Response to Short Comment by Prof. Dr. Grote

We are grateful to Prof. Dr. Grote for his interest in our work and helpful guidance. In this document, we describe our responses to Prof. Dr. Grote's comments (italicized below). Some of Dr. Grote's questions are part of our long-term research vision for this project and cannot be addressed within the scope of a single ACP manuscript.

This manuscript deals with a very important issue – namely to provide an alternative to the current emission modeling that is linked to photosynthesis. (One of) the expectations behind this is that the differences in emission responses that we see in various types of vegetation (or even species) such as differences in maximum emission intensity, temperature optimum, or the shape of responses to radiation or CO2, could be explained by differences in photosynthesis (resp. electron transport). This gives the model not only more flexibility in terms of environmental impacts but is also very important for spatial and temporal upscaling. Canopy integration is made easier because photosynthesis related gradients may be directly related to gradients in emission – so if the model reproduces gradient in light, vcmax and nutrient content, this will be reflected in emission patterns too. Also, seasonal emission patterns may – at least partly – be related to seasonal developments of photosynthesis (such as a reduction of vcmax in winter). Therefore, I congratulate the authors for being the first to approach this issue in a global climate/ air chemistry modeling approach. However, given the expectations mentioned above, it is not quite clear to me how this linkage is implemented – what seems to be important since it is already applied it on a global scale. Let me explain this in more detail in the following paragraphs:

1. Coupling the Farquhar model to the Niinemets model is based on the calculation of the electron transport rate of photosynthesis. This very much depends on the implementation of the photosynthesis model. For example gamma_star (in Farquhar et al. 1980) is calculated from the Michaelis-Menten parameters kc and and ko which vary in dependence on 4 parameters (ko25, kc25, and their activation energies). Alpha_qe depends directly and indirectly on 4 additional parameters (curvature parameter, vcmax, jmax, and activation energy of jmax, see for example Caemmerer et al. 2009). The result is quite sensitive to all of these 8 parameters and there is a range of values available for each of them. However, only vcmax is given here. How are the other parameters defined? Has the sensitivity of the model to these parameters been tested?

