

Interactive comment on “Analysis fire patterns and drivers with a global SEVER-FIRE model incorporated into Dynamic Global Vegetation Model and satellite and on-ground observations” by Sergey Venevsky et al.

Anonymous Referee #2

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Review summary

In this manuscript, Venevsky et al. describe a new fire module, SEVER-FIRE, incorporated into the SEVER dynamic global vegetation model (DGVM). SEVER-FIRE is largely based on the Reg-FIRM fire model, for whose description Venevsky was also lead author, and which provided the structural foundation upon which many modern global fire models have been built. SEVER-FIRE includes several new elements relating to fire ignition (by both lightning and humans) and fire termination, which seem

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likely to improve model realism.

Many different approaches have been used in various aspects of global fire modeling, and the new elements introduced in this manuscript are welcome as alternative mechanisms and parameterizations. A new global fire-vegetation model, moreover, could add weight to efforts to explore the uncertainty related to fire drivers and the future of fire regimes around the world. For that reason, I think this manuscript could represent an important contribution to the fire modeling literature.

That said, I recommend that the manuscript be **resubmitted with major revisions**. My explanation follows.

Main critique

Previous comments on this manuscript have highlighted the tone of parts of the paper as problematic. While I don't see it as overly hostile, I do agree that revisions should be made in the aim of reflecting the authors' respect for previous work. In their reply to Colin Prentice's comment, the authors have indicated that they intend to make changes in that direction, so I will leave aside questions of tone and language.

I do have some concerns regarding the content of the discussion, however. The modeling approach of Venevsky et al. is to minimize the use of parameterizations based on remote sensing (here, “remote-sensing approach”) and to instead favor mechanisms and relationships derived from first principles or laboratory-scale experiments (“first-principles approach”). This, they assert, may confer an advantage because their parameterizations may hold true far into the past or future (i.e., outside the satellite era) where remote-sensing-derived parameterizations do not. I can agree with that to some extent, in principle. However, Venevsky et al.—in the

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original manuscript and in their reply to Prentice—need to rethink how they discuss this.

In their reply to Prentice, the authors cite Baudena et al. (2015) as supporting their contention that including parameterizations based on remote sensing data can result in unreliable models. Specifically, they quote this passage (quoted here in full):

LPJ-GUESS-SPITFIRE simulation results do not show any low tree cover value (e.g., below 50 % cover) for rainfall higher than about 900 mm yr⁻¹ (Fig. 2b). In other words, this model (quite surprisingly) does not predict any savanna in mesic environments. In the model, though fire frequency is prescribed from the satellite data, fire spread depends on fuel load (Fig. 3c) and fuel moisture, and thus unfavorable conditions might still prevent fires. Both grass and tree presence increases fire intensity, opening up space, and thus favoring grasses. This is not strictly a positive grass–fire feedback because grass-free areas can also burn. Thus, as grasses are not fostered by the positive feedback with fire, they are always outcompeted by trees in LPJ-GUESS-SPITFIRE when water availability is high, and they do not survive above approximately 900 mm yr⁻¹. At the same time, this issue is also likely to be connected to fire intensity depending on fuel moisture. In this model, fire occurrence in a patch is calculated probabilistically from the proportion of burned area as determined from the remote sensing product. If fire occurs in a period of high fuel moisture, the intensity will be limited, thus having little effect on vegetation. This probabilistic approach is necessary because the temporal extent of the remote sensed data (now only ca. 10 years), used to generate the probability of burned area for each pixel, is much shorter than the extent of the climate data for which the model was run (ca. 100 years).

And here is the authors' interpretation, which they intend to include in their revision:

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For example, use of remote sensing derived fire frequency for Africa as an input to SPITFIRE for Africa, resulted in absence of savanna for the area with annual rainfall larger than 900 mm/yr (Baudena et al., 2015). This shortcoming of process-oriented fire model is attributed by authors to the short temporal extent of initial remote sensed data used for preparation of input data.

That is unfortunately a misinterpretation of the Baudena et al. (2015) text. As described in Thonicke et al. (2010), Lehsten et al. (2009, 2016), and Rabin et al. (2017)—and as Venevsky et al. know, given their familiarity with how relevant parts of SPITFIRE were derived from Reg-FIRM—SPITFIRE does of course have a module that, just as with SEVER-FIRE, endogenously computes fire occurrence. In Baudena et al. (2015), that module in LPJ-GUESS-SPITFIRE (and two other global fire-vegetation models) was experimentally disabled and replaced with exogenous, remotely sensed burned area, with the goal of isolating and comparing the fire-vegetation models' representation of fire's ecological effects rather than fire occurrence and spread. In the quoted text, Baudena et al. (2015) are attributing the poor performance of LPJ-GUESS-SPITFIRE not to the use of satellite data (which Baudena et al. effectively consider a true representation of reality) but rather to LPJ-GUESS-SPITFIRE not representing fuel availability and moisture in a realistic way. The relevant mechanisms in LPJ-GUESS-SPITFIRE were not derived from remote sensing data.

