

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 #2

Received and published: 23 April 2019

The contribution by Sverdrup and co-authors is an extensive description of the PROFILE and SAFE models, extending to the more recent SAFE-type models, and focusing on the development of a new parameterization of the dissolution kinetics. The article is rather long (31 pages), with the main part of it devoted to the model description. This is an unusual format, but it appears that a joint paper has been submitted at the same time to the same volume, showing an in-depth comparison between output of the latest 2D-version of the SAFE model and field measurements (Erlandsson-Lampa et al., 2019).

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I think that this exhaustive description of the Sverdrup's models is useful, either for scientists using them for simulations of the field weathering rates, or for scientists developing similar numerical tools. The article is well written and clear. I think two points should be addressed before it can be published.

One thing that confuses me is the way Al^{3+} and SiO_2 are calculated, and the consequences of this methodology on the model output. For Al^{3+} , it is assumed to be a function of the pH only, by forcing the model to be at equilibrium with “something amorphous of unknown composition”, potentially looking like “gibbsite” (page 17). This method allows the calculation of the full speciation of Al in solution, without requiring the solving of a mass-balance equation for Al. From my own experience, I know that this assumption makes the model running faster and in a more stable mode, replacing one “critical” differential equation by an algebraic equation. This is a particularity of the SAFE model. I have no specific problem with this. But then, SiO_2 is calculated in the same way, by assuming equilibrium with a kind of “kaolinite” mineral. This is a strong assumption, since the natural waters are often largely supersaturated with respect to kaolinite, due to the very low precipitation rate of this mineral. The central question is to what extent this second assumption impacts the model results? I suspect it is not insignificant, given that Al and Si are the major components of the silicate minerals.

My second point is related to the “brake” functions. Several other modeling studies (Maher et al., 2009, GCA; Godd ris et al., 2010, GCA) have shown that the development of weathering profiles can be reproduced numerically, assuming that dissolution rates slow down when the percolating waters approach thermodynamic saturation. As noted by Maher et al (2009), this is the only way to account for the difference in the propagation rates of the reaction fronts for two minerals despite their very similar kinetic rate laws. Here Sverdrup and co-authors argue that the saturation of the soil solution with respect to primary minerals does not limit the dissolution rates, because the dissolution process is not reversible. Consequently, the Sverdrup's team has developed several brake functions, which limit dissolution rates and are functions of the Al^{3+} , of the Si

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concentration, and of several additional parameters. It looks like introducing empirical inhibiting factors to make up for not considering the effect of chemical affinity, but is it really more efficient than the more classical method? The only way to answer this question is to test a variety of models for the same study site. Given that the previous study also include inhibiting factors, I find that the section 6.3 is somewhat sententious, and I would open the discussion to possible future intercomparisons. Last but not least, I'm wondering whether the need for a new parameterization of the kinetic reactions is not dependent on the way Al^{3+} and SiO_2 are calculated (previous point). These are not decoupled processes. Forcing the water composition at equilibrium with respect to kaolinite may result in an underestimation of the Al^{3+} and SiO_2 concentrations in the fluid, explaining (at least partly) why "brake" functions must be defined to slow down dissolution rates in the SAFE model family.

Once these points addressed or discussed, I think this contribution will deserve publication.

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