What is not immediately obvious from our manuscript is the intensive "behind the scenes" model development work especially over the past 3 years on implementing the fully functional FBB photosynthesis/stomatal conductance into the global chemistry-climate model for the model's 8 PFTs and constantly evaluating and re-evaluating the vegetation biophysics performance at the site level and global scale, i.e. ensuring that the model gives realistic vegetation carbon and water fluxes comparable to state of the science in land-surface modeling. Beyond this paper, we regularly evaluate a standalone site-level and distributed off-line version of the model that uses GMAO MERRA reanalysis meteorology and/or observed meteorology and site characteristics where available. For instance, please see an example of recent evaluation below (Figure A).
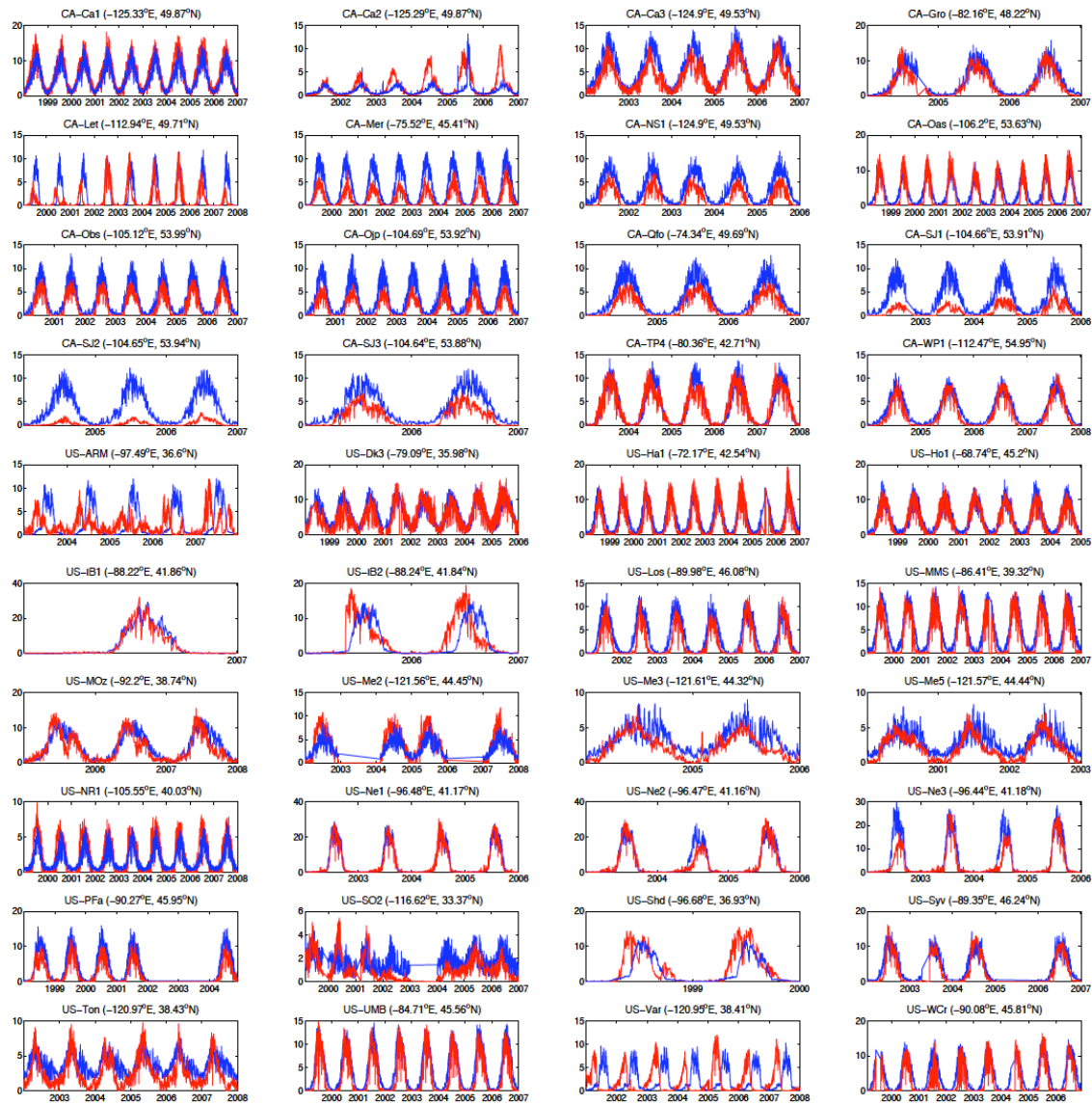
Figure A. Comparison of observations (red) and simulations (blue) at each NACP site for daily gross primary productivity (GPP, g [C] m^{-2} day-1) (see Schaefer et al., 2012 for site definitions and details). The simulation, MERRAS_VEG, is driven with meteorological forcings from Modern Era-Retrospective Analysis (MERRA) reanalyses and site measurements. The leaf area index (LAI) is from MERRA. The time span is different for each site. (*Adapted from Yue and Unger, Ozone vegetation damage effects on gross primary productivity in the United States, in submission, 2013*).

The Farquhar/Ball-Berry model is well-established, has been implemented into dozens of mature land-surface modeling systems e.g. (Sitch et al., 2008;Schwalm et al., 2010;Schaefer et al., 2012), and has been extensively evaluated at the site level in what must be 100s-1000s of research papers over the past couple of decades by many different groups. Similarly, the isoprene production algorithm has been evaluated at the site level e.g. (Arneth et al., 2007). Thus, it is already known that the FBB and isoprene production models reproduce observations well if

forced with observed site level meteorology and vegetation parameters. There is no "new science".

