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Biogeosciences Discussions

# *Interactive comment on* "Causal relationships vs. emergent patterns in the global controls of fire frequency" *by* I. Bistinas et al.

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RESPONSES TO REVIEWERS

Anonymous referee #1 As the referee points out, this is one of a series of papers analysing the controls of fire, but we are pleased that the reviewer recognises the added value of our paper because it specifically addresses the processes involved, the existence of emergent patterns and the way that these analyses can be used to improve the treatment of fire within DGVMs. We feel that this is what is novel about our work, and distinguishes the paper from previous analyses.

COMMENT:The paper is very well written although I would appreciate a summary.

RESPONSE:We did not include a final summary originally because of concern about



the length. However, we agree with the review that this could be helpful. We will therefore added a conclusion section summarising our findings as follows: "We have used generalized linear modelling to analyse the relationships between burnt area and a set of variables related to explicit controls on biomass burning. We show it is possible to describe the influence of vegetation, climate and land use on burnt area as simple monotonic functions, and that these relationships make intuitive sense in terms of the mechanisms of fire spread. Specifically, burnt area is positively related to annual net primary production, the amount of grass/shrub cover, the number of dry days per month, maximum daily temperature, the diurnal temperature range, and the area of grazing land. Conversely, burnt area decreases with increasing soil moisture, cropland area and human population density. There is a strong seasonal cycle of lightning, but once the influence of this is removed, there is no relationship between the number of lightning strikes and burnt area. Thus, although the amount of lightning may influence the number of fire starts, it does not determine how much of the area subsequently burns. We have also shown that the unimodal relationships that have been found between burnt area and climate variables such as mean annual temperature and precipitation, or measures of potential human impact such as population density and gross domestic product are not indicative of direct causal relationships. They are emergent patterns that arise because these variables are correlated with specific controls that exert different influences on burnt area. For example, the unimodal emergent relationships with population density and GDP arise because regions of low productivity generally support low populations. Our findings suggest that caution is required in the attribution of causality and the implementation of individual controls on burnt area in both statistical and dynamic-vegetation models. Specifically, the widely assumed dependence of fire frequency on ignition rates is incorrect: lightning is not limiting and the impact of increasing human populations is to suppress fire."

COMMENT:The graphics are weak and have to be improved substantially to better convey the messages. Figure 1: density scatter plot instead of scatter plot; axes labels have to be clarified, the values are meaningless to most; make 3 x 4 plots instead of

6 x 2 so the graphs can be larger Figure 2: I think you can add a few bins by using a different color scheme. Right now the bins are extremely wide Figure 3: same first two comments as for figure 1. RESPONSE:We apologise for the quality of the submitted figures. We inadvertently submitted draft figures rather than final versions. Nevertheless, the reviewer's comments on these figures are well taken. We have redrawn all the figures. We have produced a new version of the Figure 1 where the relationship and standard errors are shown in blue and shading and the actual points as black dots. We have produced a version of Figure 3 using a density plot as suggested. We have adopted the reviewer's suggestions as to layout. We have also ensured that the labels indicate what the variables actually are. For Figure 2, we have increased the number of bins from 4 to 7 and taken the opportunity to improve the labelling.

COMMENT:One thing that draws attention is that the savannas are reasonably well captured, but mismatches are large in temperate regions (e.g., the US). This requires a bit more discussion than "The broad geographic and seasonal patterns are captured well by the GLM"

RESPONSE:The first-order patterns are well captured by the GLM, and in particular the distinctions in the tropics and subtropics. As the reviewer points out, discrepancies are larger in temperate areas where the actual incidence of fire is lower. This reflects the fact that the observational record is relatively short. We will expand the discussion of the GLM results as follows: "Although the broad geographic and seasonal patterns are captured well by the GLM, there are discrepancies in regions where the actual incidence of fire is low and/or highly periodic such as boreal and temperate forests. This is an under-sampling problem and reflects the fact that the observational record is short, and thus the derived GLM has greater difficulty to capture low frequency and highly aperiodic events. This is also reflected in the predicted seasonal cycle, where the match between observed and predicted is better for the periods corresponding to the major periods of burning (DJF and JJA) than for MAM and SON. Nevertheless, the model provides a good representation of the controls on fire in those regions which are



most important in terms of the terrestrial carbon cycle."

