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

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Response to Referee #2 – In review of MS acp-2014-684

We would like to sincerely thank the referee for taking the time and effort in reviewing our manuscript. The referee brought forward several issues that need clarification, most of which require changes in the manuscript. A main focus was spent on clarifying and revising the literature review, specifically literature in the discussion section. We have incorporated these modifications and hope that the changes we are suggesting are sufficient. The changes listed below have been incorporated into a final version of

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[Discussion Paper](#)



the manuscript, which we hope will be reviewed and accepted for publication.

Foremost, we agree with the comments by the anonymous referee #2.

1) We agree that the literature review is hard to follow and should be streamlined. We believe that there are too many values summarized and acronyms from other modeling studies in the discussion section (specifically section 4.1). We have removed some of the supporting and related work from the text of the manuscript and have included them in a short table to help streamline the manuscript. This has reduced the text in section 4.1 and made it straightforward, and easier to read.

Introduction:

2) Page 23999 – Sensible heat flux is not used as a parameter in MEGAN, and therefore was not included in the Monte Carlo analysis, and was not considered in this study. Measured sensible heat flux was used as a variable to correlate against isoprene flux in the Rinne et al. (2002) study, which investigated how isoprene correlated with environmental variables. Therefore, we felt like it was appropriate to include in our literature review. However, our manuscript only evaluated the parameters that directly influence isoprene fluxes and emissions, so sensible heat was not evaluated.

We agree that including the references Ferreira et al., 2010; Müller et al., 2007; and Carlton and Baker, 2011, which also looked at MEGAN estimates in the Amazon, will help with the completeness of this manuscript. These references have been included in the introduction.

Page 23999, line 15-16; We agree that only using peer-reviewed literature on comparing multiple biogenic models should replace gray literature. Any gray literature on the comparison of models has been removed.

Results:

3) Page 24013 – we have made sure that all acronyms have been clearly defined in the manuscript. When possible we have also tried to remove unnecessary acronyms.

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If the editors would like a short table of acronyms and their definitions, that can be provided as well.

Discussion:

4) Page 24016, lines 1-4; the four studies that are referenced did use different vegetation maps, and different land surface models. We can include details on the specific vegetation maps, land surface models, and other forcing data used in these four studies in the new “Literature Review Table” that was included in the manuscript.

Page 24016, lines 25-30; to the best of my knowledge, I do not think Karl et al. (2007) reported modeled isoprene emissions before the environmental corrections were made, therefore I’m not sure if they were over- or under-predicted. While the Karl et al. (2007) did use MEGAN (the G06 version); the vegetation speciation (i.e. landcover, LAI, PFT distributions) was different from the driving vegetation variables in MEGAN-CLM. This entire paragraph has been updated in the manuscript to be clearer.

Page 24017, lines 4-25; We agree that these lines read more like a summary rather than a discussion, and they are out of place. The majority of this section (specifically lines 14-25) has been removed. We have included the review of measured Amazonian isoprene emissions in a table.

Page 24019, lines 17-18; We agree that this line is hard to follow. It was found that during drought conditions isoprene production from photosynthesis (fixed C) was 100% higher, or a hundred-fold increase. The specific values of the percent of isoprene production increased from 1% to 2%, which we wanted to include for reference.

Page 24020, lines 6-17; The reviewer makes a good point that vegetation speciation is a critical factor when looking at sensitivities of isoprene emissions, especially in the tropics where diversity is very high. Looking at the role of vegetation distribution, community composition, and well as isoprene-emitting biomass on sensitivity of isoprene emissions would be very interesting, but might be out of the scope of this manuscript.

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Our goal was to evaluate the 19 parameters that drive the MEGAN-CLM model, which is a global model, and which only has 2 tropical PFTs represented in the model. Including a wider range of vegetation composition in CLM would be out of the scope of this paper.

Additional notes –

We agree that there are lots of acronyms and model names used throughout the manuscript. These have been clearly defined, removed, or included in a table. We appreciate the thoughtful comments and reviews by the referee, and think the paper is stronger as a result.

Thank you for your consideration, J. A. Holm

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

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