Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-464-RC2, 2020

Biogeosciences Discussions

Interactive comment on "Improved parameterization of the weathering kinetics module in the PROFILE and ForSAFE models" *by* Harald Ulrik Sverdrup et al.

Anonymous Referee #2

Received and published: 13 April 2020

This paper is useful in the sense that it provides a compilation of available data on mineral kinetics. Although empirical rather than mechanistical in approach, it still can have its place in applied science, but the suitability for Biogeosciences is in doubt. The manuscript is not properly executed and does not fit the format of a paper in academic journal. I noted 3 scientific problems that have to be resolved by the authors before the paper can be considered for further review. 1) Database issue. The manuscript aims to provide a kinetic database for executing some weathering models. This task is not fulfilled. A simple compilation of available kinetic data is not the same as critical evaluation of these data and expert recommendation of rate constants (regardless of the type of equation used to model the rates). Consider some analogy with thermodynam-

ics. A database used in any thermodynamic model is a product of EXPERT evaluation of available thermodynamic information data, and recommendation of definite values based on expert judgement of various experimental (or modeling - if needed) works. The same should be clearly done for kinetic database, but this step is missing or not presented in the manuscript. The table of recommended kinetic constants should contain at least some pertinent references in the way it is organized for thermodynamic databases. Example is given in L598-605. This is totally unacceptable. Original data and their quality evaluation should be presented. Another example is L 921-923: the use of inter-mineral interpolations and of analogues is totally obscure.

2) Natural application issue. The dissolution of alumosilicates, at far from equilibrium conditions at least, is controlled by activity of free Al3+(aq) in solution (unequivocally demonstrated by group of Schott, Oelkers). The binding of Al3+ to natural organic ligands decreases the activity of main inhibitor of alumosilicate dissolution and thus increases the rates. This Al complexation is not considered (or not described) in the model because simple organic ligands (carboxylates, aromatic) cannot be used to predict Al3+ speciation in the presence of complex fulvic and humic acids, as well as organo-mineral (Al, Fe) colloids. The authors mention some acids (L306-307) but what are these acids? Some soils contain 5-10 cm of organic layer and then the mineral layer occurs. These minerals will interact with DOM. The equation 13 is not justified. Why mineral dissolution should be slowing with increasing organic ligand concentration (L395)? This contradicts to a large body of knowledge on mineral dissolution, notably of alumosilicate minerals. How the secondary mineral precipitation was modelled?

3) The quality of modeling is not demonstrated. The use of Na (Figs 13, 14, 15) without considering stoichiometry and Si release rate is not suitable. Original data allowing to see the quality to modeling should be presented. The model curves should be given for other plots (Figs 16 -18). Overall, the need of the empirical adjustments (braking functions) is not clear or not presented. There are robust equations of mineral dissolution rates, cited in L 904-905

Some technical issues (to be re-evaluated at the 2nd stage): Abstract is simply impossible to read by non-experts. The braking function should be defined. L568-569: quartz does not belong to clay minerals

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