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Biogeosciences Discussions

Interactive comment on “Modelling burned area in Africa” *by* V. Lehsten et al.

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Dear Referee of Biogeosciences, thanks for reviewing our manuscript and giving us the opportunity to improve it.

As requested by both reviewers, I added two new figures (10 11) displaying the spatial pattern of the changes in the driving data for the future simulations as well as the simulated changes in burned area. All requested changes are commented below. We have incorporated all of the reviewer’s comments related to spelling and readability of the text, and revised the manuscript’s language carefully throughout, including a tightening-up to shorten some sections. The following point by point response does not contain comments relating to readability if they are incorporated in the manuscript as suggested. Statements in *italics* indicate our responses to the comments.

Referee 2 P4386L2: ‘vegetation’, please be more precise: vegetation density, type,

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Interactive Discussion

Discussion Paper

productivity? *We choose the unspecific word ‘vegetation’ since here we aimed to refer to all aspects of vegetation including density, type and productivity, which are all related to wildfires in Africa. changed to ‘aspects of’ P1L15*

P4386L9: Later I learned tree and herb cover are only available for the year 2001, not for the full period as you state here *Yes this was imprecise and we clarified it already here in the Abstract. P1L27* P4386L15: A value of 10E4 will only mean something for those who have worked with the Nesterov index before, please add some text on what this value indicates. *Good point. We have explained this in the revised methods section (2.1.5), but judging that this information would be too specific for the abstract, we removed the value from the abstract. P9L15-19*

P4386L24: this is far-fetched and requires at least some extra text. In the main text you state that total precipitation does not change, so there could be a compensating effect. I will get back to this later, but to make these conclusions a more detailed analysis than currently provide is required. *The results and discussion sections now contain a more detailed analysis.*

In general, the abstract requires some text on uncertainty and shortcomings. You capture about 50 percent of the variability (R = 0.71), what about the other 50 percent Model shortcomings, data shortcomings? *We added a sentence in the abstract. Basically, we expect this uncertainty results mainly from applying a model that is designed to be very general (to meet the goal of applicability for global scale) by considering only the major drivers of fire, combined with the high complexity of the phenomenon of wildfires and their drivers. P2L9-10*

P4387L6: this may be misleading; carbon emissions from fires are not the same as carbon emissions from fossil fuel burning as the former may be balanced by regrowing vegetation *True, these are different aspects since emissions form wildfires are probably sequestered in the next years. However, pyrogenic emissions still show a strong seasonal cycle, which is of relevance for the climate and hence GCMs.*

P4388L17: please indicate what you mean with ‘vegetation’. *Changed to tree and grass cover P4L28*

P4389L1: what do you mean with ‘over the full availability’? think you can leave this Out *I meant that we used all available data. Taken out*

P4389L18: Does this impact the performance of your model negatively? Please elaborate.

In P4397L16 we stated that by using the training data to predict the burned area within the training data domain results in an R value of 0.74. This value is only slightly better than the 0.71 that is achieved by comparing the observed data from the evaluation data set compared to the predictions based on the training data set.This increases our confidence that in the selection procedure covered a valid area. We added a sentence in the Results section explaining this. P12L13

P4390L12: Why annual? The data is available on a daily step, or weekly if you take uncertainty into account. You predict burned area also on a higher temporal resolution, would make sense to compare modeled and measured data at this temporal scale.

In the first part of the analysis we analyze the annual burned area fraction, while in the second part we distribute the fire activity over the seasons. If we would keep the quasi daily resolution this task would require a much more complex model since the seasons would have to be taken into account specifically into a time series model instead of a GLM and this resulting model would be highly complex and could not easily be integrated into a DVM which was one of the key objectives of this work. The second part of the analysis, the distribution of the fraction of burned area within the year uses the daily data.

