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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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Answer to Reviewer #1.

- 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 evapotranspiration 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 evapotranspiration? 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.

We agree with the reviewer that the way we were quantifying the contribution of non-rock sources to the spring cationic load can be improved. Indeed, some springs show a fairly high Cl concentration (> 100 μmol/L). A closer look at the data show different sources of chloride, one of them being clearly linked to release of nitrate and therefore to anthropogenic activities (agriculture, domestic input and/or use of road salt). This nitrate-rich component is particularly prominent in the stable part of the slope, where a couple of villages are present. In the unstable zone, an additional source of chloride is also found, but not linked to nitrate. In the most unstable zone of the landslide, we propose an additional source of Cl derived from the leaching of bedrock minerals, and thus to the release of Na or K.

To differentiate these different inputs of chloride, in the revised manuscript, we will include a new scenario in the inversion scheme, in addition to the previous scenario where Cl was just contributed by the atmosphere (subtraction of the critical Cl*): in this new scenario all Cl will associated with Na such that it will be necessary to subtract from the Na budget the whole Cl amount for each spring. This will obviously translate in additional uncertainty in the output parameters ("R" and "Z" parameters).

Regarding the reviewer's suggestion that we should rather "includes atmospheric deposition as a solute source (as oppose to performing a fixed correction beforehand)", we emphasize that this would not necessarily solve the issue of non-cyclic sources of Cl - we would still need to make a strong assumption about the X/Cl ratios of these sources. This is why we prefer to keep this "fixed correction beforehand", and to modify it to address the - fully valid - concern of the reviewer. We also note that the new

scenario will place an "upper bound" on the correction that should be performed on Na concentrations, as some Cl could still be derived from other sources than NaCl. As a consequence, we believe that the combination of our previous scenario and of this new one will constitute a sort of "sensitivity analysis" allowing us to explore the whole range of possibilities for the impact of Cl sources on the cationic load of the springs at Séchilienne.

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 endmember (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.

This is a fair point too. To address this comment, we will significantly modify our inversion scheme by: (1) using the actual composition of local carbonate rocks to convert Sr fractions into Mg and Ca fractions (based on new major and trace element data obtained on HCl and acetic acid leachates of local carbonate bedrock samples); (2) including an explicit, quantitative treatment for the precipitation of secondary carbonates based on the method of Bickle et al. (2015). To that effect, we will determine the local mixing array between the different rock type based on new major and trace element data obtained on local carbonate and silicate bedrock samples (acetic acid and HCl leachates, residues, and bulk rocks; see Figure 1 below). These major modifications will relax a strong assumption of the previously used model - namely that the composition of the carbonate end member was an "adjustment variable".

Figure 1: local mixing array (mixing line is the blue dotted line) between the different rock types (the black square corresponds to one of the rock samples used to constrain this mixing array; the other rock samples are not visible at this scale) in the Sr/Ca vs. Na/Ca space based on new major and trace element data obtained on local carbonate

and silicate bedrock samples (including quantitative treatment for the precipitation of secondary carbonates based on the method of Bickle et al. (2015)). The colored circles correspond to the "raw" composition of springs, the colored triangles to their composition corrected for gypsum inputs, and the colored stars to their composition corrected for secondary carbonate precipitation. The colored lines reflect the evolution of the water composition upon precipitation of secondary carbonates. Symbols colors have the same meaning as in the submitted manuscript.

- 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 endmembers, 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.

We are not sure to understand fully the reviewer's concern. As for the extrapolation of "linear regressions of solute data" generally speaking, we believe that this is a pretty conventional method yielding robust results when correctly performed (as was done by the reviewer himself and his co-authors in previous studies; see the determination of the rain end member in Torres et al., GCA, 2015). As for the specific case of the gypsum end member in our work, again we do not see where the issue is. Our rationale is as follows: (1) some springs are only very marginally affected by silicate inputs (hence, **in the case of Séchilienne** by sulfide inputs; see Fig. 4d of the previous version of the manuscript) as shown by their 87Sr/86Sr ratios < 0.710 (these are springs S12-S21); (2) data from these springs do form linear trends in X/ SO4 vs. (Ca+Mg)/SO4 diagrams, which in this type of diagram implies that their composition result from a binary mixture; (3) the SO4-rich end member of this mixture is gypsum, the composition of which lies by definition at (Ca+Mg)/ SO4 = 1. Thus the extrapolation of these linear trends at (Ca+Mg)/ SO4 = 1 does constrain the X/ SO4 ratios of the gypsum end

member. In the revised version of the manuscript, we will better explain this approach, in particular by modifying Fig. C2 (see Figure 2 below); in its new version, Fig. 2 will feature only four panels with 87Sr/86Sr, Sr/ SO4, Ca/ SO4, and Mg/ SO4 vs. (Ca+Mg)/ SO4, with only data from springs S12-S21 plotted. We hope that some additional text and a more focused figure will help the reader understand this approach.

