Response to Referee #1

We are grateful to the reviewer for their time and energy in providing helpful comments and guidance that have improved the manuscript. In this document, we describe how we have addressed the reviewer's comments (italicized below). Page numbers refer to Discussion paper (*Atmos. Chem. Phys. Discuss.*, 13, 17717–17791, 2013).

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.

We did think about possible summary figures, however, it is rather challenging because there are not obvious compact patterns (e.g. across biomes, seasons) in the model-measurement comparisons. Instead, we have included Tables 5, 7 and 8 in the paper that show statistical diagnostics and data useful for benchmarking model performance.

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".

Done.

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.

Changed to: "comparable to the total flux of methane"

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

Fixed.

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.

We disagree. The statement is accurate because the sentence specifies "Current generation **CCMs and CTMs** usually neglect the response of isoprene emission to soil moisture". MEGAN and Muller et al., 2008 are complex canopy environment models and not CCMs or CTMs as far as we know. We already discuss the soil moisture function in MEGAN and Muller et al., 2008 throughout the paper. CCMs and CTMs that use MEGAN-type algorithms can easily incorporate the soil moisture function in the isoprene emission algorithm (an active land model isn't necessarily a requirement, because gridded soil moisture can be used from a reanalysis product e.g. GMAO MERRA). However, they usually don't include this function.

Our point is that variability in atmospheric composition has a dependency on soil moisture that is usually not accounted for in global chemistry models (e.g. pg. 17722, line 25, "It follows that water availability must play an important role in isoprene emission that will in turn affect the atmospheric composition and climate-air pollution interactions").

To avoid confusion, we have added "**global** CCMs and CTMs usually neglect the response of isoprene emission to soil moisture…."

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

Corrected.

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.

We have added: "and incorporates interactive terrestrial ecosystems, a dynamic carbon cycle module, and 2-way coupling between the on-line vegetation and atmospheric chemistry".

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?).

We removed: "The major difference between the standard and SiB2 vegetation cover datasets is in the extent of the crop PFT cover. The standard dataset has more extensive crop PFT cover in the tropics and subtropics whereas this land is classified as the savanna and grass PFTs in SiB2. Conversely, the SiB2 dataset has more extensive crop PFT cover in Europe whereas this land is alternately classified as grass and deciduous PFTs in the standard dataset. On the global average, the standard dataset has a higher crop PFT fraction than in SiB2."

And replaced with: "The fractional coverage of global vegetated land area by each PFT for the standard and SiB2 datasets is shown in Table 1 (bright and dark bare soil fractions not shown). In the standard dataset, the fractional cover of crop PFT is about double that in SiB2 (36.9% versus 19.1%) because tropical and subtropical land that is classified as crop PFT in the standard dataset is classified as savanna and shrub PFTs in SiB2."

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

Since this paper is being considered for Atmospheric Chemistry and Physics whose readers may not be so familiar with the terminology, we have added a short definition: "α_{qe} is the intrinsic quantum efficiency for photosynthetic CO_2 uptake in the chlorophyll reaction system that absorbs PAR to drive the oxidation of water and the reduction of enzymes (photosystem II)."

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

Firstly, it is important to recognize that the simple scaling function has the same shape as the parameterization in Possell & Hewitt, 2011. The primary reason that we adopted this simple function is to be consistent with Arneth et al., 2007 and Pacifico et al., 2011, the other published photosynthesis-dependent global models. Secondly, we might argue that our model includes greater complexity because we compute fully dynamic C_i whereas models that use the laboratory-based parameterizations from Wilkinson et al., 2009 and/or Possell & Hewitt, 2011 typically assume $C_i = 0.7*Ca$ for C3 plants e.g. Guenther et al., 2012; Heald et al., 2009.

That said, our model framework is flexible enough for us to explore the use of alternative functions/parameterizations in future work. The exact form of the CO_2-inhibition parameterization likely imposes a considerably smaller source of uncertainty than assuming it applies to all plant species. For the 3-5 year research plan, our group ultimately intends to explore the use of the new conceptual model (Harrison et al., 2013) that does not require parameterization of the CO_2-inhibition.

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

We deliberately used the exact form of the model that is described in Arneth et al., 2007 and Pacifico et al., 2011 (to facilitate comparison of the identical isoprene scheme in different host simulation frameworks). Furthermore, we state in this Section 2.1.3: "Such high temperature conditions in isoprene emitting biomes rarely occur in nature at large ecosystem scales…."

