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Discussions

Interactive comment on "The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model" *by* K. Thonicke et al.

Anonymous Referee #2

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General comments:

Because this paper describes the new fire module of one of the most promising DGVMs in use right now, it is important and has scientific merit. The manuscript is clear and well written, but it flows a bit like a technical report instead of a paper in a scientific journal. The review of previous DGVM fire modules and the description of real advances in SPITFIRE need more detail and structure. In addition, many of the assumptions and potential limitations of SPITFIRE are given only cursory treatment or are ignored altogether.

Specific comments:

1) In the second paragraph of the Introduction, the role of fire in global emissions budgets is reviewed briefly, with fire being cast in a generally negative light. There is no differentiation, however, between emissions from regions where fire is a natural ongoing process (i.e., emissions would naturally be offset by post-fire vegetation regeneration) versus where fire does not naturally play a role (i.e., emissions are a net source and land cover may be more-or-less permanent). This is an important difference, as one of these types of sources is a net contributor to global warming and the other much less so.

2) The review of existing fire modules (p. 700-701) reads like a list, with minimal explanation of why other fire modules are really in need of improvement. Similarly, when SPITFIRE's advances are described, a reader has no good criteria to understand how close the authors have come to providing the full functionality required to adequately model fire (or, by their omission, what components SPITFIRE has yet to incorporate). I see this as one of the bigger problems in the paper, and I think it could be remedied fairly easily. Although it is over a decade old, the Fosberg et al. 1999 paper (in International Journal of Wildland Fire) describes several capabilities that a fire module in a DGVM should have. Other papers have also summarized characteristics and/or limitations of how fire-related interactions are handled in different models (e.g., Keane et al. 2004 in Ecological Modeling) or requirements for fire-vegetation dynamics to be captured correctly in certain biomes (e.g., Pausas et al. 2004 in Ecology). A chart showing what types of functionality are needed in a DGVM fire module, which of these are included or missing from other DGVMs, and how many of these now included in SPITFIRE is needed. This would greatly improve a reader's understanding of SPITFIRE's novel contribution and what is left to be incorporated as future work.

3) Although SPITFIRE is called "process-based", models of this complexity could produce similar outputs from vastly different combinations of input parameter settings. The reader is left wondering how many of the parameters came from published values, versus those user-specified and "tunable" parameters that the authors had to vary in order

to successfully recreate the patterns of interest (i.e., historical fire rates and locations). In addition, how many sources of stochasticity are there in the model (if any)? It would be helpful to have a short summary – either written or as a table – to provide this type of information.

4) The ROS of fire is modeled according to Rothermel (p. 702, 708), which is very sensitive to wind speeds. Furthermore, we know that in many parts of the world most of the area burned occurs under relatively high wind conditions. In contrast, we are told that winds are input as monthly average values. How is this apparent mismatch justified/rectified? ROS is also quite sensitive to topography, which is not represented in SPITFIRE. Although it is obvious why coarse-scale topography has limited meaning, why is the omission of topography not a cause for concern in accurately modeling area burned?

5) The role of "fire danger" (FDI) appears to be important in many of SPITFIRE's equations, but its description and justification are extremely brief (section 2.2.3). First of all, it seems problematic to apply eq. 7 to all fuels classes, since ignition and fire spread are primarily constrained by characteristics of the finest fuels classes. Allowing FDI to be driven by larger fuels classes, which could also be heavier and thus carry a larger weight in the composite estimate of fuel moisture (eq. 6)., needs more explanation. Also, what is the rationale for the linear decrease in fire danger with respect to fuels drying out? This is a potentially problematic assumption, especially since there are threshold-type responses in fire danger for live fuel moisture. In short, there is not enough description/justification/testing of the assumptions made concerning FDI, particularly given its pervasive role in SPITFIRE.

6) It is disturbing that the section on fire damage to plants (2.2.6) is based almost entirely on literature pertaining to North American conifers. What about all of the other PFTs around the world? This bias toward coniferous trees ignores many important fire-plant dynamics (e.g., see Pausas et al. 2004 noted above), possibly even the majority. Because getting this particular interaction right is so important to modeling fire-plant

dynamics correctly, I think that this is one part of SPITFIRE that needs additional work; at the very least, some discussion of how this limited set of relationships may alter overall patterns at larger scales is warranted.

7) The claim about fire danger being zero in deserts (p. 714) does not appear to always be true, based on visual inspection of Fig 2a (e.g., desert SW of US, Arabian-Thar deserts). It would be interesting (and helpful for the point you are trying to make) to see some graphs of fire danger vs fuel load for many biomes.

8) The last sections of the paper do a decent job of evaluating the modeled fire patterns, but little is said about why SPITFIRE performs well in some regions and not in others. Which assumptions or simplified processes need to be refined? It might be helpful to compare simulated vs actual fire, to highlight patterns in residuals and the key biomes where improvement is needed. This might also aid in identifying potential refinements. How often do you get the wrong vegetation type but correct fire-related patterns? This manuscript documents a tremendous amount of work, but it is not very critical of its own performance.

9) The final paragraph of the paper makes several assertions about the potential uses of SPITFIRE. What are the current limitations? Given the many PFTs that are not accommodated, why would we expect model outputs for future climate scenarios to be realistic? Which simplifications and assumptions must be further tested, and which appear to have worked well?

Interactive comment on Biogeosciences Discuss., 7, 697, 2010.