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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The global atmospheric budget of ethanol revisited" *by* W. V. Kirstine and I. E. Galbally

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Comments to Referees

We thank the two anonymous referees for their constructive comments and useful suggestions for improving the presentation. Below, we quote each referees comment followed by our response.

Referee # 1. It would be interesting to see if the difference between the biogenic emission estimates is mostly due to using different emission factors or the different scaling procedures. The uncertainties for the biogenic source of atmospheric ethanol in this study are given as roughly ±50% and some explanation of how this uncertainty

was determined is discussed. Naik et al. gives an uncertainty of a factor of 3 in their biogenic ethanol emission factor, without much discussion, and before scaling the emissions to the global level. So this article seems to take a large step in improving the precision of the global biogenic ethanol emissions, assuming that uncertainties were propagated correctly. It is the opinion of this reviewer that the work should be published, but could be more interesting and useful if the authors could specifically pin down why their estimate of biogenic ethanol emissions is higher than those previously published.

Our Response. Naik et al. (2010) state "the emission factors (Fig.2) for ethanol are primarily based on the recommendations of Guenther et al (2000) except that emissions from coniferous trees are based on ...(Schade and Goldstein, 2001, 2002)". Thus, the ethanol emissions in Naik et al. (2010) are based on 3 sets of plant ethanol emission observations. Then later, Naik et al. (2010) state "the dependence of ethanol emissions on root flooding is not considered here".

In our discussion paper, 4 additional sets of data on plant ethanol emissions are included, as well as the initial 3 included in Naik et al. (2010), more than doubling of the emissions database. Furthermore, inclusion of these extra studies in our discussion paper allows the inclusion of the effects of root flooding and plant stress, which were not included in the Naik et al. (2010) paper.

Referee # 2. I had big difficulties in following the way how the sources and sinks have been calculated. This is crucial for the reader of the paper, as these values are considerably higher than those found in previous peer-reviewed literature. Therefore, I would suggest that authors either describe it in a way that the reader can really follow how these numbers have been created. This could be done in the main part of the paper or by adding a section with supplementary material.

Our Response. The brevity of the description of the extrapolation from short-term to annual fluxes our discussion paper was imposed to limit the length of the paper. We

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agree with the reviewer's comment, and are attaching Supplementary Material providing explicit calculations of each annual emission flux. See Supplementary Material.

The reviewer correctly noted an error in the paragraph referring to the study of Fukui and Doskey (1998). It will be revised as follows in the final paper:

Fukui and Doskey (1998) measured ethanol emission fluxes from a grassland ecosystem in the midwestern United States over most of the growing season. The average of all the ethanol fluxes measured during the study period was 206 μg m-2 h-1, with the largest fluxes occurring after a severe frost event or after the plots were waterlogged following heavy rain. Using the data of Fukui and Doskey (1998), we estimate that for a simulated growing season in a typical temperate region with 1000-2000 daylight hours, two water-logged periods and one severe frost event, as observed by Fukui and Doskey (1998), the average ethanol flux would be 0.2-0.4 g (ethanol) m-2 y-1 (Fig. 1).

Referee #2. I also cannot follow the global sink calculation of the authors for the OH degradation. If I just use some very basic assumptions I get 27 Tg/yr, as a maximum for the OH gaseous sink term. My calculation goes as follows: I assume globally 1 ppb of ethanol (which is the highest value used in the paper (in the CBL)), and globally 1E6 OH radicals/cm3. If I then take the Jimenez value of 3.2E-12 cm3/molecule/ s for k in the ethanol-OH reaction. The calculation would be: 1/(3.2 x10-12 x 1E6)= 3.6 days lifetime of ethanol relatively to OH This means that the ethanol in the atmosphere is overturned 365/3.6, roughly 100 times a year. Then I calculate the burden (only in the troposphere) 1ppb x 1.44E20 mol. This results in 1.44E11 mols of ethanol, which is 270 Gg of ethanol. This multiplied by the 100 times overturn per year gives 27 Tg/yr. . . . Also here I strongly suggest that authors provide the information of how this was actually calculated in the supplement.

Our Response. We agree with the reviewer's calculation. The issue is one of units. The reviewer calculates the mass of carbon turned over as a sink of ethanol per year. Alternatively, this calculation indicates that 52 Tg y-1 of ethanol is removed from the

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atmosphere under the reviewer's scenario. The source and sink estimates presented in our discussion paper are for the full molecular mass of ethanol, as, we conclude, are other recent budget estimates of ethanol (Naik et al. 2010, Singh et al. 2004). Supplementary Material presenting our full atmospheric sink estimates will be provided with the final paper. See Supplementary Material.

Referee #2. P. 25910, Line 1: I would not label ethanol as biogenic (as it also has anthropogenic sources, also mentioned in the paper). Oxidised VOC?

Our Response. The global sources of ethanol arise from either the fermentation or burning of biological material. Nearly all industrial or beverage ethanol is produced by fermentation of plant material. As such, we would prefer to leave the term 'biogenic' unchanged.

Referee #2. P. 25910, Line 8-10: The sentence starting with "The observational : : : could possibly be omitted, as it contains no valuable information

Our Response. This sentence will be removed in the final paper.

Referee #2 P. 25920, Line 21: Albeit the fact that the ocean is a net source, the deposition has to be taken into account. Only if the study which is referenced has taken this into account already and the sum of both processes is calculated then it would be ok. Please check and specify in the text if this has been done in this way.

Our Response. Beale et al. (2010) calculates net flux, which takes into account of any deposition.

Referee #2 P. 25922, Line 5-14: The short-term differences of sources and sinks should level themselves out during the course of the year. I don't think that this point is justified to be made. So I would suggest to delete it.

Our Response. We think that it is useful to comment on the expected variability of atmospheric ethanol concentrations in the context of this paper.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C12718/2011/acpd-11-C12718-2011-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25909, 2011.

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Interactive Comment