

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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We appreciate the reviewer for valuable comments.

»The main weakness of the study is that the global soil sink has not been determined really independently but was tuned via a parameter, i.e. the depth of the inactive soil layer which acts as a diffusion barrier for H₂ uptake by soils. With such a tuning parameter it seems always possible to close the global budget of H₂ sources and sinks (but possibly not to correctly reproduce the seasonal cycle or mean north-south gradient of H₂). This needs to be made clear already in the abstract of the paper.

We have modified the abstract to mention about the tuning as follows; “Given that C3980

the thickness of biologically inactive layer plays an important role in the soil uptake of H₂, its value in the model is chosen to achieve agreement with the observed H₂ trends. Uncertainty of the estimated soil uptake flux in the semi-arid region is still large, reflecting the discrepancy in the observed and modeled seasonal variations.”

»Abstract line 4: Explain the term “AGCM”

The word "(atmospheric general circulation model)" has been added after the "AGCM".

Page 4062, »Line 6: It may be confusing for the reader who is not totally familiar with the problem, that here it is said that the uptake takes place in a “thin soil layer near the surface” while in the abstract it is stated that there is a “biologically inactive layer near the surface”. A bit more explanation would help here.

We have modified the sentence to " Previous studies have shown that most of this absorption is accomplished within the several centimeters of soil below the surface".

»Line 10: reference: Do you mean “Schmitt et al., 2009” (also at a few places further down in the text)

»Line 12: should read “Hauglustaine”

These are typos. So the names have been corrected.

Page 4065: »Lines 15: Why should the distribution of ocean and land H₂ emissions be similar to that of CO if the associated processes are totally different?

We did not have enough information about the distribution of these emissions. Therefore, we used the distribution of CO emission as proxies of the distributions of the biological activity. We have modified the sentence to clarify this intent.

»Lines 25: If most of the H₂ uptake happens in the first 5 cm of the soil (with an inactive layer of 0.7 cm) why is the vertical resolution of the soil model with a layer thickness of 5cm, 20cm, 75cm etc. sufficient to model H₂ uptake?

We adopted the resolution to be suitable to express heat and water balance near the surface. It is difficult to reproduce detailed structure of the soil variables under the coarse horizontal resolution (~240km) of precipitation and temperature. We expect the simulation with higher resolution in the future.

»Page 4066, line 3: explain "MIROC"

The name of "MIROC" have been explained in Page 4063, line16

Page 4067: »Line 11: unit of C should be kg m⁻³ to be in agreement with Eq. 2

A density has been introduced in equation 2 to correct the unit of C.

»Lines 15: The sentence "Furthermore, R_c is described as . . ." is unclear. It would help if the authors explicitly present the respective equation.

The details of R_c in the method of Wesely(1989) is well known to the model developer and requires a long description. So we omitted those sentences.

»Lines 23: Why should the permeability of snow be always low?

»Lines 25: It is not clear to me how the deposition on snow is calculated. Do the authors mean "ratio" instead of "rate"? Why is this related to the "depth" of snow?

We have modified the sentences to be more concise. We calculate the deposition of each gas onto the soil and snow separately and summed by the fraction of snow cover.

Page 4068: »Line 9: Please correct flux unit.

»It would be helpful to write "D_s" (the diffusion coefficient in the soil) as "D_s" (D_index_s) in the Equations (and text) not to confuse the reader as it may also mean "D multiplied by s".

»Line 15: Correct air ratio unit.

The unit have been corrected and "s" of D_s has been indicated by a subscript.

C3982

»Page 4069, line 17: Why "reduction"? Do the authors mean "estimate"?

The thickness of the inactive layer δ is an important parameter in the reduction of the soil uptake flux compared with a case without an inactive layer. We have modified the sentence to represent the meaning mentioned above.

Page 4070 »Equation(13):the term Θ_{sat} seems a bit misleading here. I guess what is meant is the total porosity (which may be completely filled with water in which case the soil is saturated).

The sentence have been modified to " Θ_{sat} is the maximum aerial or liquid water volume per unit volume of soil (total porosity)"

»Equation(17): Θ_w needs to be defined.

The sentence have been modified to "M is the ratio of liquid water volume per unit volume of soil (Θ_w) to Θ_{sat} ."

»Page 4071, lines 10: "In addition . . ." It is unclear what was made here.

We have removed these sentences according to the comment by the another reviewer.

»Page 4073, lines 25: "However, the model did not capture . . .Japan Island" This sentence is unclear to me.

The sentence has modified to "However, our model shows a constant value of 2×10^{-2} cm s⁻¹, without a seasonal cycles, due to the coarseness of the model grid to resolve the arable land in Japan."

»Page 4074: For comparison with observations, the proper way would be to select the atmospheric model estimates only for those dates and times when the flasks have been collected at the stations. Correspondingly, only these model data should be compared with measurements in Figure 3. I understand that these selected data were used to calculate the bias numbers reported in Table 1?

C3983

The observed value doesn't have information on the day when the observation was not done. We plot the model value on the non-observed day in Figure 3 to show the range of day-to-day variation. We have described more about the comparison between modeled and observed values in Figure 3 as follows; "In Figure 3, the overall patterns of seasonal cycles and inter-annual variation calculated by the model are in relatively good agreement with those obtained at many of the observation stations. The differences between the observed and the fitted curve indicate short-term fluctuations comparable to the simulated day-to-day variations."

Moreover, we have added the detailed explanation about the method of comparison in table 1.

»Page 4082, lines 22: "In this study the correction . . ." This sentence and correction are unclear to me.

We have modified these sentences and put some sentences. Please see our reply to the reviewer #1.

»In Figures 1, 6 and 7 the axis are practically unreadable, please increase the numbers
The font size of axis numbers have been modified, as suggested.

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