Atmos. Chem. Phys. Discuss., 12, C4981–C4984, 2012
www.atmos-chem-phys-discuss.net/12/C4981/2012/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Determining water sources in the boundary layer from tall tower profiles of water vapor and surface water isotope ratios after a snowstorm in Colorado" *by* D. Noone et al.

**Anonymous Referee #1**

Received and published: 24 July 2012

This manuscript makes use of relatively high-frequency measurements of deuterium in water vapor from a very tall tower research facility in Colorado. Combined with other pieces of information (isotope ratios of a decaying snowpack, meltwater ponds, etc.) the authors attempt to determine/constrain the sources of water vapor in the atmospheric boundary layer. The underlying scientific rationale for the investigation is related to the argument that... "inadequate representation of surface fluxes and their dependence on surface conditions are among the key sources of uncertainties in quantifying regional hydroclimate". Here, isotope measurements throughout the at-

mospheric boundary layer in combination with “mixing line” analyses are used to help constrain the various surface contributions (evaporation, sublimation, etc). . ..i.e. reduce uncertainties in the representation of the surface fluxes. Overall, I like the subject of the manuscript and provide the following comments/criticisms:

1.The introduction, methods, and discussion sections omit numerous key references related to the discussion of mixing line (Keeling plot, gradient, flux ratio) analyses. Since 2004, work by Griffis, Lee and others have involved laser-based isotope flux measurements of 18O-CO2, 13C-CO2, 18O-H2O etc. The rationale for flux- based approaches has been discussed in detail and the limitations of Keeling/mixing line analyses have been well documented- especially for water vapor where the two end-member mixing assumption is certainly violated. The similarity assumptions in terms of turbulent transport for each isotopologue have also been discussed. Griffis applied the flux-gradient approach on a tall tower and compared it to eddy covariance based isotope flux ratios of 13C-CO2. The work by Lee et al., 2006 (Tellus); 2012 (Boundary-Layer Meteorology, in press) has demonstrated that the Keeling approach (when time-based, 1 measurement height as a function of 1/concentration) is not reliable for water vapor or at best can only be applied for a narrow range of environmental conditions.

2.Performance of the Picarro L115-i. To what extent is this analyzer stable/reliable under field conditions? How does changing ambient temperature impact the measurement? To what extent do vibrations associated with its movement impact the measurement? To my knowledge, changing temperature and vibrations could have a detrimental impact on the optical alignment, fringing etc. These issues should be addressed. Further, how does the calibration method account for the changing mixing ratios observed over the 300 m profile?

3.Have other investigators used this Picarro analyzer for insitu field measurements? What were the performance characteristics, reliability?

4.Profiles. My understanding is that it takes 15 minutes to obtain a profile. The elevator



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takes the instrument on a 9 min ride to the top where it sits for a 3min measurement interval and sits at the bottom of the profile for 3 min. Based on the turbulent time scale – can you obtain a reliable gradient with this type of sampling scheme?

5.Equations 4, 5, 6. If using the flux ratio method, the transport coefficients can be neglected if they are the same for each isotope (i.e. assume similarity). In your equations this scaling applies in the surface layer, but is it valid in the mixed layer?

6.Mixing line assumptions. It is difficult to accept that a two end-member mixing model could be applied anywhere with reliability. i.e. there is entrainment, advection, and multiple surface sources so how can such a model be justified?

7.There is a need to comment on the footprint of the tall tower observations. These will differ dramatically for concentration vs fluxes. There is also a need to provide some information regarding the source footprint that influences these tall tower measurements. What are the surface conditions that influence these measurements? Snow packs are highly variable and their decay typically forms a complex patchwork on the landscape. One can imagine that the isotope ratio of the snow pack, ponds, bare soil etc are highly variable in space and time. There should be supporting information in the form of land use/land surface conditions derived by satellite.

8.Throughout, it is not always clear if the mixing line refers to gradient or traditional Keeling type analyses. Further, it is not always clear if these two methods are being applied within the surface layer or mixed layer. These terms and their use should be made clear throughout. It is only later in the manuscript that specific heights etc. are noted with more detail.

9.What are the typical heights of the convective and nocturnal boundary layers and how does entrainment/top-down diffusion impact the results (i.e. what is the isotope ratio of the vapor in the layer of air being entrained through daytime mixing).

10.Mass balance assumptions – surface runoff seems like a key component since the



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soils are still frozen and there is limited infiltration. What is the spatial extent of the snowpack isotope ratio observations? I see no mention of how these were sampled spatially or temporally. I know from person experience that vertical variations in isotope ratios are very strong – this raises the question of what is the surface and where does sublimation originate from.

11.Why is there no mention of the actual total flux observation from the tall tower? How large is the total latent heat flux that is to be partitioned? Given the amount of energy available for this time of year it seems like it will be very small (a few W m-2) and that partitioning this small uncertain flux is limited by its own uncertainties (large error in isotope composition of flux, varying end members on short time scales etc).

12.Kinetic effects. How does turbulence influence the fractionation associated with sublimation and ponding water evaporation. I did not see that this was discussed, but would need to be parameterized for the landscape by taking into account factors such as snow roughness length etc.

13.Page 16344 section 20. Good point, but this section should be cited appropriately.

14.Conclusions. Page 16349 Section 5-10 This not a novel finding. Section 15 – this is weak as written because it does not discuss source footprint differences etc. Section 20-25. – this section ignores previous research.

15.I wonder if the 18O-H2O data would provide similar information and partitioning or would we arrive at a very different conclusion?

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 16327, 2012.