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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Relationships between photosynthesis and formaldehyde as a probe of isoprene emission" *by* Y. Zheng et al.

Anonymous Referee #1

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Zheng et al. present a study that attempts to link large-scale observations and simulations of isoprene emissions and concentrations. They do so with a regression analysis between observation-derived estimates of GPP and formaldehyde variability and their climatic drivers (as well as regression between the GPP and formaldehyde estimates), which are compared with the respective regressions obtained from an Earth system model, and with the respective regressions to simulated isoprene emissions.

The study is a brave attempt to overcome one of the largest problems in isoprene chemistry modelling: The lack of constraints from large-scale observations on the magnitude and spatial patterns of isoprene emissions (or concentrations). Although the results obtained in the study give little confidence in the possibility to obtain such constraints from observations, the attempt is worth documenting and can provide insight for many

researchers in the fields of emission modelling and atmospheric chemistry modelling. As such, I think that this manuscript should be published.

However, I have a major concern that should be addressed prior to publication. The two data sets used (global estimates of gross primary production derived from upscaled eddy covariance measurements of CO2 exchange and variability of column-integrated formaldehyde content obtained from satellite observations) bear large uncertainties in their estimates, and so do the isoprene models, which is illustrated e.g. by the large spread in global isoprene emissions obtained from the three models used. These uncertainties are mentioned in the manuscript, but I think that they should be acknowledged more concretely in the methods used for the analysis. Specifically, the comparison between observations and models is performed very indirectly, by comparing correlations between the drivers and the isoprene proxies for the models and the observations separately, which is probably a result of the abovementioned uncertainties. I think the authors should discuss why they have chosen to do this rather than comparing simulated GPP or formaldehyde concentrations directly with the observations. Maybe such an attempt could be added as an illustration to show the large discrepancies in absolute amounts between models and observations.

All in all, I think that this paper, despite the lack of a strong conclusion with regard to the applicability of large-scale data sets for constraining isoprene emissions or concentrations, should be published after accounting for this, as well as for the remarks below:

Major remarks:

I consider it misleading to present the GPP product as a "global observation" (11765/22): It is essentially an empirical upscaling from a large, but geographically unevenly distributed, set of CO2 exchange-derived GPP estimates. Although the product is indeed often presented as a global one, there are large regions (notably the tropics and subtropics) for which it is poorly constrained with observations. The nature

of this product, which uses amongst others climatic parameters to do the upscaling, differs fundamentally from the satellite-derived HCHO observations. It is no surprise that the product results in good partial regression coefficients (11775/4): the climatic parameters used to determine the regression are an important subset of the parameters used to perform the upscaling. I suggest that the authors highlight this difference when discussing the results of the regression and account for it in their interpretation.

The regression analysis is consequently shown for three rather than four seasons. Why do you ignore DJF in your analysis?

In the description of the regression performed, it should be noted that all regressions are temporal, and that spatial regression is not attempted here (if my interpretation is correct). As the regressions are performed on standardized variables (expressed in units of standard deviations), it is the temporal variability that is addressed. I do not understand why the authors express this explicitly by referring to HCHO variability as "HCHOv", but do not use "GPPv" for GPP variability.

The application of a soil moisture dependence in the models is presented as a crucial aspect to get correct large-scale estimates, but the study does not prove that such a dependence is necessary for obtaining the right response - the improved correlation may be the result of other differences between these algorithms (e.g., the soil moisture-independent algorithm Y-MEGAN obtains considerably higher isoprene emissions). The improved correlation does not necessarily point at a causal effect, and interpretation of this should be done with care. Also, the discussion of water stress effects on isoprene is rather concise: There is ample literature on these effects at smaller scales, which could be used here to support the conclusions regarding soil moisture.

Minor remarks:

11765/15: Please define "short-term" and "long-term"

11769/27: Is the PFT-specific LAI a value that is globally applicable, or do you deter-

mine it for each grid cell separately?

11770/3: It seems somewhat crude to refer to the Farquhar model as a Michaelis-Menten model: It bears important elements from Michaelis-Menten kinetics to determine e.g. temperature dependences of the involved enzymes, but the model contains many important elements that are not related to Michaelis-Menten kinetics.

11771/17: The semicolon at the end of the line is not appropriate here, as there is a new part of the equation starting here. Please add a few words to introduce the empirical parameters C_T and C_L.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11763, 2015.