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

W. Knorr

wolfgang.knorr@gmail.com

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OVERALL IMPRESSION:

This is a welcome advance towards general understanding of the drivers of wildland fires at the global scale. For example, it is another study that demonstrates that ignitions are just one of many factors that facilitate fires. This is important, because all currently available process-based global fire model put the act of fire ignition straight at the centre of their modelling philosophy. However, as this study again demonstrates, number of ignitions in most fire regimes has only a negligible impact on fire frequency (even if it has on fire density, i.e. number of fires per area and year).

PRO:

Well written and mostly clear.

Significant advance in understanding - not many studis of this kind have been carried out at the global scale (Krawchuk et al. 2009 refers to fire density, not fire frequenc, and only used one "human footprint" measure; Moritz et al. 2012 focusses on climatic and vegetation factors; Knorr et al. 2014 does not include land use explicitly, only via population density), and no global study as used GLMs (but Lehsten, V., P. Harmand, I. Palumbo, and A. Arneth, Modelling burned area in Africa, Biogeosciences, 7(10), 3199-3214, 2010 used GLMs and should be acknowledged). The study provides further indication that the ignition-centred approach used in all process-based fire models to-date leads to incorrect results and is generally misleading - ignition is just one of several factors, and it may be the factor that is "easiest" supplied, and therefore does not play a very big role in controlling fire frequency.

Concept of emergent properties interesting, and the results are intuitively interesting (which is always a good thing).

CON:

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).

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.

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.

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

Improvement of graphics - some are extremely small.

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

MINOR POINTS:

p3867l24: not sure GAM fits here p3871l16: Better to put : instead of . (the same for l25 etc.) p3873l11: 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). p3874l6: o -> degree sign

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.

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