COMMENT:One other comment is that parameters derived from a global study are not necessarily regionally applicable. For example, on a global scale lightning may not be important but in the boreal region most (large) fires are started by lightning. This should be much more clearly acknowledged RESPONSE:We are not claiming that lightning is unimportant, nor would we disagree with the statement that lightning is the cause of most large fires in boreal regions. Indeed, we suspect that most large fires are caused by lightning, regardless of the ecosystem. Our point here, and it is consistent with other studies, is that the number of lightning strikes per se is not the determinant of burnt area. Our study implies that there are always sufficient lightning strikes to start fires and that the limitation on the spread of fire is the flammability of the material, which is determined by fuel availability and by fuel dryness. This was the point of our comment about the difference between numbers of lightning starts and impact of lightning on burnt area, because many modelling groups assume that increasing the number of lightning strikes will automatically increase the burnt area. However, clearly we need to make this distinction clearer in the text. Specifically, we propose to expand our comment about the relationship between number of ignitions and burnt area in the beginning of the discussion to explicitly include natural (lightning) ignitions as follows: "Knowing that people start fires, some model developers have focused on human ignitions as a supposed determinant of burnt area (e.g. Thonicke et al., 2010). In a similar way, because observations indicate that large fires in e.g. boreal ecosystems are started by lightning and that the number of fire starts increases with convective activity and number of strikes (e.g. Peterson et al., 2010), it is frequently assumed that increasing the number of lightning ignitions will lead to an increase in burnt area (e.g. Pfeiffer et al., 2013). Fire size however is extremely skewed, and burnt area is dominated by large fires that can only occur under weather conditions suitable to fuel production and rapid fire spread." We also propose to expand the existing discussion of whether lightning is a limiting factor on biomass burning and the treatment of lightning ignitions in DGVMs as follows: "Lightning has no significant effect, indicating that the number of lightning strikes is not a limiting factor on biomass burning and specifically on burnt area. These conclusions support the suggestion of Knorr et al. (2013) that ignition sources rarely or never limit fire frequency. Although there may be considerably spatial and temporal variability in the number of lightning-induced fire starts, as well as regional differences in the relative importance of natural and anthropogenic fire starts, this variability is ultimately unimportant in determining burnt area. Imposing a strong and explicit link between number of fire starts and burnt area in a model will lead to erroneous predictions." Finally, we will add an explicit statement about lightning in the newly added summary section (see above) as follows: "There is a strong seasonal cycle of lightning, but once the influence of this is removed, there is no relationship between the number of lightning strikes and burnt area. Thus, although the amount of lightning may influence the number of fire starts, it does not determine how much of the area subsequently burns."

Minor comments RESPONSE:We thank the reviewer for pointing out the typo on page 3874 – 6. This has now been corrected to 0.5o COMMENT: 3876 - 13: this is for the tropics at adds with the Hadley circulation which makes for fires burning in the winter. Since the patters look okay in Figure 2 it may be just a typo? RESPONSE: This is not a typo, but we realise that the sentence is less than clearly written. We will change it to read: "The negative signs on the coefficients for the seasonal variables indicate a global tendency for fires to occur in the summer half year in each hemisphere."

COMMENT: 3877-15: 38 g C / m2 / year is extremely low (desert), please look into this. The original graphs used a log transformation and thus we incorrectly reported the value for the peak in the text. This should have been "380 gC/m2". We will correct this in the text and we thank the reviewer for pointing out this mistake.

Short comment W. Knorr

We thank Wolfgang Knorr for his positive and helpful review.

RESPONSE: He points out that although GLMs have not been used at a global scale



before, this approach was used for a regional study by Lehsten et al. (2010) and suggested we acknowledge this. We agree that it is appropriate to cite this earlier regional study, and will add a sentence in the introduction as follows: "We use a generalized linear model (GLM) to relate the fractional burnt area to a series of predictors representing the potential vegetation, climatic and human controls on fire ignition and spread. GLM modelling has previously been used by Lehsten et al. (2010) to model burnt area in Africa."

COMMENT: Mathematical formulae should be added (fitting criterion, how were the points and lines on the partial residual plots calculated?) so that the reader can better understand what was done (for example in a technical annex).

RESPONSE:We will include an Appendix with the fitting criteria and cross-correlation matrix. The partial residual plots were obtained with the "visreg" package in R. They represent data values corrected for the (fitted) effects of all of the other predictor variables apart from the one plotted. As such, they are the closest equivalent to standard scatter plots in bivariate regression.

COMMENT: A table or some other form of summary of the different optimisations and what the input data, output data and observations were that were used to fit the model should be added.