Our research focus is global chemistry-climate modeling. For our purposes, it is relevant to provide a validation of the global scale model because it will be used in global scale applications, for instance simulation of chemistry-climate interactions in past and future climate states. In the limited space of the present manuscript that is focused on isoprene emission (an application of the vegetation biophysics module critical for chemistry-climate interactions), we offer a broad evaluation of GPP zonal and seasonal average performance. It is notable that the RMSE for the Yale-E2 model runs (shown in Table 3 in the paper) is of comparable magnitude RMSE obtained for the well-established JULES land-surface model that was constrained with observed LAI at each of the sites (Blyth et al., 2011). Indeed, Yale-E2 performs better than JULES for the tropical site Santarem. Our standalone vegetation model will participate in the next phase of the multi-model intercomparison project: Trends in net land carbon exchange over the period 1980-2010 (TRENDY). We will submit the model to be considered for any future phases of NACP multi-model intercomparison.

For clarification, we have added in Section 2.1.2: "the biophysical fluxes at the leaf level in each canopy layer based on appropriate parameters for each of the 8 PFTs from (Friend and Kiang, 2005) and the Community Land Model (Oleson et al., 2010) with updates from (Bonan et al., 2011) (Table A1)."

We have added an Appendix Table A1 that documents the PFT-specific photosynthesis parameters applied in this work.

2. If vcmax depends on nitrogen (P17727, L15) – is nitrogen dynamically described? If not – why is it mentioned at all? In fact, the literature seems to judge it as quite important to consider vcmax (nitrogen, sla) differences across the canopy for upscaling (usually linked to nitrogen gradients, see e.g. Niinemets 1997, Niinemets et la. 2004, 2010) – particularly in a model that accounts for up to 16 canopy layers. Some of the pitfalls that come with canopy scaling have been outlined in Keenan et al. 2011. Wouldn't it be reasonable to demonstrate at least the suitability of these simplifications before using them?

In the model, the canopy nitrogen profile follows a negative exponential decline with depth but we do not yet have fully dynamic nitrogen. We have removed: "V_{cmax} is prescribed for each PFT based on the specified leaf nitrogen content" and instead refer readers to Table A1: "maximum carboxylation capacity at the optimal temperature, 25°C, (V_{cmax}) (Table A1)".

In our research, we investigate the short-lived climate forcers, the impacts of ozone and aerosols on regional and global climate. Therefore, our work necessarily is geared towards spatiotemporal scales that have actual relevance for climate change, e.g. large-scale changes in the ozone and aerosol composition across hundreds of kms. Thus, we are simulating the vegetation biophysics within an IPCC-class global climate model framework that concurrently simulates the entire climate state and the reactive atmospheric chemical composition (ozone and aerosols) that are fully coupled to the climate state (and now the vegetation biophysics). Our strategy is to develop a model that represents the magnitude and climatic sensitivity/variability of carbon and water

fluxes on ~100km grid-cell scales. Parameterizations are unavoidable. We fully recognize the importance of: Keenan et al., 2011 "Overlooking the canopy: The importance of canopy structure in scaling isoprenoid emissions from the leaf to the landscape", for example, in terms of a local ozone air quality application. But that is not our business. To our knowledge, there is no quantitative study demonstrating the importance of fully explicit resolved canopy structure for regional and global ozone and aerosol radiative forcing applications. Perhaps this idea will be a good subject for a future research project.

3. Drought impact is quite large and in a similar magnitude for GPP and isoprene. This is somewhat surprising because the majority of measurements find photosynthesisrelated emissions less sensitive to drought stress than GPP (see overview in Niinemets 2010, referenced as 2010a in the manuscript). It is also one of most uncertain impacts in the MEGAN model, which is parameterized from only one publication and heavily depends on wilting point parameterization (according to Guenther et al. 2012). It would therefore of particular interest to get more insight into the indirect drought impact via stomata conductance. If I understand it correctly, a decreased conductance decreases the internal co2 concentration (ci), electron transport limited photosynthesis and thus emission. The degree of reduction thus depends on implementation and parameterization. Given the different suggestion to do this (see different formulation e.g. in Farquhar et al. 1980, von Caemmerer and Farquhar 1981, Harley et al. 1992, and von Caemmerer et al. 2009), I feel that some more description and sensitivity analyses is needed to show that the implemented emission model responds reasonable. Additionally, - since the decreased ci should directly increase the emission by the k-term (as stated in P17729, L9) – It would be interesting to see of what magnitude the two counteracting effects actually are and if the relation between both depends on the degree of drought stress?

