Earth Surface
Dynamics
Discussions

Interactive comment on "Landslides as geological hotspots of CO_2 to the atmosphere: clues from the instrumented Séchilienne landslide, Western European Alps" *by* Pierre Nevers et al.

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The manuscript by Nevers et al. entitled, "Landslides as geological hotspots of CO2 to the atmosphere: clues from the instrumented Séchilienne landslide, Western European Alps" reports new measurements of groundwaters emanating from a large landslide. In addition to major element concentrations, the authors also report measurements of the isotopic compositions of dissolved sulfate and strontium, which are used to help apportion the solute load between different lithologic sources. The solute source partitioning provides new insights into flow paths through the landslide as well as the effect of local chemical weathering on atmospheric CO2 levels. Overall, I think that this manuscript

is appropriate for publication in *Earth Surface Dynamics* after some minor revisions.

To me, one of the most exciting conclusions of this manuscript was that the detection of gypsum dissolution in some samples implied water flow through the Sabot faults. This result demonstrates that detailed hydrochemical measurements and mixing models can be used to improve our understanding of flow through the subsurface. The interpretation that some proportion of the solute load is sourced from gypsum dissolution also impacts the other main conclusion of the paper: that landslide weathering is a source of CO2 to the atmosphere. Multiple previous papers have come to this same conclusion recently and they are appropriately highlighted in the Nevers et al. manuscript.

Given how important the detection of gypsum dissolution is to the Nevers et al. manuscript, most of my main comments concern the mixing model that the authors use to come to this conclusion. Firstly, I am slightly confused about the rainwater correction that the authors employ in this study. I get the concept of using chloride critical values, but I am slightly concerned that the assumption of spatially and temporally uniform evapo-transpiration is not appropriate for this specific study site. The data presented in Figure 3 show a wide range of chloride concentrations, including some below the chloride critical value. How does such a range arise if all samples are presumed to originate from rainwater with a constant concentration of Cl that undergoes the same amount of evapo-transpiration? It is also well known that rainwater element to Cl ratios can be significantly different than seawater element to Cl ratios especially for sulfate (e.g., Stallard & Edmond 1981), which is an important ion in the present study. The authors also acknowledge that some Cl remains after they perform the correction, which implies that there is an additional solute source. However, this additional source is not included in their subsequent mixing model, which is concerning to me as this might be a source of error.

The 3 component mixing model the authors utilize is based on Na, Sr, and Sr isotopic ratios as these are expected to behave more conservatively. I agree with the authors

about this and trust that their mixing model yields reasonable estimates for the fraction of Sr sourced from each end-member (with the caveat that they ignore the source of excess Cl as mentioned above). However, the extrapolation of the mixing results for Sr to Ca and Mg effectively assumes conservative mixing (Equation 8), which negates the whole motivation for performing the mixing calculations with Na and Sr alone.

The manner in which some of the end-member ratios are defined is concerning to me as there may be some statistical issues and/or circular logic applied. For some end-members, linear regressions of solute data are extrapolated to yield constraints on their characteristic elemental ratios. For example, the text on line 638 describes regressing element to sulfate ratios versus Ca+Mg to sulfate ratios. Since sulfate is the denominator for both ratios, there is potential for spurious correlation that might affect the best fit line and thus the calculated end-member ratio. Similarly, since the sulfate to Sr ratio of the silicate end-member is estimated from the data, the finding that the d34S correlates with the fractional contribution of S from this end-member might be forced to be the case as oppose to being an entirely independent test of the mixing model. Given the large range of d34S values observed in the solid-phase data, I am somewhat surprised that a single d34S value for sulfide mineral weathering can be applied to all of the water samples.

My recommendation would be for the to employ a more robust mixing model in their revised manuscript that (1) includes atmospheric deposition as a solute source (as oppose to performing a fixed correction beforehand), (2) includes an additional end-member besides atmospheric deposition that supplies Cl, and (3) uses all of the major elements and either allows for secondary mineral formation (e.g., Bickle et al. 2015) or assumes congruent weathering. This model should also test broader end-member ranges that rely less heavily on the linear regression approach. My concern is that the current way in which gypsum dissolution is being identified is not the most robust. Since detecting and quantifying gypsum contributions are so important to the main conclusions of the manuscript, I think that a more robust assessment of the data, either

through an improved mixing model or other technique, would greatly strengthen the manuscript.

Minor comments:

Line 111 - I do not know what "inertial circulation" means and would appreciate a brief definition here.

Line 395 - I am not sure that figure 5 is a histogram. Is it not a stacked bar plot? Also, one standard deviation of the mixing model results may not be a sufficient representation of the uncertainty. With the Monte-Carlo approach that is applied, there is no reason that one of the more "rare" solutions could not be the most accurate. Focusing on the median (and results nearby) will be sensitive to the assumption of a priori distribution shapes for each end-member.

Figure 7 - A more complete definition of R and Z in the figure caption would be useful.

References

Stallard, R. F. & Edmond, J. M. Geochemistry of the Amazon 1. Precipitation chemistry and the marine contribution to the dissolved load at the time of peak discharge. Journal of Geophysical Research 86, 9844–9858 (1981).

Bickle, M. J., Tipper, E. D., Galy, A., Chapman, H. & Harris, N. On discrimination between carbonate and silicate inputs to Himalayan rivers. American Journal of Science 315, 120–166 (2015).