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Interactive comment on "Implementation and comparison of a suite of heat stress metrics within the Community Land Model version 4.5" *by* J. R. Buzan et al.

Anonymous Referee #2

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The manuscript by Buzan et al. documents the implementation of an online calculation of heat stress indices in one of the leading Earth System Models. This effort is very valuable and potentially useful for a large scientific community running this climate model or analysing its output. As far as I can judge without having access to the source code, the approach used and its implementation are sound and the set of indices is comprehensive and justified.

However, in my opinion the manuscript is not ready for publication. The manuscript is poorly organized, in places lengthy, and generally hard to read. There is a high level of detail in the first part including the discussion of indices that are not even implemented, or lengthy discussions on aspects that are not directly relevant for the manuscript,

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whereas in the second part it is very hard to find all the relevant information documenting the figures and results. The first half of the manuscript is fine but should be substantially shortened to the essential information documenting the indices and their implementation, whereas the second part requires major revisions including revisiting the selection of results presented.

The authors argue that "The three advantages of adding these metrics to CLM4.5 are (1) improved thermodynamic calculations within climate models, (2) quantifying human heat stress, and (3) that these 20 metrics may be applied to other animals as well as industrial applications." Given the results presented I am not yet convinced that (2) and (3) would require an online calculation of the indices, and I do not see much evidence that (1) makes much of a difference for the temperature range relevant in the troposphere.

Based on a first application of the new code the authors conclude that some indices are more sensitive to temperatures and others more to humidity, and that "arid regions consistently have higher temperatures and lower humidities than the non-arid areas." I am afraid I do not see why an online calculation would be needed to draw these pretty obvious conclusions.

Instead of presenting these obvious findings, I would expect the results and discussion section to demonstrate the added value of the implemented online calculation of heat stress indices. I understand that the primary objective of the manuscript is to document the code including new implementation of the manuscript. Thus, there is no need for ground breaking research and highly innovative new findings but at least I would expect the results and discussion section to demonstrate the accuracy and relevance of the online calculation of heat stress indices over a post processing of the daily output. How do the results in the online calculation differ from the indices calculated based on the model output? To what extent does a calculation based on daily average output miss the extreme heat stress values? In this regard, I wonder why the authors decided to output the 6h average values rather than the maximum and minimum values at any

time step in the 6 hour or daily interval. Furthermore, it would be desirable to evaluate the model's performance at some places with in-situ measurements or gridded data in order to see whether potential model biases in temperature and humidity add up or cancel out. In my opinion the complete analysis of the model experiment output needs to revisited, the corresponding sections need to be rewritten and the figures need to be redone with the goal of demonstrating the added value of the great effort done in this project. Please find more detailed comments below.

(1) The author emphasize the implementation of more accurate moist thermodynamic calculations. However, based on the results shown here I am not yet convinced that this is particularly relevant. At least Fig.1 suggest that there is hardly any difference in the typical range of tropospheric temperatures. Please quantify the effect for the application here.

(2) The discussion of the existing literature on page 5200, line 4-22 is misleading. It suggest that there are major issues in the existing literature on heat stress, pointing to the inaccurate moist thermodynamic calculations. However, from Fig.1 it seems that this effect is either small or even completely irrelevant for the findings. I am also surprised about the claim that there is an error in Benestad (2010). I do not know the study but please provide more detail about and verify with authors before making such a claim in a side remark. Also the criticism of the studies looking at monthly values is not justified as for instance Dunne et al. are carefully motivating why they choose the monthly time scale.

(3) I do not see why the paper defines and discusses indices such as the indoor WBGT when in the end it is not even implement in the code. The entire first half of the manuscript should be substantially shortened to the essential parts. I recommend defining the indices in a table or list to enhance readability and give only a short discussion of their strength and weaknesses in the text.

(4) The joint distribution analysis is not really convincing, it is not clear what research

question it addresses and thus it does not add any novel understanding. Do you want to understand which indices give more weight to temperature or to humidity? If so you could basically do that in an xy-plot showing temperature on one axis and relative humidity on the other axis, and then add the isolines for the individual indices. The slopes would then tell you which indices give more weight to temperature or to humidity. Another approach would be to produce a QQ-plot of temperature and humidity versus each index or correlate their time series. If the emphasis is more on the spatial pattern I would like to see a more quantitative analysis like a pattern correlation of the contributing variables and the indices. But again, emphasis in the results section should be put on demonstrating the added value of the new code implementation.

(5) The figure captions are highly cryptic and the readability of the figures is poor. Please spell out the abbreviations in the caption and clearly describe what is shown. The caption should allow a reader to understand the key message of a figure without having to read the whole manuscript.

Since the manuscript requires major reorganization I provide only few detailed comments on the current version of the manuscript. Abstract: the abstract is too long and does not provide a concise summary of what is new in this manuscript 5199, line 6: replace "heat death" by "heat-related mortality" or similar 5199, 12: note that there are very large uncertainties in these numbers, see e.g. Robine, J., S. et al. (2008), Death toll exceeded 70,000 in Europe during the summer of 2003, C. R. Biol., 331(2), 171–178, doi:10.1016/j.crvi.2007.12.001. Instead of giving a number that may be very inaccurate, provide a range of values given in the literature or just refer to "tens of thousands". 5199, 18: Statement "Another study shows that the trend in hot extremes has continued despite the warming hiatus". The statement "even without El Niño" is confusing. I would argue if there hadn't been an El Niño in 1998, the trend would have been even more pronounced and I think this is also what the paper says. 5202, 11: "has" -> "have" 5207, 14-15: Motivate why you do not use wind and radiation. I assume there are good reasons why we do not have too high confidence in these variables in

coarse resolution models. Anyway, the limitations may not be obvious for non-experts and should be briefly discussed. 5212, 24: Quantify how much difference it makes. 5214, 2-7: Why don't you output minimum and maximum values across the 6 hour intervals? 5214, 10-11: "within the 99th percentiles", I assume you mean exceeding the 99th percentile. 5214, 13-14: Rephrase sentence, it is unclear. What do you mean by "the time domain"? 5214, 24-26: Sentence unclear. 5214, 26-28: What figures are you referring to? 5215, 18: "This is unsurprising given their underlying similarity as a buoyancy measure." I am not sure I understand. 5216, 18 and Table 5: These categories seem unconventional. Either you group the locations by latitudes or by regime but then I would expect an objective criterion with no overlap. E.g. I do not see why the tropics are not considered as "moist convective". 5217, 4-5: Extremely long tail in Fig. 6. It is not clear what you are referring to. 5217, 19: I think Diffenbaugh et al. used daily data and Dunne et al. were very clear that they never intended to quantify peak heat stress but rather to explore sustained stress causing work inefficiency. 5218, 7-19: This is a very detailed feature and its discussion seems odd in this part of the manuscript. If the authors think this bias is crucial and needs to be documented, it would need to be illustrated with a figure or introduced and discussed with more background so that it is understandable for a non-expert knowing all the details of this parameterization.

Interactive comment on Geosci. Model Dev. Discuss., 7, 5197, 2014.