focus on the economic value (or not) of such a change rather than the potential impact on road safety.’ (p. 28)

Comment 5) Where this article could be stronger is in the justification for not conducting a meta-analysis. The search strategy specifically selects quantitative analyses with primary data, yet these values are not reported in detail in the results section. Further reasons as to why a meta-analysis could not be performed would strengthen the paper considerably.
Response 5) Thank you for this comment. We have added further justification regarding the decision not to undertake a meta-analysis as part of the quality assessment discussion: ‘While most of the papers included incidence or mean/standard deviation descriptive results (19 papers), there was considerable heterogeneity across studies in terms of the time periods around the DST transitions selected for the analyses (see Tables 1 and 2 for details); this rendered the data as a whole difficult to subject to meta-analysis.’ (p. 24). We have also added a note about this to the limitations section: ‘The review is limited by the heterogeneous nature of the literature base. This includes variation in time sampling, statistical analyses, and populations of interest. One consequence of this heterogeneity is that we were unable to complete a meta-analysis of the studies. For example, the papers report findings based on differing time periods before and after DST (e.g. timeframes among short-term studies varied from one day to two weeks around the transitions). It is very difficult to reconcile this heterogeneity of comparisons into meaningful mean or dispersion effects during meta-analysis’ (p. 27). Additionally, we have updated the wording of findings within the table, where needed, to be maximally clear.

Comment 6) The section stating inconsistent findings in the impact of DST on road traffic collisions, injuries and fatalities may benefit from additional details, such as the size of the studies involved and how conclusive the results were. Where long-term impact is discussed, it would be useful to report both the number and percentage for reductions in crashes. Numbers as well as percentages would also be beneficial in the section describing different types of road users.
Response 6) Thank you for this comment. We attempted to draw out data on size of studies to add to Tables 1 and 2, however, this proved extremely challenging due to the variability with which individual studies reported these details. We have added a paragraph to the Methods and Discussion section to raise this issue: ‘It is important to note that we attempted to extract information that would provide an estimate of the size of the collision datasets used in the papers. However, this proved extremely difficult as this information was often not explicitly provided by the authors; in some cases we were left to derive estimates from graphical representations rather than tabular data. To compound this issue, where incidence was reported, varying units of measurement (e.g. weeks, days, parts of days, etc.) were often used, that could not reliably be reconciled into a standard reporting unit.’ (p. 9)

Comment 7) Seasonal effects and the time of year (such as data collected around holiday periods) could also be discussed further.
Response 7) On review of the 24 papers, we were unable to find any paper that considered the issue of holiday periods (note that there were from a range of jurisdictions, thus, it is likely that holiday dates would vary). We have added a note about this to the Discussion section of the paper: In particular, individual studies tended not to attempt to isolate light effects by statistically controlling for other potential explanatory variables, such as traffic volume, for example, or holiday periods. (p. 26)

Comment 8) The biases present in the individual studies and any bias in the data collection for this review could also be included in greater detail.
Response 8) As outlined in comments above, we have added paragraphs on quality assessment to the Methods (p. 9) and Results (p. 24) sections, and we have also included a table summarising our findings (p. 25): 'The quality of the papers included in the review was assessed using a bespoke set of quality assessment criteria. These criteria were identified by the research team to capture the extent to which the information provided answered the review questions [...]' We have also added details of potential limitations relating to our data collection methods as part of the Discussion section: 'The research team decided to double screen 15% of papers when isolating papers that met our inclusion/exclusion criteria, rather than double-screening all papers. The original review protocol proposed that double screening would continue up until high inter-coder reliability was reached; in practice, this was achieved after 15% of papers were screened.' (p. 27)

Reviewer 4
Comment 1) The numbering of the research questions looks strange (only 1, with 1.a and 1.b). Perhaps there is something missing from the list?
Response 1) Thank you for bringing this to our attention; we have now numbered these '1' and '2' for clarity.

Comment 2) The double check for the selection and the classification of papers needs to be considered.
Response 2) As mentioned in our comment to Reviewer 3 (above), we have included our approach to screening as a potential limitation in the Discussion section: 'Finally, the research team decided to double screen 15% of papers when isolating papers that met our inclusion/exclusion criteria, rather than double-screening all papers. The original review protocol proposed that double screening would continue up until high inter-coder reliability was reached; in practice, this was achieved after 15% of papers were screened.' (p. 28)

Comment 3) Highlighting papers in which there is a breakdown by road-user type and time-of-day is also useful. I agree with the authors' decision not to attempt a meta-analysis, as data and applicable methodologies are too widely different.
Response 3) Thank you for this feedback, we appreciate it.