RESPONSE:We included a table giving the source of the input data and the transformations used (Table 1), but unfortunately the ordering of the tables was reversed in the submitted manuscript and thus the table with this information was mis-labelled. We apologise for the confusion this has caused. The output of the GLM is predicted probabilities of burnt area. This should now be clear from the text.

COMMENT: Add a caveat about using the GRUMP data. Upon visual inspection, it becomes evident that the GRUMP data have uniform population density over large areas because the density is based on administrative units, as opposed to the HYDE population data (based on Landscan), where ancillary data were used to re-distribute population along roads and other points (or lines) where people crowd.

RESPONSE:We are aware that the GRUMP dataset is based in administrative units, and that this results in uniform population densities in some areas. The HYDE dataset is also based on population by administrative units, and although this is redistributed using ancillary data, close inspection suggests that it also contains areas with unrealistic uniform population densities as well as having the additional constraint of being to some extent a modelled (and untested) product. We have preferred to stay as close as possible to the observations, and we think that the GRUMP data sets is sufficient in most regions of the world at our scale of analysis. Nevertheless we will add text in the section describing the data sets as follows: "As it has been widely assumed that the number of human-set fires increases as human activities increase (see e.g. Aldersley et al., 2011) we used population density (static) as a general index of human influence. These data were obtained from GRUMP v1.0 (CIESIN, 2005). The GRUMP dataset is based on information of population for administrative units, and the size of these units varies by region. Unlike the HYDE data set (Klein Goldewijk et al., 2011), no attempt is made to use ancillary information to redistribute population within the administrative unit. We have avoided the use of such modeled population data, given that that it is difficult to test the realism of such redistribution. The resolution of the GRUMP data set is sufficient for our analyses over most of the globe, but the large size of some administrative units in e.g. northern North America could lead to slight biases in the assessment of the impact of humans on ignitions."

COMMENT: It is not known how the results would look if alternative indices (e.g. Nesterov index using Tmin for Tdew) had been used.

RESPONSE:Even though we show that area burnt is decreasing with $\alpha$ (plant available moisture), it is true that we have not demonstrated what the results would be using other fire danger indices or replacing the formulations of the Nesterov index. Our point here was that several components of the Nesterov index exert independent controls on fire, and that these components also figure in several alternative fire danger



indices. However, we realise that the original text was not quite clear and we modify the text accordingly: "We have shown that burnt area declines with soil moisture while increasing with all three components of the Nesterov Index, representing time since rain, maximum temperature, and vapour pressure deficit, respectively. Many other fire indices include similar components (see Alexander et al., 1996; Noble et al., 1980; Hardy and Hardy, 2007)."

COMMENT: Improvement of graphics - some are extremely small.

RESPONSE:We apologise for the submitted figures, which as we explained above, were drafts submitted in error. Following the suggestions of referee #1, we have now redrawn all of the figures (including increasing the size) and we believe this considerably improves their readability.

COMMENT: It would have been good if the posterior uncertainties of the optimised parameters were shown.

RESPONSE:We have added these uncertainties to Table 2.

COMMENT: p3867, l24: not sure GAM fits here.

RESPONSE:We will delete the word GAM.

COMMENT: p3871, l16: Better to put : instead of . (the same for l25 etc.)

RESPONSE:We have changed the punctuation as suggested for all the sub-heads.

COMMENT: p3873, l11: I would say fuel moisture behaves similarly to soil moisture, but it certainly does not depend on it when we refer to dead fuel (and most fuel is litter or dry grass).

RESPONSE:We agree that the moisture of dead fuel does not depend on soil moisture, but rather on the drying potential of the atmosphere. This is effectively taken into account by our inclusion of diurnal temperature range as a controlling variable, because diurnal temperature range is a surrogate for the vapour pressure deficit (vpd).

Here we are interested in exploring the impact of soil moisture on live fuel (which can be consumed during fires) and on maintaining living material, because this effectively determines the rate of conversion e.g. of grasses from living to dead fuel and hence could impart a lag in the timing of the fire season. We agree that we have not stated this clearly enough, and have modified the sentence justifying the inclusion of this variable as follows: "the moisture content of living fuel, and the rate of conversion to dead fuel in the case of grasses, is determined by soil moisture, which varies much more slowly through the season than either vapour pressure deficit or fuel moisture because the water-holding capacity of soil allows moisture to be retained for several months."

COMMENT: p3874, l6: o -> degree sign

RESPONSE:We will make this correction

COMMENT: Knorr et al (2013) should now be cited as Knorr, W., T. Kaminski, A. Arneth, and U. Weber (2014), Impact of human population density on fire frequency at the global scale, Biogeosci., 11, 1085-1102.

