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Geoscientific Model Development Discussions

Interactive comment on "Modelling fires in the terrestrial carbon balance by incorporating SPITFIRE into the global vegetation model ORCHIDEE – Part 1: Simulating historical global burned area and fire regime" *by* C. Yue et al.

Anonymous Referee #1

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The manuscript by Yue and coauthors describes the integration of the SPITFIRE fire model into the ORCHIDEE land surface model, and includes detailed comparisons with various aspects of the fire regime. Fire is increasingly recognized as an essential process for simulating ecosystem dynamics, distributions, and interactions with the atmosphere and climate. The authors do an especially nice job of recognizing the various dimensions of fire regimes, which include not only burn area but also the number of fires, their size, and their intensity, and carefully comparing the model to different observational datasets. I also appreciated the 'quality flags' presented for the ORCHIDEE

comparisons that utilize the variety of observational datasets.

I was somewhat disappointed that the authors did not attempt to address some of the major flaws in the model that were highlighted by their analyses. While I appreciate that some of the comparisons themselves are novel, a few relatively large model biases were discovered, including fire season, fire sizes, fire duration, spread rate, and regional burn area. I realize some of these are beyond the scope of this paper. The major flaw in my view, however, is that the authors did not take advantage of or even seem to be aware of recent work that addressed some of these issues with SPITFIRE in LPJ by Pfeiffer et al. 2013 (Geosci. Model Dev., www.geosci-model-dev.net/6/643/2013/ doi:10.5194/gmd-6-643-2013). A revised version of the model was presented here which included improvements for fire duration (including multi-day burning), coalescence of fires, and interannual lightning variability.

Specific comments

-The authors did not mention a relatively recent paper by Pfeiffer et al. 2013. This describes a revision of SPITFIRE in LPJ. Although the improvements were focused on better representing burning in preindustrial time, this version contains many model developments advantageous for the simulation of present-day fires. It's unfortunate that the authors of this study did not take advantage of this development, as there are a number of areas for improvement with SPITFIRE that the authors acknowledge which have already been developed.

In my view the the major improvement from the Pfeiffer study that is applicable for this paper/analysis is the coalescence of fires within grid cells and the allowance of multi-day burning. The authors discuss in numerous places the bias in fire size introduced by the 4 hour restriction on fire duration, as well as the discrepancy between fire patch sizes in reality and in the model. Both of these would seem to be improved with Pfeiffer et al.'s modifications.

The authors use climatological lightning frequency to derive natural lightning ignitions.

However, there can be considerable variability in this during the fire season. Pfeiffer et al. 2013 accounted for this by deriving a relationship between lightning frequency and ancillary meteorological data.

-In general I would like to see an expanded discussion on what's causing some of the specific model biases. This includes fire season, high burn area and fire intensity in the tundra, big regional biases in North America, the Middle East, southern Africa, Australia, etc.

-I would advise the authors to be REALLY careful of the long-term burned area observations for Russia. Comparisons are mentioned back to 1920, but the observations are highly uncertain pre-MODIS era, and especially before 1980 (look at the discussion of how Russia data were created in Mouillot and Field (2005), Appendix A, and their uncertainty estimates in Table 2).

-[Fig. S7] Related to above, I'd like to see these graphs combined for boreal North America, and aggregated to decadal like Fig. 10. The problem is that in the text the authors claim there is good agreement in boreal North America long term. Compared to Mouillet and Field (2005) in Fig. 10, the comparison is decidedly not favorable. It's hard to tell in Fig. S7 what the overall decadal trends are in the national fire databases vs. ORCHIDEE. The authors claim that this reflects the model's ability to capture fire trends driven by climate variation relatively well. I'd also like to see a decadal statistic here, because as it's presented the reader is not convinced, and is hard-pressed to believe that the long-term trends are actually captured.

-The parameter for ignitions per person per day was spatially explicit in Thonicke et al 2010, and here the authors discarded that and used a global constant. Even though the results are comparable, why move away from something that's arguably more sophisticated? This needs to be justified better I believe. The authors state that the overall average is better when compared with GFED using the spatially-explicit parameter [pg 2392, line 19].

-Similar to above, why not keep the fire suppression algorithm? It improves the simulation especially in the western US, where fire suppression is known to have decreased burn area by almost an order of magnitude since the mid-20th century.

-The authors don't discuss how fire intensity is calculated in the model, although it's being compared to data. Is it strictly from the Roethermal equaitons?

-This is somewhat beyond the scope, but if fire intensity and duration are explicitly simulated, why do they not affect combustion completeness? These are crucial drivers, and was an area that was augmented somewhat for the ORCHIDEE integration presented here.

