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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of soil uptake on the global distribution of molecular hydrogen: chemical transport model simulation" *by* H. Yashiro et al.

H. Yashiro et al.

h.yashiro@jamstec.go.jp

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We appreciate the reviewer for valuable comments.

Major issues:

1) » The only serious remark I would like to make is that the H2 budget does not seem to be closed. Figure 8 presents the various budget terms, which are presented as anomalies from 2000. The caption mentions that the 2000 total H2 tendency amount -3.9 Tg. The upper panel, however, shows a much smaller change in the total burden in 2000. Also for the other years I could not link the budget terms with the change in the global burden.

I have to apologize for the mistakes of the values in the graph. Now we re-calculated the total burdens and the averaged value matches that we wrote in the abstract. We also corrected the total of budget terms in 2000, resulting in the value of -1.9Tg/yr. These were simple miscalculation. Furthermore, the inconsistencies of the total annual budget terms were due not to count the amount of inflow from the stratosphere. In this study, annual total of net stratosphere-to-troposphere exchange were in the range of 3.6 to 5.9 Tg/yr. For the a small gradient of concentration near the tropopause, net stratosphere-troposphere exchange of hydrogen was ignored as usual. However, our simulation results(Fig. 7) show that the concentration below the tropopause is slightly lower than the value in the lower stratosphere throughout the year and we estimate the small amount of net influx form stratosphere. The net S-T exchange of ozone in our model has been evaluated by Sudo et al.[2007], and it's value is about 490 Tg/yr.

Table 2 : We added the term of S-T exchange. Figure 8 : We corrected the value of upper panel and add the anomaly of S-T exchange. We also corrected the value in the caption to -1.9Tg/yr.

2) » Specifically the "soil wetness" and "inactive layer" could be modified and the effects on the simulated H2 concentrations could be shown (e.g. for one year).

We append the modified version of Fig. 10. In the figure, we also discussed about the impact of global bias of soil moisture. If we apply $\pm$0.05 m m-1 change of global soil moisture, deposition velocities change largely compared to the change of inactive layer thickness. In this study, we adopted δ = 0.7 cm to optimizes the model agreement with the observation. However, as reviewer suggested, this is not a unique solution. We can have different combinations of δ and the air ratio correction (free parameters) to optimize the model-observation agreement for trends. For example, we can employ a combination of δ = 0.3 cm and the air ratio correction of 0.19 m3 m-3 or δ = 1.0 cm and 0.24 m3 m-3, to obtain similar results. The impact of these calculations on the seasonal variation at the observational sites is minimal.

Minor issues:

» p 4060, l 12: semi arid regions (add s)

» p 4060, l 16: Tg H2 (add H2)

» p 4060, l 20: dominant causes (plural)

» p 4060, l 22: observations (plurar)

» p 4060, l 23: in the tropics (add the) and at northern high-latitudes (add at).

» p 4062, l 6: previous studies have shown

» p 4064, l 7: details . . . are described

» p 4064, l 11: unit of the rate constant is wrong!

» p 4064, l 26: which were obtained

» p 4065, l 24: a set if 10 vegetation .. (leave out “of defined”)

» p 4067, l 11: unit is not correct. Should be kg/m3 or a density should be introduced in formula 2.

» p 4068, l 9: unit of the flux is not correct

» p 4068, l 15: air ratio should read m3/m3 I guess?

» p 4071, l 17: the uptake rate of enzyme activity. . ...replace by: the uptake rate due to enzyme activity.

» p 4072, l 13: velocities

» p 4075, l 4-5: noticeably discrepancy, replace by: “noticeable discrepancies”

» p 4076, l 11-12: the H2 concentration is well mixed, replace by: “ H2 is well mixed globally”.

» p 4081, l 6: from large biomass burnings, replace by: “from large scale biomass burning”.

» p 4082, l 22: The unit 0.2 g/g seems strange to me, since earlier the air ratio was given in the (wrong?) unit of m/m.

» p 4093: caption too small.

» p 4097: at selected 10, replace by: “at 10 selected”

The sentences and captions have been corrected, as suggested.

» p 4069, equation 9: The concentration in the inactive layer is not relevant. Why not simply give the formula for C(δ)?

The concentration in the inactive layer is used to give the flux in that layer. I described equation 9 to emphasize contrast with equation 10.

» p 4071, l 11-13: I am a bit surprised by this approach. This seems very ad hoc. Why is the value of delta not linked dynamically to a temperature surplus? Now a step function is introduced based on a very arbitrary temperature limit (40 C).

This treatment is aimed to increase the thickness of inactive layer by extreme temperature. However, as the reviewer pointed out, evidence was weak on how to give the parameters. These sentences have been deleted. The effect of this treatment on the hydrogen concentration was very limited, so there is no graph that should be specially replaced by the recalculation of the model.

» p 4073, l 11: with the diurnal, replace by: “with the seasonal” (I guess?)

We have modified the sentence to "$\sim$ the range of 5-10 $\times$ 10-2 cm s-1 that is associated with the variation of precipitation and $\sim$"

» p 4078, l 1-3: This seems speculation to me. I would expect a more detailed analysis here. One could conduct a sensitivity analysis that enhances soil uptake in this specific

region.

We conducted a sensitivity study by changing the soil moisture (decrease 0.05 m m-3), thickness of inactive layer (0.7 to 0.3 cm), and the ratio of freeze and non-freeze water (increase non-freeze water of 30%) only around the model grid near UTA. The results show that the seasonal variations of deposition velocity change obviously near the station, but any case does not capture the sharp decrease of H2 concentration at the station. To solve this problem, it may be necessary to apply finer topography and higher horizontal resolution, and to improve reproducibility of boundary layer mixing. I modified the sentences to fit this result.

