SOIL Discuss.,
https://doi.org/10.5194/soil-2018-17-AC1, 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.

Eric McGivney et al.

gustafjp@kth.se

Received and published: 29 November 2018

RE1 = referee 1 ; AU = author's response, (y) = comment no. y

RE1(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.

AU(1): We have added the following sentences to the end of the introduction to clarify the objective of our paper: “The objectives of this paper are to (i) investigate possible long-term impacts of two theoretical future harvesting scenarios on the acidification and base cation status using a novel dynamic model, HD-MINTEQ, and (ii) compare biomass-induced acidification to the historical acidification that took place during the 20th century due to acid rain.” We have also changed some of the language in the discussion section to deemphasize these simulations as a comprehensive predictor of soil chemistryâĂŤthat was not the intent of our paper. One sentence that exemplifies our stance on this follows: “However, because the HD-MINTEQ simulations here produced stronger effects on soil Ca compared to those observed in the field (Zetterberg et al., 2016), it seems probable that the acidification effect as predicted by HD-MINTEQ may be regarded as a ‘worst-case scenario’, and that the real acidification, at least over the first rotation period, may be even smaller.”

We would also like to point out our admitted shortcoming concerning N cycling: “However, at this point it needs to be added that these conclusions may not be relevant in cases when nitrification following harvest is substantial, in which case the acidification effect could be considerably larger; this possibility was not considered in our simulations.”

RE1(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.

AU(2): The unique aspect of this work is the comparison between the acidification effect of historical S deposition, and the effect that can be expected from forest harvesting. We have tried to clarify this a little more, especially in the Conclusions section, which is partly rephrased to highlight this in a better way.

RE1(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.

AU(3): Thanks for pointing this out. It was certainly not our intent to push this model off as 100% correct. We have made some changes to the language to clarify that these were modeled simulations and that there are of course limitations to them. Below are some examples that will be found in the revised text: “. . .because the HD-MINTEQ simulations here produced stronger effects on soil Ca compared to those observed in the field (Zetterberg et al., 2016), it seems probable that the acidification effect as predicted by HD-MINTEQ may be regarded as a ‘worst-case scenario’, and that the real acidification, at least over the first rotation period, may be even smaller.” “. . .at this point it needs to be added that these conclusions may not be relevant in cases when nitrification following harvest is substantial, in which case the acidification effect could be considerably larger; this possibility was not considered in our simulations. Most Swedish forests are N-limited (Högberg et al., 2017), but increased nitrate concentrations are found in soil solution for some years after final felling. Nitrification is dependent on site productivity, which is between 4 and 8 m3 ha-1 yr-1 in the sites studied. According to the estimates of Futter et al. (2010), the total accumulated harvest effect should generally not exceed 220 and 500 meq NO3- m-2 for site productivities of 4 and 8 m3 ha-1 yr-1, respectively (Futter et al., 2010), indicating rather modest nitrification effects on the long-term acid-base status of the soils. As an example, this value represents between 5 and 15 % of the atmospherically deposited BC over a full rotation period, hence nitrification is a relatively minor proton source as compared to other processes in the forest soils under study.”

RE1(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!).

AU(4): We have tried to improve the text (i) by providing more discussion on the reasons behind the differences between model and observations, and (ii) by identifying areas where the model can be strengthened. As concerns the first of these aspects, we have provided new text towards the end of section 3.1, as follows: “For the Gårdsjön and Kindla sites, the modelled results align well with the lysimeter data, with a few exceptions. Interestingly, the model underestimated Mg2+ concentrations at both sites. This could be caused by the use of A2M estimates, i.e., that the normalization model underestimated the presence of easily weathered Mg-containing minerals. In a recent study, Casetou-Gustafson et al. (2018) compared A2M with the mineralogy obtained by X-ray powder diffraction (XRPD) for two soils that were similar to the soils studied here. They found that trioctahedral mica and hydrobiotite were consistently underestimated by A2M, which is consistent with our modelling results as these Mg-containing minerals have relatively high weathering rates. Moreover at Kindla, SO42- concentrations were underestimated. There may be several explanations, but one possibility is mineralization and oxidation of organically bound S (Löfgren et al., 2001, 2014). The delay in SO42- decrease at Kindla was, however, predicted well in the B1 horizon of the model.

