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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can we explain the observed methane variability after the Mount Pinatubo eruption?" *by* N. Bândă et al.

Anonymous Referee #2

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General comments: This is a detailed analysis of how the sources of methane and its removal by atmospheric chemistry sinks might have changed when there were clearly some significant effects to these following the Pinatubo eruption. While some of the results are similar to earlier analyses, the focus here is to do a more objective comparison of possible changes in a carefully balanced way. In particular, this addresses the roles that can be played by changes in sources and/or in oxidation by OH, as well as the extent to which atmospheric mole fraction data can distinguish between the ORCHIDEE and LPJ inventories for wetland emissions.

A key result from this analysis is that, while a combination of different factors can explain much of the magnitude of changes that were observed, these did actually occur more rapidly than explained by the model analysis.

However, some of the background for this work could still be covered more clearly. Patra et al 2014 is being cited in the section on potential sources of uncertainty, and that review paper raised major questions about the extent to which atmospheric chemistry models can reproduce the spatial distribution of OH, particularly the ratio of Northern Hemisphere to Southern Hemisphere concentrations. Given this level of structural uncertainty in atmospheric chemistry models, it would be useful to include a short comparison of the TM5 model, as used here, with others used elsewhere.

Also while the Patra et al 2014 paper was focussed on the spatial distribution of OH, the earlier paper by Montzka et al 2011 (Small Interannual Variability of Global Atmospheric Hydroxyl. Science 331, 67-69. 2011) was a review of how different tracers for OH can bring in some constraints on the extent to which this has varied over 1985 - 2007. A brief comparison between that and the results of this paper would be helpful in the conclusion.

Specific comments: It would be helpful if the introduction included a reference to the general summary of the CH4 budget given in Table 6.8 of Ciais et al, 2013 (Chapter 6: Carbon and Other Biogeochemical Cycles, Working Group I Contribution to the IPCC Fifth Assessment Report, Climate Change). That is a more recent assessment than Denman et al 2007 and clearly shows the very large discrepancies that still exist between bottom-up and top-down budget analyses, and therefore the relevance of this work.

The long paragraph over lines 58 – 112 would be clearer if it were reorganised into separate paragraphs summarising the previous treatments of sources, then of sinks, and then factors such as cross tropopause transport, rather than merging these together as currently done.

It would also read a bit more clearly if a new paragraph started on what is now line 108 with "In the present . . . " as that would specifically bring out the focus of this paper.

At some point it would be useful for the paper to say whether the approach is to exclude

CH4 removal by soil processes, and by reaction with tropospheric chlorine, or to treat these as static over the period 1991 – 1995. Relative variations in these would have to be large to explain much of the atmospheric CH4 variations in this period, however, soils involve a mix of methanogenic and methanotrophic processes and the net flux to the atmosphere can vary across a range of 10 – 40% of what is actually being produced (e.g. Le Mer, J. & Roger, P.; Production, oxidation, emission and consumption of methane by soils: A review. European Journal of Soil Biology 37, 25-50, 2001).

Section 2.1 is clearly written and shows why this work is an extension of what has been published previously.

The way in which tropopause variability and cross tropopause transport is treated can be very relevant for the period after the Pinatubo eruption. While section 3.1 mentions that a tropopause based on Lawrence et al 2001 is used, it is not clear to what extent that would differ from a more detailed treatment of its seasonal and latitudinal variations such as the NCEP/NCAR reanalysis project (Kalnay et al., 1996 Bull Am Met Soc and the continual updates at ftp://ftp.cdc.noaa.gov/Datasets/ncep.reanalysis.dailyavgs/tropopause).

In section 2.2 it would be useful to know how the altitudes for the Mauna Loa and Niwot Ridge sites are being dealt with as they are outside the lowest 2 km of the atmosphere being used from TM5. For example there is a significant seasonal cycle in the differences between mole fractions for Cape Kumukahi at the surface and Mauna Loa at 3397 m. Similarly no sites are being used over the 0-30°S region and again there are quite different seasonal cycles for the mole fractions at Samoa and Ascension Island. However, these differences are being reflected to some extent in Figure 4 where differences between the GLOBALVIEW analysis and for the Pacific sites is shown.

Section 3.1 gives a good summary but the paragraph from lines 375 to 406 could be clearer if it were broken into two that covered what was similar between ORCHIDEE and LPJ, and then what was different.

Line 383 has a repetition of words. Figures 6a and 6b are not referred to in the text.

The summary in Section 4 is well organised. The conclusion in Section 5 is clear but as noted earlier it would be useful to make some comments on the differences between this analysis and that of Montzka et al, 2011.

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