Hydrol. Earth Syst. Sci. Discuss., 9, C4145–C4149, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C4145/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Impact of elevation and weather patterns on the isotopic composition of precipitation in a tropical montane rainforest" *by* D. Windhorst et al.

Anonymous Referee #2

Received and published: 6 September 2012

The scope of this paper addresses a need for more hydrologic studies and stable isotope measurements of precipitation in tropical, mountainous environments. The topic fits within the scope of HESS. However, there are significant revisions that need to be made before it can be accepted for publication. Special attention to organization of argument and corroboration of observations with additional data sets would improve the paper. Some of the larger-scope issues are presented below.

1. The paper attributes an observed shift toward lower isotope values to a seasonality of weather patterns, and it distinguishes this seasonality from "the amount effect"

on isotopic composition, when in fact these two things are related. Orographic rainfall associated with trade winds may experience limited rainout, yielding heavier stable isotopic compositions, whereas other precipitation systems may be derived from already depleted air masses, thus producing lighter isotopic compositions. Therefore, different prevailing weather systems and the amount effect on isotopic composition are linked. The paper could benefit from added discussion that elaborates and explains its reasoning behind causes of the seasonality, and two the heavily referenced publications in the paper (Rhodes et al., 2006 and Scholl et al., 2009) can help with thinking through a restructured discussion.

2. The data set represents only 3.5 months of data. Describing a "strong" seasonal isotopic signal is over stated given the limited data set. Are there other data sets in the region collected over longer time spans that could corroborate the observed pattern? More discussion of the "Amaluza" data, or other data sets if they exist, could help here.

In addition, the interpretation of the "seasonality" of the data needs to be connected better to changes in prevailing air masses that bring precipitation to the study area. If wind direction, shown in Figure 5, is not clearly defined after Mid-October, then I am confused as to the "prevailing" air mass that is responsible for the lighter isotope compositions after Mid-October. The introduction section implies that the Pacific Westerlies prevail during this time. Is Figure 5 at odds with this description, or are the wind directions significantly different? How do the authors know that wind direction data are linked to a change in larger-scale prevailing air masses that bring precipitation to the region? The authors should be able to find other data sets to firm up this interpretation.

3. Discussion of the deuterium excess data (section 3.3) needs to be improved. The paper describes an "abrupt decrease [in deuterium excess] in mid-October," yet Figure 6 shows differences in this shift between the four sample sites, and the decrease seems more gradual than abrupt at the"El Tiro" location. Make sure the written descriptions carefully describe the observations. The discussion of effects of relative humidity vs. recycled water on deuterium excess needs to be explained further within the context

of conditions in this study area. The observed difference in deuterium excess at the highest altitude site is intriguing but not adequately explained.

Additionally, the discussion in Section 3.3 is confusing about whether lower deuterium excess is attributed to changes in moisture recycling during the year, or differences in the proportion of recycled water within certain air masses that bring precipitation to the study area. The picture of what the authors interpret as happening at this location needs to be improved.

4. One source is not cited properly, which brings suspicion that other miscitations exist. Scholl et al. (2009) do not describe a seasonality in deuterium excess, as is stated on p. 8437, lines 7-8. Rhodes et al. (2006) do, but this is not mentioned. Also, the interpretations presented on p. 8436, lines 3-15 are similar to findings of Rhodes et al. (2006).

5. Figures 2 and 6 need to be presented differently. The graphs need to show that each sample represents a discrete event. Therefore, the isotope data must be represented as points rather than as a continuous curve. Each sample represents an average isotopic composition for a particular event, and it is unknown how the composition may be changing over the sampling interval (yet the curves imply that this is known). The individual points need to be shown, and some indication of sampling interval would be helpful.

Additionally, the x-axes do not show a linear representation of time; instead, the dates are categorical. Because time is a variable, the spacing of tick marks on the x-axis needs to represent an equal number of days.

6. Figure 1 needs a locator map within South America, not just the watershed delineation. Presenting a broader view of the study area will help communicate interpretations of the results within the context of large-scale wind patterns and regions that could supply recycled water to the study area.

7. The paper's interpretations of the "altitude effect" and lapse rate from the stable isotope data are reasonable for the data presented.

8. The grammar and writing generally is good. I did catch a few typos. Others may exist.

a) p. 8427, line 24: add a comma after citation, and change "Though" to "though".

b) p. 8428, line 21: add "s" after "dominate."

c) p. 8432 line 9, ($p<0.05$): The p-value should be "greater than" 0.05 to be not significant.

9. Other sections needing clarity:

a) p. 8429, hypothesis 1: This first hypothesis isn't specifically addressed in the discussion.

b) p. 8429, first paragraph: Clarify altitude changes for sampling sites in addition to the region.

c) p. 8431, lines 1-11: Are the Pacific westerlies the dominant wind pattern for Oct-Dec? What brings moisture to region if not the trade winds?

d) p. 8433, line 22: Rozanski et al. (1993)have a more updated global meteoric water line than Craig (1961).

e) p. 8434, lines 22-24: Clarify differences in the temperature ranges in the study area versus those referenced. It's not clear from the text that the lapse rates for the other mentioned studies are also in the tropics and might experience similar temperature changes with elevation(although reading the reference list clarifies that a little bit).

f) p. 8435, lines 9-11: Connection between times of SE trade winds and changes in stable isotope composition is not clear in Figure 2. Make relationship between the timing of the seasonal precipitation patterns and changes in the data more clear.

g) p. 8437, line 23: Sentence "Elevated deuterium excess values..." is missing a verb.

h) p. 8438, line 7-8: "...but still revealed no significant effect" This point is unclear as presented.

i) p. 8438, lines 12-19: The paper would be better off stating this information up front. Acknowledge that the monitoring period was short at the beginning, and strengthen the argument through elaboration with other data sets.

References cited:

Rhodes, A. L., Guswa, A. J., and Newell, S. E. (2006) Seasonal variation in the stable isotopic composition of precipitation in the tropical montane forests of Monteverde, Costa Rica, Water Resour. Res., 42, W11402.

Rozanski, K., L. Araguas-Araguas, and R. Gonfiantini (1993), Isotopic patterns in modern global precipitation, in Climate Change in Continental Isotopic Records, Geophys. Monogr. Ser., vol. 78, edited by P. K. Swart et al., pp. 1– 36, AGU, Washington, D. C.

Scholl, M. A., Shanley, J. B., Zegarra, J. P., and Coplen, T. B. (2009) The stable isotope amount effect: new insights from NEXRAD echo tops, Luquillo Mountains, Puerto Rico, Water Resour. Res., 45, W12407.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 8425, 2012.