Atmos. Chem. Phys. Discuss., 6, S4142–S4144, 2006
www.atmos-chem-phys-discuss.net/6/S4142/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Process-based estimates of terrestrial ecosystem isoprene emissions" *by* A. Arneth et al.

Anonymous Referee #2

Received and published: 30 October 2006

This manuscript nicely presents the "current state of knowledge" with respect to process-based modeling of the relationship between elevated atmospheric CO2 and leaf isoprene emission. Although numerous laboratory-based studies have indicated that elevated CO2 suppresses leaf isoprene emission, few studies have attempted to integrate this observed suppression into larger ecosystem-scale emission scenarios. A true highlight of this manuscript is the observation that the CO2-inhibition of leaf isoprene emission can be significant enough to offset the increase in emission due to CO2-stimulation of productivity and LAI. Specifically, this manuscript highlights the need for incorporating both direct and indirect effects of CO2 on isoprene emission when developing emission scenarios. These results have important implications for the future of biosphere-atmosphere interactions as well as atmospheric chemistry in

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general. This manuscript seems ideally suited for publication in ACPD, as it is likely to generate a fair bit of discussion within the VOC and global change community. In general, I found this to be an interesting, albeit somewhat wordy, manuscript. As such, my comments are relatively trivial but are listed below:

1. Although the author's do a nice job of comparing the various process-based isoprene emission models, it is unclear to me why the Niinemets et al., model was the one chosen for coupling to the dynamic vegetation model. Normally, this would not be an issue, but since the author's have spent so much time comparing and contrasting the various process-based models I believe they should better develop the rationale for using the one they did. This seems particularly relevant as ultimately the choice of the process-based model clearly drives the modeled isoprene production at the various 'ecosystems' presented.

2. I think the observed discrepancy at the Harvard Forest site merits further discussion. I find these results particularly perplexing considering that modeled LAI exceeds actual LAI by 30% and if you enhance the basal emission rate from 100 to 160 the model data the discrepancy is still 20%? Are the author's simply suggesting that the problem is inaccurate basal emission estimates in this system? Or, do these results tell us something more fundamental about the limits of their coupled vegetation-isoprene model? As constructed, I'm wondering if their model approach only works well in ecosystems where emitting species are generally dominant? This ultimately begs the question: where is the bulk of the uncertainty in the model estimates presented? This is an area of the manuscript the author's could more fully develop.

3. The author's do a nice job of using a 'semi-mechanistic' approach for modifying the Niinemets et al., model for CO2 sensitivity (CO2 dependence of epsilon). But ultimately, this is still a 'best-fit' approach to modeling the CO2 response. Considering the nature of ACPD, it would be useful for the author's to indicate

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what the future of a truly mechanistic process-based model might look like and/or discuss if modifications to existing models (as presented here) are good enough.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8011, 2006.