RESPONSE:We will update the citation. We will also take the opportunity to put in the missing Knorr reference (Knorr and Heimann, 1995).

Anomymous referee #2 Introduction: 1. In lines 5-10 (p. 3867) the authors state that global controls increase wildland fires and attribute it to climate change. However, the cited literature do not present analyses about climate change attribution. This must be corrected.

RESPONSE:We agree that the studies cited have not demonstrated that the recent increases in wildfire are a result of climate change; there has been no formal attribution analysis. Rather these papers point out that the observed recent changes are consistent with what might be expected with climate change and have thus ascribed these changes as the result of climate change. We do not wish to claim that causality has been established, and our point here is rather that the possibility of increasing risk of



fire in the future motivates the search for a better understanding of the controls on fire. We will change the wording of the sentence as follows: "Understanding global controls on fire regimes is of practical importance because the incidence of wildfire is already increasing and many authors have suggested that this could be a response to climate change (e.g. Running, 2006; Westerling et al., 2006; Baltzer et al., 2007; European Environmental Agency, 2012)."

2. Why is the term ''process-based" in parenthesis?

RESPONSE:The name process-based mentioned in quotation marks (not parentheses) because this term is frequently used to distinguish mathematical models which explicitly represent key processes from statistical models. We agree that the quotation marks are unnecessary and we will remove them.

3. The authors should include work from Kloster et al and Li et al. (BG 2014) on fire modelling and future climate applications using the fire component of the CLM model. Also the model approach used in those models to simulate human-caused ignitions must be included in the introduction.

RESPONSE:We were aware of these papers. We did not intend to provide a comprehensive review of fire modelling in the introduction, but rather to cite a few of the more obvious problems and differences in the treatment of specific fire processes in such models. The treatment of human ignitions in CLMfire is essentially the same as in Pechony and Shindell and SPITFIRE. However, we will cite the CLMfire references as requested. We should have cited the reference to the future simulations and will also rectify this and add: "simulations with fire-enabled dynamic global vegetation models (DGVMs) have shown a decrease (Scholze et al., 2006), an increase (Kloster et al., 2012) or no change (Harrison et al., 2010)."

Data and Methods 4. Why are only 5 years of burnt area data used in the statistical analysis? This is a very short time frame to conclude about the climatic influence.

RESPONSE:There were two reasons for confining our attention to this period. We did not want to use the less reliable pre-MODIS (i.e. pre-2000) results. We were also constrained by the lack of data on e.g. population after 2005. Thus, the period 2000-2005 represents the time when we had reliable information about our predictors and burnt area. but also because for the MODIS era, we did not have all the available data. We stated the first constraint in the original manuscript (3871 – 6-9), ''We confined our attention to the post-2000 period, for which the GFED data were derived by combining MODIS satellite observations with biome-dependent modelling of the relationship between burnt area and observed fires." However, we did not make the second constraint clear. Given that we are using spatial patterns in monthly burnt area and their relationship with controlling variables, the 72 months of data provide enough information. We will modify the text to make these two points clearer as follows: "We confined our attention to the post-2000 period, for which the GFED data were derived by combining MODIS satellite observations with biome-dependent modelling of the relationship between burnt area and observed fires. We do not analyse the period post-2005 because some of the required ancillary data sets were lacking. However, since we are considering spatial relationships between burnt area and the predictor variables using monthly burnt area, this 72 month time period is adequate to diagnose the key relationships."

5. The authors used the GFED3 product to analyse burnt area data. Why was not the GFED4 product used? Have burnt area attributed to deforestation & degradation and agricultural waste burning been eliminated from the analyses? IF not, please explain why?

RESPONSE:The GFED4 product was not available when these analyses were made. We have now re-run the analyses using GFED4 (aggregated to 0.5o) and the GLM results are not significantly changed. We have added these results to Table 2, for comparison. We will add a sentence in the methods section to make this clear, as follows: "(The fourth version of the GFED data became available after our analyses were completed; we have checked that the results do not change materially when



the newer data set is used.)" We will also add a sentence in the results section, to point out that the choice of GFED3 or GFED does not affect our conclusions about the nature and strength of the relationships, as follows: "Analyses using the GFED4 database (Giglio e al., 2013) show that the relationships are not affected by the choice of burnt area data set." We use the full dataset at 0.5o resolution; we did not screen out deforestation fires or agricultural burning because of difficulties in identifying such fires unambiguously and because the burnt area associated with these anthropogenic fires is still controlled to some extent by climatic factors. Given that the reviewer assumed that we might have screened the data, we propose to add text to emphasise this is not the case and to explain our motivation for not working with screened data as follows: "We made no attempt to screen the data for deforestation or agricultural fires, in part because of the difficulty of unambiguously identifying such fires and in part because it is clear that the extent and timing of deforestation and agricultural fires is influenced by climatic factors (e.g. van der Werf et al., 2010)."

