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Geoscientific Model Development Discussions

Interactive comment on "Modelling the role of fires in the terrestrial carbon balance by incorporating SPITFIRE into the global vegetation model ORCHIDEE – Part 2: Carbon emissions and the role of fires in the global carbon balance" *by* C. Yue et al.

Anonymous Referee #1

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Review of "Modelling the role of fires in the terrestrial carbon balance. . ." by Yue et al., Geosci. Model Dev. Discuss.

Yue et al. present model results of the impact of fires on the global carbon cycle from simulations with SPITFIRE running inside ORCHIDEE. This paper focuses on carbon emissions from fires and the effect of fires on the terrestrial carbon sink, building off of a companion paper that introduces the model coupling and assesses model performance

with regard to fire area burned. The model fire emissions are validated against the GFED although deforestation and agricultural fires are not included in this version of SPITFIRE. This makes the comparison difficult in many respects, in particular the fuel consumption is vastly different between the model and the GFED. The authors do a nice job pointing out this major caveat wherever relevant and make the comparison to GFED natural-fires-only where possible.

The analysis of changes in the terrestrial carbon sink uses the "fire-induced sink reduction" extensively and I find the use of this metric to be an effective way to look at the impacts of fire on the carbon cycle. The authors introduce the idea of a fire "respiration equivalence" which I think is an interesting concept, although I have some suggestions below on how to place this in a clearer context. Overall the paper is nicely presented with appropriate and effective figures, and I list suggestions for improvements below, mostly minor in nature.

General comments:

1. Although this is a companion paper, it would be helpful to the reader who is not familiar with this version of SPITFIRE to include a brief description (one-two paragraphs) of the major aspects of the model somewhere in Section 2. The combustion completeness is discussed in detail but there are a few other things that came to my mind while reading: how does the model treat fire-caused mortality, how are human impacts on fires included (i.e. changes in ignition based on population density but no fire suppression). In addition to these topics, which both come up in subsequent comments here, a basic description of how the model predicts fire events/spread would be helpful.

2. The definitions of the land-atmosphere carbon fluxes in Section 2.4 could be simplified in my view. The use of both NEP and NBP seems to be superfluous since NEPOFF is the same as NBPOFF, given that the subscript "OFF" implies no fire carbon emission. If only NBP is used, the SRFIRE equation can be simplified to "SRFIRE=NBPOFF – NBPON" so that it is clear that the SRFIRE is the difference of the same quantity from

the two simulations (fire on and fire off). Maybe there is a reason for using both NEP and NBP that I am not seeing, but to me this seems a more clear way to express this quantity.

3. The idea of a "fire respiration equivalence" in the model is interesting, however I think the presentation of this concept could use more explanation. Firstly, Fig. 9d shows that, with the exception of one recent year, SRFIRE is always positive. This should mean that, while increased heterotrophic respiration (RH) in the FireOFF case may compensate for some of the enhanced sink without the fire C flux, it only very rarely compensates for all of the enhanced sink, not only when the fire year was extreme (Pg 9036, Lines 16-19)? If this is the case maybe it would be better to remove references to an "equivalence" and use an alternative description, such as "large-part compensation". Although this does not have the same simplicity of concept even if it may be more appropriate.

Also, some references to the respiration equivalence I think may give the wrong impression about the difference between the real world and a fire/no-fire world comparison. For example, Pg. 9019 Lines 18-19 reads ". . .fires mainly compensate the heterotrophic respiration that would happen if no fires had occurred." I think this gives the impression that in any given year if fires were suddenly turned off then, because of the increase in HR, about the same amount of carbon would be emitted as if fires were still turned on. In this case of the sudden fire switch off, while litter C might increase a bit, and RH with it, it would be a much smaller increase than is seen in a "no-fire" world that has had many years of litter C build-up, such as the FireOFF case in this study. In other words, the RH compensation might only matter relative to a "no-fire" world. So the phrase in the abstract "if no fires had occurred" might be better stated as "in a world without fires". This may seem like a subtle point to make but it is important in my view since readers may get the two ideas confused.

Specific comments:

Pg 9023, Line 16: The fuel consumption timescale "h" may not be familiar to some readers and could use a definition here.

Pg 9025, Line 23: It might be worth adding here that using constant land cover will mean not only that fires associated with land cover change (deforestation fires) will not be included but also that wildfires will not be affected by changing PFTs.

Pg 9026, Lines 9-10: It is not clear to me how the model as described here is capturing deforestation fires. This should be elaborated on, especially since prior statements indicate that deforestation fires are not included.

Pg 9026, Line 26: Here is where I wondered how population density was taken into account when predicting fire events. A short description of the model in Section 2 would clarify this.

