Review: Improving the LPJmL4-SPITFIRE vegetation-fire model for South America using satellite data

General comments

Process-based global fire models are widely considered critical components of dynamic global vegetation models, with certain biomes—especially tropical and subtropical savannas and grasslands—being strongly regulated by fire disturbance. However, many such fire models have been developed based on parameterizations from extratropical biomes. In this manuscript, Drüke et al. use an automated technique to reparameterize a global fire model to improve its performance in the Brazilian Caatinga and Cerrado biomes with regard to both burned area and biomass. The authors perform this optimization using actual runs of the vegetation model rather than in some kind of offline mode—something that has only rarely been done before for fire models, but which could be a valuable component of the global vegetation-fire modeling toolbox. This, combined with the fact that the authors describe their methods thoroughly and walk through the results in a logical manner, lead me to recommend that this manuscript be ***accepted for publication pending minor revisions***.

Specific comments

- P2 L13: The authors describe the Caatinga as fire-prone, but the referenced map (Fig. 1) does not provide much support for that assertion. The authors should clarify in the text what they mean.
- P6 L1: "their effectiveness to ignite a fire is 0.04" is unclear. Better would be something like, "4% of cloud-to-ground strikes can start a fire."
- P6 L9:
    - As far as I can tell, this is the first time the parameter named p_d has been used in regard to SPITFIRE. I suggest using some other symbol, as this p_d could be easily confused with P_D (population density) from Thonicke et al. (2010).
    - "per day" is misleading; SPITFIRE as described in Thonicke et al. (2010) does not allow for multi-day fires, and thus this is simply the maximum fire duration.
- P11 L1–2, P21 L21–27: Because LPJmL does not allow fire on managed lands, the authors exclude cropland burning from the observed data in their comparisons. This is reasonable, but ignores the fact that a fair amount of Cerrado is actually used as pasture, primarily in the southern part of the region (Sano et al., 2010; Parente et al.,

2017). I don't think this makes a huge difference in the context of this manuscript, because (a) only a few of the 40 sampled gridcells were from the southern Cerrado, and (b) the main takeaway from this paper should be the use of the optimization algorithm, rather than the exact parameter values it gives. However, the authors should (briefly) address this issue in the text.

- Sect. 2.4 and/ or Sect. 3.2: For the benefit of other researchers interested in using this or a similar optimization algorithm, it would be helpful to know various pieces of info about the process. How many model runs were required? How long did they each take? How was the decision made to halt the optimization—was it manual, or did the algorithm reach a stop condition? If the latter, what was/were the stop condition(s)? Etc. This level of technical detail is more than appropriate for *GMD*.
- Sect. 2.6: The authors do a good job describing how to interpret values of the NMSE, but they should do the same for the Willmott coefficient of agreement. What are the possible values? What are "milestone" values (e.g., for NMSE, 0 vs. 1 vs. >1)?
- P13 Fig. 5: It would be helpful to use the same Y-axis for all subplots in the right column (subplots b, d, f), as was done for the left column.
- P13 Fig. 5 and P16 Fig. 6: Nesterov and VPD rows should be swapped, since in the rest of the paper the Nesterov Index is usually discussed first.
- P16 Fig. 6: The use of lines here is confusing, since that usually implies some kind of change over time. The authors should seriously consider using a bar graph here instead.
- P17 L3–4: "The model optimization scheme focuses on fire parameter [*sic*], hence the model performance can only improve in fire-prone biomes, i.e. not in, e.g., wet tropical forest where fire is absent." This is not strictly true. Model performance could improve in wet tropical forest if the initial parameterization (a) performed badly there with regard to burned area (i.e., simulated almost any fire at all) and (b) underestimated biomass. It just so happens that neither of these conditions are met by the initial LPJmL configuration. This may seem like a minor quibble, but it could mislead other researchers interested in applying this or a similar optimization algorithm to their own models. It is important to be clear that optimizing a fire model can improve performance with regard to vegetation parameters not necessarily where fire is frequent, but rather where fire is *modelled poorly*.

- P20 L7–8: Presumably the authors are making this assertion based on the fact that the indicated region is modelled as ~50% tropical evergreen, but Fig. 8 does not appear to say anything about the tropical raingreen PFT—just evergreen. The authors should clarify this.
- P21 L23–27:
    - How do the authors reach the conclusion that including fire on managed land would increase "fire amplitude" (this phrase should be reworked, by the way) and improve interannual variability? Why might it not also (or instead) improve annual mean?
    - Citations should be added regarding the real-life use of fire on managed lands (e.g., Laris, 2002).
    - Citations should be added regarding the simulation of fire on managed lands (e.g., Pfeiffer et al., 2013; Rabin et al., 2018).
- P22 L1–2: This is incorrect; Rabin et al. (2018) did indeed optimize FINAL.1 within a dynamic global vegetation model (LM3). Also, the authors should (at least briefly) discuss the pros and cons of their method relative to the one used by Rabin et al. (2018); this would be valuable for other researchers interested in optimization methods.
- P22 L14–15: The authors address availability of the model code, which presumably refers to LPJmL. But what about the genetic optimization code?

Technical corrections

- P9 L5: "form" should be "from"
- P10 L22: "dependend" should be "dependent"
- P11 L19: "simulations" should be "simulation"
- P11 L25: "Caating" should be "Caatinga"
- P17 Table 3: "Evergreem" should be "Evergreen"
- P17 L3: "parameter" should be "parameters"
- P18 L12: "significante" should be "significance"
- P18 L13: "particular" should be "particularly"
- P19 L6: "particular" should be "particularly"
- P21 L12: "seperate" should be "separate"

Works cited in review

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Parente, L., Ferreira, L., Faria, A., Nogueira, S., Araújo, F., Teixeira, L., & Hagen, S. (2017). Monitoring the brazilian pasturelands: A new mapping approach based on the landsat 8 spectral and temporal domains. *International Journal of Applied Earth Observations and Geoinformation*, *62*, 135–143. http://doi.org/10.1016/j.jag.2017.06.003

Pfeiffer, M., Spessa, A., & Kaplan, J. O. (2013). A model for global biomass burning in preindustrial time: LPJ-LMfire (v1.0). *Geoscientific Model Development*, *6*(3), 643–685. http://doi.org/10.5194/gmd-6-643-2013

Rabin, S. S., Ward, D. S., Malyshev, S. L., Magi, B. I., Shevliakova, E., & Pacala, S. W. (2018). A fire model with distinct crop, pasture, and non-agricultural burning: use of new data and a model-fitting algorithm for FINAL.1. *Geoscientific Model Development, 11*(2), 815–842. http://doi.org/10.5194/gmd-11-815-2018

Sano, E. E., Rosa, R., Brito, J. L. S., & Ferreira, L. G. (2010). Land cover mapping of the tropical savanna region in Brazil. *Environmental Monitoring and Assessment*, *166*(1-4), 113–124. http://doi.org/10.1007/s10661-009-0988-4