Comment 4) Concerning the Tables, I suggest specifying the sample size for each study (total or average number of subjects per year, for example). This is particularly important since other numbers are given in the "findings" column. The figure should be presented in the other direction to be more readable.
Response 4) Thank you for this comment. We attempted to draw out data on size of studies to add to Tables 1 and 2, however, this proved extremely challenging due to the variability with which individual studies reported these details. We have added a paragraph to the Methods and Discussion section to clarify this: 'It is important to note that we attempted to extract information that would provide an estimate of the size of the collision datasets used in the papers. However, this proved extremely difficult as this information was often not explicitly provided by the authors; in some cases we were left to derive estimates from graphical representations rather than tabular data. To compound this issue, where incidence was reported, varying units of measurement (e.g. weeks, days, parts of days, etc.) were often used, that could not reliably be reconciled into a standard reporting unit.' (p. 9)

Comment 5) The results are clearly presented. The reason why DST could have a short term effect on crash risk - due to sleep deprivation - is explained in the results. However it may be better to move that to the methods section (to better understand the choice of structure for Tables 1 and 2).
Response 5) Thank you for this suggestion. We have now added a note on sleep deprivation in the

Methods section: ‘We extracted data relating to short- and long-term impact separately, where available. Examining short-term changes in collisions following DST transitions is thought to control for factors, such as traffic volume, that can vary over longer periods of time. We also extracted information relating to spring and autumn transitions separately, where available, in part because the spring transition leads to a shortening of the transition day by one hour, which can impact on sleep duration and latency. The autumn transition, conversely, adds and extra hour to the transition day and short-term collision trends are less likely to be linked to sleep duration and latency.’ (p. 8-9).

Comment 6) Finally the discussion could be more comprehensive. I suggest to enrich it by discussing what is missing from current studies for them to have more meaningful results (whether positive, or not)? For example, what data and what study design are needed to efficiently adjust for variations in weather and lighting conditions?
Response 6) We have included a section on quality assessment, which we think captures this point: ‘The ‘ideal’ paper, in terms of the review questions, would a) use an official road safety database (e.g. maintained by a statutory agency), b) report both short and long term analyses, c) examine morning and evening trends, d) explore both spring and autumn transitions, e) probe the impact of light transitions on a range of road users (e.g. pedestrians, cyclists, vehicle occupants, adults, children etc.), f) report statistics that could facilitate a meta-analysis, g) report data on factors, such as traffic volume, that could explain (in whole or part) any trends that emerged, and f) focus specifically on the impact of light (rather than sleep) on road safety.’ (p. 9-10)

Comment 7) Also, there is no mention of cyclists’ collisions. Is such information missing from every reviewed paper, as the consequences of DST on cyclists should be close to that for pedestrians.
Response 7) Thank you for highlighting this. Few of the studies provided data for cyclists; however, we have added details on findings from two studies that did provide this detail: ‘Whittaker reported that the onset of DST in spring was associated with a reduction in casualties that was particularly pronounced for pedestrians (36%) and cyclists (11%). Sarma & Carey found a significant reduction in cyclist casualties following the autumn transition, though the authors noted that the darker evenings may have led to reductions in bicycle use, which would have impacted on total incidence.’ (p. 23)

Thank you again for these helpful comments and for considering this paper.

VERSION 2 – REVIEW

REVIEWER	Keeble, Claire University of Leeds, UK
REVIEW RETURNED	20-Feb-2017

GENERAL COMMENTS	The authors have addressed the points as suggested and have considerably improved the article. An estimate of, or reasons against, the heterogeneity or summaries of the differences in, for example, time sampling would have been useful. It would also have been helpful to include a critical assessment of the studies used in the review and a discussion of any biases within these studies due to factors such as poor study design or data collection approaches.

REVIEWER	Jean-Louis Martin IFSTTAR, France
REVIEW RETURNED	07-Mar-2017

GENERAL COMMENTS	All my remarks and comments have been taken into account. I think this paper deserves to be published in the BMJ open