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Interactive comment on "Impact of climate variability and land use changes on global biogenic volatile organic compound emissions" *by* J. Lathière et al.

J. Lathière et al.

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We first would like to thank the referee for his comments and remarks that will be very helpful to improve the quality of our manuscript. We detail in the following part the modifications and corrections that have been made.

General comments:

1-The authors include the effect of CO2 on photosynthesis, but have not considered the effect of CO2 on isoprene emissions. Some studies (e.g. Rosenstiel et al., Nature, Vol.421, pp.256-259, 2003) show that isoprene emissions decrease with increasing CO2. Could the authors estimate how much of an impact the change in CO2 they

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used for their simulation (1983-1995) might have on their calculated isoprene emissions? We are aware that the consistency of biogenic VOC emissions estimates is closely linked to the parameters considered in the models. Rosenstiel et al. (2003) showed, measuring the evolution of isoprene production and biomass for atmospheric CO2 levels of 430, 800, and 1200 ppmv, that the percentage of fixed CO2 converted to isoprene is decreased by CO2 fertilization. The influence of atmospheric CO2 level on isoprene production could be critical on long time-scales, characterized by a strong atmospheric CO2 variation, such as it could be the case in the future. Nevertheless, our study is performed on a 13 years period, with a 5% atmospheric CO2 increase, and not a nearly-doubling atmospheric CO2 as it is the case in Rosenstiel et al. (2003). We can thus reasonably consider that the misestimate of isoprene emissions due to the fact that we do not take into account the impact of changing atmospheric CO2 on isoprene emissions is rather small. Indeed, doing a coarse estimate based on Rosenstiel et al., 2003, we calculate that the change (in %) of the carbon fixation, defined as the ratio between isoprene production and biomass, is equal to 0.6 time the atmospheric CO2 increase (in %), and of opposite sign. Considering the 5% atmospheric CO2 increase over the 1983-1995 period leads to a 3% decrease of carbon fixation, which is not a major change. This point is now discussed in conclusion: “Rosenstiel et al. (2003) showed that under increased atmospheric CO2 level from 430 ppmv to 800 and 1200 ppmv, the isoprene production was reduced by 21% and 41% while aboveground biomass accumulation was enhanced by 60% and 82%. We can reasonably consider that considering this influence in our study would not change significantly the estimates calculated over the 1983-1995 period, characterized by a 5% increase of the atmospheric CO2 but could however be subsequent on longer time-scales.”

2-The authors model emissions of methanol, yet they have not referred to the paper on the global methanol cycle by I.E. Galbally and W. Kirstine. “The production of methanol by flowering plants and the global cycle of methanol”, J. Atmos. Chem., Vol.43, No.3, pp. 195-229, 2002. I would suggest the authors include a reference to this paper, and compare the results with their own estimate of the emission flux. A reference to the

ACPD

5, S5651–S5655, 2005

Galbally and Kirstine (2002) has been included in the text, in the section 3.1: “Galbally and Kirstine (2002) estimated methanol emissions by flowering plants to 100 Tg of methanol per year (37.5 TgC/yr), based on plant structure and metabolic properties.”

3-The authors calculate emissions of formic and acetic acid, yet do not discuss the modeled fluxes anywhere in the paper. Are they significant? A quick estimation of the yield of formic acid from the reactions of isoprene and methacrolein with ozone, using the mechanism of Poschl et al. [J. Atmos. Chem., Vol.37, pp.29-53, 2000] and some modeled chemical fluxes gives about 6 TgC/yr, which is four times the direct emission modeled in the paper. The authors should discuss the formic/acetic acid emission fluxes, or remove them from the paper. To better appreciate the importance of formic acid biogenic emissions, a comparison with the study by Baboukas et al. (2000), giving and estimation of formic acid emissions from the ozonolysis of organics, has been included in the section 3.1: “Biogenic emissions of acids total 1.8 TgC/yr from which formic acid emissions contribute to 1.5 TgC/yr, 3 times less than the production of organics by ozonolysis reaction, estimated to 5 TgC/yr (Baboukas et al., 2000)”.

4-Section 4. The authors have compared their estimated VOC emissions with the southern oscillation index (SOI). However, the correlation coefficients obtained are very small, and indicate that changes in SOI have little if any impact on the VOC emissions. Much of this section, and figures 4 and 5 could be removed from the paper. We have strongly reduced the corresponding section and modified as well the figures 4 and 5, according to the referee’s remark.

Specific comments:

1-Abstract - line 1, Change “is incorporated” to “has been incorporated” Change made in the text

2-p.10615, line 7-8, Change to “Global VOC emissions by vegetation areĚ” Change made in the text

ACPD

5, S5651–S5655, 2005

3-p.10616, line 8, “Ě30 and 270 Tg/yr.”. Should the unit be TgC/yr? If not, how did the authors calculate these masses? The secondary organic aerosol production is given in mass of organic matter. This has been clarified in the text.

4-p.10617, line 12. Could these numbers be converted in TgC/yr to be consistent with other emission values quoted in this paper? Unfortunately, we do not have the corresponding forest area to assess the impact on annual emissions. Nevertheless, this emission level can be compared to the emissions calculated in our study and illustrated in the Figure 3.

5-p.10628, lines 10-11. “..number of grasses species emitterĚ” needs rephrasing, “number of species of grass that emit isoprene..” Change made in the text.

6-p.10628, line 27. Change to “Considering the difficulty in estimating leaf temperatureĚ” Change made in the text

7-p.10630, lines 17-20. Sentence is very long and needs rephrasing. The corresponding sentence has been finally removed, in relation with the reduction of this section, in response to the general comment n°4.

8-p.10631, lines 19-23. Rephrase sentence. “In the tropical regions with low precipitation, such as the southern part of Brazil, the LAI is quite small in the control run (1-3 m^2/m^2). Deforestation leads to a small increase in LAI in this region between 1 and 1.5 m^2/m^2. In significant parts of Amazonia, Central Africa and Indonesia, a large decrease in LAI in the range 2-4.5 m^2/m^2 is modelled.” The sentence has been modified accordingly.

9-p.10632, lines 11-15. Rephrase sentence. “A decrease in zonal mean methanol emissions is also modeled north of the equator in March, and south of the equator in September(Ě). This reduction in methanol emissions is due to the large decrease in LAI in these regions, and occurs despite the significantly larger methanol emission factors of crops compared to tropical trees (Table 1).” Change made in the text.

ACPD

5, S5651–S5655, 2005

10-p.10633, lines 12-14. Are these fluxes the wrong way round? They show that the methanol emissions increase, not decrease. The text has been corrected and clarified: “Zonal mean methanol emissions decrease by 0.5’10-11-1.5’10-11 kgC/m^2/s, between January and March and after October, and by 1.5’10-11-5’10-11 kgC/m^2/s, during spring and summer”.

11-p.10638, lines 8-13. Split this sentence in two. I guess you are talking about the sentence p.10636, lines 8-13, which has been modified accordingly. “Nevertheless, important points, such as the pattern and dependency of emissions on environmental conditions for compounds other than isoprene and monoterpenes, and the response of foliar density and biogenic emission factors to CO2 and climate change, are still under debate. Those various influences could affect emissions levels significantly, and more information is needed to reduce the estimates uncertainty and improve our understanding of biosphere-atmosphere interactions.”

12- Figures 5 and 7 seem to be “squashed” horizontally, and are difficult to interpret; expanding them in the horizontal direction would make them clearer. The figures have been modified.

13-The different vertical bars for each region in Figure 6 are almost impossible to distinguish. Could this figure be redrawn in colour? The corresponding figure has been modified.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10613, 2005.