Review of "Projected Impact of Heat on Mortality and Labour Productivity under Climate Change in Switzerland" by Stalhandske Z., Nesa V., Zuwald M., Ragettli M., Galimshina A., Holthausen N., Roosli M. and Bresch N.B. (Manuscript ID: NHESS-2021-1361)

Extreme heat poses a major threat on society and economy. It is therefore important to quantify the magnitude and extend of this threat in the framework of climate change. The current study addresses this need by exploiting a high-resolution climate dataset and adopting a risk assessment framework in order to investigate the impact of heat on mortality and labour productivity in Switzerland under the present and future climate conditions. The topic is very interesting and significant added value can be provided by the study. However, the paper is subject to certain major limitations in its current form.

First of all, the authors should provide a better description of the study's conceptualization and merit in the "Introduction" Section. The statements in Lines 44-48 ("A few authors [...] for national assessments.") are confusing for the reader (previous relevant studies were actually implemented at national scale, e.g., Zhang and Shindell, 2021).

The "Data and Methods" Section is too lengthy (6/15 pages of the manuscript + 14 pages in the supplementary information document) and lacks a concrete structure. It is really hard for the reader to follow the methodological concept, when he has to go through several sections and to continuously jump from the main document to the supplementary information (virtually in every paragraph in Lines 81-174). The authors should adopt a more precise and comprehensive way that will assist interpretation and will also reduce the size of this Section, including the supplementary information. In this direction, I would suggest moving Table S1 in the main document and avoiding distinguishing sub-sections based on the risk components.

Certain methods contain critical defects:

- Hourly temperature values: Which other models did you test (please provide relevant documentation)? Why did you evaluate the applied method(s) only over four stations and only for summer 2018? Which are the "few days" presented in Figure S1? Could you please provide the whole JJA model-observation timeseries over all stations used for the evaluation (Bern, Geneva, Sion and Lugano)?
- Indoor temperature values: The model used for computing indoor temperatures is based on energy and daylighting simulations (Roudsari and Park, 2013). However, it is unclear if and which weather-related data were used for the application of the model. This is very important, as outdoor weather conditions are highly associated with the indoor temperatures. More specifically, the evolution of indoor building temperatures depends not only on the energy production and consumption of a building, but also on the radiation coming through the windows and exchanged between the indoor surfaces, the natural ventilation, the generation of heat due to occupants, and the impact of the air conditioning system (Salamanca et al., 2010; Salamanca and Martilli, 2010; Matzarakis et al., 2020). The method applied by the authors neglects these critical factors.
  Further, the analysis that implemented for deriving the relationship between the indoor and outdoor temperature, lacks clarity, robustness and validity: How the *percentage difference* equation came up? Did you perform any kind of regression analysis? Why

did you apply the analysis for only three hot days in August 2018? Did you validate the proposed formula against in-situ indoor observations?

- WBGT values: It is unclear how the authors concluded to the approximation equations for computing WBGT based only on temperature. Based on Figure S2 (Why did you plot only temperatures over 20 ^{O}C? Does this also apply for the computations?), I assume that they applied linear regression analysis between WBGT and temperature. However, it is confusing for the reader because they refer to a "model" (black line) in the legend. They also provide observations mean (red line) on the Figure for the "model" verification. However, it is not correct to validate a linear regression relationship ("model") against the same data that have been used for the development of the linear regression relationship.
  Further, the developed approximation WBGT formulas are based solely on outdoor temperature and humidity data, and a constant wind speed (Why constant? Please provide the equations used for the WBGT computations through the R package HeatStress). However, the authors apply the same formulas for the indoor environment (particularly, the shadow formula, assuming that the only difference between the indoor and outdoor environment is the lack of solar radiation in the first case, which is not true), using the estimated indoor temperatures. This cannot be considered valid. A different "model" needs to be developed and validated based on indoor measurements.
- Heat-Mortality association: How the polynomial function (equation (4) in supplementary information document) is associated with RR computed by Ragettli et al. (2017) and used in the current study? I do not agree with the assumption that the mean RR based on 8 Swiss cities is the same for all regions of the country. It may be a small country in terms of extends, but it characterized by high elevation variability, which is important for temperature and heat stress related studies. Further, the assumed low variability in RR among the 8 cities is not strongly supported by Figure S1 in Ragettli et al. (2017), e.g., Basel vs Berne.
  Mortality data are used for the period 2010 to 2019. The authors refer to them as daily deaths, but also as average daily deaths (Spatial? Temporal? It is unclear). Further, what is the geographical distribution of these data (e.g., at canton level)? What kind of deaths do you consider (e.g., all-natural)? What is the point of excluding heat attributable deaths by dividing the daily number of deaths (or average) with RR? What do you mean "evenly distributed deaths"? Why estimating the daily number of deaths per cell, when you have the actual number of deaths? In which maximum temperature do you refer in Line 143-144? Since the mortality data cover the present period (2010 – 2019), how do you estimate the number of daily deaths per cell in the future? There are so many questions raised, as the described methodology is very complicated, not adequately justified, and not easy for the reader to follow.
- Uncertainty and sensitivity analysis: Better justification needs to be provided concerning the uncertainties' distributions. Especially the assumption of a triangular distribution for the indoor temperature uncertainties is arbitrary. Further, the RMSE computed only for four stations is used in equation (9) in the supplementary information document for considering hourly values uncertainty. Also, further details are necessary concerning the sensitivity analysis process in terms of tools and methods applied and the coverage of the data used (Figure S6 refers only to 2050 under RCP8.5).

As a result of the above drawbacks in methodology, unfortunately, I believe that the outcomes of the study cannot be trusted at their present form. The authors honestly acknowledge the importance of the uncertainties and limitations in their work. However, they argue for the robustness of the study's outcomes (Lines 12-13 in Abstract, Lines 235-26 in Discussion, and Lines 272-273 in Conclusions). I am afraid that this cannot be adequately supported.

Other comments in the direction of improving the manuscript include:

- Lines 7 and 50-51: Please clarify that the two impact categories are (i) mortality and (ii) labour productivity, as in Lines 52-55.
- Line 63: What is the spatial resolution of the population geographical distribution used?
- Line 65: Please clarify that SI corresponds to "Supplementary Information" document.
- Line 106: Please replace "(see (Kjellstrom et al., 2018))" with "(Kjellstrom et al., 2018)".
- Line 113: Please replace "(Ragettli et al., 2017)" with "Ragettli et al., (2017)". The same format change also applies to other references in the text (e.g., Line 121, Line 128).
- Figure 2 is repeated in the supplementary information document. I would suggest replacing Figure 2 with Figure S3. The same also applies for Figure 3 and S4.
- Mean and uncertainties in Figure 4 can be presented in a single plot. The same also applies for Figure 5.
- The authors argue for the added value of using a high-resolution climate dataset in their analysis, but the canton level analysis is limited to two tables in the supplementary information document. I would suggest giving more details and emphasis, and promoting the regional-scale analysis.
- Line 211: Please delete "4.0.1 Discussion of the results"
- Line 266: Please separate "Conclusions" Section (i.e., "5. Conclusions")

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