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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new estimation of the recent tropospheric molecular hydrogen budget using atmospheric observations and variational inversion" *by* C. Yver et al.

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Thank you for the helpful comments. The comments are in italics and the answers in plain text. The comments about typo or legends will be taken into account for the revised version of the manuscript.

p. 77, L. 5: Section 3.4 could be rearranged a bit to be more clear. p. 28977, L. 20: It is not clear what a 'summer total', 'yearly total', or 'global total' of deposition velocity is without a definition. It wouldn't make sense to have a cm s^{-1} unit for a year's worth of soil uptake, but a mean could make sense. As could integrating over all latitudinal mean bands. Please explain more clearly. Without a clear explanation it's hard to know

how the units are in tens of centimeters instead of tenths of centimeters.

According to the reviewer's comments and to these two comments, section 3.4 will be modified and better organised with a more detailed description of scenarios. The authors have decided to suppress the map plotting the deposition velocity against latitude as it was more confusing than helping to investigate the deposition velocity maps. Therefore, the authors do not use 'yearly total' or mean anymore. 'As stated in the previous paragraph, we use three different soil deposition velocity maps as prior in the model. These maps are presented in Figure 4 for the months of July and January. They present some common large scale features but differ for the magnitude and distribution of regional deposition velocity. On a large scale, the highest values are found in July corresponding to the favorable temperature and moisture conditions for high deposition. In January, the maximum values are located in the Southern Hemisphere (austral summer) and in July they are located in the Northern Hemisphere except for the S3 map where high deposition velocities are found in the Southern Hemisphere throughout the year. The first two maps (S0 and S3) are more detailed since they are based on vegetation maps. The last one (S4) was created using deposition velocity measurements combined with the driving meteorology of the Oslo CTM2. These measurements remain sparse and were thus extrapolated to latitudinal bands. The first map (S0) is the one having the highest pixel velocities, up to 0.14 cm s^{-1} in July in the Northern Hemisphere, whereas in the S3 map the maximum pixel deposition velocity reaches only 0.07 cm s^{-1} and in the Oslo one (S4) the maximum pixel deposition velocity only reaches 0.06 cm s^{-1}. S0 presents important spatiotemporal variations with marked hotspots. In the winter, these hotspots are observed in Brazil and southern Africa (United Republic of Tanzania, Republic of Mozambique, Zambia and Angola). In summer, hotspots are observed mostly in North America and in the north of Russia. These high values are due to the direct link existing between NPP and deposition velocities in the assumptions of scenario S0: High NPP produced by favorable meteorological conditions may lead to too high deposition velocities. In the Southern Hemisphere, these hotspots reach 0.1 cm s^{-1} in a gridcell while in the Northern Hemisphere, they reach up to 0.14 cm s^{-1} in a gridcell. In Lallo et al. (2008), the highest values found in boreal forest was 0.07 cm s^{-1} which about two times lower than the values here. These high deposition velocities are then to be considered with caution as possible artifacts of the use of NPP as a proxy of H_2 deposition velocity. S3 is characterised by the absence of large spatiotemporal variations. In this map, the deposition velocity is lower above 30N than below (except for the Sahara region with the desert and Australia). In S4 map, the latitudinal deposition velocity presents spatiotemporal variations, but contrary to S0, there are no hotspots. In winter, the larger values are found in South America and southern Africa too but more so at the southern latitudes (Argentina and South Africa). Since the soil uptake is extrapolated from latitudinal bands, there are also large values in southern Australia. In summer, the higher deposition velocities are observed above 30N.The three maps thereby present large differences in their distributions and we can expect to find important differences in the first-guess simulations.'

Fig 4: You might plot your S1 map, which as the scaled product of S0 wouldn't show a different pattern, but would be on a comparable scale to S3 and S4. Why are deposition values of zero filled in for some maps with the black color and left blank in others? I would consider not using the black color if it is a zero value.

