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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Photosynthesis-dependent isoprene emission from leaf to planet in a global carbon–chemistry–climate model" *by* N. Unger et al.

Anonymous Referee #1

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This manuscript describes the development and evaluation of a new biochemical model of isoprene emission (based on the Niinemets et al. (1999) scheme) in the GISS Model-E2 global climate model. This is a straight-forward model description paper, however, one distinguishing factor is the collection of isoprene flux measurements over different ecosystems and seasons, which could serve as an excellent resource for comparing isoprene response and model performance. The authors might consider whether some summary figures could more easily compare the observed behavior across sites. Additional minor suggestions and corrections are noted below.

1. Page 17720, line 8: "reactive carbon" may include carbon monoxide, which puts the anthropogenic source well above the numbers implied in this sentence. I suggest replacing "reactive carbon" with "VOCs".

2. Page 17720, line14: Total methane emissions (natural+anthropogenic) are typically estimated ∼500 Tg/yr, comparable to (not significantly less than) isoprene emissions. RCP anthropogenic emissions total ∼300 Tg/yr in 2000 and do not include the large natural wetland and termite sources. Thus the statement in the text appears incorrect. It should be removed or suitable citations provided.

3. Introduction: The authors may want to update their references to include the latest MEGAN inventory described by Guenther et al., 2012.

4. Page 17722, line 24: This statement doesn't seem completely accurate. Certainly many CTMs neglect the response to soil moisture because they do not include an active land model. However soil moisture is part of the Guenther et al., 2006; 2012 algorithms and can easily be incorporated in models, such as Muller et al., 2008 cited here on page 17732, line 15.

5. Page 17723, lines 11, 20, 23: typo "online" not "in-line"

6. Section 2: It would be helpful for the authors to clearly demarcate in the text what is new in Yale-E2 compared to the NASA Model-E2.

7. Page 17725, lines 18-24: Could the authors provide some numbers here to convey some sense of scale? What are the total crop areas? What are the relative differences? (i.e. differences of a factor of 2 or 20% in crop cover in standard vs SiB2?).

8. Page 17728, lines 16-17: What is "photosystem II"?

9. Page 17729, lines 3-4: Why did the authors use this simple linear scaling rather than the existing laboratory-based parameterizations (eg. Posell and Hewitt, 2011; Wilkinson et al., 2009)?

10. Page 17729, lines 15-17: It's not clear why the authors chose not to use a temperature relationship that simulated this optimum.

11. Table 1: it would be useful to add PFT land cover fractions here (for standard and SiB2) either as a fraction of total vegetated surface or absolute area.

12. Page 17731, lines: 13-14: Was the standard deviation of the 10 year simulation used to evaluate the significance of results? It would be informative to include the stdev of total isoprene emission and GPP of the SimCONT simulation in Table 2 for this purpose.

13. Page 17732-33/Figures 1b/2: Please harmonize the units in the text and figures (either kg/m2/s or mgC/m2/h).

14. Figures 3a/3b: Are both of these figures required? The seasonal differences are not really discussed, so perhaps just show summer?

15. Page 17733, line 18: suggest adding to sentence: "in most regions of the world in the model."

16. Figures: It would be useful in many of the timeseries that show the model average value for 10 years, to also display the interannual variability (perhaps as a shaded region representing the stdev) so that the potential errors associated with mis-matched measurements and simulation time horizons could be assessed.

17. Figures 5a/5b: Why at Tharandt does the model underestimate GPP and overestimate latent heat flux? It is the only site that exhibits this behavior.

18. Section 4.2.3: It would be very useful accompany the data in Table 5 with a scatter plot (perhaps colored by biome and/or season) to help visualize the model performance. I realize that such a comparison will be inexact due to gaps in data records, etc, and therefore would suggest retaining Table 5, but a simple plot would be far more informative/useful to the reader.

19. Page 17741, lines13-14: I'm confused by these sentences – Figure 8a shows comparisons that are certainly not within a factor of two! This may be the result of mis-captioning? The Figure 8a caption doesn't specify the sites and season. In general, all the Figure 8 captions would benefit from the addition of a time horizon for the observations (are the stdev for multiple years or over single years?) or reference to a Table that provides this info.

20. Section 4.2.5: This section is lengthy and there is a lot of information to assimilate from the plots, but it seems to largely conclude that one can't use in situ flux sites to validate large scale models (or that there are many caveats to doing so). I wonder if the section could somehow be simplified around this discussion?

21. Page 17743, lines 23-24: It's hard to agree with this statement. The model-measurement comparison is terrible here. The measurements are zero in winter, are highly structured and variable. Thus even "qualitatively" the model does not reproduce the observed behavior.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 17717, 2013.