6. On page 3873, l. 3-4, the authors take the maximum daily temperature as a surrogate that there will be enough energy to heat the fuel particle to the ignition temperature. I think this assumption implies an error because it ignores the energy of the ignition source. Air temperature can only lead in extreme circumstances to self-inflammation which is usually supported by the content of volatile substances in the fuel. Please correct your assumption to what the use of Tmax actually means in the text.

RESPONSE:We did not mean to imply that the maximum temperature determined the energy needed to heat material to ignition point. Rather, and as we go on to say in the next sentence diurnal temperature range and maximum daily temperature determine flammability on a given day. We realise that our current text is ambiguous and we will therefore delete the sentence "Maximum temperature determines whether an ignition event will heat the fuel enough to start a fire."

7. Building a simple soil moisture model is interesting, but the underlying assumptions

require a sensitivity study as to what extent the data and equations used in the model influence the calculated soil moisture.

RESPONSE:We wanted to include soil moisture in our analyses because the moisture content of live fuel is related to soil moisture, and because the availability of soil moisture determines when grasses die and thus the seasonal availability of dead grass fuel. However, there is no universally accepted soil moisture dataset and indeed modelling soil moisture in detail is a highly complex topic. Hence we used instead an estimate of the ratio of actual to equilibrium evapotranspiration based on a standard soil-moisture accounting algorithm. This ratio is widely used in the literature as a measure of plant-available water. We describe the derivation of this ratio following the standard references. We will modify the text in order to make the argumentation clearer, as follows: "There is no generaly accepted global data set of soil moisture and there are differences among various available modelled products. We used instead a standard estimate of the ratio of actual to equilibrium evapotranspiration ($\alpha$), widely used as an index of plant-available moisture (Prentice et al., 1993), as a surrogate for soil moisture. This index is calculated from the CRU TS3.1 climate data as described in Gallego-Sala et al. (2010)."

8. Paragraph starting in line28 (p.3873) can be merged with the above paragraph.

RESPONSE:We will merge these paragraphs.

9. The description of the predicted global fire distribution (seasonality maps) in Fig. 2 must be expanded and describe where results are different from the observations and discuss later in the discussion section what are the possible reasons for the model errors. Presenting a R2 for each season map is not sufficient. In addition, it must be explained why the R2 in MAM and SON is so low? Is this related to the simple soil moisture model? How is this behaving in seasonal dry areas and classical grazing areas? Isn't the latter reducing fuel load, thus observed area burnt?

RESPONSE:The key results of this paper are the individual relationships between



burnt area and specific controls, and these maps are only meant to be illustrative of the fact that the final model produces are reasonable prediction of seasonal and spatial patterns. We did not expect, nor is it reasonable, that the GLM will predict burnt area in a specific year exactly. Nevertheless, we have expanded the description of these results as requested by Reviewer 1 (see response above). It is possible that the lower R2 valuesin MAM and SON reflect the fact that these seasons are preferred timing for fires associated with land management. We have no particular reason to attribute this to the use of $\alpha$ as an index of soil moisture, but in any case, such attribution is beyond the scope of our analysis.

10. P. 3878, l. 10-20. Discuss all published model approaches regarding human-caused ignitions, see e.g. Kloster et al. (2010, 2012) and Thonicke et al. 2010. Also make clear in the text that the drivers of fire ignition differ from factors determining area burnt. This is what your objective of this study is, but it requires a clearly written text to make this understandable.