-Regarding model spinup: a spinup of 200 years seems quite short for aboveground processes. For example, Moorcroft et al. 2001 demonstrated that over 200 years are needed for the accumulation of biomass in tropical rainforests. A second spinup of only 150 years for fire dynamics seems quite short as well. Many fire-prone boreal systems have FRIs of around 150 years, and others are well over 500 years. As such their dynamic equilibrium wouldn't seem to be reached, although I realize the large grid cells burn more frequently than this. Some demonstration of how this spinup was enough, not just for the carbon sink, but also for fire frequency, aboveground biomass, etc., would help.

-[2386, lines 27 - 29] I wouldn't argue that the model is capable of capturing deforestation fires if the land cover map is static. To support this claim, can the authors somehow generate an estimate of how much 'deforestation; fires there are in their simulations?

-I believe Archibald et al. 2013 generated 'fire patch' data for the entire globe. Why restrict the patch analysis to boreal North America and southern Africa using the Archibald et al. 2010 data?

-[2389, lines 22 - 25] This 'pooling of fire patches' requires more explanation here.

-For the comparison with GFED3.1, it might be good to also look at the more recent version that includes small fires (Randerson et al. 2012, JGR-biogeosciences) since burn area and emissions increase by approximately one-third globally

-[2390, Seasonal similarity] This statistic makes sense, but it's new to me. Has it been used before? When looking at Table 2 its value means little without context. Is there a way to provide statistical significance or at least more context with good/bad correlations?

-[2392, line17] The statement that the model result agrees best with GFED3 is not supported by any number/statistic. Please provide.

-[Figure 8] SPITFIRE has some peak fire months in February in Alaska, October in Canada, and April/May in the Far East of Siberia. This is very surprising and I think quite unrealistic. What causes this? There are also some December/January grid cells in boreal forests for GFED, which I find somewhat hard to believe. Is this correct?

-Regarding fire size distribution in boreal North America, Lehsten et al 2014 (JGR Biogeosciences) show a different function for Canada than the strict power law.

-[2397, line 21] Be careful what you call tundra fires. These are still quite rare, and the model greatly overestimates them. In the observations many are in fact forests, just open or sparse in the northern limits

-The high intensity tundra fires jump out at me as a large and somewhat surprising bias. Why is this happening in the model? This may have unfavorable implications for black carbon deposition, etc. They are also spreading incredibly fast, faster than in the tropics. Is this because of grass coverage in the static land cover map?

-[2403] "To fully represent the big fire process in reality, improvements need to be made to the model to allow fire to span multiple days when the climate is favourable. . ." Again, this has been done in Pfeiffer et al. 2013.

-Regarding the influence of human ignitions. the authors could also cite Knorr et al 2013 (Biogeosciences Discussions) who show that human population seems to have

little positive influence on fire occurrence except at very low densities, and even then it's quite minimal.

Technical corrections

-[Title] I know a companion paper will focus on carbon, but as is this paper has no mention of carbon whatsoever, yet the title focuses on fires in the global carbon balance. Also, I think the following would read better: "Modelling (the role of) fires in the. . .global burned area and fire regime(s)"

-[2379, line 1] I would suggest stating that fires help determine, or are one of the major determining factors, for the distribution of biomes. They are certainly not the sole determining factor, as this sentence implies.

-[2379, line 7] As with the title, I think the plural 'regimes' is more appropriate, as there is no single global fire regime

-[2379, line 10] It's quite unclear what this 78 - 92% number actually means. Please be more specific if possible.

-[2380, line 16] Would sound better as ". . .Earth system models is needed to investigate. . ."

-[2381, line 3 - 5] This statement, particularly the word the 'infrequent', is certainly true for many boreal/temperate forests and even chaparral, but not for tropical savannas or grasslands where fire frequency is less than 5 years.

-[2381, line 17] Doesn't 'fire regime' here also include intensity, as this is mentioned previously and included in the analyses.

-[2387, line 8] VIRS, not IRS

-[2387, line 10] This should probably say ". . .by applying a modified version of the CASA model. . ."

-[Figure 6] I think an annual mean would be easier to look at here. Perhaps consider adding a panel below. This could also be merged with Fig. 5.

-I think Fig. S4 is very helpful for visualizing the regional biases and could be moved to the main text.

-Figures 2 and 3 could potentially be merged to cut down on the total number of figures.

-[2395, line 14] The variability in modeled burned area is much less than the data, which should be stated

-[Discussion, first paragraph] There is no mention of Pfeiffer et al. 2013, which should be included.

Interactive comment on Geosci. Model Dev. Discuss., 7, 2377, 2014.