

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

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General comments

I know of no scientific field that is quite so replete with unsubstantiated myths as pyrogeography. It is therefore most welcome to see an objective analysis based on data sets that have become available during the past decade, recording the spatiotemporal patterns of fire at a global scale. This MS is particularly welcome because it tackles head-on one of the most persistent of these myths, namely that the probability of fire increases as a result of human activity. Knorr et al. demonstrate not only that this is false, but also that it is at 180 degrees to reality.

The emphasis on fire frequency (as expressed in burnt area) as the key quantity to be

C7230

predicted is well justified in the Introduction. This is important. Much confusion has arisen between fire frequency and density, which may have helped cause the persistence of the myth that "fire increases with human population density".

The MS is concise and readable, and the methods well explained and sound. My comments are therefore relatively minor.

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. 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.

Specific comments

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 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.

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

C7231

with deserts where there is nothing to burn.

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 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.

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

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.

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

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.

C7232

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 $\delta^{13}\text{C}$ 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.

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.

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".

Technical correction

Penultimate paragraph of the Discussion: challange => challenge.

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C7233