RESPONSE:The purpose of this paper is not to review the various ways in which modellers have parameterised anthropogenic ignitions. We have cited a series of modelling papers in the introduction (incuding those by Kloster et al and Thonicke et al.) because the erroneous treatment of human ignitions in many models provided one of the motivations for our analyses. The paragraph identified by the reviewer describes results of analyses of observations which have shown the unimodal relationship between population density and burnt area – a relationship which we can now explain as a non-causal artefact that emerges from correlations between population density and other controlling variables, specifically the fact that in general areas of low NPP (and hence low fuel loads) cannot support large populations and that areas that have high NPP, and can thus support dense populations are by their very nature areas with high rainfall and thus too wet to burn. In response to the reviewer's comment, we propose to modify the opening sentence of this paragraph to emphasise the fact that the papers we cite are based on analyses of observations at a variety of different scales, as follows: "A number of analytical studies of both regional and global datasets have shown a unimodal relationship between burnt area and human population density, when log-transformed to emphasize the form of the relationship at very low population densities (Archibald et al., 2009; Aldersley et al., 2011; Bistinas et al., 2013)." We agree that the distinction between controls on burnt area and controls on e.g. number of fires needs to be emphasised. We do not think this paragraph is the appropriate place to deal with this, but will emphasise the distinction in response to the next comment.

Discussion

11. The argumentation of the first paragraph in the discussion is not clearly written. Please revise what you wanted to express here.

RESPONSE:We agree that this paragraph was written in an abbreviated form and that we should make the argument clearer. We will revise it as follows: "We have focused on analysing plausible mechanistic controls of burnt area, which is the aspect of the fire regime most directly relevant to vegetation disturbance and the carbon cycle, and have demonstrated that there are strong, monotonic relationships with NPP and climate variables related to the Nesterov Index. At the same time, we have shown that there is no significant relationship between burnt area and either anthropogenic or lightning ignitions. This latter finding may appear surprising, and runs counter to the assumption currently adopted in many fire models that burnt area is related to the number of fire starts. Knowing that people start fires, some model developers have focused on human ignitions as a primary determinant of burnt area (e.g. Thonicke et al., 2010). In a similar way, because observations indicate that large fires in e.g. boreal ecosystems are started by lightning and that the number of fire starts increases with convective activity and number of strikes (e.g. Peterson et al., 2010), it is frequently assumed that increasing the number of lightning ignitions will lead to an increase in burnt area (e.g. Pfeiffer et al., 2013). However, the controls on burnt area are not necessarily the same as the controls on other aspects of the fire regime, especially fire numbers. Fire size is extremely skewed, and large fires only occur under weather conditions suitable to



fuel production and rapid fire spread. Thus, and as demonstrated here, burnt area is determined not by the number of fire starts but by vegetation productivity and climate controls on drying. Confusion about the distinction between the controls on burnt area and on fire starts is rife in the literature. Part of this confusion may stem from the fact that remotely sensed data on active fire counts have been available for longer than burnt area products. Indeed, pre-2000 burnt area products (including the early years covered by GFED) strongly depend on active fire counts (Giglio et al., 2010). By using post-2000 burnt area data, we avoid this bias. "

12. P. 3879, from line 22, the point discussed here is not new in fire modelling science. The reason presented here is exactly why in the past modellers working on fire risk and fire regimes have introduced fuel classes. You need to reflect on this literature and relate your outcome to this.

RESPONSE:We acknowledge that the introduction of fuel classes in fire risk or mechanistic fire models reflects the recognition of the need to take into account differences in fuel drying times, but nevertheless such treatments do not account for the timing of conversion between live and dry fuels – which is the point we are making here with respect to the independent control exerted by alpha. We have therefore made no change to the paragraph.

13. P. 3880, l. 13-19. Human population has been related to fire suppression in fire modelling approaches before. Please analyse respective literature and include it in your argumentation. RESPONSE:We agree that the text here is abbreviated, and will expand this to discuss both the observational evidence for fire suppression and by adding a paragraph describing the modelling approaches that have been used to mimic fire suppression by humans as follows: "Human population density shows a strong and consistently negative relationship to burnt area, which is significant despite the separate inclusion of cropland and grazing land area. The role of people in suppressing fire has been identified through previous analyses of observations on biomass burning on recent, historic and palaeo-timescales (e.g. Krawchuk and Moritz, 2009; Carcail-

