Author's response to reviews

Title: Are daylight saving time transitions associated with changes in myocardial infarction incidence? Results from the German MONICA/KORA Myocardial Infarction Registry.

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Author's response to reviews: see over
Response to Reviewers’ Comments

Dear editor and referees,

Thank you very much for giving us the opportunity to revise our manuscript. The comments provided by the reviewers were very helpful.

Please find our modifications and responses to the reviewers’ comments below. All modifications are highlighted in yellow in the manuscript.

Referee 2:

Title: Are daylight saving time transitions associated with changes in myocardial infarction incidence? Results from the German MONICA/KORA Myocardial Infarction Registry.
Version: 2
Date: 29 July 2015
Reviewer: Glen Pyle

Reviewer’s report:
This study by Kirchberger and colleagues examines changes in AMI incidence in a region of Germany during the transitions to and from daylight savings time. Using data from a relatively large patient cohort they have found some changes in incidence rates, and further mined the data to explore subgroups. This has led to the novel finding that ACE inhibitor use may impact susceptibility to AMI at these periods of time.

1. The authors claim that there are deficiencies or differences in previous studies that have been addressed by the current study. In the introduction, 3rd paragraph they present findings from previous works that appear to (unwittingly) minimize the novelty of the current study. Although some differences are listed in the following paragraph, the differences between past studies and the current one should be more explicit. What were the limitations of the previous studies that are addressed here? Please explain after the description of each study, rather than in a generic fashion. This issue arises again in the Discussion when it is noted that the findings are largely in agreement with the Swedish study. There is no immediate indication as to why this study is novel.

Thank you very much for this comment. As proposed, we have included the limitations for each of the studies reported in the introduction section:

“However, this study only investigated age and sex as factors that might influence the association between DST time shifts and AMI incidence. A further study from the Swedish authors examined subgroups of AMI cases and found a higher risk of AMI for the spring transition in individuals taking cardiac medication or having low blood lipids, and a lower risk of AMI for the
autumn transition in persons with hyperlipidemia, and persons taking statins or calcium-channel blockers, but these differences were not statistically significant [7]. Moreover, this study sample was restricted to hospitalized AMI cases and used the time of hospital admission instead of onset of AMI symptoms, which may have influenced the results. Potential meteorological confounders such as air temperature, relative humidity, and barometric pressure were not analyzed.

“However, this study did not include fatal AMI cases and consider meteorological variables as potential confounders.”

“Limitations of this study refer to its small sample size, an exclusion of fatal AMI cases, the use of the time of hospital admission as AMI onset, and the lacking consideration of meteorological confounders.”

In addition, we have tried to highlight the novelty and strengths of our study in the discussion section. We have placed the following sentence at the beginning of the discussion section:

“Our study is the first one which investigated the association of DST shifts and AMI incidence in Germany and considered meteorological variables as potential confounders.”

Moreover we have expanded the paragraph reporting the strengths of the study. It now reads:

“The strengths of this study include the population-based sample of consecutive cases with coronary death and survivors with validated AMI, inclusion of patients in a defined area and according to defined criteria, and the standardized collection of risk factors, treatments and mortality data. Contrary to most prior investigations which were restricted to hospitalized patients with AMI [7-9], we have also considered coronary deaths and used the time of symptom onset instead of time of hospital admission as indicator of AMI onset [7,9]. A major strength is the adjustment of the regression models for air temperature, relative humidity, and barometric pressure, which were reported to trigger AMI events [13]. Moreover, in the subgroup of AMI survivors, we were able to consider a number of characteristics which might influence the effect of DST on AMI incidence.”

2. There are no data on number of patients excluded. 25,499 patients were included in the analysis, but how many were excluded for reasons such an inability to obtain medical records?

In order to clarify this issue, we have included the following information into the “Data analysis” section:

“Since information on a number of these variables could not be obtained for all patients (e.g. employment status was only requested in the interview and therefore not available for people who died before being interviewed), most of
the stratified analyses are restricted to a limited sample size ranging between 89.9% (diabetes) and 53.3% (smoking) of the total population."

3. Tables 1 and 4 age groups should be greater than or equal to 65, or less than 65.

We have modified the age groups in tables accordingly.

4. There are points in the Discussion where differences between the findings of this study and earlier ones are noted, without much development. For example, on p 11 it is written that Janszky et al (6-7) did not find a gender effect, but that the current study did. The only discussion of this is a line stating that men are expected to be later chronotypes. This explains the current findings, but does not attempt explain why some studies find gender differences and others do not. Some discussion of this points should be included, beyond simply noting them.

Thank you again for this comment. We have added the following sentences in order to give more specific information, however, we can only speculate about the underlying reasons. As already mentioned on page 11, lines 11-20, geographical location may an influential factor leading to differences in the results across studies:

“The reasons for the conflicting study results are unclear. One may speculate that differences in study designs have contributed to these findings. In addition, as stated above, geographical location may be an overall factor that affects the association of DST transitions and AMI incidence.”