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Interactive comment on "Oxidation of sulphides and rapid weathering in recent landslides" *by* R. Emberson et al.

Anonymous Referee #3

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The authors argue that landslides place unweathered material at the land surface, exposing pyrite to oxidation which drives weathering. They discuss many implications.

The paper was interesting and this phenomenon deserves attention because it is important. I have seen dissolved Fe emanating from beneath landslides in Puerto Rico (and soon thereafter precipitating), right after a landslide, so I know this happens and the central idea is not surprising. Pyrite oxidizes fast when exposed to oxygen and water and especially when bacteria are involved. So that is not that surprising. I think what the authors are trying to do that is interesting is quantify this effect by looking at different watersheds with different densities of landslides. This is an interesting idea. But I don't think they quite nailed down their story. I have a hard time even knowing exactly what they did show, and what this means exactly. I suggest that the authors work hard to probe their data more. With that effort, their results will be more impactful.

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Discussion paper

Part of the problem is that much of the phrasing is a bit vague and confusing. I got tired trying to figure out what they meant. Even the abstract has such phrasing. For example, this phrasing in the abstract is awkward and unclear and should be revised: "Bedrock landslides create conditions for weathering where all mineral phases in a lithology are initially unweathered within landslide deposits, and therefore the most labile phases dominate the weathering at the outset and during a transient period of depletion." I can't really read the abstract and get the main point. Given that I am not surprised about pyrite oxidation being accelerated by landslides. . .what is the big take-home message that they can show? One interesting aspect that is not explained clearly is why Taimali operates differently than WSA: shouldn't this basic difference be in the abstract?

I do think the authors should rework their paper before final publication. Here are some points of various importance to consider:

1. The key question in my mind is the depth to which landslides scour the landscape in comparison to the depth of the pyrite depletion zone and the carbonate depletion zone. The authors sort of mention this but not fully. Where are the reaction fronts for pyrite and carbonate in these rocks? And what is the depth to which landslides scour? Often pyrite is oxidized down to the water table.

2. Please discuss the calculation of bicarbonate by charge balance. This would usually have a large or reasonably large error just because it is a small difference between larger numbers. But, for example, the calculation could also have error due to neglect of organic anions in the waters, or any other anions. What is the error in bicarbonate? (and it should not be labelled DIC, it should be labelled bicarbonate). I see that the measurements of sulfate and chloride are each +_10%...seems like the calculation of bicarbonate by difference might have big error bars. In that regard, there are most likely too many significant figures on the bicarbonate (and probably some of the other element concentrations?)

3. The calculation of bicarbonate was then used to assess calcite supersaturation. I would think there is a big error in this calculated SI as well.

4. I wouldn't capitalize element names (Calcium versus calcium).

5. Generally, I would not call Si "cationic load" although we do usually refer to it as a cation simply because we analyze it with other cations. But it is present as a neutral species unless the pH is very high. For this reason, Si does not contribute to cation balance.

6. With respect to Sr: what does this mean? "peaks at 30 umol/L

7. The paper has a lot of assertions that are really undefended assumptions. Such assertions need to be defended more thoroughly: "the depth of a landslide scales with the area"...is this always true for all locations? or was Larsen reporting about one locality more than another? I am also not sure about "the proportion of calcite available correlates with the size of the issuing landslide". I think this statement could be true but doesn't have to be true. Are there data corroborating this assumption? The authors later state that "the lack of correlation between landslide size and seepage concentration suggests that the distribution of pyrite is highly heterogeneous.." I am not sure I would say the pyrite distribution is heterogeneous but I would argue that in many places there could be a pyrite oxidation front like those identified in Chigira et al. 1990; 1991; Drake et al., 2009; Brantley et al., 2013. I suppose that the base of landsliding in any given area might be somehow correlated with the depth to pyrite, but I think it is also very possible that the depth of landsliding might not correlate with the depth to pyrite.

8. The paper needs some mineral abundance data and element abundance data.

9. The authors start using the phrase "Landslide weathering efficiency" without defining it. What exactly is this?

10. Please explain why you are using Kendall's tau value?

11. Line 243, acidity means the base neutralizing capacity of a water. Do the authors mean acidity or protons? (Pyrite is a source of protons.)

12. I suggest the authors include some sort of figure or more text to defend this statement: “the lower proportion of purely silicate derived cations in landslide seepage . . .supports the interpretation that weathering in the sampled landslides is dominated by the dissolution of carbonates”.

13. I don’t understand this: “. . .a significant proportion of the sedimentary pyrite contained within the rock mass survives exhumation. . .” I think in general, pyrite moves up and out of the system like any other mineral until it gets oxidized at some depth related to the rate of erosion and the amount of water. Is this is what is meant by survival (death by oxidation)? It is generally oxidized down to the water table unless it is a very pyrite rich rock.

14. The authors argue that Na and Si do not vary with sulfate but Ca and K do vary with sulfate. They argue that this points to multiple pathways relevant for silicate weathering on a catchment scale. I don’t see how an observation about landslide seepage can tell us about the other pathways: it can only tell us about the landslide contribution, not the other contributions. I think there are nested reaction fronts. . .deeper flowpaths will interact with parent lithology and shallower flowpaths will not. The authors are arguing that landslides bring some of the deeper material to the surface: landslides are egg beaters. What we need to know is, how deep do they scour relative to where the minerals are?

15. I think this is unsupported as written: “the relationship between landslide volume and stream solute concentration will also saturate above a hypothetical maximum concentration”

16. Waters cannot be supersaturated with respect to an element. . .only wrt a mineral (unless we are talking about native sulfur or gold or some such). See line 356. This needs to be fixed.

17. I don't understand this sentence at all! I think it should be deleted or re phrased. "We describe this style of weathering as non equilibrium weathering as the depletion rates of the various minerals involved are likely to be very different."

18. What is important is not the "minority minerals" but "reactive minerals". Once the reactive minerals are removed, then the landslide material will weather like all the other weathered material in the landscape (i.e. material above the reaction front for the reactive mineral). . .unless the landslide is grinding up the material and increasing surface area, which is another possible effect. I do think that pyrite is often a minor mineral but that is not what is important about it.

19. Once pyrite oxidizes, weathering will revert from H2SO4 to carbonic plus organic acids.

20. The authors' use of "ambivalent" is incorrect: "the impact of landsliding on climate is ambivalent". I think the authors mean ambiguous.

21. I am not sure that the authors can defend the statement, "the area most affected by erosion due [to] typhoon Morakot is now a net source of CO2"

22. Conclusions: I don't understand this sentence: Five years after landsliding, carbonic acid is a weathering agent of lesser importance and the weathering of silicate mienrals is subsumed.

23. The term "lithological phases" is not correct. A lithology is a lithology and a phase is a phase. . .not sure what a lithological phase is. . .a mineral?

24. In figure 4, SiO2 is silica, Si is silicon.

25. The way the figure on page 20 is plotted is confusing. The fonts on the y axis differ above and below the break. I don't really understand what the tick marks mean on the y axis, because of the break in the axis. Why plot the two figures together in this way?

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