The bottom line is that including the temperature optimum will have little or no impact on the results, data comparisons and evaluation for present climate presented in this study. We could easily incorporate the temperature optimum (or temperature "turnover") for use in studies of past and future hot greenhouse worlds. However, we are also currently exploring a particularly exciting new research area, impacts of the temperature acclimation of photosynthesis. We have added to the end of Section 2.1.3:

"In future and past hot greenhouse worlds, plant photosynthesis may acclimate to the higher temperatures (Arneth et al., 2012). This plastic adjustment of photosynthesis will indirectly impact isoprene emission. Whether the temperature optimum for isoprene synthase will similarly shift in warmer climates is not known."

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.

Done.

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.

Done.

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

Done. Harmonized units to $mgC/m^2/hr$ i.e. changed the units in the figures.

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

Agreed. Removed Figure 3(b).

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

Done.

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.

"Mis-matched" is the wrong word because a CCM always simulates climatologies and not specific meteorological years, unless it is being driven by off-line met fields in which case it has become a CTM.

We appreciate the reviewer's thoughtful suggestion. In earlier versions of the time series plots, we did attempt to represent the model's stdev as shaded regions (for instance similar to Figures 9a-c that show average diurnal cycle). However, specifically for the time series cases, we found that the plots were too cluttered to the extent that it became difficult to make out the individual model/measurement lines. The stdev (n=10 years) is in general rather small for the model (a few percent at most) because of the use of fixed SST boundary conditions. Therefore, we prefer to keep Figures 7(a-f) and Figures 8(a-e) in the original submission format. (Reviewer #2 has not raised any concerns about these Figures).

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.

The behavior is in part a result of the model land cover being dominated by crop PFT in this grid cell (e.g. Table 3) and in part a result of our choice of photosynthesis parameters in the Michelis-Menten Enzyme-Kinetics FBB model for the evergreen PFT. Blyth et al., 2011 found a similar underestimate of photosynthesis and overestimate of latent heat flux at Tharandt site using the JULES land surface model.

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.

Done. As the reviewer indicates, this figure is not strictly scientifically correct because there is no real relationship between the different measurement campaigns (x-axis). However, since both reviewers have requested this figure, and we agree that it makes the model-measurement comparison much easier to visualize/assimilate, we have added Figure 6 scatter plot.

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.

The sentence on page 17741, lines 13-14 reads: "The model simulates the magnitude of the average diurnal cycle in **the dry season tropics and the growing season temperate zone** (except at Belgium) to within a factor of 2….". Therefore the statement is correct because April-July is the **wet season tropics** (where the model overestimates by > than a factor of 2 for reasons discussed in the paper).

There was a typo in the Figure 8(a) caption [now Figure 9(a)]. It is Table 6 not Table 5 that specifies the measurement and model time periods used to compute 30-minute average diurnal cycles and is referred to at the start of this section "**4.2.5 Isoprene and GPP diurnal cycle"** e.g. on page 17740, lines 26-27: "The measurement days included in the averaging are indicated in Table 6….". We have corrected the relevant captions, and in Figure 9(a) additionally indicated that the left column shows wet season results and the right column shows dry season results.

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?

We disagree and respectfully invite the reviewer to reconsider this comment. Section 4.2.5 does not "largely conclude that one can't use in situ flux sites to validate large scale models". The main conclusion from this section is that the model reproduces the measured variability in the 30-minute average diurnal cycle but is less successful in reproducing the absolute flux magnitude. This conclusion is clearly apparent in the text. We assert that the length is justified because there is a large amount of interesting information in the plots (the length has already been condensed for the manuscript). In addition to a succinct quantitative statistical assessment of the model performance at each site, this section discusses some novel findings, for instance on the interactions between drought-canopy-temperature and isoprene emission, such that at dry season Manaus the water-stressed model simulates higher emission. We also provide a comparison of our results to the other published photosynthesis-dependent model results at the sites where data is available.

21. Page 17743, lines 23-24: It's hard to agree with this statement. The modelmeasurement 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.

It is not surprising that the model-measurement comparison is "terrible" because it is actually comparing "apples to oranges". The climate model results are decadal average climatologies that are not designed to reproduce high frequency weather variability in a specific meteorological year. We would like to clarify that there are no measurements in winter (not that the values are zero). We explored making plots of the modeled and measured weekly and monthly 10am-3pm averages but there is hardly any data left (6 points at most). Since the paper is a bit long anyway, we have decided to remove Figures 9(a)-(c) and Section 4.2.6 from the paper. Quantitative information on the seasonality of model isoprene emission is indicated in Figure 1(b).

We have added to Section 5 (page 17745):

"We will explore daily and seasonal average isoprene emission model performance using an off-line version of the vegetation model that is driven by meteorology from the GMAO Modern Era-Retrospective Analysis (Rienecker et al., 2011)."