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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of MEGAN-CLM parameter sensitivity to predictions of isoprene emissions from an Amazonian rainforest" *by* J. A. Holm et al.

Anonymous Referee #1

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The goal of the study of Holmes et al. is to explore the sensitivity of the MEGAN-CLM model to its different drivers and to evaluate the capacity of the model not only to reproduce isoprene emissions but also to reproduce the leaf temperature (Tleaf) and the photosynthetic active radiation (PAR), two key drivers of isoprene emission.

If I have no issue with the general goal of the study, I found the methodology followed by the authors to get their results extremely confusing if not completely wrong. The main issues I have being:

1) The sensitivity analysis. The procedure followed to determine the influence of the

model parameters on isoprene emissions is rather unclear. §2.2 'A Monte Carlo simulation was conducted for varying all 14 of the non-constant uncertain parameters used in MEGAN-CLM for a combined output uncertainty. Next, additional Monte Carlo simulations were conducted which varied each of the 19 parameters one-at-a-time, while all other parameters were held constant.'

Were the Monte Carlo simulations conducted varying 14 or 19 parameters? How did the authors decide to vary the parameters? What is the range used and how is it defined? In the 'one-at-a-time' experiences, to what values the other parameters were held constant? The methodology is vague and confusing. As an example, Figure 3 shows the top 10 variables contributing to most of the variability in the MEGAN-CLM model. Three of these parameters are defined in the paper as model constants (CCE, CT1, CT2). From the methods, it seems that indeed these constants were kept constant. . . Therefore, how could they contribute to the model variability?

2) Evaluation of the modelled isoprene emissions The authors built an 'observed' annual cycle of isoprene emission for the tropics using a mixed of different measurements, done at different time period, at different places and using different measurement techniques. They are hence using an "artificial" dataset to evaluate the capacity of the model to reproduce isoprene emissions at the tropics. This methodology is a non-sense as it disregards spatial and inter-annual variability that naturally influences isoprene emission.

3) Evaluation of the modelled leaf temperature If I understood well, the authors use observations of leaf temperature from a field campaign done in 2003 to evaluate modelled leaf temperature simulated using two atmospheric forcing datasets taken for the year 2000. Again the authors dismiss the inter-annual variability of meteorological data that surely does influence leaf temperature. The authors also compare directly hourly model outputs to minute data, and conclude that the model is not able to catch the measurements extremes (i.e. Fig.4). I would expect that averaged hourly data (atmospheric forcing of the model) do not catch as much variability as minute data. At least

the authors should average the data from minutes to hours before comparing. Finally, before concluding that the CLM model is not able to reproduce the observed leaf temperature, the authors should have analysed the consistency between meteorological data (i.e. air temperature or cloudiness) forcing the model with the one observed at the site where leaf temperature has been measured. Using a coherent forcing dataset is essential to judge the capacity of a model to reproduce a given variable.

Beside the methodological issues, I found the manuscript overall hard to follow. The methods part is unclear. The authors should not forget that they address their manuscript to an audience that is not necessarily familiar with CLM, technical jargon i.e. carbon-nitrogen (CN) or biogeochemistry (BGC) option is rather vague. The discussion uses too many numbers within the text (consider using tables to summarise other studies' results), and the main message (if any) gets lost in those numbers. Finally, there are too many inaccuracies in the text, which leave the reader with the impression that the authors do not fully master the subject, which is rather surprising given some of the names in the co-authorship list. Based on the scientific incoherence of the study and the general style of the manuscript, I would recommend rejecting the manuscript in its current form.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23995, 2014.