Pg 9026, Lines 16-26: The criteria for determining model equilibrium is mentioned briefly later on in this section (Pg 9027, Lines 15-17) but it would be helpful to include a description of the criteria used here on Pg 9026 when the spinup is being explained.

Pg 9028, Lines 22-24: The authors could make more of an effort here to note that peatland fires are not exclusively anthropogenic (especially in N. Hemisphere high latitudes) even though it is convenient to group them this way for the purposes of the study.

Pg 9028, Lines 23-24: Another note to include explanation of how some deforestation fires are captured by the model.

Pg 9031, Lines 4-15: This is an excellent point and really adds nicely to the overall analysis. It could be even better with a sentence added that connects the idea laid out in the first sentence of the paragraph (emission variability driven by forest fires, burned area variability driven by grassland/savannah fires) to the difference between the model and GFED.

Pg 9033, Lines 1-3: The difference in fuel consumption between the model and GFED

in tropical grasses/savannah especially is very large, as pointed out in this text and shown in figure 5. Could the authors provide some insight at this point in the text into why the numbers may be so different for these fires (i.e. combustion completeness)? Maybe something as simple as noting that the differences in combustion completeness are driving this discrepancy and they will be discussed in the next section.

Pg 9035, Lines 1-3: The van Leeuwen et al. (2014) combustion completeness values might not change the global emissions by much, but maybe they improved the spatial distribution of fuel consumption as compared to the GFED? If so, this could warrant a new figure, or a new panel in Figure 5 showing the difference.

Section 3.3.1: The figure is really nice, a great concept. Are the data used from the model? If so, would it make more sense to use the full century of model output? This might change the look of the high latitude regions which have longer fire return intervals. I had difficulty understanding parts of this section – Line 12 should read that agricultural harvest PLUS heterotrophic respiration account for 91% off NPP, correct? And I might be missing something but 100% minus RH plus CH (91.0%), minus FE (3.4%) leaves 5.6% for NBP instead of 5.2% as written in the text.

Pg 9036, Line 7: What measure of variability or uncertainty is the plus/minus representing here?

Pg 9036, Line 14: Is soil organic matter considered available fuel for model fires? This could also be addressed in the brief SPITFIRE description in Section 2.

Pg 9036, Line 19: This is where I thought that discussion of the model treatment of fire-caused mortality of vegetation would be helpful. This role of fires, converting live vegetation C to litter C, should be mentioned here when explaining how fires are analogous to respiration.

Pg 9037, Lines 14-16: This contention would carry more weight if half of the lowest SRfire years were not also after 1970 (after 1980 in this case). Also, precipitation

patterns also have a role in determining the high/low years (as the authors note in the following paragraph) and the uncertainty in these fields in the climate data used for atmospheric forcing is high for the first half of the 20th century. I recommend removing this sentence. Also Hartmann et al. (2013) is not listed in the references.

Pg 9038, Lines 12-17: This is stated very nicely. In some ways the SRfire analysis is better without the deforestation fires since it provides a nice wildfire-only baseline to compare anthropogenic impacts against.

Section 3.3.4: It is a good idea to compare the results of this study to a previous study, Li et al. (2014) in this case. To my mind this discussion can be greatly reduced in scope, maybe even down to just a few sentences. The important parts to note are the big difference in the sink reduction predicted in the two studies, and the major differences in the studies that could lead to this discrepancy.

Table 1: It would be great to have global totals for emissions and burned area included in this table.

Figure 6, caption: It was unclear to me what was meant by “based on grid cell area” here. This seems inconsistent with the units given which are per meter squared.

Technical changes:

Pg 9022, Line 11: Delete “issues of what”

Pg 9025, Line 7: Change to “A ratio of simulated GPP to MTE-GPP was. . .”

Pg 9032, Line 1: “possibly” might be better word choice than “probably” here.

Pg 9034, Lines 7-8: This sentence could be stronger if “indicating that the simulated fuel load might be comparable to GFED3.1 data” was deleted.

Pg 9037, Lines 11-13: Note that the SRfire numbers used here are global.

Pg 9037, Line 16: Change “The SRfire. . .” to “The average SRfire. . .”

Pg 9038, Line 23: A couple work choice recommendations – change “limit” to “average” and “accelerated” to “increased”.

Pg 9039, Line 13: Change “by the two” to “from the two”

Pg 9040, Line 13: Change “NPP by fire” to “NPP lost to fire”

Pg 9041, Line 17: Is this Table 1 “in” Archibald et al., 2013?

Table 1 caption: Fix “GEFD”

Figure 8 caption: Delete “returned to the atmosphere”

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