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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 #3

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This paper utilizes a dynamic model to assess the future impact of hypothetical timber harvest scenarios on soil chemistry at 3 sites in Sweden and compares the simulated response to the simulated impact of acidic deposition. The main findings are that timber harvesting will acidify soils (WTH slightly more than CH) but not to the extent simulated by acidic deposition. The authors also report that weathering rates will generally increase. Overall the paper is well written and the findings are what one would expect and which have been reported elsewhere – namely timber harvesting can acidify soils through base cation removal. Therefore in that sense I do not disagree with any of the main findings and conclusions of the paper. The authors cite many of the previous papers that show essentially the same thing. However, the main question I have is whether the actual values reported are in any way meaningful. Throughout the paper the authors report changes that are relatively small (e.g. "At Aneboda, the pH across all horizons dropped by an average 0.13 (WTH) and 0.12 units (CH) compared to the NH scenario by the year 2080"). However when I look at the calibration figures and simulation figures (3-5) it is quite clear to me that parameters such as pH and base cations are very poorly matched. For example, Figure 3b shows simulated Mg in soil solution and observed values for the various horizons – they don't match very well and I don't see any clear separation of the observed data by horizon. Likewise, there is no Al3+ chemistry shown nor any description of how well the model simulations match the observed chemistry for the various soil horizons. Hence, the authors have a model that performs as expected but I have no confidence in the actual numbers nor the timeframe of the reported changes. Without a detailed evaluation of the model performance that provides the reader with some confidence that the simulated changes are in any way meaningful I cannot recommend that this paper should be published. Section 2.6 describes model calibration but looking at the figures it seems to be a very poor match for the various horizons. Other Points. 1. Quality of Figures is poor. 2. The authors show several spikes in soil solution chemistry but these cannot be validated. 3. There is no real discussion – the results/discussion section is primarily a description of the simulations and how the model is parameterized. 4. The reported weathering rate changes (see Figure 6 and 7) are so small as to be insignificant compared with other inputs/outputs (deposition/plant uptake) and of course there is no way of knowing whether this is actually happening (assumes PROFILE is correct). 5. Ignoring N dynamics is an issue – trees take up N and it can change both because of deposition and/or harvesting. 6. Conclusions – first sentence depends where in the world you are.

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