

## ***Interactive comment on “Short Communication: Aging of basalt volcanic systems and decreasing CO<sub>2</sub> consumption by weathering” by Janine Börker et al.***

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Summary: Basalt weathering and the associated sequestration of carbon dioxide via alkalinity fluxes is an important and often overlooked part of the overall silicate weathering story. Weathering of volcanic provinces may represent an important part of the proposed negative feedback between climate and chemical weathering. The authors explore a key detail of this process, namely the extent to which the age of the basalt affects the overall weathering fluxes. Building on prior work by several of the authors (Li et al. 2016), the authors provide evidence in this short communication that basalt younger than Holocene age tends to sequester carbon dioxide at a higher rate than would be

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expected based on prior characterisations of the kinetic limits of basaltic weathering in older basaltic units. They suggest that this is likely due to short lived effects associated with magmatism, such as magmatic CO<sub>2</sub> contribution, the input of fresh volcanic material, and geothermal-hydrothermal activity. They compare their new estimates for global CO<sub>2</sub> sequestration due to basaltic weathering with other prior estimates.

I found this an interesting and valuable contribution that is appropriate in scope for ESurf. I suggest that it should be published after moderate revision to address a few issues and improve clarity, as detailed below. Overall, the results are intriguing and I would like to see where the authors go with their work! I hope my comments are helpful. If any clarification of the comments is required, I would be happy to help.

Robert Emberson, Washington DC, 6th August 2018 (review requested 3rd August 2018)

I have ordered the following comments based on how important I think they are, with minor grammatical and typographic corrections at the end.

Comments:

1. I think it is imperative that the authors provide uncertainty / error estimates on all of their results, and describe how these estimates were made (probably in the supplemental material). Without estimates of uncertainty, the reader is unable to assess the significance of the results. Even if the uncertainty on the measurements has already been made in the prior work (Li et al. 2016), I feel it's essential that the authors also show these estimates (and additionally explain again how these estimates were derived – see comment number 5 for further discussion). Specifically, I'd like to see uncertainty estimates attached to all plotted points in the figures, in equations 2 and 4, and in the estimates in Table 1.

2. The different behavior of IVF and AVF areas based on reactivity is a novel way to explore the basaltic weathering regime, but perhaps it would be useful for some read-

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ers if the authors also explained this in the context of supply-limited or kinetic-limited weathering (e.g. Ferrier et al. 2016 *Geochemistry Geophysics Geosystems*). The IVF behavior shown in Fig 1b certainly looks like classic kinetic-limited weathering, but as the authors point out in the AVF this relationship no longer holds. I would suggest plotting the AVF data in the same way as the IVF data in Fig 1b. to see if there is a similar kinetically-limited relationship; if so, this could indicate that the boundary conditions for the kinetically limited system differ (e.g. due to less clay precipitation in younger lava bodies), whereas if such a kinetically-limited behavior is not evident (or the residuals are very large) then perhaps other effects (e.g. magmatic CO<sub>2</sub> degassing) may be relevant. Showing the reader these data could help demonstrate the effects seen, as well as making the relationships easier to understand for geochemists who work in the kinetic/supply limited paradigm.

3. The authors explain that they use the Holocene transition as a way to split the AVFs from the IVFs. This makes sense in terms of data availability, and I think it is an appropriate (if arbitrary) way to separate the data. However, the Holocene transition involved a series of global climatic changes, and I think it would be useful for the authors to explore whether this transition and the climatic changes associated with it could explain the differences observed in 'reactivity' between pre and post Holocene basaltic fields. I appreciate that the AVF and IVF areas both experience the same climate today, but I think it would be at least useful to discuss whether or not there could be a legacy effect in the IVF areas from weathering under a different climatic regime.

4. It would be helpful to provide detail of the water chemistry measurements or data involved in this study in the supplemental material. From the information provided, it is not possible for the reader to tell how variable the HCO<sub>3</sub><sup>-</sup> measurements made were, which makes it hard to judge the estimates of CO<sub>2</sub> sequestration. While I appreciate that a full assessment of the chemical composition of the rivers in question is significantly outside the scope of the study, it would be helpful for clarity if the authors discussed the following points, either in main or supplemental text: • How variable

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is the HCO<sub>3</sub> in the water? • Are the rivers super-saturated with respect to calcite? This is a fairly important issue, as there is potential for secondary carbonate precipitation to form in the rivers and soils of a catchment, which means the final estimates of HCO<sub>3</sub> flux may be biased. • What are the concentration – discharge relationships for HCO<sub>3</sub> in these rivers? If concentration is relatively high even at high discharge (i.e. are the rivers near-chemostatic? - e.g. Godsey et al. 2009, *Hydrological Processes*) then the largest storm events have the greatest importance for HCO<sub>3</sub> flux – and as a result, changes in climate across the Holocene transition (e.g. different storm frequency) may be relevant for the findings (see point 3 above). As I say above, I fully appreciate that data on these points may be lacking, and certainly addressing these points in full is outside the scope of this study – I would just suggest explaining how these issues may relate to your results, and what your assumptions are with regards to the river chemistry. This would really help the reader appreciate the results. A discussion of the assumptions could be incorporated into supplemental material.