let et al., 2009; Marlon et al., 2008), and attributed in part to direct intervention but largely as a result of landscape fragmentation and fuel reduction. Given the independent relationships with cropland and grazing area – terms which already incorporate some aspects of landscape fragmentation or fuel removal – our analyses indicate that other human activities are also important in suppressing fires and thus limiting the area and biomass burnt. Possible mechanisms include the removal of wood for heating and cooking, roads and clearings creating barriers to fire spread, and fragmentation through urbanization. It is likely that the mechanisms are different at different levels of population density and in different regions (Bistinas et al., 2013; Knorr et al., 2014). Many modeling groups have included some form of fire suppression by humans. The most common approach is to mask fire in cropland (e.g. Thonicke et al., 2010; Prentice et al., 2011; Kelley et al., 2014) or as a universal (e.g. Pechony and Shindell, 2009; Kloster et al., 2010; Li et al., 2012) or spatially variable (tuned) function of population density (Thonicke et al., 2010), or by reducing fuel loads in grazing areas (Krinner et al., 2005). Clearly, given the independent relationships with cropland area and human population density, the reliance on crop masking is insufficient. The reduction of fuel loads in grazing land is not supported by the positive relationship between burnt area and grazing land area. Suppression of fire as a function of population density is consistent with our findings, but should be applied even at low population densities. Thus, models incorporating algorithms that increase fire as a result of increasing population density and then subsequently allow for fire suppression at higher population densities are not mechanistically consistent with the observed relationships. "

14. P.3881, this part of the discussion is not well written and arguments are not clearly developed. In my opinion, the evidence presented here does not convince why previous model assumptions have to be corrected, since the model assumption have not been discussed. So the reader does not know what should be corrected.

RESPONSE:This section was meant to be a summary of the implications of issues already discussed with regard to modelling, but this may not have been clear because



of the formatting of the text, which should have appeared as bullets, as separate paragraphs. We propose to reinstate the bulleted summary form. As we have now added an explicit paragraph on modelling approaches to ignitions and fire suppression (see response above), this format should make it clear exactly what modelling assumptions need to be addressed.

15. What are the other effects that counteract the acceleration of burnt area with increasing fire ignitions? The way it is presented here, it reads like a misconception. Also, existing fire models do take into account that the number of potential human-caused ignitions decreases with increasing human population density.

RESPONSE:We do indeed believe that the idea that increasing ignitions leads to an increase in burnt area is a misconception. We will remove the sentence about human ignitions, because it was poorly written and implied that there might be an effect of number of human set fires and burnt area, which is not supported by our analyses. We will add text at the end of the paragraph to make the relationship between human ignitions and burnt area, and between population density and fire suppression, and to list some of the possible effects that lead to decreased burnt area with human population, as follows: "We have shown that apparent increase in burnt area with increasing population density at low population densities is an artefact of the relationship between NPP and population density. Given that models already incorporate the effect of fuel limitation on burning, including a second constraint through population density will lead to erroneous predictions. The strong negative relationship between population density and burnt area, which is independent of constraints on burnt area due to land use, implies that it is important to include fire suppression in a modeling framework. In addition to fire-management activities, fire suppression could be a result of non-agricultural landscape fragmentation, creation of artificial barriers to fire spread, and removal of fuel for domestic use. Given that the causes of fire suppression are likely to vary spatially and with population density and cultural factors, further analyses of this phenomenon would be worthwhile."

Short comment V. Lehsten

COMMENT: The authors claim the novelty of their approach, but the same has been done (actually with even very related data) for the African Continent so the results should be similar (Lehsten et al 2009, this Journal).

RESPONSE:There are several distinctions between our study and that of Lehsten et al., most specifically the fact that our analyses are global. Our study predicts monthly fire frequency based on the total data set, i.e. both burnt and non-burnt pixels. In addition, we show partial residual plots (which graphically demonstrate the goodness-of-fit of the relationships to each predictor variable); and we demonstrate how unimodal responses of area burnt to various predictors can result from combined, monotonic effects of more than one predictor. Thus our work has a broader scope than the analyses published by Lehsten et al. Nevertheless, we were remiss in not citing this study, and have now done so (see response to the review by Wolfgang Knorr, above).

COMMENT: The Nesterov index sums up a drought unit (based on temperature and dewpoint temperature) and goes to zero once the precipitation is above 3 mm, at least this is the definition that is given in the stated reference. The dataset used by the authors does not allow to calculate this original index. So the authors call it the simplified Nesterov which is fine. The use of diurnal temperature range as an estimate for the difference between dewpoint temperature and average temperature assumes that the air is saturated at night. This has been demonstrated to work in mountainous areas but is not the case in most fire prone regions at the time that the fire occurs. Just take any daily climate data set that contains these parameters and check yourself by using this proxy versus the real value. I also thought first it is a valud idea but it does not work at all, you get differences in orders of magnitude. Using monthly data to calculate an index which goes to zero in case of rain event of 3 mm per day is somewhat unsuitable I think how do you assess the temporal distribution of the rain? The monthly precipitation and the number of dry days does not allow this, except if you assume that all rain days in the month have the same precipitation.



