Atmos. Chem. Phys. Discuss., 11, C975–C978, 2011
www.atmos-chem-phys-discuss.net/11/C975/2011/

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.

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Received and published: 20 March 2011

This paper describes the global distribution of hydrogen, and specifically the role of the hydrogen surface deposition. The authors applied a newly developed deposition scheme in a global model. There is a prominent role of a small inert layer of soil that separates the air from the active H2 depletion region (where it is consumed by bacteria). With the new deposition scheme implemented, the model is able to reproduce the observed seasonal cycle and the hydrogen concentration in the atmosphere. However, the uncertainty is large, especially in semi-arid regions where the top layer and soil moisture can change rapidly over short time periods.

The paper is interesting and relatively well written. The language is sometimes a bit hard to read, and contains some mistakes (some listed below). The analysis that is presented is mostly sound. Some issues are mentioned below. 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. This issue should be resolved. Apart from that I would have expected a more "in detail" analysis of the possible weaknesses of the new parameterizations. 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). Now we have to wait to the end of the paper (page 4083, line 20) to learn that the value of δ was actually tuned to enhance the comparison with observations.

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).

This shows that the language should be scrutinized, preferably by a native speaker. In the remainder, I will not list all the little mistakes I found.

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

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).

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

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 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.

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.

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

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.

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 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”.

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

Page 4093: caption too small.

Page 4097: at selected 10, replace by: “at 10 selected”

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