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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 #1

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This paper is a by and large clear description of the current state of the PROFILE model (and its derivatives) together with a summary of how the model has evolved over the course of over 20 years.

It links to other papers in this issue that use the revised version of the model.

As such it is fine for publication and is a useful explanation of how the PROFILE model works. It provides necessary background for other papers in which the model is applied.

Although cited in the references, the paper Introduction and section on "Earlier development work and background" makes no reference to the papers coming out of Langan's group in the 1990s that were critical of a number of aspects of the model. This paper is written by the model authors so it is perhaps understandable why they don't refer to papers that have highlighted aspects of the model that need improving but I think this should be done. For example Langan et al. (1997) reviewed the reworking of data from dissolution studies used to generate the kinetic parameters in the profile model (rate constants and reaction orders) and the default mineral compositions used in the model and found both to be inaccurate. As far as I can tell (first paragraph of Section 4 "Theory") this has now been addressed, though due to time constraints I haven't checked any reworking of the data in the many dissolution papers cited in the current manuscript.

Fig. 1 appears to show positive and negative feedback loops – it might be clearer to call these by those names.

Despite the lengthy reference list it appears to be incomplete – e.g. I couldn't find the Sverdrup and Stiernquist 2002 reference listed which is a shame as it appears to be a paper that describe the modelling philosophy of the Sverdrup group

I would argue that the PROFILE model isn't widely used – it is extensively used by Sverdrup and his group – see Steady-state weathering rate models" section

As stated in the paper, when assessing model performance the model authors are "comparing uncertain model estimates with equally or more uncertain field estimates at best". This is a fair comment. As the current manuscript describes the kinetic workings of the model and acknowledges uncertainty, it would be useful to include a comment on that major source of uncertainty facing all weathering calculations – mineral surface area. The core equations in PROFILE, e.g. equation (1a) require a measure of surface area. Surface area normalised dissolution rates are multiplied by the available mineral surface area on a mineral by mineral basis. The model does a good job at producing weathering rate estimates that approximate those measured in the field but the surface area term remains a challenge. Many applications of the model assume that the distribution of surface area between minerals is equivalent to the distribution of their masses as determined by XRD. From a practical perspective it is hard to see what else can be done but it is always worth acknowledging this issue. Another issue is the total mineral surface area term. In this manuscript it is simply referred to, in previous papers it is suggested that this can be calculated from a consideration of the clay, silt and sand fractions present in samples. I'd encourage the paper authors to suggest how model users should measure this term (geometric, BET or their texture formula) and to comment on the uncertainty this introduces. If they still advocate their texture based equation for calculating surface area I would strongly encourage them to publish the data from which the relationship is derived – they have never done this and the only independent test of this equation by Langan's group showed a poor match between surface areas predicted by that equation and real measurements. The values were all of the same magnitude and so, given the uncertainty in the model, this isn't a particular issue for the model producing rate estimate similar to those measured in the field but it remains a cause for concern that perhaps the surface area term functions as a scaling factor to ensure comparability between modelled and measured rates rather than deriving rates from first principles.

Figures 12 and 13 – it needs to be clearer what the numerical values in the legend refer to

In Section 4.10 "The parameterization of the kinetic rate equations" it is stated "Of these minerals, the regressions of ∼20 have yet to be published. In due time, these will get their own proper publications. . ..". I do understand the sentiment here and the authors have my sympathies but it makes me slightly nervous since it would be good to publish the data and processing of data used in a model before publishing the model as we can't predict whether these regressions really will be published or not. Also, when the original regressions for reaction orders and rate constants were checked by

Langan's group there were errors in them. If the current model uses orders and rates that haven't been published and can't be checked it calls into question confidence in the model – with the caveat that the model does seem to predict mineral weathering rates in the right ball park compared to other models and so is presumably functioning OK. The authors make this point at the end of Section 4 "The ultimate test. . .how well they describe both laboratory experiments and field data. . ..such tests have been generally successful. . .more on this will be forthcoming as the publishing of further comparisons are made" – as above it would be nice to see these comparisons published now though I acknowledge that the paper would be too long if the comparisons were included in it.

There are minor language issues through out.

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