For the Aneboda site, the discrepancies between model and observations were more substantial. For example, while SO42- and pH were grossly underestimated, Ca2+ and Mg2+ were overestimated. It is important to note that the lysimeter data plotted in Figures 1, 2 and 3 (previously 3, 4 and 5) are averages based on data from several lysimeters, and it has previously been observed that there are large variations in the results of individual lysimeters at the Aneboda site (Löfgren et al., 2010; Löfgren et al., 2011; Löfgren et al., 2014). As an example, for the B horizon the averaged results are based on 8 lysimeters. Three of these, nos. 7102, 7104 and 7105, had results that were clearly divergent from the others (new Fig. S4, Supplement). Dissolved SO42-, Ca2+ and Mg2+ were all considerably higher, whereas the pH was lower. Possible reasons include a net mineralization and oxidation of organically bound sulphur in response to decreased S deposition (Löfgren et al., 2001, 2014), a process which was not taken into account in the model. It may also be observed that if the results from the three lysimeters were removed, there would be a clearly improved agreement between the model and the observations."

Discussion of possible improvements is provided in the final part of section 3.3, as follows: "Although the HD-MINTEQ model provided simulation results that appear reasonable, a future task is to upgrade the model to include N transformations, so that the effects arising from e.g. N deposition and nitrification can be more accurately assessed than was possible in the current work. Further, improved estimates of the mineralogical composition through e.g. X-ray diffraction would be desirable to avoid the mismatch in individual base cations, as was observed for Mg2+."

RE1(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 management 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.

AU(5): As should now be clear from the objectives, our focus is not to present a toolbox for accurately predicting the effects of different future management practices, but rather to contribute to the discussion of these impacts by comparing different scenarios with a simplified model, by for example comparing these effects to those of historical acid rain.

RE1(6): 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. AU(6): It is held constant according to the values in Table 1. We have added the following text to clarify this: "HD-MINTEQ does not simulate N chemistry; instead dissolved NH4+ and NO3- in the different horizons are given as input data, and are held constant (Table 1)"

RE1(7): 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?

AU(7): Yes, Al and C are assumed to be completely replenished, and the soil pool of Al does refer to the geochemically active pool. We have included the word "geochemically active" to clarify the last point, also see model description in Löfgren et al. (2017), which is cited in the paper.

RE1(8): p5 line 10. Why was ForSAFE only used at Gårdsjön?

AU(8): For the purpose of this work we have relied on parameterisations made previously by two different research groups, by Belyazid and Moldan (2009) for the Gårdsjön site and by Zetterberg et al. (2014) for the two other sites. In the latter case, ProdMod was used to estimate uptake values, as is written in the text.

RE1(9): 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?

AU(9): It was. Lysimeter pH measurements were used to calibrate the model. BC concentrations were adjusted to achieve modeled pH values that matched empirical pH values. This has been added to the text in section 2.6: “The modeled values of pH, dissolved inorganic Al and other BC concentrations from 1993 to 2014 were then compared to soil solution data from the same period taken from various depths and binned into either horizon O, E, or B before being plotted.”

RE1(10): 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? AU(10): We have including new texts concerning the comparisons of model predictions with observations. C.f. replies and texts in response to comments 4 and 9.

RE1(11): 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.

AU(11): Agreed, we have moved this text to section 2.6, immediately after the description of how the model was calibrated.

RE1(12): 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?

AU(12): The pH values in B1 and B2 were consistently higher than pH in E, both for the

Printer-friendly version

Discussion paper

observations and for the model. The captions were wrong! We have changed Figures 1-3 (previously 4-6) to correct this.

RE1(13): 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).

AU(13): We thank the reviewer for the comment. We have changed the colors of the observed data markers to match the modeled horizon in Figures 1, 2, and 3 (previously 3, 4, and 5).

RE1(14): 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.

AU(14): We have made these changes, thank you.

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

Printer-friendly version

Discussion paper