5. This study is a useful addition to the study of Li et al. (2016), from which much (if not all?) of the data seems to be drawn. While I appreciate that the prior work is a published study, I think it is important that the authors explain their methodology in this publication too. For example, it would be useful to explain how alkalinity calculations were made, and some of the assumptions associated with the chemistry data; it would also be useful to summarise all of the uncertainty estimates made in that prior study in this study (see point 1). I notice that the Li et al. 2016 paper is open-access (much appreciated!), so I can appreciate that some researchers may feel it is sufficient to just cite the methods in the prior work. My personal preference is for as much of the relevant methodology for a given study to be described in that study as possible, but I leave this to the editor's discretion in this case.

6. A small point, but one I feel worth mentioning – the HCO<sub>3</sub>/reactivity figures in the supplementary material (lines 188-189) are a really useful accompaniment to Figure 1 in the main text, and I would suggest incorporating at least some of them into the main

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text. It really helped me understand the importance of temperature and runoff, and I think they're important enough to include. Even if you choose not to do so, it would be useful to ensure that the symbology in the figures corresponds to one another (i.e. red-blue points in the main text, red-green points in the supplement). I'd suggest using red-blue as in the main text, to avoid any issues with red-green colour blind readers.

7. It may be useful to compare these findings to those relating to weathering in aging glacial moraines, to help contextualize the importance of aging? Your results are really intriguing, and provide impetus for research questions focused on e.g. weathering in lava flows of known age (via e.g. cosmo dating) and comparing to glacial moraines would be direct comparison.

8. In line 40, you refer to 'geogenic nutrients'. I think this is a jargon term that many readers won't understand; I'd suggest defining this term before you use it. Additionally, I think this statement needs a citation to support it.

9. In a couple of locations, you separate the arid and non-arid locations based on a rainfall total of >74mm per year – why was this figure chosen? It would be helpful to have some explanation as to this number.

10. I would suggest arranging references in chronological order where there are multiple citations in parentheses, e.g. Line 43 and other locations.

Typographical / Syntax points In general I found the paper to be well written and easy to read. There are a handful of places where the language is somewhat idiosyncratic, and I've tried to make suggestions where possible. As a general point I would recommend using the active voice rather than passive voice to improve readability, but that's probably a matter of personal preference.

Line 12: Consider being more specific than the word 'information' – perhaps describe which types of data you mean.

Line 20: Instead of "from surface near material in the critical zone", consider "from

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material in the shallow critical zone"

Line 22: Remove comma after suggests

Line 23: "Active basalt areas" is jargon – I'd suggest it's best to define what you mean in the main text and avoid using jargon terms in the abstract

Line 30: "Basalt areas, despite its limited. . ." should be "Basalt areas, despite their limited. . .". Also remove the 'the' before CO2.

Line 39-40: Consider changing "The role of basalt weathering in the carbon cycle and its feedback strength in the climate system depends, besides the release of . . ." to "The importance of basalt weathering in the carbon cycle and the climate-weathering feedback loop depends in part on the release of geogenic nutrients but crucially on the amount of associated. . ."

Line 59: Replace "However, the here suggested aging effect" with "However, the effect of aging on weathering rates from a volcanic system discussed here has not been evaluated."

Line 68: Consider replacing "the fraction of the Holocene area on the total studied area" with "the proportion of total area occupied by Holocene lavas"

Line 92-93: The word order and verb agreement in this sentence is somewhat unclear – I haven't suggested a revision since I don't want to mess with the meaning, but I'd suggest revising it to clarify what you mean.

Line 100: Replace "For allowing comparison with" with "to allow for comparison with"

Line 103: Replace 'reporting' with e.g. 'describing'

Table 1: The first two columns in the table have no label? Please add a label to explain what these are.

Line 152: Consider changing the phrase 'time stamp' – it isn't necessarily clear what

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you mean.

Line 159: Replace 'Results' with "Our results". Also change 'considering' to 'exploring'

Line 160: 'Displacement' – do you mean 'emplacement'?

Line 164: I would suggest rephrasing to remove the comma.

Final paragraph: Consider re-stating your key finding in the final paragraph.

Supplementary Material: Generally I found the supplementary material to be clear and helpful. Please ensure that uncertainty estimates are included where possible. I would also suggest checking with the editor as to whether the citations in the supplementary material will be indexed or not – it may be advisable to move them to the main text to ensure they get indexed.

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Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-10>, 2018.