P4390L27: not sure what you mean here, please elaborate

We added an explanation.P6L14-17

P4391L9: there are reasons for this large range and it could be accounted for (see

Giglio et al. 2006), on the scale you work uncertainties will be smaller than you want the reader to believe here. Burned area products are preferred above fire hot spot data though, fully agree. *We fully agree and still think that the conversion from active fire data into burned area was a very important step, since before the availability of burned area products based on reflectance differences it was the only way to estimate burned area. However the fact remains that burned area products seem to be the more reliable information. In any case, the wide range of conversion factors is not really needed in case of our development, and therefore took the sentence out.*

P4393L21: why not a moving fire season? With the definition now you do not handle the propagating timing of the fire season; in October rain has started in Angola while in Madagascar it is still raining in May. This will impact your analysis of interannual variability. *You are right that this change in seasons will have an impact on our interannual variability. The reason for introducing the fire seasons was that it allowed us to pool the northern and the southern Hemisphere of the continent. We defined the seasons by looking at the average total burned areas for each hemisphere. Applying a similar type of approach to a larger number of regions would have made the analysis more complex since one would have to objectively define a set of rules about how to define such a moving fire season. However we are appreciating the idea for future development of the model.*

P4394L3: The Nesterov index was developed for northern regions where temperature is an important factor. THis is probably not the case in Africa, please elaborate why you still chose this index *We added a sentence explaining our reasoning. P9L19-21*

P4398L5: 'well'. Not sure, from the figure it seems burned area in all major biomass burning regions are underestimated. In combination with the underestimation of the satellite burned area as mentioned in the paper this points to a serious error in the model. This makes me wonder: you report an R of 0.71, so about 50Is the absolute value somehow included in this estimate, or just the variability? *The absolute burned area (continental sum) is relatively well estimated due to the statistical method which*

BGD
7, C2775–C2782, 2010

tends to even out the differences spatially and temporally but retains the total value when estimating the model parameter.

I am asking this because in the beginning I was impressed by the 0.71 (50 perc of the variability),but when looking more closely at the figure (most importantly 4, which I think is the absolute value instead of the standard deviation as mentioned in the caption) the underestimation makes the model look worse. I may be missing something here. *It is true that the model under-represents areas with high biomass burning. Looking at figure 3 one can see that there is a large variability in the data, compared with the smooth model response. Additionally there are other variables which are not used here and which are not available at a global scale or for projections such as land use habits or other socioeconomic factors influencing land use. The derived model can probably be substantially improved by adding additional variables or increasing the complexity of the statistical model but both would decrease its applicability for predictive burned area modelling e.g. within DVMs.*

P4399L7: it would be good to quantify this. I have a several issues with the statement that fires burn in the beginning of the season, most importantly: 1) are these model results? If yes, then please be careful with this statement because readers will think that fires in general burn in the beginning of the dry season. *These are no fire model results, but represent a comparison of the applied climate data with the remotely sensed burned area, basically by visually comparing the two Hovmöller plots. .We added a line in the results section as well as the figure caption stating this. P14L25*

In figure 9 you show (for one grid cell) that the model peaks earlier than remote sensing indicates, so your modeled results may not be good enough to make these statements 2) the nesterov index increases over time, so although the red colors are more striking in figure 7, they actually indicate the 2nd half of the fire season. *Figure 9 is now figure 8, since we combined figure 6 7. This statement was not based on Figure 8,which would be questionable since it only displays a single cell, but on results displayed in the panels of Figure 6 covering the whole continent. Figure 7 shows that the burned* *area peaks at around a Nesterov value of 10000 The reviewer is right in that that this Nesterov value does not indicate whether the fire occurs in the beginning or the end of the dry season. The figure rather links the fire event to a certain Nesterov value; and these values are reached at different times of the year depending on the length of the dry season (mostly in the first half). We revised the sentence accordingly. P19L30*

P4400L2: ‘good agreement’. I would not rate this as ‘good’; the peak is at least one month too early and the second half of the fire season is poorly captured. *We took out the word ‘good’. However, given the high variability of the data, the complexity of the phenomenon and the simple model parameterized we still consider the fit quite satisfying. P15L5*

