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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interpreting methane variations in the past two decades using measurements of CH_4 mixing ratio and isotopic composition" *by* G. Monteil et al.

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Answers to major comments:

The referee is concerned that the scaling factor that we apply to our OH field may not be valid, since this factor was derived using the TM5 model and since the spatio-temporal patterns are derived from a full chemistry run of TM3 at a different resolution than our TM3 setup.

Indeed, our model simulations are sensitive to OH, since the reaction OH+CH_4 is the dominant methane sink, and therefore the amount of OH in the model determines the amount of methane emissions needed to match the measured concentration. However, the purpose of our study is not to determine absolute values for methane source and sink strengths, but to find which relative variations in the different budget terms are compatible with the observed methane and δ^{13}C-CH_4 growth rates. A slightly different amount or distribution of OH would have to be compensated by corresponding changes in methane sources, but the impact of such adjustments on the simulated trend in global CH_4 and its isotopic composition would only be minor. We illustrate this in Figure 1 of this document, which shows a comparison between the S0 and P2 scenarios of the paper and two new identical scenarios (S0B and P2B) with OH increased by 8%. Emissions, soil sink and stratospheric sink were increased by the same relative amount. The resulting concentrations and isotopic ratios are very comparable to those presented in the article.

If the referee claims that the required reduction in emissions is strongly dependent on the OH level then this is true for the average level of the global emissions. However, a constant adjustment of OH cannot explain the differences between simulated versus observed trends, which are investigated in this study.

Answers to minor comments

- page 6773, line 10: Edgar was changed to EDGAR, as suggested.
- page 6773, line 12: Reference to Rigby et al. (2008) was added.
- page 6773, lines 16-18: The OH sink is slightly lower after 1995 in Bousquet et al. (2006), however the reduction in emissions is stronger.
- page 6774, line 23: "joined" was changed for "coupled".

- page 6775, lines 20-21: This remark was taken into account by a small modification in section 3.2.4 of the discussion paper. Meteo fields from the year 2000 are recycled through the whole simulation period, mostly for technical reasons, however we verified that the influence of this was negligible on multi-year trends (the influence of inter-annual variability of temperature will be mostly visible in the amplitude of the seasonal cycle). In any case, an increase of the OH sink can be interpreted as an increase in the OH concentration or as an increase in temperature.
- page 6776, line 6: The OH fields used in our study are from a full chemistry run of TM3, described in Houweling et al. (1999).
- page 6776, line 22: The 0.1×0.1 resolution is the format in which the data were provided by EDGAR. We further aggregated then into a 7.5×10 format.
- page 9776, line 22: in the TM3 model, the top of the atmosphere is 0 hPa. In our coarse resolution setup this corresponds to 8 tropospheric levels + 1 level for the stratosphere.
- page 6777, line 3: The source of the statistics is EDGAR4 (G. Maenhout, personal communications, 2010). This was added in the revised manuscript. We were initially using rice emissions from GISS, including seasonality; we lost this seasonality when we switched to the EDGAR4 inventory. The loss of seasonality resulted in a degradation of the simulated seasonal cycle, but did not affect the multi-years trends. This is because rice emissions are kept relatively low in our model: most of the seasonality comes from OH sink and wetland emissions.
- page 6779, lines 4-10: The referee interpreted well our writing, the sources section was modified for more clarity in the revised article.
- page 6779, lines 21-22: We verified that the conclusions of Tans (1997) apply to our model: the difference in initialization time appears more clearly when the dif-

ferences between the simulations shown in Figure 1 of the discussion paper are plotted. We prefer to show a range of simulations with different initial conditions, which corresponds better to the way we determine our initial conditions. To avoid confusions, we deleted the reference to Tans (1997) in the revised manuscript.

- page 6782, lines 18-20: There is a small offset between the simulated and the observed concentrations in the southern hemisphere, this is due to the tendency of our model to overestimate inter-hemispheric gradients, discussed in section 3.1. Despite this offset, the growth-rate is better reproduced before 1990, in both hemispheres. The corresponding paragraph have been modified to make this more clear.
- page 6787, two first paragraphs: As described in answer to major comments, a slightly different OH would mostly offset our simulated concentrations and isotopic ratios, without significant effect on the trends, as long as the amount of OH stays constant.

References

Bousquet, P., Ciais, P., Miller, J. B., Dlugokencky, E. J., Hauglustaine, D. A., Prigent, C., Van Der Werf, G. R., Peylin, P., Brunke, E.-G., Carouge, C., Langenfelds, R. L., Lathière, J., Papa, F., Ramonet, M., Schmidt, M., Steele, L. P., Tyler, S. C., and White, J.: Contribution of anthropogenic and natural sources to atmospheric methane variability, Nature, 443, 439–443, doi:10.1038/nature05132, 2006.

Houweling, S., Kaminski, T., Dentener, F., Lelieveld, J., and Heimann, M.: inverse modeling of methane sources and sinks using the adjoint of a global transport model, J. Geophys. Res., 104, 26137–26160, 1999.

Tans, P. P.: A note on isotopic ratios and the global atmospheric methane budget, Glob. Biogeochem. Cy., 11, 77–81, doi:10.1029/2002GB001895, 1997.

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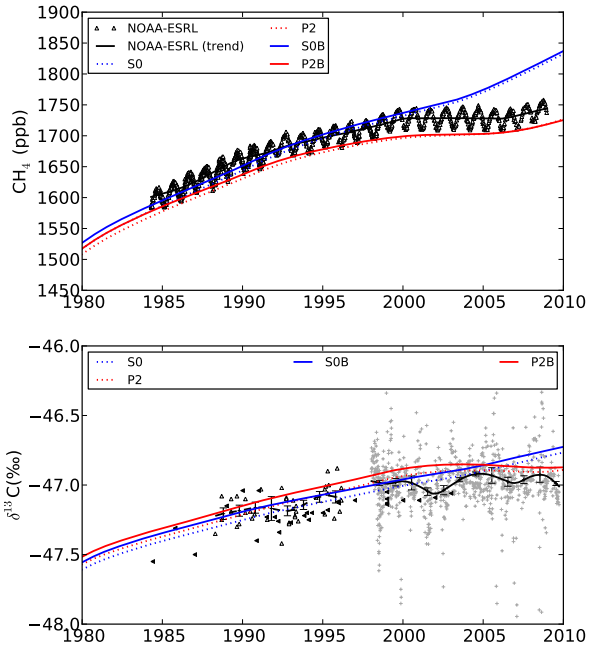

Fig. 1. omparison of scenarios S0 and P2 of the discussion article, with corresponding S0B and P2B scenarios, using different OH burden.