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Interactive Comment

Interactive comment on "Improved simulation of fire-vegetation interactions in the Land surface Processes and eXchanges dynamic global vegetation model (LPX-Mv1)" *by* D. I. Kelley et al.

Anonymous Referee #2

Received and published: 19 March 2014

Review

Keeley et al: Improved simulation of fire-vegetation interactions in the Land surface Processes and eXchanges dynamic global vegetation model (LPX-Mv1)

The manuscript describes improvements of the LPX model in terms of the fire-vegetation interactions with a focus on Australia. These improvements include various aspects of the fire model as well as of the hosting vegetation model (a new lightning ignition parameterization, a different treatment of bark thickness, different fuel moisture treatment and litter decomposition and the introduction of resprouting in the model). The model improvements are evaluated for Australia using a benchmarking scheme

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introduced by Keely et al. (2013).

The manuscript does a very good job in describing the single modifications introduced into LPX. The Result section, however, reads a bit difficult. Some conclusions are hard to follow and the single benchmarking metrics terms used are sometimes confusing (see specific comments below).

As reviewer #1 mentioned it would be nice to see how the modified LPX model performs globally and not only for Australia as presented here. As long as this is not shown in the manuscript statements such as “The new model incorporates a more realistic description of fire processes and produces a better simulation of veg etation properties and fire regimes across Australia, and is expected to produce a considerable improvement in the simulation of fire-prone vegetation worldwide.“ are not justified.

Overall, the topic and the manuscript is well suited for Biogeosciences and I recommend publication after the specific comments below have been addressed.

Specific comments:

Title: That the evaluation of the model is restricted to Australia should be reflected in the title

Abstract: “The introduction of adaptive bark thickness and resprouting produces more realistic fire regimes in savannas, including simulating biomass recovery rates consistent with observations” – This must not be the case for African savannahs. You only tested for Australia.

The new model (LPX-Mv1) improves Australian vegetation composition by 33 % and burnt area by 19 % compared to LPX. – the improvement in % is hard to understand out of context. Here you should be more specific, e.g. referring to your benchmarking score used. Also the values refer probably just to one metric (annual average) and do not summarize all the metrics applied (including seasonal variation and interannual variability).

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Introduction: The introduction is very short and misses, for example, an overview of other global fire model activities.

The shortcomings of the LPX model must be demonstrated somewhere. If this is part of Keeley et al. (2013) than it should be properly referenced, otherwise it should be shown in more detail here.

Page 934/Line 16: Here it should be mentioned that the work by Prentice et al. is based on an earlier version of the SPITFIRE model by Thonicke et al. This than also helps the reader to understand the numerous references to Thonicke et al. in the following paragraphs.

Page 938: Lightning ignitions

(i) I have problems to follow your description on lightning ignitions. The total flash rates are taken from the LIS sensor and the CG flashes from NLDN, and you combine those two to derive a relationship between total flashes and CG flashes? How does RL than relate to L? I think I'm missing here something.

(ii) How does your relationship compare to the findings of Baldocchi et al., 2000?

(iii) The formula gives flashes in (flash m2 month) The figure (1) in flashes / km*/day . This should be unified. In the LIS product the lightning frequency ranges between 0.1 and 70 flashes/km**2/year. According to your equation this leads to CG ratio way below 1%, which is increasing with increasing total flashes and not decreasing as the figure suggests. Please clarify.

(iv) Where does CG_dry come from and how is Pwet defined?

(v) What does "Discontinuous current lightning" refer to?

Page 940/Line 17: In the revisions Pfeiffer and Kaplan named their model differently, please refer to the new model name.

Page 941: Fuel drying. This new treatment than fully replaces the Nesterov index as

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described in the LPX model description?

Page941/Line23: Please define Hr

Page 941/Equation 18: Could you indicate how much this formulations differs from the simple assumption T_dew=Tmin– 4 .

Page 944/Line7: Is it too high or too low?

Page 950/ Model configuration: Did the spin-up not use lightning ignitions?

Page 951/ Line 2: “in the original model (see Table 7)“ in Keeley et al. 2013?

Page 951: "We re-gridded the data for the period 1997–2006“ Why did you restrict your analysis to this timperiod? GFEDv3/4 is now available for longer timeperiods.

Page 952/Line 12: Table 6 refers to a bootstraping mean model, this should be named similar in the main text and in the table.

Page 952/Line 22: Manhattan Metric is MM in the following?

Page 953: The description of test of the resprouting treatment on plant recovery should have an own section. It does not really fit unter Model configuration and test.

Page 955: Model performance simulations values are sometimes compared among each other, sometimes to the mean model and sometimes to the random model. This should be done more consistently.

Page 955/Line 9: which NME score? Annual average S1?

Page 956/Line 20: “Despite an improvement of 65–95 %“ this refers to what?

Page 956/Line 25: "around ca. 1–5 %“ this refers to what?

Page 956: Figure 8/9/10 should be introduced in the main text. Throughout the analysis you refer to these figures. It would be easier for the reader to get an idea in the beginning of these sections what is shown in the single graphs.

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Page 957/Line1: “The simulated distribution of trees in climate space is improved in LPX-Mv1-rs compared to LPX.” How to you evaluate this?

Page 958/Line14: The discussion of Figure 7 appears here a bit out of context maybe this should be done earlier, when discussing the overall resprouting treatment. Also Figure 7 needs a more descriptive caption.

Page 958/line 23: “LPX-Mv1-rs compared to LPX is 18–19 % for burnt area“ Here and throughout the manuscript I do have problems to understand the measure "burnt area“. Does this refer to the performance of the model to reproduce the spatial distribution of the annual burnt area.

Page 957/Line9: “fAPAR was already on average 59 % higher in LPX compared to observations (Table 7)“ Does this refer to the NME or the absolute value?

Page 961/Line2:” We have used the conceptual framework of Clarke et al. (2013), which is based on extensive field observations, to evaluate our simulations of RS-dominance in a qualitative way“ it is not clear to me to what kind of framework this refers to.

Page 961/Line 26: “and is expected to produce a considerable improvement in the simulation of fire-prone vegetation worldwide“ – without showing any global results this is still a very speculative.

Table6/7: the step1,2,3 notation is confusing. The table should be extended to include the information directly.

Figure 8: applies a crop mask. The differences are actually hard to distinguish. I can not find a reference to the crop mask in the main text. What fields are used in the benchmarking, the one with or without the crop mask?

Interactive comment on Geosci. Model Dev. Discuss., 7, 931, 2014.