The authors have chosen to plot the S0 map as it is the map used in the original version of the inversion as performed by Hauglustaine and Ehhalt. (2002) and Pison et al. (2009) The black values are zero values whereas the white values are missing values. This information will be given in the legends of figure 4 in the revised version.

p. 77, L. 15: The emphasis on the minima and maxima, and later on 'hot spots' is somewhat confusing in this discussion. I would emphasize that you are just listing the grid-cell maxima in each plot and not use the word "reaches". Then, it will be more clear when you show that it is the 'mean' or 'total' soil sink that is more important.

We agree with this comment. For more clarity, the paragraph will be modified to highlight that we deal with grid-cell maxima.

p. 77, L. 25: Why are the variations important? It might be useful to explain the derivation of the S0 (Hauglustaine and Ehhalt, 2002) map very briefly on p. 76, L. 6 to give your reader some grounding.

In the manuscript 'important' was meant to be a synonym of 'large'. This will be changed for more clarity. The derivation of map S0 is explained in section 3.1 as this is the map used in the original version of the inversion. 'In the original version of the inversion which constitutes scenario S0 in this study, the H2 prior emissions and monthly deposition velocity maps are as detailed in Hauglustaine and Ehhalt (2002). Briefly, as no emission inventory exists for H_2 emissions and as CO and H_2 share the same sources (transportation, biomass burning, methane and VOCs oxidation), the H_2 emission distribution is inferred from the CO emissions distribution (Olivier et al., 1996; Granier et al., 1996; Brasseur et al., 1998; Hao et al., 1996). Emissions are then scaled to fit the estimates given by the various studies presented in Hauglustaine and Ehhalt (2002). N_2 fixation-related emissions are scaled from CO emission maps for marine emissions and from NOx emission maps for terrestrial emissions (Erickson and Taylor, 1992; Müller, 1992). Finally, the deposition velocities are estimated using the dry deposition velocities for CO, which are based on net primary production (NPP) variations and a ratio between the deposition velocities of H_2 and CO of 1.5 (Hough, 1991; Brasseur et al., 1998; Hauglustaine and Ehhalt, 2002). This leads to deposition velocities between zero and 0.1 cm s^{-1}.'

p. 28980, L. 23: It might be better not to simply early on that the S0 soil sink is too weak, if you show later that it might not be the case. Maybe one sentence earlier in the paper explaining that it was assumed to be too weak, but as you'll see, you don't conclusively find that with your analysis. Otherwise, the allusion to it being too weak is a bit confusing.

The sentences about the weak soil uptake will be modified in order to highlight the fact that this was an hypothesis formulated by Hauglustaine and Ehhalt (2002)in their paper to explain the mismatches between model and data and not an admitted fact.

'The first-guess modelling leads to a strong offset with a simulated mean mixing ratio ≈ 115 ppb higher than observed. Hauglustaine and Ehhalt (2002) attributed this mismatch between model and data to the underestimation of the soil sink in the northern hemisphere during winter and spring. Moreover, using the same scenario, Pison et al. (2009) found an urrealistic accumulation of H_2 in the atmosphere attributed partly to the same cause. In scenario S1, to test this hypothesis, we have therefore scaled the initial mean mixing ratios to the observed mean mixing ratios. Moreover, we have used updated prior surface emission fluxes from Lamarque et al. (2008) with H_2 /CO mass ratio of 0.034 and 0.02 for anthropogenic and biomass burning emissions, respectively (Hauglustaine and Ehhalt, 2002; Yver et al., 2009) and optimised HCHO concentrations from Bousquet et al. (2011). The deposition velocity map has been scaled by a ratio of 1.28 to take into account the hypothesised underestimation and produce a better balanced budget assuming that the other terms (production, emission and OH loss) are known and fixed.' It seemed important to the authors to highlight early in the manuscript that this S0 deposition velocities are supposed to be too weak in the Northern Hemisphere to explain why we scaled the map in the scenarios S1 and S2 as a test of this assumption.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28963, 2010.