Venevsky et al. also, in their reply to Prentice, suggest that the Baudena et al. (2015) example shows a disadvantage of the remote sensing approach in the present as well. However, the example does not support their case:

- It is the result of a contrived experiment that does not reflect how most global fire-vegetation models actually work.
- The only global fire-vegetation model I can think of that does directly input

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satellite-derived burned area (LM3-FINAL.1; Rabin et al., 2018) would not be negatively affected by that input in the present. This is because LM3-FINAL.1 (a) only applies those burned areas on cropland and pasture, thus avoiding the problem with bad fire inputs leading to bad community composition, and (b) uses constant combustion completeness and fractional mortality factors that would not be affected by fire occurring on wet vs. dry days. Rabin et al. (2018) do acknowledge that the use of this input is problematic when applied outside the period of its derivation.

As I've said, I agree with the authors that a first-principles approach could be advantageous because it seems more likely to result in parameterizations that are more robust outside the satellite era, but I cannot think of how any example using historical data would support their case. Instead, I think the best thing the authors could write is what they wrote in their reply to Prentice:

We argue that it would be advantageous if one can produce long-term fire relationships without depending on remote-sensing, which is available for a relatively short period of time (a few decades). Fire return intervals can be of the order of hundreds of years, whereas remote sensing is available for several decades. Therefore using remote sensing to derive relationships implicitly assumes a space for time substitution, which may or may not hold. Also our approach in turn allows the remote sensing to be employed as a valuable evaluation dataset, albeit over this limited time interval.

However, I am actually not convinced that SEVER-FIRE even *is* more grounded in first principles than most other global fire-vegetation models! I see at least one instance where remote sensing or other large-scale, recent historical datasets have been used:

- Equations 1–6, governing lightning ignitions, were derived from national networks
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of ground-based sensors in the United States and Canada in 1997 (Allen & Pickering, 2002).

- Equation 9 may also have used such a dataset, although it's not clear exactly how it was parameterized. In their reply to Prentice, the authors mention that the value of \bar{a} for peninsular Spain was derived in the Reg-FIRM description (Venevsky et al., 2002); while I was not able to totally follow the chain of logic presented there, I do understand generally the strategy. However, I do not see the parameterization for the Sahel that, according to the authors' reply to Prentice, is also supposedly in Venevsky et al. (2002). More importantly, even in their reply to Prentice, the authors do not describe what historical fire occurrence data they used to derive Equation 9. Was it satellite data? If so, that undermines the authors' insistence that SEVER-FIRE has an advantage due to independence from parameterizations based on remote sensing data. Or was it instead based on national statistical databases? There are issues with those as well:
 - They only exist in certain wealthy countries.
 - They may not be reliable going back into the mid-20th century.
 - They depend to some extent on the satellite record for recent decades.
 - It would still be basing a part of the model on some external data which, although based on a longer time period than the satellite record, could still fail to be representative of mechanisms far in the past or future.

This is not to say that SEVER-FIRE is an outlier; essentially all global fire-vegetation models are designed to reproduce a limited time series of historical data, either through explicit parameterization processes or through manual model tuning. Global fire models are typically classified into two groupings—purely empirical models and quasi-mechanistic models—which differ in their reliance on parameterizations derived from historical data. See, for example:

- The anthropogenic ignition components of (most of) the eight models included in Table S1 in the Supplement of Rabin et al. (2017)
- The parameter estimation (using the Levenberg-Marquardt algorithm) described for the quasi-mechanistic FINAL.1 in Rabin et al. (2018)
- Purely empirical models such as SIMFIRE (Knorr et al., 2014, 2016)

Thus SEVER-FIRE, rather than being categorically different from most other global fire-vegetation models (a “purely mechanistic” model, perhaps) as Venevsky et al. contend, seems instead to be more first-principles-based only by a matter of degree (i.e., it derives lightning flash rate from weather rather than from a historical-derived climatology, although that derivation does itself depend in part on historical data).

Finally, I agree with Reviewer 1 that the satellite record is not unique in its susceptibility to non-representativeness. Even completely accurate, decades-long, ground-based measurements could only be assumed to be representative of the time period covered, with whatever plant species, climate/weather patterns, and anthropogenic activity was there at the time. And of course such records are not completely—or even consistently—accurate anyway! Furthermore, such records are not global in coverage, so even though the problem with space-for-time substitution is lessened relative to the satellite record (not eliminated completely), a *space-for-space* problem is worsened. Likewise, laboratory-based experiments, such as those regarding the ignition efficiency of lightning strikes, depend on the species of plant litter involved—even an experiment sampling a wide variety of plant species from across the planet could fail to be representative of species far into the past or future. The brief temporal coverage of the satellite record may make it especially vulnerable to failures of robustness, but other datasets have their own problems.

