Biogeosciences Discuss., 10, C8009–C8016, 2014
www.biogeosciences-discuss.net/10/C8009/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Impact of human population density on fire frequency at the global scale" *by* W. Knorr et al.

W. Knorr et al.

wolfgang.knorr@gmail.com

Received and published: 17 January 2014

Authors' response in normal text, referees' comments in *italics*.

The authors could go further in drawing out the implications of the study. The Conclusions, for example, are too brief. The results have implications that are not spelled out. For example, even though it was not the main focus of the MS, the results do confirm the profound influence of climate on fire frequency.

We agree: if humans have a rather small and generally negative impact on fire frequency, then historical fluctuations in this quantity as for example shown in the charcoal record are most likely due to climate. The current text alludes to this in Section 4 for the increase in fire activity prior to the late 19th century decline (p. 15756, line 23).

We have also added a statement related to future changes in fire frequency to Section 4 after the place mentioned:

“Because the inferred impact of population density is small compared to the large historical changes in population density, we can expect that future climate change will have a major impact on fire frequency, even with further substantial changes in human population.”

We now also pick up on this point in Section 5 (Conclusions), as suggested by the referee.

The negative effect of population also has important practical implications. It points to a direct conflict between land management policies (for conservation and for safety). But it also suggests that the likely effect of global warming in increasing fire frequency in many regions doesn’t have to be passively accepted, and that predictions of future fire need to take regional demographic trends into account.

We find the point about fire management is well taken, even though we had originally considered it out of scope for this global-scale analysis. We thank the reviewer for this valuable comment and have appended a short paragraph to the end of Section 5 (Conclusions). We did not include a reference to practical fire management because we believed that the results are insufficient to fully support those arguments:

“This has consequences for the way we perceive the problem of landscape fires. For example, future climate change does not necessarily need to lead to increased fire risk because of the multitude of negative impacts from human activities. Also, models aimed at simulating future fire risk should take into account both climate and demographic variables. While the exact mechanisms still need to explored, such models should allow for the existence of ignition-saturated fire regimes.”

A key article that should be cited in the first paragraph is:

Harrison SP, Marlon JR, Bartlein PJ (2010) Fire in the Earth System. In: Changing

BGD

10, C8009–C8016, 2014

Climates, Earth Systems and Society. International Year of Planet Earth, pp 21-48. Springer.

This deals with many of the issues addressed in the MS, as well as the mythology of pyrogeography.

We have added this key reference to Section 1 (Introduction), first paragraph.

The third paragraph of the Introduction starts by describing a "frequently observed" pattern (of increase in fire frequency with human population at very low population densities), but goes on to cite several references where such a pattern is not seen! I suggest, therefore, that this phrase be replaced with "sometimes reported". I suspect that some such reports are artefacts due to the coincidence of low human population with deserts where there is nothing to burn.

It appears that the referee has overlooked the word “fire density” in this paragraph – these patterns were indeed observed for numbers of fires per area, not burned area per surface area (fire frequency), and we could cite more such examples. To avoid this misunderstanding in the future, we have added “ – as opposed to fire frequency – “.

The fifth paragraph of the Introduction mentions relationships of fire frequency with GDP per area. However, to my knowledge, a credible global map showing GDP on an areal basis does not exist. There is a map available which proves, on analysis, to have been derived as the product of GDP per capita on a large-area basis (large political entities) with population density. No map to my knowledge reflects e.g. the enormous disparities in GDP per capita within the less populous regions of Australia or Canada. Thus, I suggest not referring to the influence of GDP on fire regime, as it cannot currently be demonstrated.

The corresponding reference indeed refers to GDP per km2, but it is not essential to this discussion. In order to avoid raising further questions, we have elected to delete the following text: “, or found that higher human population density or gross domestic

BGD

10, C8009–C8016, 2014

product per area are associated with a more regular temporal pattern of fire density at the interannual time scale (Chuvieco et al., 2008)."

The Introduction should make clear that the patterns analysed are multi-annual, i.e. that the study does not attempt to analyse interannual variability or seasonal timing of fire.

Good point, even though the optimisation takes interannual changes of fire frequency into account. We have added a sentence at the end of Section 1 sketching briefly the general method, while stating that the purpose of the study is the analysis of spatial patterns, even though interannual fluctuations are being taken into account during parameter estimation:

"We will address these questions based on an analysis of both observed and modelled spatial patterns of fire frequency at the global scale, while developing and optimising a model of fire frequency at annual time steps."

The last sentence of the chapeau to "Methods" does not make sense. Please reword.

We have modified the following sentence: "Fire frequency is therefore assumed proportional to the product of two factors, one of which becomes zero when zero fuel load is indicated, the other, an indicator of fire risk, when the indicated risk approaches zero." It now reads: "Fire frequency is therefore assumed proportional to the product of two factors, where the first is a function of some quantity approximating fuel continuity and load, the other a function of some indicator of fire risk. The functions are formulated such that the first becomes zero at zero fuel load, the second when the fire risk is equivalent to zero."

A reference or URL is needed for the WATCH data set.

The WATCH interim data used here have not been published anywhere and there is no permanent URL available. However, we have added a reference for the original WATCH data at the beginning of the 2nd paragraph of Section 2.1, 1st sentence:

BGD

10, C8009–C8016, 2014

“As climate data we use the Max-Planck-Institute for Biogeochemistry WATCH ERA Interim daily global climate product at 0.5° by 0.5° resolution for 1999 to 2010, produced according to the method by Weedon et al. (2011).”