Figure 2 (new version of Fig. C2 of the submitted manuscript): Mixing diagrams of E/SO4 (with E = Sr, Mg, Ca) and 87Sr/86Sr vs. (Ca+Mg)/SO4 used to determine the E/SO4 (and then E/Sr ratios) ratios of the gypsum end member.

- Similarly, since the sulfate to Sr ratio of the silicate endmember is estimated from the data, the finding that the d34S correlates with the fractional contribution of S from this endmember 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.

Again, we are not sure to understand the reviewer's concern here. Fig. C3 does show an independent test of the inversion outputs: The X-axis is not constrained by any isotope ratio, while the Y-axis is "purely isotopic". The isotope composition of sulfur is indeed used to constrain the structure of the inversion model (Fig. 4d), showing that at Séchilienne it can be safely assumed that sulfides are most likely physically associated to silicate minerals, and thus that a SO4/Sr ratio can be defined for the silicate end member (and not for the carbonate end member). But this is a rather qualitative constraint on the model structure, and not a quantitative constraint on the model output themselves which would induce some "circularity" in Fig. C3. As for the variability in solid phase d34S data, we would first like to emphasize that the correlation of Fig. C3 is far from being perfect, such that some of the scatter at least could well be attributed to this type of variability. However, it should also be reminded that small rock samples are expected to be much more variable than waters (and end members determined thereby) because of the naturally integrative nature of the latter.

<p style="text-align: center;">C5</p>

Text: Line by line detailed comments:

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

"Inertial circulation" here refers to the inertial behavior of groundwaters in aquifer. This term means that these waters exhibit a slower transit through the rocks, resulting in a slow response of the flow rate at the outlet in response to rainfall. By contrast, a "re-active system" shows a quick response. Here, the behavior of the aquifer is influenced by rainfall over long durations, showing its inertial behavior.

We will add a sentence to explain this term.

- 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.

The reviewer is right, this is a stacked bar plot and we will change the text accordingly. Showing uncertainty in a stacked bar plot is challenging, but we will add whiskers to the figure to express the level of uncertainty associated to these estimates (e.g. 16th and 84th percentiles).

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

This will be added.

Please also note the supplement to this comment:
https://esurf.copernicus.org/preprints/esurf-2020-42/esurf-2020-42-AC2-supplement.pdf
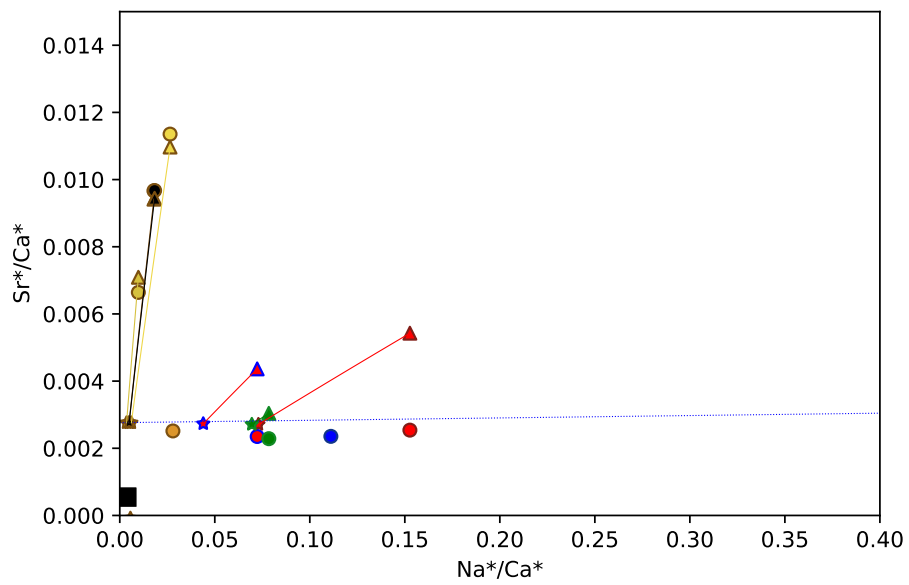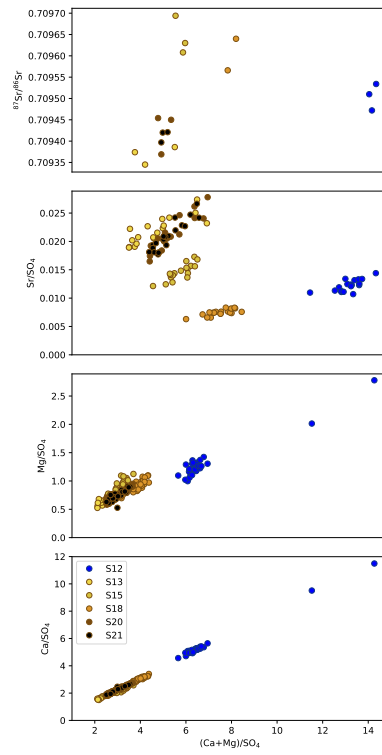
<p style="text-align: center;">C6</p>

C7

Fig. 1.

Fig. 2.

C9