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Comment on esurf-2021-87

Anonymous Referee #1

Referee comment on "The effect of lithology on the relationship between denudation rate and chemical weathering pathways – evidence from the eastern Tibetan Plateau" by Aaron Bufe et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-87-RC1, 2021

This manuscript explores how the relationship between riverine solute geochemistry and denudation rate varies between catchments with different bedrock compositions. The authors find that the concentrations of silicate-derived cations are relatively constant with increasing denudation whereas contributions from carbonate weathering and sulfide oxidation increase with increasing denudation rate. The authors also find differences in sulfate concentrations between lithologies at a given denudation rate, which affects the calculated amount of CO_2 drawdown.

The results of this study are fairly non-controversial. Nevertheless, it is nice to actually observe in data some patterns that may have been predicted/expected. Moreover, many similar datasets lack the tight constraints on denudation from ^{10}Be measurements that are available for this study. While one could nitpick about some of the assumptions that go into the mixing model (i.e., no secondary mineral formation and the congruent dissolution of bulk silicate rock), I think the conclusions that the authors come to are the most parsimonious and that their approach is sufficient to explain the major trends in the data. Accordingly, I think the manuscript is appropriate for publication after some minor revisions. In particular, there were some methodological details that I was confused about that could be explained better in a revised version of this manuscript.

Line 42: It is probably better to be more precise here and state that carbonate weathering by carbonic acid is CO_2 neutral over timescales longer than the characteristic timescale of carbonate precipitation in the ocean.

Line 43: I would recommend against the Lasaga 1984 reference here. There are many other options out there that report measurements of silicate mineral dissolution rates. I would also suggest Johnson et al. (2019) as a more recent reference on pyrite oxidation kinetics. Lastly, it might also be worth citing the work by Kanzaki et al. (2020) on the reactive-transport modeling of silicate weathering and pyrite oxidation.

Johnson, Aleisha C., et al. “Experimental determination of pyrite and molybdenite oxidation kinetics at nanomolar oxygen concentrations.” Geochimica et Cosmochimica Acta 249 (2019): 160-172

Kanzaki, Yoshiki, Susan L. Brantley, and Lee R. Kump. “A numerical examination of the effect of sulfide dissolution on silicate weathering.” Earth and Planetary Science Letters 539 (2020): 116239

Line 50: It might make sense to cite Ibarra et al. 2016 here as well as it also compares basaltic and granitic weathering fluxes.

Ibarra, Daniel E., et al. “Differential weathering of basaltic and granitic catchments from concentration–discharge relationships.” Geochimica et Cosmochimica Acta 190 (2016): 265-293.

Line 144: My interpretation is that, depending upon the geologic map data, one of three different potential silicate end-members was used for each river sample as opposed to, for example, trying all three different silicate end-members for each catchment. I would appreciate a very clear statement about which data constraints were applied to which catchments just to avoid any confusion.

Line 184: “We corrected all major elements for atmospheric inputs…”. I am confused by this. I thought that rainwater was an end-member in the set of mixing equations described at the start of section 3.2. This sentence here makes it sound like the data were corrected for rainwater contributions and then inverted for carbonate vs. silicate contributions. It would be helpful if the authors could clarify their exact approach.

Line 192: “…we did not consider the hot spring end-member in finding the best-fit model in the inversion”. This confused me. The start of section 3.2 describes a four end-member mixing model (silicate, carbonate, rain, and hydrothermal inputs). However, this sentence makes it seem like hydrothermal inputs were completely ignored such that the authors actually use a three end-member mixing model. If that is the case, I think it is very confusing to describe a hydrothermal end-member only to ultimately ignore it. Again, it would be helpful if the authors could clarify exactly how potential hydrothermal contributions were considered.

Line 284: I am not sure if the authors *have* to make this argument that concentrations are proportional to fluxes. I think it is best to stick to what is actually measured (i.e., concentrations and concentration ratios) as opposed to making untested assumptions about discharge variations based on imperfect proxies (mean annual rainfall) that do vary quite considerably (factor of 3) given the range of concentration variability.

Line 307: "… to the dissolution of soil waters by fluids from other parts of the landscape…". This sentence was confusing to me. I am not sure how fluids dissolve soil waters. I recommend that it be edited for clarity.

Line 316: It might make sense to cite Kemeny et al. 2021 here as well given that they also looked at seasonal changes in the carbonate weathering fraction at a similar site.

Kemeny, Preston Cosslett, et al. "Sulfate sulfur isotopes and major ion chemistry reveal that pyrite oxidation counteracts CO2 drawdown from silicate weathering in the Langtang-Trisuli-Narayani River system, Nepal Himalaya." Geochimica et Cosmochimica Acta/294 (2021): 43-69