Weedon, G. P., S. Gomes, P. Viterbo, W. J. Shuttleworth, E. Blyth, H. Osterle, J. C. Adam, N. Bellouin, O. Boucher, and M. Best (2011), Creation of the WATCH Forcing Data and Its Use to Assess Global and Regional Reference Crop Evaporation over Land during the Twentieth Century, J Hydrometeorol, 12(5), 823-848.

In introducing the Nesterov index, it should be explained why temperature range (from a mechanistic point of view) is an appropriate quantity to include in a prediction of the drying rate of fuel – that is, its strong relationship to vapour pressure deficit.

Thank you the suggestion. We have modified the following statement (p. 15741, line 4): “The daily temperature range plus 4 âŮęC is an approximate indicator of dryness because of the strong relationship between the diurnal temperature range and atmospheric humidity.”

The beginning of 2.4 refers to Marlon et al. (2008). Another key reference here is:

Z. Wang, J. Chappellaz, K. Park, J. E. Mak. Large Variations in Southern Hemisphere Biomass Burning During the Last 650 Years. Science, 2010; DOI: 10.1126/science.1197257

This reference uses independent measurements (CO isotopes from Antarctic ice) to show that the patterns shown in charcoal records by Marlon et al. (2008) are real, and in particular that the present-day pyrogenic CO source is lower than at any time during the past 650 years.

Section 3.5 puts a number on the decline of fire frequency since 1800 (14%) and this is also cited in the Abstract. The Marlon et al. (2008) data do not quantify the magnitude of the decline, so it isn’t possible to make a quantitative comparison. However, first-order estimates could be obtained from the Wang et al. (2010) study mentioned *above, or from the published records of $\delta 13C$ in methane from ice cores. This comparison should be made. Without having yet made these calculations, I suspect that the estimate 14% may be on the low side. If this proves to be so then there should be some comment on why the magnitude might be under-estimated.*

We are grateful for the encouragement to broaden the discussion of the wider implication of the results. In addition to Wang et al. (2010), we now also include Ferretti et al. (2005), who use carbon isotopes in methane from ice cores, as well as van der Werf et al. (2013), who provide a model perspective on the plausibility of the inferences by the other studies. We have added a discussion of those issues after the current 3rd paragraph of Section 4.

"Extrapolation of our results back in time yields an estimate of 14% for the decline in burned area since 1800, or about the same since the late 19th century. While Marlon et al. (2008) do not put any numbers on their observed changes in fire frequency, inferred changes in biomass-burning emissions of based on carbon-13 isotope measurements from Antarctic ice cores and mass balance calculations can be used as a qualitative proxy. Of these, methane isotope data indicate an approximately two-fold increase since 1800 (Ferretti et al., 2005), but carbon monoxide (CO) data a 70% decline since the late 1800s (Wang et al., 2010). A further constraint is given by van der Werf et al. (2013), who used bottom-up calculations and atmospheric transport modelling to conclude that the strong decline in emissions reported by Wang et al. (2010) is difficult to reconcile with what we know about emission sources, and that emissions were likely not as high during historical periods. From this we conclude that a moderate decline in fire frequency and emissions as suggested by this study is in general agreement with other studies."

Ferretti, D. F., et al. (2005), Unexpected changes to the global methane budget over the past 2000 years, Science, 309, 1714-1717.

van der Werf, G. R., W. Peters, T. T. van Leeuwen, and L. Giglio (2013), What could

have caused pre-industrial biomass burning emissions to exceed current rates?, Clim. Past, 9, 289-306.

The fourth paragraph of the Discussion addresses problems with the way in which human population effects are represented in global models. I would like to see a stronger statement here. Venevsky et al. introduced the concept of the propensity of humans to start fires (number of fires started per person per day), which cascades through the model in such a way that the burned area ends up being proportional to the product of this term with population density. This approach has been adopted with modifications in some other models (including one of which I am a co-author). The opportunity should now be taken to say that this concept should be abandoned.

Thank you for the encouragement. We had a closer look at the four fire models cited in the Discussion and added the following text after the first sentence of the 4th paragraph of the Discussion:

“In all four models cited, fire suppression is modelled exclusively via reduction of the number of ignitions at high human population density. There is no consideration allowing human population density to influence the average burned area per fire, and thus – by design – no possibility of a fire-saturated regime, where the number of ignitions can increase with no impact on the total area burned (but an increase in number of fires).”

The seventh paragraph of the Discussion concludes that the separate representation of human fire ignitions and fire suppression in models is not "necessary". I would say that it is not "necessary or justified".

We believe the reviewer here refers to the sixth paragraph. This is the place where we have now added further suggestions on how fire frequency should be modelled in more mechanistic models:

“Alternatively, more mechanistic models should not only consider the impact of humans on the density of successful ignitions, but also on fire spread, possibly by describing fire as a process with multiple limiting factors (such as fuel availability, fuel moisture, fuel connectedness and ignitions)."

Technical correction – Penultimate paragraph of the Discussion: challange => challenge.

Done, thank you for spotting this.

Interactive comment on Biogeosciences Discuss., 10, 15735, 2013.