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Comment on hess-2022-294

Anonymous Referee #2

Referee comment on "Soil–vegetation–water interactions controlling solute flow and chemical weathering in volcanic ash soils of the high Andes" by Sebastián Páez-Bimos et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-294-RC2, 2022

This manuscript evaluates soil water and solute fluxes in two soil profiles under two different vegetation, i.e., the cushion-forming plants (CU- UR) and tussock grasses (TU-UP). This evaluation was based on measurements of soil water content, water flux density, and solute concentrations and the outputs from the well-known HYDRUS-1D model. These measurements and modeling allowed for evaluating the role of soil water balance on soil chemical weathering, which was one of the paper's main objectives.

Overall, the paper is well-written and suits the scope of HESS. Nevertheless, essential points must be included or further analyzed, especially in the methodology and discussion sections. These major points are described below, and a few minor considerations are described afterward.

In the introduction, the statement "we hypothesize that vegetation type has an impact on water and solute fluxes (...)'' does not suit well as a hypothesis since it has been well demonstrated, especially in the context of root water uptake modeling. Therefore, the hypothesis should be stated just as a particular case for these two vegetation types, not as an overall case.

There needs to be more information about how ETa and ETp were determined. Only lines 229-230 say that "ETa was derived from potential evapotranspiration (ETp) according to the surface pressure head and soil moisture'' and in the supplementary material 3 it is stated that "... ETp [was] based on the Penman-Monteith equation''. Notice that ETp from the Penman-Monteith method requires values of parameters such as crop canopy resistance and albedo. What were the values for each vegetation type? Furthermore, ETp is usually partitioned into potential transpiration and soil evaporation in hydrological modeling. Therefore, the authors should also shortly describe this and present the values of the parameters for each vegetation.

The methodology lacks information about root measurements, yet Figure 3 shows the vertical distribution of root diameter and abundance. Only the maximum rooting depth for each vegetation is given (lines 133--134). The authors also need to show how they determined the relative distribution of root length density over depth. How was it considered in the HYDRUS model? What about the transpiration reduction function? As far as I know, Hydrus-1D has two options for transpiration reduction functions: the Feddes and Jarvis model. I would like to know about values used for the empirical threshold parameters and if they can impact the simulation results.

At the beginning of section 5.1, the authors compare ETa from CU-UR and TU-UP and cite values from other studies(in the second paragraph) but do not explain why ETa from CU-UR is higher than ETa from TU-UP. This difference is enhanced in dry periods but is also not explained. This discussion is brought back only in the fourth paragraph. First, in my opinion, this discussion should be placed right after the first paragraph. Second, the authors show some evidence for the higher (lower) ETa from CU-UR (TU-UP) but need to address why this happens. For instance, the authors should explain why in 2A horizon under tussocks, which has about 50% of the roots (roughly looking at Figure 3f), the highest soil moisture is observed, but annual ETa is well below ETp. Overall, the discussions are mainly based on soil water content, but when it comes to actual transpiration, one should look at the soil pressure head. Thus, the differences in soil hydraulic functions between soil layers and the two soil profiles need to be considered in the discussions. Papers related to root water uptake modeling might be useful to enhance these discussions.

The discussion about soil water flux needs further analysis (section 5.1, 494--506). The authors attribute the differences in vertical water fluxes and deep drainage to the vertical distribution of soil water storage. However, notice that soil water content distribution is also affected by water flux. Also, stating that "soil water storage capacity is limited by lower θT AW and KSAT'' is misleading since a high hydraulic conductivity promotes the reduction of soil water content in the soil layer. I think this discussion should be based on the soil hydraulic functions from each soil profile, considering the hydraulic conductivity and soil retention curve rather than on soil water content or soil water storage.

Minor comments:

1) l187. There is no Figure 2e.

2) l218. The Kling-Gupta efficiency (KGE) may not be well known as the other measures for goodness-of-fit and may need some description and citation.

3) l228. What do you mean by surface pressure head?

4) l334--336. These relations are not clear to me. Can we see them in any table?

5) l410. Measured or simulated values in Table 3?

7) Fig 7. The line colors for each soil horizon are hard to recognize in the figure.

6) l494. ``There is approximately 3-fold less water flux transmitted from the A to the underlying horizons under cushion plants (Table 3, Fig. 7b)''. It does not match to what is shown in Table 3.

7) The Equation 1 from the supplementary material 3 must have the sink term for root water uptake.