

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-116

Anonymous Referee #1

Referee comment on "Modeling the short-term fire effects on vegetation dynamics and surface energy in southern Africa using the improved SSiB4/TRIFFID-Fire model" by Huilin Huang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-116-RC1>, 2021

This study investigates seasonal patterns of fire using the SSiB4/TRIFFID-Fire model. Thereby analyses focus on variables representing the energy balance such as latent and sensible heat. While the topic is generally interesting, I have several comments.

The study is motivated by the lack of short-term studies of fire impact and analyses at monthly temporal resolution. For example l 53 "the short-term fire effects at monthly to annual scales have not been quantified in current fire-vegetation models". This is misleading because many models do include temporal aspect in fire models and monthly variability has been studied, even by the authors of this study: Huang et al 2020 GMD Fig 6,9; Fang et al 2010 Atm Chem and Phys, Fig 4. This statement and similar statements in the manuscript should be revised to clarify differences and novel aspects (eg energy balance) and that seasonal aspects are represented in other models as well.

Fig 7 and others suggest that there is a huge mismatch between burnt area and considered state variables. The peek of state variables occurs around 3 month after the peak of burnt area. One would expect a more or less immediate effect of fire on vegetation cover as fire removes biomass within a few days. It seems that vegetation is hardly vulnerable to direct fire impacts and mortality. How can this be explained? I think that a more detailed model description (particularly of the fire model) should be added to be able to understand the behavior. Further this should be in the explained discussion section. Given these delayed responses, I'm not convinced that the model can actually simulate fire seasonality and its impacts on the energy balance adequately.

The results section contains several sentences and points that are more appropriate for the discussion, eg how the results compare to previous studies. The discussion, in turn, contains results of sensitivity analysis (FIREONdark simulations) that has not been introduced previously. I suggest to describe these experiments in the methods section and report results in the results section and therefor focus on interpretation and discussion of the results in the discussion section.

Generally the manuscript provides many results in detail, however, I think that the study could strongly benefit from a specific key questions or hypotheses. Such questions could deal with the expected fire impacts on the energy balance or the expected outcome of surface darkening in FIREONdark simulations.

The manuscript also requires careful language editing. Some points are listed below.

l 23 "vegetation clearance" I suggest to reword here and elsewhere, a 1% reduction tree cover and 4-8% reduction is (at least for me) not a vegetation clearance; there are still 99% of trees left.

l 29 I suggest to add some conclusions to the abstract.

l 31 reword "Earth's ecosystem" to ecosystems, I would argue that there is not one single Earth ecosystem but multiple ecosystems.

l 35 in the boundary layer.

l 49 "These studies" reword, this suggest that all studies in previous sentence are "world without fire" studies.

l 63 reword/delete "we start with", does that relate to the presented study or are further studies planned?

l 78 "These variations can be explained by ...".

l 92 I suggest to reword and delete "makes first attempt" and again, this is not the first attempt to simulate temporal variability of fire but to conduct the specific analyses related to the energy balance.

l. 104 as mentioned, the manuscript could benefit some specific questions or hypotheses.

l. 108 not entire Southern Africa has typical savanna climate, for example the Fynbos is a winter rainfall region and the western parts are deserts.

l. 108 what does "most representative savanna fire" mean?

l. 111 delete "in climatology"?

l. 113 "and local ecosystems have evolved".

l. 116 "Over SAF, ...".

l. 124 dry season mentioned previously.

l. 130 Shrubs dominate the...".

l. 157 tundra is not a PFT but a vegetation/biome type.

l. 170 "has been updated" I understood that this has been updated in previous study and suggest to clarify.

l. 194 what is a "vital driver"?

l. 176, 204 it seems that the inclusion of land use and modification of the soil moisture function are the only model developments presented in the study. Apart from that, the simulations were done using an existing model version. The model development aspect of the manuscript seems to be minor.

l. 204, 206 how was the adjustment done? Please clarify.

l. 215, 216 I suggest to add CO2 values.

l. 225 not clear to me why fire was suppressed only for 2 year periods, why not for a longer period? Please clarify and explain why this experimental setup/modeling protocol has been chosen and developed.

l 359 specify "short PFTs".

l 365 I would not denote fire as an important "contributor to deforestation" given that it only removes 0.2 to 0.6% per year (per fire?). At what % does vegetation recover until the next fire? Does such a fire regime have long term impacts on vegetation or is vegetation in equilibrium because regrowth compensates fire removal?

l 400-402 does that relate to model results or reality?

l 409 "LAI consumption by fire" I would argue that fire does not directly consume LAI, rather fire removes biomass and causes vegetation mortality which in turn implies changes of LAI. I suggest to reword.

l 465 "In the fire season".

l 559 "highly vulnerable to fire ... large-scale deforestation" as mentioned, 0.2-0.6% don't suggest that trees are highly vulnerable.

l 571 I suggest to add some conclusions and take home messages.