SOIL Discuss.,
https://doi.org/10.5194/soil-2018-17-RC1, 2018

Interactive comment on "Assessing the impact of acid rain and forest harvest intensity with the HD-MINTEQ model – Soil chemistry of three Swedish conifer sites from 1880 to 2080" *by* Eric McGivney et al.

Anonymous Referee #1

Received and published: 23 August 2018

General comments

The authors present a modelling study of the past effects of acid deposition and projected future effects of varying harvesting practices on the soil and soil solution chemistry of three forested sites in Sweden. This is clearly a topic of current interest as the impacts of past soil acidification recede and the potential future impacts of forest management practice become more important. The manuscript is well organised and written and is reasonably straightforward to understand. Having read the paper, I do however feel that significant amendment is needed, the reasons for this being largely

Printer-friendly version

Discussion paper

to do with the broader context of the work and the manner in which the results are presented and discussed:

1. it is not clear exactly what the purpose of the study is. Is it to demonstrate the utility of the HD-MINTEQ model for the projection of future soil chemistry under different forest management practices, to specifically make projections that can inform forest management practice, or a combination of both? The manner in which the paper is written suggests largely the latter, in which case I have reservations regarding the confidence that can be placed in the model as currently constructed as a comprehensive predictor of soil and soil solution chemistry. The lack of a specific submodel for N species transformations is in my opinion a potentially significant shortcoming.

2. It is not clear what specifically is new about this work in the context of modelling soil and soil solution chemistry. The model is state of the art for its description of ion-organic matter equilibrium interactions and the description of BC weathering, but it falls somewhat short of the state of the art in other areas.

3. Given these acknowledged shortcomings of the model, it would seem appropriate to me to place more emphasis than has been placed, on assessing the ability of the model to predict the trends and/or magnitudes in soil and soil solution chemistry. The results and discussion section effectively takes the model predictions as 'correct' in its assessment of future trends.

4. I feel that the paper would be strengthened by an assessment of the model capabilities alongside the discussion of the future projections, coupled with an assessment of where the model could be strengthened e.g. a better description of N transformations (or, of course, arguments as to why such 'improvements' would not greatly improve predictions!).

5. Finally (and returning somewhat towards my first bullet point) I cannot see any broader context - why is there a need to make these predictions in the first place? For example, can the results tell us anything about the effects of different future man-

Printer-friendly version

Discussion paper

agement practices on forest viability? I appreciate that this is somewhat beyond the scope of this paper as it relates to relationships between soil chemistry and tree growth/performance, but I do feel that some broader context is needed to emphasise the importance and usefulness of this work.

Specific comments

p3 line 23. How is the supply of N species concentrations realised over the whole simulation period? It is currently unclear how exactly N species are handled. p3 line 24. It is not clear what is meant by 'The soil pools of Al and organic C were assumed to be constant over the simulated time period. Does this mean that losses of Al and C in drainage are assumed to be completely replenished? Does the soil pool of Al mean the geochemically active pool, or is there a non-reactive component that supplies active Al by weathering? p5 line 10. Why was ForSAFE only used at Gårdsjön?

p7, line 4. It is unclear what is meant by uneven depletion rates or how the BC uptake rates were adjusted to compensate for this.

Section 2.6. My interpretation is that pH was not used in calibration? This seems unusual given the importance of pH changes as a consequence of soil management and acidification/recovery. Why was this not done?

Section 3 (Results and discussion). There appears to be no comparison of the model predictions of pH, BC and sulphate with the observations. This to me is a serious omission since the usefulness of the model in predicting the future effects of forest management depends critically on its ability to predict how the soil chemistry has responded to changing inputs and management in the past. Does the model predict the trends and/or magnitudes of the observations over time to a degree that provides sufficient confidence for it to be a useful tool for investigating the projecting future trends? If not, can potential reasons for discrepancies be suggested and means to further develop the model to address such reasons be proposed?

Printer-friendly version

Discussion paper

p10, line 4 onwards. As an explanation of a modification to the model in the light of initial runs, this discussion would be better placed close to the model calibration section.

Figures 3-5. It is somewhat difficult to follow the graphs but my impression is that pH in B1&B2 is modelled as being consistently higher than pH in E, but that the observations suggest that pH is either quite similar in both horizons or tends to be higher in E. This is an example where the model capabilities need to be assessed against the observations - is the model a useful tool for assessing what is being projected here?

Technical corrections

Figures 3-5. It is rather difficult to match observations and model results on those panels where observations are present. I suggest redrawing to make the connection more obvious - a simple way would be to colour the observation points according to horizon in the same way as the model lines (though the B1/B2 lines might need to be recoloured also).

Figures 3-5. The symbols noted in the captions do not correspond with those on the graphs - specifically the graphs have crosses, which are not listed in the captions. This needs correction. I've assumed that crosses are supposed to be open squares.

Interactive comment on SOIL Discuss., https://doi.org/10.5194/soil-2018-17, 2018.

Printer-friendly version

Discussion paper