P4400L17: This part is confusing. If the total precipitation remains the same AND the Nesterov index increases then there is a redistribution of precipitation. This needs to be discussed and visualized because it is the basis for your statements about future fire activity that is the most novel part of the paper. *In response to this request we have introduced new sections (last section of Results) and figures (Figure no. 10 11) considering the spatial distribution of the projected precipitation and population and the projected burned area.*

P4401: the Discussion section is long and would benefit from being better organized using subsections *We separated the Discussion into subsections with the hope to increase the readability. In addition, we restructured and shortened some sub-sections in the discussion to strengthen the focus on our main arguments.*

P4401L1-8: this is an excellent section

P4401L16: these kind of analyses actually perform better with decreasing resolution because several factors usually average out. So I found it difficult to see this was used as an explanation why the model did not perform perfect. *In terms of nonlinear relationships (e.g the derivation of the Nesterov index), applying a grid-averaged (lets say on a 5 degree scale) precipitation will result in a quite different value than an av-*

BGD

7, C2775–C2782, 2010

eraged Nesterov value calculated from fine grid-scale resolution. This is because an averaged precipitation might be 'on average' strong enough to reset the Nesterov index continuously. A geographic fine-scale pattern with, for instance, the northern half never experiencing such a reset (leading to a high fire danger), and frequent resetting of the index in more southern locations would however result in a quite different fire pattern for the whole area. This is just one example; we also expect an increase in performance at a higher spatial resolution for the other nonlinear responses used in our analysis.

P4401L23-24: please add a reference or elaborate, so the term 'probably' can be deleted. *Since there is no study concluding exactly this, we deleted this sentence.*

P4403L8-11: That may be true but is population density a good proxy for the number of human ignitions? Everybody agrees that fires can be set in deserts or during the wet season in a tropical forest, clearly climate is important. But in the climate zone where fires occur, humans may be more important than you suggest. As long as you cannot explain the majority of the variability with parameters other than humans, there is a chance humans are more important than climate. *We are aware that human population density as it is used here is not the best proxy for number of human ignition. However, there arises a question of scale (being applicable on global scale) and availability of potential proxies for future projections. Human population has been used as proxy in other examples (e.g., Thonicke et al., Kloster et al., both BG) – which in itself may not be enough as an argument. The point however is, that at present there seems no suitable alternative (considering spatial and temporal scale of our interest) variable that has some explanatory power and does change strongly in the future according to the population development projections. As long as there is no better proxy available we use population density.*

P4405L1: I do not understand this statement; as far as I know all DGVM studies (e.g., Venevsky, S. Maksyutov,S. (2007) SEVER : a modification of the LPJ global dynamic vegetation model for daily time step and parallel computation Environmental Modelling and Software 22, 104-109) somehow use a Nesterov index and thus have a higher

BGD

7, C2775–C2782, 2010

Interactive Comment

time step than annually. *Yes but they all use a dynamic approach. Here we are using a purely statistical approach. We clarified that in the sentence.P19L24*

P4405L20: Again, this requires a more thorough regional investigation because the decrease in burned area is not expected when precipitation remains constant. *See additions in the results and discussion section*

P4405L25: Why not compare to the burned area dataset you used? And matching burned areas seem difficult to reconcile with figure 4c where the model seems to underestimate burned area in all major fire regions. *See additions in the results and discussion section.*

P4406L17: Good to see the discussion here. This needs graphics to be backed up though, now it remains speculation. *See additions in the results and discussion section.*

P4407: The conclusions section should be expanded in my opinion to a condensed discussion section and include quantitative information and some text on uncertainties. *As a conclusion section it is meant to highlight the results and possibilities very short, since the actual information is already stated somewhere before.*

Fig. 1: By painting everything below 3 percent white a lot of fire-prone areas areas are masked. It may be better to set this threshold lower. This also applies to the other maps *The original threshold was at 5 percent we have redrawn all maps with a threshold of 2 percent.*

Interactive comment on Biogeosciences Discuss., 7, 4385, 2010.