Responses to Referee 2 for Stalhandske et al. "Projected Impact of Heat on Mortality and Labour Productivity under Climate Change in Switzerland"; https://doi.org/10.5194/nhess-2021-361

We would first like to thank the two referees for the helpful comments and suggestions to improve our work. Both reviewers expressed that our study have significant added value but that several points need to be addressed. These points are very relevant and will improve our study, as described in our point-by-point answer. The text written in black are the original comments provided by the referees.

In summary, both referees first thought that the methods part was hard to follow, especially when we explain the methodology regarding heat-related mortality. Based on these comments, we need to restructure this section, and include parts of the supplementary information in the manuscript. Both referees also mentioned some short-coming in the methodology regarding heat-related mortality estimations and provided helpful suggestions to improve those. One referee also pointed out that some results were unexpected, which allowed us to find a mistake in the code. We are very thankful for that and have made sure to check the rest of the code. Finally, the first referee also pointed out that the methodology to estimate the experienced temperature of workers inside and outside building should be improved. We can also answer to most of these concerns by adapting our methodology and including other data.

This study presents spatially explicit and aggregated projections of heat-related mortality and heat-related losses in labor productivity for Switzerland under different climate change scenarios. The topic is important given that more frequent and more intense heat events are one of the major manifestations of climate change to be expected in Central Europe. Yet, I do have a number of concerns regarding the methodology adopted, especially with regard to the estimation of heat-related mortality.

Major:

1. 1) Currently, the description of the methods to compute heat-attributable mortality is difficult to follow. I would suggest moving the formulas from the supplementary material to the main text, making sure that all of the variables and parameters are properly explained.

   The other referee also indicated that this part of the methodology was hard to follow. We agree that it will be easier to follow if we include the formulas an explain the steps based on each variable of the formulas.

2. 2) To the extent I have understood the approach taken, my specific doubts concern the following
   - i) Why did the authors choose to model the functions coming out of Ragettli et al. (2017) as polynomial functions instead of adopting the model structure employed in Ragettli et al. (2017)? In that case, the uncertainty in the RR curves could have been assessed via the distributions of the original parameter distributions. Most importantly, there is a well- established methodological framework for computing attributable mortality based on distributed lag nonlinear models (dlnm, e.g., Gasparrini & Leone 2014, Gasparrini et al. 2015, Gasparrini et al. 2017) that the authors could have employed here.

     Ragettli et al. (2017) follows the distributed lag nonlinear models methodology as done in the cited publications. We employ the resulting RR for the lag following the heat days from this study. This is the same methodology as adopted by Gasparrini et al. (2017) for example. As for the polynomial function, this was adopted instead of the commonly used cubic splines functions, as we only look at the rising part of the function above 22 degrees.

ii) In the presentation of the current approach, it is unclear what is meant by “the fraction of the population exposed to heat” in Section 8 of the supplementary material. Using the notation of Perez & Künzli 2009, one would think that $AF_{pop} = AF_{exp}$, because all of the population should potentially be exposed to heat. Therefore, my understanding is that the formula for computing the attributable fraction per cell and T_{max} value should be AF = (RR- 1)/RR

We thank the reviewer for the relevant comment. This is indeed the case; we do consider the fraction of the population exposed as being the full population per cell o the corresponding age category. We copied the formula given by Perez & Künzli (2009), but we should specify that in our case it is simplified to AF = (RR- 1)/RR. We agree that this is confusing and will explain the simplification of the equation.

iii) In Section 9 of the supplementary material, it seems like a binning of temperature values are introduced by computing the number of days within certain limits of T_{max}. I don’t see the necessity of this. As mentioned under point (i) I would have employed the dlnm framework directly, but if you would like to start from RR curves, to my understanding you could compute the total heat-attributable mortality as

$$I_{Mortality} = \sum_{c=cells} \sum_{d=days} N_c AF_{c,d}$$

$$AF_{c,d} = \begin{Bmatrix} \frac{RR(T_{c,d})-1}{RR(T_{c,d})} & if\ T_{c,d} \geq reference\ temperature\ from\ Ragettli\ et\ al. 2017 \\ 0 & if\ T_{c,d} < reference\ temperature\ from\ Ragettli\ et\ al. 2017 \end{Bmatrix}$$

This is the same equation as we are using, but it is again a sign that our methodology was not clear and that it must be better explained, by also including the equation in the methods part.

You could even consider using the cell-specific, average daily death per day of the year ($N_{c,d}$), in order to account for the seasonal structure of the baseline mortality (or for present-day the observed daily mortality). The population fractions would be used to scale the nationally available mortality data to the cell level.

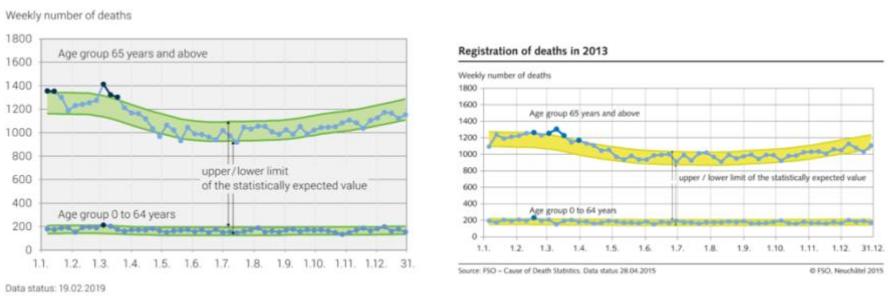

Figure 1: Weekly mortality in Switzerland for the years 2018 and 2013. Source: https://www.bfs.admin.ch/bfs/en/home/statistics/health/state-health/mortality-causes-death.html

This is also something that we tried to implement; we were however limited by computational power as it doesn't allow to sum all days of same temperature. The next best estimation is to considerer the average daily deaths in summer months, which does not vary much as shown in Figure 1.