Every development team has their own principles that they bring to model construction.

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If those principles represent a significant break with the dominant mode of thinking in the field, it makes sense to spend time in the model description discussing them. However, Venevsky et al. seem to have a perfectly normal quasi-mechanistic fire model in SEVER-FIRE. Thus, this manuscript should be rewritten to focus on the model itself (especially where it differs from previous models) rather than the philosophy that governed its design.

Other major comments

1. Apparent from the comments of Prentice and Reviewer 1, as well as my read of the manuscript, is that the authors need to improve the Introduction, Methods, and Discussion sections to better highlight the novel aspects of SEVER-FIRE.
2. When explaining novel parts of SEVER-FIRE, the derivation process should always be fully explained—as the authors did for their equations regarding lightning strikes. Such explanation needs to be added for:
 - The wealth dependence of anthropogenic ignitions (Eq. 9; as they mention they will do in their reply to Prentice)
 - The limitation of fire duration to two days. This limitation may have contributed to SEVER-FIRE's underestimation of burned area in the boreal region: Korovin (1996) found that almost 70% of the burned forest in Russia over 1947–1992 resulted from fires that burned for more than *ten* days.
3. The factor *timing_j*, which modulates the frequency of human ignitions depending on the time of year, seems rather ad-hoc but could nevertheless be of use for many fire models. The authors should demonstrate that including it actually improves the simulation of annual total and/or seasonal timing of burned area.

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4. A glaring hole in many global fire models is that they do not allow multi-day burning, and so SEVER-FIRE's inclusion of this is most welcome. However, as with *timing_j*, the authors should demonstrate that including this parameterization improves their model.
5. I disagree with Reviewer 1's critique that the paper should be condensed by removing previously-published model components and instead directing readers to those publications. It is too easy to gloss over important differences that may have arisen in the time since the original publication, and makes it too difficult for the reader to learn about the model. One alternative could be to move explanation of non-novel model parts to one or more Appendices (or, less preferably in my opinion, a separate Supplement). The authors should also consider constructing a table-based description of their model to match the form of the supplementary tables in Rabin et al. (2017). This would enable a much simpler comparison between SEVER-FIRE and the models described there, and would ensure a complete description of all relevant aspects of the model.
6. The authors should explain why the model outputs were compared to GFED2, instead of the more recent GFED3(s) or GFED4(s), which would have a number of advantages:
 - These datasets cover nearly twice the time period as GFED2, which would increase the time period available for comparison—which the authors acknowledge as a weakness.
 - GFED3 incorporated an improved burned area detection algorithm (Giglio et al., 2010).
 - GFED4 incorporated further improvements to the burned area detection algorithm (Giglio et al., 2013).
 - The “s” versions of GFED3 and GFED4 are boosted by burned area estimated for small fires that the original algorithms fail to detect (Randerson et

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al., 2012).

Minor comments and technical corrections

1. P10 L24–25: This sentence should cite the “other global fire models,” as well as perhaps Rabin et al. (2017), which provides a comprehensive overview and comparison of a number of global fire models.
2. P11 L15 (Eq. 12): This equation structure does not seem to account for the fact that, for a given rate of linear spread, an older fire has a longer fireline and thus will add more burned area per unit time than a more recent fire. This could be a contributing factor to the underestimation of burned area in boreal regions, where large, long-lasting fires contribute significantly to total burned area. I do not consider this a critical issue, but it's something the authors should definitely mention.
3. P12 L17–26 (Sect. 2.2.2): It would be nice to see, probably in a Supplement, figures showing the input data described here.
4. P13 L18: “As a DGVM” should be deleted—there are certainly DGVMs that have the capability to output results that reflect the vegetated area in a gridcell. The authors should also explain (a) why they found it necessary to adjust the GFED data, rather than simply adjusting the SEVER outputs, and (b) what the net impacts of their adjustments were on global burned area.
5. P14 L19–21: A citation of Lasslop et al. (2015) should be made here.
6. P16 L14–16: This text implies that the overestimation of fire in India may have something to do with the fact that the model simulates grass there. In reality, it's

probably because of strong fire suppression resulting from high fractional coverage of cropland.

7. P18 L14: Mention should be made of the fact that these regions were originally created for use with GFED (Giglio et al., 2006).
8. Work is needed on the Discussion paragraph about anthropogenic impacts on fire (P21 L7–19):
 - Pfeiffer et al. (2013) should be mentioned, since they introduce a number of interesting ideas for modeling of human fire use.
 - “In Africa for example, the combination of a strong seasonal wet-dry climate with regular human ignitions favours high fire incidence.” This sentence does not seem to fit with the idea introduced in the previous sentence; namely, that land use and agricultural practices are likely more directly related to fire incidence than wealth in certain regions.

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