» p 4078, l 10: Also here it is stated that “This problem is connected with the physical property of the uppermost soil”. Either you say that the problem “may be connected” or you show with sensitivity studies that the situation is sensitive to the physical properties of the uppermost soil.

We modified the sentence to "may be caused by$\sim$".

» p 4081, l 16: Please refrain from speculation about increasing OH trends due to water vapor trends. OH chemistry is more complicated. In fact you could analyze the OH budget in the model. OH recycling (by NOx) and OH sinks (methane, CO) also play an important role. So either analyze the OH budget, or simply state that the trend in the H2 sink is small compared to the deposition trend.

We don’t focus the detailed analysis of the OH trends and budget in this study, so the sentences have been removed as mentioned.

» p 4083, l 20: I would have expected this statement about tuning earlier, e.g. in the method section. Something like: “with sensitivity experiments we found that a delta value of 0.7 leads to an optimal comparison with the available observations”.

We modified the sentences in p 4071, l9 to " In this study, we use a uniform δ of 0.7 cm to achieve a good agreement with global observations (see section 3.3 below)".

» p 4091: Please state in the caption how the averaged concentrations were calculated (based on daytime/nighttime?).

We have added some explanations to the Table 1. The individual observed value is compared with the daily average output of the model calculation value.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 4059, 2011.

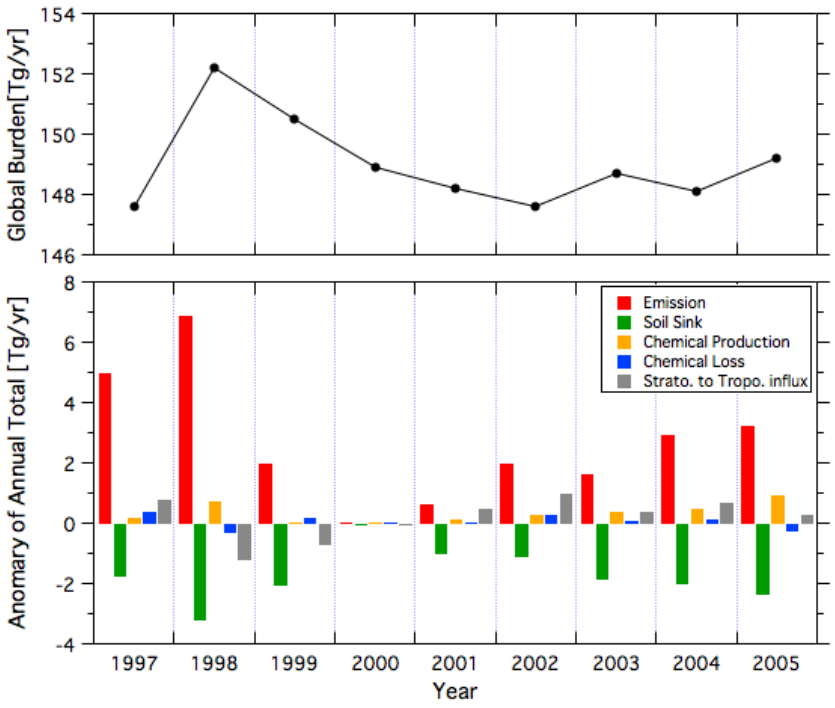

Fig. 1. (modified from Fig.8) Global annual averages of the sources and sinks of H2 (lower panel) and global burden of H2 (upper panel) during the period 1997-2005.

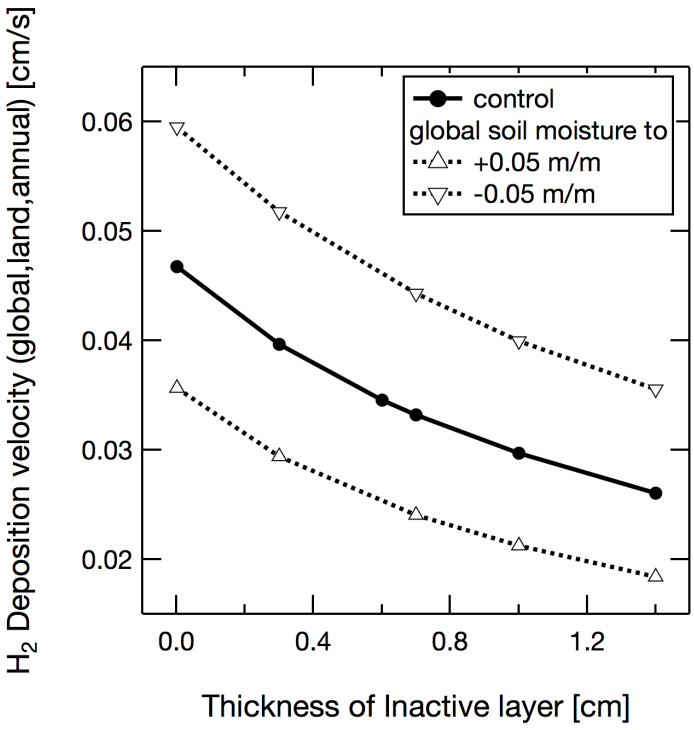

Fig. 2. (modified from Fig.10) Sensitivity experiments for change in the global averaged H2 deposition velocity on land as a function of the thickness of inactive layer.