

Interactive comment on “Landslides as geological hotspots of CO₂ to the atmosphere: clues from the instrumented Séchillienne landslide, Western European Alps” by Pierre Nevers et al.

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Answer to Reviewer #2.

- 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 water. However, more data on chemical/mineralogy 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

C1

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.

We indeed have additional information on the local rock composition, and some text material will be added in the text, part 2.1 "Geological setting":

"Borehole logs are available within the instability (Lajaunie et al. 2019). Within these logs, observations have shown that the rock formations below the slope are relatively unstructured, and pyrite is heterogeneously distributed therein. Rock samples along this borehole seem to have been subjected to oxidizing conditions, but no clear sulfide reaction front is present at the scale of the instability".

In addition, unpublished petrological observations on thin sections from these boreholes, as well as associated mineralogical analyses based on X-ray diffraction (XRD) have shown the presence of pyrite disseminated within the rocks, but with no particular association with calcite. XRD analyses does not show any evidence for gypsum in the sampled rocks. In support of these observations, the work of Vallet et al., 2015 showed by inverse modeling that sulphates in waters from the unstable zone (UZ) originate essentially from pyrite. Information from this work will be added as supplementary material, featuring in particular thin sections photographs and XRD spectra.

According to the saturation indices calculated for the spring waters, the saturation allowing the precipitation of the gypsum was never reached. Thus, the simplest interpretation is that the presence of the signature of gypsum in the waters is linked to the dissolution of an "external" source, rather than to a weathering product of pyrite. The possibility that gypsum is a pyrite alteration product is unlikely and the manuscript will not be modified about this topic.

- 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 elemen-

C2

tal 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?

As for the specific question of the reviewer regarding spring S10, we emphasize that this outflow represents a sample taken in a "stable" area, above a lowly weathered and slightly fractured basement. In contrast, G1 and G2 are located in the unstable context of the slope, where the basement is destructured. Thus, the weathering degree of rocks and minerals (pyrite weathering at the origin of sulphate concentrations in the water) will be higher at G1 and G2 with respect to S10. This explains the lower sulfate contents at the level of S10. Furthermore, interpreting S10 as an interflow and G1 and G2 as originating from deeper groundwaters does not seem to us like the simplest scenario, as topographically, outflows G1 and G2 are higher than S10. The unstable zone actually consists in a rather superficial context for water circulation (Vallet et al., 2015a), characterized by the quasi-absence of deep groundwaters.

This information will be integrated in the text and we will extend the discussion on hydrogeological pathways in the discussion section.

Specific comments:

- Line 35: I agree that silicate weathering by sulfuric acid does not directly influence atmospheric CO₂, but I will argue it will reduce the potential for CO₂ sequestration by silicate weathering.

Yes, we agree. We will modify the sentence to reflect this fact.

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

These references will be added: Vengeon, 1998; Meric et al., 2005; LeRoux et al., 2011; Guglielmi et al., 2002; Vallet et al., 2015; Lajaunie et al., 2019.

- Line 278: The authors showed a more complicated mass balance approach later, then is the correlation from atmospheric input necessary here? The atmospheric input

C3

could be another endmember in the mixing model.

As replied to reviewer 1, we believe that this would overly complicate the mixing model, without specifically addressing the issue related to this correction - and rightfully raised by reviewer 1: that a larger fraction of Cl (larger than Cl_{crit}) might be accompanied with cations such as Na. Consequently, we will not include another end member in the mixing model, but we will refine the correction done beforehand.

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

Labels a-d will be added to the figure, and some legend will be added to the grey bar in Fig. 4a ("Jurassic carbonate").

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

Quantitative results will be summarized in the abstract.

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

The color of the river will be changed.

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

Citations will be listed in references.

Fletcher, R. C., Buss, H. L., and Brantley, S. L.: A spheroidal weathering model coupling porewater chemistry to soil thicknesses during steady-state denudation, *Earth Planet. Sci Lett.*, 244, 444-457, <https://doi.org/10.1016/j.epsl.2006.01.055>, 2006

Behrens R., Bouchez J., Schuessler J. A., Dultz S., Hewawasam T. and von Blanckenburg F. (2015) Mineralogical transformations set slow weathering rates in low-porosity metamorphic bedrock on mountain slopes in a tropical climate. *Chem. Geol.* 411, 283–298, <https://doi.org/10.1016/j.chemgeo.2015.07.008>, 2015

C4

- 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.

Yes, we agree. We will add "provided that enough carbonate and pyrite is present in the bedrock".

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

There are no data available for dissolved oxygen.

- Figure C1: the y label is unreadable.

Y label will be arranged to be readable.

Please also note the supplement to this comment:

<https://esurf.copernicus.org/preprints/esurf-2020-42/esurf-2020-42-AC3-supplement.pdf>

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2020-42>, 2020.