3. I was also surprised to read that the authors only find a doubling of heat-related mortality by the end of the century, even under the high-emission scenario RCP8.5. Other studies, such as Gasparrini et al. 2017, Huber et al. 2020, usually find much higher rates of change for heat-related mortality under high emission-scenarios in Central Europe. Maybe this is due to the unusual approach chosen here for estimating heat-related mortality.

   We are extremely thankful for this comment. Upon checking the referenced papers, we realized that indeed our numbers were out of the range that we should expect. As explained above, the methodology remains very similar although it is slightly simplified, and we would expect similar results. it also made us realize that we were predicting a larger increase for the mortality of people in the category of people under 75 compared to those over, which should not be the case. Upon checking the code, we found that we exchange the RR curves for the two categories. This was corrected, and the new results that we would get for mortality are shown in Figure 2.

   Huber et al. (2020) report a 2.8 average factor of change in mortality due to heat under a global 3 degree increase in temperature for Germany, which is equivalent to an RCP8.5 scenario in 2080, for which we estimate a change factor of about 2.6 In the study by Gasparini

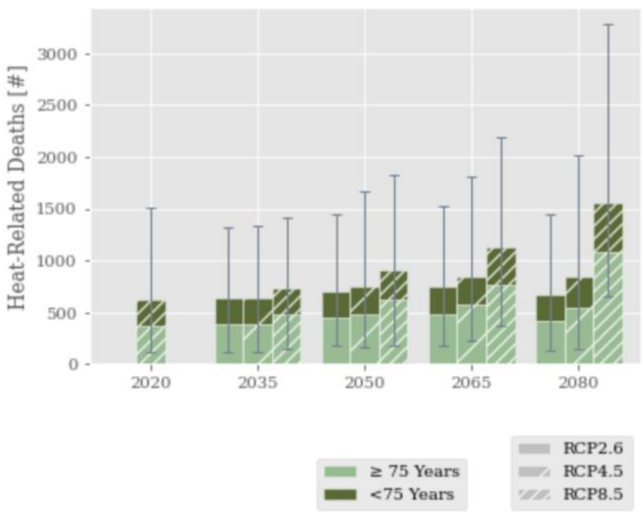

Figure 2: Updated results for heat related mortality, as we ran only 100 simulations, these may still slightly change.

   et al. (2017), they project about a 4 time increase since 2020. We think that this difference may be explained by the fact that the authors look at all central Europe. Also, we can expect differences coming from the climate models, as we see an important variability between them for our model. The CH2018 RCP8.5 data are based on 32 simulations of 15 global and regional climate models with different initial conditions and two resolution, while the model by Gasparini et al. (2017) and Huber et al. (2020) are run on 5 global climate models. We will however make sure to compare our results in the discussion with previous literature.

4. Another aspect that is currently not well explained in the method section is the definition of the baseline and future time periods. ll. 158-160 do not, e.g., mention the central years chosen. Also, it should be made explicit that "today's mortality" is based on simulated temperatures, and averaged mortality data. Given that the authors seem to have access to

observed mortality data for 2010 to 2019, the question arises why the authors did not choose the 2010s as the present-day baseline, computing future impacts for selected decades in the future. If the authors decide to keep the timeslice definition adopted now, it should be justified why the 2020 baseline is only shown based on RCP8.5 data, and not for the other RCPs.

We think that it is important to also show what we model for today's climate to see the change resulting from the climate signal. We will however make this clearer in the manuscript. As for the observed excess mortality, we prefer to provide it in the text as it becomes too much information in the same plot.

Minor:

We agree with all following comments and are happy to implement those.

l. 8: Better „We estimate that about 670 death per year are associated with heat exposure today in Switzerland." – To me "die because of heat" is quite strong, since your models are based on statistical associations

l. 22: I would suggest: "the impacts of heat on human health" rather than "metabolism". Alternatively, I would use the word "physiology" instead of "metabolism"

l. 29: Better: "people older than 75 years"
ll. 54-55: "The data and methods..." seems to be a repetition. Delete sentence?
l. 79: Would be good to include 1-2 sentences explaining the type of impact functions used. ll. 238-9: "The influence of heat waves...": incomplete sentence
Section 4.2. Check referencing format; author names appear outside of parentheses

l. 242: references cited do not point to the pertinent literature; there are a number of studies out now including adaptive processes/demographic changes into projections of heat-related mortality under climate change (e.g., Rai et al. 2019; Lay et al. 2021, Wang et al. 2018)

ll. 202-209: You discuss here the spatially explicit results on labor productivity. It would be nice to also mention the results for mortality (currently only shown in the supplementary material) and discuss whether the areas of high risks are the same for both labor productivity and mortality.

Fig 4 and Fig. 5. To me no need to show panel (a) without error bars as differences between RCPs are visible even at larger y-axis scale.

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