

## ***Interactive comment on “Reviews and syntheses: Weathering of silicate minerals in soils and watersheds: Parameterization of the weathering kinetics module in the PROFILE and ForSAFE models” by Harald Ulrik Sverdrup et al.***

**Anonymous Referee #3**

Received and published: 26 April 2019

This ms provides an overview of various aspects of the kinetic weathering model in PROFILE and ForSAFE, focussing on kinetic parameterization. The stated purpose is to describe the updated mineral kinetics database used in these models, and revisions to the brakes on weathering reactions in the model. The brakes are discussed at various junctures throughout the document, while the kinetics data are presented largely in a sizeable table appended to the main body.

In addition to the improvements in the brakes, there is value in the conceptual diagrams that provide the reader with a more complete understanding of how the weathering rate

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model works (eg. Fig 8,9,14). These are perhaps the clearest characterization of the model to date. Perhaps this could be revisited as an objective of this contribution, as much of what the paper contains is outside of the stated scope. Additionally, another addition to the model is the inclusion of reaction kinetics for OH. It would seem this would enhance the capacity to model weathering rates in agricultural soils, more so than improving rates in much of the acid sensitive areas that have been a focus point of model use to date. It seems this should be discussed, as otherwise this inclusion would have limited impact?

It seems clear that this ms originates as a report that has not had the necessary work done to transform it to a clear and coherent ms. The variety of statements about e.g. historical funding sources, contributions at different times, etc. belong in the acknowledgements and not as informal prose scattered throughout different sections of the paper. Likewise the ms is poorly put together, with mis-numbered sections, figures presented out of order, strange numbering of equations etc. The reference list is both incomplete and not reflective of the citations in the text. There are grammatical issues throughout. In the absence of line numbers, I won't point out these and other line-specific problems. Table formatting and figure captions, which can be inconsistent to the figures themselves, should also be addressed (e.g. fig 17) as should axes for which units are not always provided. Not all figures cited in text are shown.

Additional things to address: It is not adequately explained why the model fails at greater depths. This is referred to as an issue of depth, but since depth is not a parameter than is tied to weathering reactions it seems much more likely that this may be an issue of water table presence. If the water table is the issue, then certainly brakes are a necessary step to improving the rates, but only if the soils are not freely draining. A more comprehensive overview of this is needed, including how the model can address the issue of weathering reactions that occur transiently as soils are wetted after infiltrations, versus deeper soils that may be wet due to the water table but of a very different chemical composition. How are the necessary hydrological dynamics represented?

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Most of the first 8 pages, and section 4 and 3 are not needed. This contribution would carry much more potential impact if it were focussed on the improvements to the model rather than rehashing a long-history that is tangential to the stated purpose. The plethora of figures included from previous work add little value.

It is remarkable that comparing uncertain model estimates of weathering with 'equally or more uncertain field estimates' yields error of +/-5%. What is missing from the section on page 5 is that error is often much larger than this, and this is shown in some of the citations in the ms. The sensitivity of the model has been documented in different ways over the last >2 decades. Problematically model sensitivity is strongly linked to parameters that are 'unparameterizable' or parameterized in highly uncertain ways (e.g. Hodson et al. 1997 WASP). It is troubling that this work has been overlooked while focussing on select examples where there was relatively good performance of the model. This raises important questions as to whether the model is producing realistic outcomes for the right reasons.

While the effort here has no doubt resulted in improvements to the model, it is difficult to assess the potential impact of this work as no thorough demonstration of improvements in capacity are illustrated. Considering the companion paper by Erlandsson Lampa will be helpful to this, but it remains unclear what the role of the 'results' and 'discussion' sections are in their current formulation in this ms, as they don't link back to the stated purpose.

There is potential to confuse the reader. The weathering sub-model requires no calibration, but it can only be carried out after calibration of the main ForSAFE model?

Section 4.2 has been presented numerous times elsewhere. What is the rationale for its inclusion here?

Where do data in Table 1 originate?

It is unclear how equation 23 arises from eq 12, but then reverts to eq. 29.

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Equation terms are not always defined.

Page 20 and 21. How is something updated to be consistent with previous (rather than new)? It is difficult to discern the message of the remaining part of the paragraph.

Figures 16. Are the lines fitted to the observations, or generated with the model?  
Figure 19. Are these plots comparing individual base cations, with other cations with base cations as a group? The legend does not provide clarity

Figure 23. Why Figure B? Was this previously published and borrowed here?

The major result of the paper, the provision of an updated kinetic dataset is finally introduced on page 28, and receives modest attention relative to the numerous bits that came before it.

Section 5.2 and testing of the model comes here as a surprise. It was not described as an objective, and appears to be covered comprehensively in the companion paper. Its role here is unclear.

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-38>, 2019.

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