

## ***Interactive comment on “Analysis of global methane changes after the 1991 Pinatubo volcanic eruption” by N. Bândă et al.***

### **Anonymous Referee #1**

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The manuscript presents an analysis of the processes that lead to the global methane changes after the Pinatubo volcanic eruption. The authors give a very comprehensive overview of the literature related to studies of the different aspects of the change in methane after the Pinatubo eruption. They summarize the partly contradictory outcomes of these studies, stating that no complete study has been made, which includes all competing processes. However, with their simplified tropospheric chemistry model they are also not able to investigate all potential processes, in particular changes in the atmospheric dynamics. The authors clearly state the limitations of their model. Nevertheless, it should be pointed out more clearly that a quantification of the processes influencing the methane concentration is of limited significance with the strongly simplified model they are using. The manuscript rather presents a sensitivity study – which is very interesting in itself and a prerequisite for a study using a more

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sophisticated model system.

In general, the paper is well structured and well written. The sensitivity studies are systematically evaluated. However, it is not stated clearly enough which simulation represents the base line for the evaluation. Furthermore, the need of having both, steady-state and transient simulations, should be motivated in the beginning.

The paper is appropriate in scope and content for ACP and should be published after some revisions.

Specific comments:

P18030, L9: To really quantify the effects of the processes a more sophisticated model approach would be needed. With a simplified, one-dimensional model this is rather a sensitivity study.

P18031, L28: It should be mentioned that the higher interannual variability found by Prinn was for an earlier time period, ie. pre-1995.

P18032, L6: State what was emitted?

P18040, L3-11: List more clearly the simulations performed. Some more details for the reference simulations are needed. Which simulations are shown in Fig. 3? Why are these simulations starting at methane concentrations considerably higher than the observed value in 1890? Does the 'base simulation' contain anthropogenic CH<sub>4</sub> emissions, which vary in time, like those shown in Fig.3?

P18040, L20-21: That 'Surface concentrations . . . fall well within the range of observations' is not at all shown in this section. But this would be required to allow the reader to judge this statement. Please add a comparison with observations here.

P18041, L20-28: The comparison with budgets from other studies needs to be evaluated more critically. Differences in some components are substantial, eg. for ozone stratospheric exchange. Consequences for the validity of this study should be dis-

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cussed.

P18042, L14-28: Given the large overestimation of sensitivity factors of CH<sub>4</sub> and OH to changes in the ozone column compared to estimates from more sophisticated 3-D CTMs, the question arises whether the results of this study is significant at all. At least the authors point out that their results should be interpreted with care. Please state why the study should nevertheless give meaningful insights.

P18043, L21: Why does an underestimation of the methane lifetime lead to an overestimation of atmospheric concentrations.

P18043, L22-23: Why should it be expected that the model is able to represent the temporal evolution following a volcanic eruption if it is not able to represent the temporal evolution of the concentration in the century before? Please explain.

P18044, L6: the term 'base simulation' should be introduced already at the end of Sect. 2, see comment above.

P18046, L21ff: It is not immediately clear where the statements concerning the changes in OH come from. Where is this shown?

P18047, L3: It is not easy to extract this number from Spahni et al.(2011). Could you please explain from which part of the paper you have extracted this number.

P18047, L9: The reference (Spahni et al., 2011) refers probably only to the first part of the sentence.

P18047, L21-23: In this sentence it is not unambiguous what a lower growth rate is. Hence the argumentation is not completely clear.

P18047, L27-29: The statement of 'a general correspondence' is overoptimistic. This should be assessed more critically.

P18049, L20: 'remarkable comparison' should be rephrased to be more modest even in view of the simplicity of the model.

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P18049, L24-26: This statement is certainly true. However the model used in this study is highly simplified and does not fulfil the stated requirements. This should be recalled here in the conclusions.

Technical corrections:

P18038, L12: add a reference for 5 TgN of NO<sub>x</sub> from lightning.

P18034, L10: ... observed d13C...

P18039, L10-12: add here again the reference (Guo et al., 2004)

P18039, L13: explain briefly what 'GISS data' are

P18041, L17: ...possibly due to...

P18043, L6: replace 'this study' by Etheridge et al. (1998)

P18048, L4: ...differences...

P18051, L18: ...tropical tropospheric...

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18029, 2012.

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