That would be excellent except there is not enough (if any) suitable field measurement data to evaluate if the "implemented emission model responds reasonably". Precisely for this reason, we have shown all model results throughout the paper for the regular standard present climate model and the model with artificial removal of water stress, so that we can judge the importance of the current model implementation within the context of all available above canopy flux measurements. As we clearly state in the paper, there remain unresolved issues regarding the isoprene emission response to drought. Generally, the response pattern to drought stress is a short-term initial increase in isoprene flux followed by a longer-term decline. Our model formulation simulates this response pattern. In fact there are 3 competing drought effects that influence isoprene emission: increase in kappa, decrease in gpp and increase in canopy T. We discuss extensively in the paper the evidence at Manaus for drought induced canopy T increases leading to stimulated isoprene emission. If a suitable drought-impacted field measurement dataset that includes isoprene, gpp, meteorology and soil moisture were to become available to us, we would certainly be eager to assess the model response behavior using our standalone site level model version.

4. It is a bit irritating that vegetation types such as the 'deciduous forests' are put into one PFT, given the huge differences in species-specific emission potentials and the considerable importance of differentiating vegetation classes (e.g. Schurgers et al. 2011). In the MEGAN this has been to some degree accounted for by providing regional specific emission factors. This is not the case here, isn't it? Thus, I wonder 1) if the dominant forest within a PFT actually

behaves similar regarding emission independent from its location (resp. species composition?), and 2) if the average parameterization of photosynthesis is able to reproduce the average emission response of the PFT. An indication that this might need to be considered is the finding that the given representative vcmax values for forest PFTs are a lot smaller (30 for deciduous) than those assumed for high isoprene emitters (e.g. all deciduous species listed in the isoprene emission data set of Pacifico et al. 2009 have vcmax values between 50 and 100 umol m-2 s-1). Can you comment on this? The latter point, of course, is also the reason why the comparison with a specific site doesn't work. Shouldn't the model be run with two parameterizations – one PFT specific and one site specific - to show that the model is at least able to provide reasonable estimates if correctly parameterized?

In the model, the isoprene emission from a particular PFT is location-dependent because it will respond to the different meteorology/climatic conditions in different locations.

Our global model provides more reasonable site emission estimates than (Pacifico et al., 2011) that substantially overpredicted the isoprene emission at most sites. Our model authentically simulates the measured variability in isoprene emission 30-minute average diurnal cycle across a wide range of different ecosystems (R^2=64-96%). The isoprene emission magnitude in the temperate zone and dry season tropics is within a factor of 2. Considering we use a global climate model that simulates its own meteorology and atmospheric chemical composition at ~200 km resolution and applies only 8 PFTs, this performance may indeed be viewed rather exceptional. That said, Prof. Dr. Grote's idea to publish the model-measurement comparisons using the off-line site level model version driven with site-specific vegetation variables and meteorology is very well taken, and we will endeavor to do this in follow-on work.

V_{cmax} = 30 μmol m^{-2} s^{-1} is from (Bonan et al., 2011). It is possible that our LAI model gives too high LAI in some regions that may be compensated for by the lower V_{cmax} than in Pacifico et al., 2009. It is an excellent idea to explore these relationships more closely in an off-line site level version of the model.

It is my view that increasing to 20 PFTs and ~50-100km horizontal resolution is not likely to have any significant impact whatsoever on the quantitative findings in this paper. A step change improvement in global isoprene modeling can only come with many more measured values of above canopy flux isoprene emission across a much greater range of species that could then be implemented into the global model at the species level.

The questions above are only referring to the direct linkage between photosynthesis and emission models. There might be some more questions regarding for example the simulation of seasonality of emission. It is very well known that the onset of emission lacks behind that of photosynthesis which is generally attributed to the need to build up enzyme capacity/ activity of emission products (see e.g. Monson et al. 2012). But this might be addressed in another discussion.

We already have discussed the delayed onset of isoprene emission relative to photosynthesis throughout the paper and our approach and plans for treating it in this model.

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