

## ***Interactive comment on “Isotopic fractionation during soil uptake of atmospheric hydrogen” by A. Rice et al.***

**L. Meredith**

predawn@mit.edu

Received and published: 27 December 2010

I've added some specific comments to this discussion that I hope will be helpful.

p. 8002, L. 9: The wording in this sentence is awkward: "Considerable variability in deposition velocity observed during winter was not found to be closely related to soil moisture."

p. 8002, L. 22:  $H_2$  is not really an energy source, but could be used as an energy carrier and fuel.

p. 8003, L. 11: "Furthermore, there is not consensus" should be "no consensus" or "not a consensus".

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

p. 8003, L. 23: The final sentence of this paragraph would be better if broken into two.

p. 8004, L. 14: Why were sites cleared of duff before experimentation? This has been shown to affect the measured strength of  $H_2$  uptake by Smith-Downey (2008).

p. 8006, L. 13: "Though in all experiments consumption outweighed production, evidence for a subsurface  $H_2$  source is demonstrated by the decay of  $H_2$  to non-zero asymptote (Fig. 1a) and previously measured below ground non-zero  $H_2$  concentrations (Conrad and Seiler, 1985; Smith-Downey et al., 2008; Yonemura et al., 2000)."

This is interesting. It would be useful to know what was the longest duration the chamber studies were run and what precautions were taken to avoid pressure effects within the chamber. If overlying air is depleted in  $H_2$  relative to soil pore-space  $H_2$  concentrations, the observed change in  $H_2$  concentrations may just be diffusion-driven out-gassing from the pores instead of active production. Whether concentrations remained stable over a longer duration might be informative here.

p. 8007, L. 17 and p. 8009, L. 16: You write there is no significant correlation between the deposition velocity and soil temperature, but there appears to be some negative correlation between temperature and deposition velocity in the summer. Perhaps you could include a p-value to show the case quantitatively. This comment also applies to KIE vs soil temperature.

p. 8010, L. 18: You found a striking variability in the wintertime  $H_2$  uptake and KIE that is not related to soil moisture content as you measured it. In this line, you might add that only "some of this variability may be associated with summertime soil moisture content" then. It would be useful to add why you think your winter time measurements of deposition was so variable and had a larger range than what you measured during the summer.

p. 8015, Fig 1: It might be more interesting to plot the delta value instead of HD versus

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

time in Figure 1-b, or to include a third plot. The concentration of HD is just changing with the same scaling as the bulk  $H_2$  mixing ratio, so that HD tells us little information.

p. 8016, Fig 2: It would be better to include more information on these plots by using different symbols for summer vs. winter. I would also consider using a color scale to illustrate temperature of each data point - i.e., red for warmer to blue for cooler temperatures or by using different sized markers for different temperatures. It would be interesting to see whether more patterns emerge. You don't plot  $v_d$  versus  $T$  anywhere.

---

Interactive comment on Biogeosciences Discuss., 7, 8001, 2010.

**BGD**

7, C4515–C4517, 2010

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C4517

