

## ***Interactive comment on “Landslides as geological hotspots of CO<sub>2</sub> to the atmosphere: clues from the instrumented Séchillienne landslide, Western European Alps” by Pierre Nevers et al.***

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In this manuscript, Nevers and colleagues present major element chemistry and isotopes ratios ( $^{87}\text{Sr}/^{86}\text{Sr}$ ,  $\delta^{34}\text{S}$ ) measured in different water types from a highly instrumented landslide (Séchillienne) in France. This study includes a comprehensive dataset and highlights the coupling of pyrite oxidation and carbonate dissolution. I feel the manuscript is interesting and fits the journal. But several points need be addressed or clarified to increase the strength of the paper:

The evidence from the rocks: Nevers and colleagues did a nice job by using the  $^{87}\text{Sr}/^{86}\text{Sr}$ ,  $\delta^{34}\text{S}$  of the rocks to constrain the interpretation on dissolved species in

C1

water. However, more data on chemical/minerology composition of rocks might be necessary, e.g. the pyrite and carbonate abundances. It might be interesting to know how deep is the reaction front of pyrite (if samples from several boreholes are available). Also, whether weathering is driven by carbonic or sulfuric acid (as shown in Fig. 5) is related to the pyrite and carbonate abundances in rock. Could gypsum be a weathering product of pyrite weathering? Since the isotope values were reported, I suspect the solid samples might be still available to make the analysis.

The discussion on hydrology: I like the authors' approach in section 5.2 on how this source identification may help to refine the hydrogeological model. But I feel the authors could discuss more about how the hydrological process may affect the chemistry of different water types. For example, is it possible that outflow S10 with low elemental concentrations represents an interflow, where pyrite has already been depleted in surrounding rocks and samples from G1 and G2 represent deeper groundwater where pyrite oxidation is occurring?

Some specific comments:

Line 35: I agree that silicate weathering by sulfuric acid does not directly influence atmospheric CO<sub>2</sub>, but I will argue it will reduce the potential for CO<sub>2</sub> sequestration by silicate weathering.

Line 51: some references should be added to guide readers.

Line 278: The authors showed a more complicated mass balance approach later, then is the correlation from atmospheric input necessary here? The atmospheric input could be another endmember in the mixing model.

Line 364: I didn't find the label (a-d) in the figure. The gray bar in Fig. 4a (left upper panel) needs explanation.

Line 420: I love this figure. I think some quantitative results should be summarized in the abstract.

C2

Line 460: It is better to use another color for the river

Line 535, 536: These two citations are not listed in references.

Line 538: In general, I agree with the authors. But the significance of such feedback really depends on the pyrite and carbonate abundances in bedrock.

Table A1. Are the dissolved oxygen data available? Given the importance of pyrite oxidation, such data might be interesting.

Figure C1: the y label is unreadable.

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