Review of Zhu et al "Building a machine learning surrogate model for wildfire activities within a global earth system model"

Apologies for the late review. The request came through whilst I was on a month's annual leave and then got Covid when I returned.

Zhu et al. present a deep neural network that 1) emulates the fire model used in CLM and 2) optimises on observed burnt area from multiple products. They include an evaluation and the new "surrogate" model and a discussion on potential uses.

While there are no flashy results, the DNN approach presented shows great promise in many applications of global fire modelling, and its documentation is a perfect fit for GMD. The m/s is very well written and easy to understand - quite an achievement for such a novel and potentially complicated approach. It is perhaps the most enjoyable read I have seen in GMD.

I have two slight methodological concerns, though I think they just require some modifications of the text rather than any new analysis or evaluation:

1. ***Normality assumption in equation 8***. The distribution of burnt area is extremely non-normal, and so should the probability in the model-to-observational error in the cost function. Having said that, the evaluation of the DNN shows it still performs well, so I won't make a new analysis a requirement for the m/s to be accepted. The authors should discuss the limitation of this normality assumption on model use, and possible ways of developing it. (Kelley et al., 2021) for example, uses a zero-inflated logit transformed least squared approach (though that is for a maximum likelihood approach), which might be useful? If your method struggles with non-unimodal cost functions, maybe a simple logit transformation would be better?
2. ***Lack of biomass feedback in DNN model in evaluation.*** Presumably, the "base" model still represents vegetation and biomass feedbacks that the new scheme does not? (Correct me and the m/s if I misunderstood). This means 1) evaluation of base model will be at a disadvantage, as vegetation/fuel feedbacks represent another source of non-linear interactions that could exasperate errors in the base model. And 2) it limits the applications for the surrogate model for future climate and environmental change. Excluding biogeochemical-fire feedback is fine for this study (as the authors point out in the discussion, this study is just a first step). Still, there should be some acknowledgement in the model evaluation and discussion. For example, how much might biogeochemical feedbacks explain variations in base model regional biases vs the small biases in the DNN?

Other than that, I only have a small number of specific comments.

Great work, and I look forward to seeing where this surrogate model gets used next.

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Specific comments

Line 41/42: (Lasslop et al., 2020) suggests significantly small changes in tree cover across the models in their study (I think 3-25%)

Line 83-85: "*Two types of wildfire models are widely used: process-based models and data-driven statistical models.*" (Hantson et al., 2016) first suggested this distinction, so it should be cited.

Line 127-129: It sounds like you're not making any developments to CLM4.5 fire processes here? If that's the case, can you state that (sorry if you do somewhere and I missed it)

Line 134/135: As mentioned in the intro, human activity also fragments the landscape (Kelley et al., 2019; Andela et al., 2017). Is this effect represented in the model? Dont worry if not - new processes aren't what this paper is about. But if it is, please briefly describe how.

Line 140-141: *"shaped region controlled by wind speed and fuel wetness…"* is this a rate of spread model such as (Rothermel, 1972)? If so, please cite

Line 173-176: on taking the mean of the observations. I wonder if there's a way of capturing the uncertainty from the disagreement between products using your approach? (I'm not suggesting new analysis, but maybe a discussion point for future work)

Line 198-199: Would it work if you swapped around and used 20% for training and 80% for testing?

Equation 8: is the "i" each gridcell?

Line 230-232: How do these numbers compare to fireMIP model results in (Lasslop et al., 2020; Hantson et al., 2020)

Line 308/309: It would be more appropriate to look IAV (and also look at trends) within regions. A few areas dominate global IAV, so at the moment, IAV evaluation does not provide a very good assessment of the global model. Given you split your model into regions anyway, I wonder if you could split this evaluation as well?

Line 358/359: *"This study is an important step towards fully coupling E3SM and the DNN-Fire models in the future."* I'd be interested to see how this progresses, so feel free to suggest me as a review in the future.

Line 362-365: Very good point!

Line 382-383: *"will produce a model dominated by gridcells that have high burned area (e.g., Africa)."* using a logit transformation on your cost function might help with this.

Line 385-391: I quite like this pragmatic regional approach, but as the m/s points out, GFED regions were designated partly on present-day bioclimate and fire conditions. Future shifts in bioclimate (i.e. vegetation transitions, climate extremes etc., which are more likely at extreme future scenarios (Swaminathan et al., 2021; Burton et al., 2021)), may impact the performance of future projections given this regional optimisation. A brief comment here on this impact would be great.

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