RESPONSE:We are surprised by the reviewer's mention of “orders of magnitude” difference between vpd estimated from temperature range, and “real” vpd values (presumably, values conventionally derived from relative humidity and temperature measurements). In other work (with H. Wang and B.J. Evans, unpublished results) we have compared the two methods globally. We obtained a correlation better than 0.9 and did not find extreme outliers from the correlation. We have also shown (with D. Sahagian, unpublished results) the existence of a precise relationship between monthly dry-day frequency and the expected value of the number of days since a rainfall event. This relationship was derived using a well-established distributional assumption, used in many weather generators, about the persistence of wet versus dry conditions. Thus, the number of dry days in the month is a good statistical indicator of the time elapsed for fuel drying. Additional assumptions would be needed in order to provide a temporal distribution of the amount of precipitation among rain events, but this information is not required for the calculation of the Nesterov index. Both topics are beyond the scope of this paper, but are the subject of papers in preparation.

COMMENT: Spatial autocorrelation and colinarity of input variables. The statement that the correlation between input data is not affecting the significance of the results is simply wrong. It is however true that the estimated parameters are only minor affected. If you estimate a GLM with highly correlated input data, the R-square will be strongly affected and you will overestimate the significance of the model.

RESPONSE:First, we do not account at all for spatial auto-correlation, and with good reason: we assume that each data point represents an independent realization of the underlying process that generates burnt area, and this is a reasonable assumption, because it is extremely rare for fires to burn an area corresponding to several adjacent 0.5ËŽ grid cells. It is important to remove the effects of spatial autocorrelation when the data values at one point are influenced by the data values at adjacent points, but this is not the case in our study. Second, we certainly agree that correlations among predictor variables tend to reduce the significance of a fitted model. Our text does not say anything contrary to this. Nonetheless, a GLM is not invalidated by the presence of moderate correlations among predictors, and the significance values assigned to individual predictors have a clear interpretation.

COMMENT: The authors do not state how they found their final model. I assume that they used a forward step procedure, since they used the R-package. In this case it has been show that the order in which the variables enter the analysis strongly influences the result especially when the input variables are highly correlated. /Burnham & Anderson, Behavioral Ecology and Sociobiology 65:23–35; Whittingham et al. 2006. Journal of Animal Ecology 75:1182–1189). The R value as such is actually not usefull to state whether a model is significant. That has to be done using a p-value.

RESPONSE:We add the cross-correlation matrix between the dependent variables for information. The variables are not highly correlated. Contrary to this reviewer's assumption we did not use a forward stepwise procedure. The significance levels of the variables rely on the p-values, as they should, and not on the r-values (''two-tailed p-value corresponding to the t or z ratio based on a Student t or Normal reference distribution": see summary.glm in the ''stats" package in R). As we stated in the manuscript 3874 21: ''Correlation among predictor variables increases the sample size required to achieve statistical significance (Maxwell, 2000), but does not compromise the validity of the regression coefficients or their estimated significance levels. By defining the combinations of predictors a priori we avoided the bias towards significance that is characteristic of stepwise methods (Cohen et al., 2003)."

COMMENT: One of my main criticism is the final result which the authors claim to have found: "most notably, the widely assumed dependence of fire frequency on ignition rates – are evidently incorrect." With the data that they used, it is not surprising that they first found a negative relationship. The authors used the OTD lightning seasonal data. This data is highly interpolated in time. If you look at a single pixel you often see a sinus curve with the maximum at the summer or rain season. This type of data is not representing the lightning that actually causes the fire. These flashes are so



called dry lightning. They are only comprising a minor proportion of the total number of flashes, they will not be apparent in any seasonal product which is averaged over many years. While the independence of the fire on lightning in the tropics might be true since there most fires are caused by humans it is certainly not the case in the boreal region. Though even here I would argue that the statement as "the widely assumed dependence of fire frequency on ignition rates – are evidently incorrect." Since humans cause the ignitions there. If the authors would have found that fires in the world (and hence also in boreal regions) are independent of ignitions that would be very interesting, but neither the used data, nor the methods used allows such a statement.

RESPONSE:Following the reviewer's reasoning here, we re-ran the analysis by using only the cloud-to-ground fraction of the lightning dataset according to Thonicke et al. (2010), and excluding ''wet" lightning as defined by Kelley et al. (2014). Dry lightning flashes constitute only 18% of the total flashes. There were no changes of any importance; and after including seasonality variables the result was exactly the same as in the main analysis, i.e. lightning became non-significant.

Interactive comment on Biogeosciences Discuss., 11, 3865, 2014.