

## ***Interactive comment on “Modelling acidification, recovery and target loads for headwater catchments in Nova Scotia, Canada” by C. J. Whitfield et al.***

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**General Comments** This paper is an excellent contribution to the literature on recovery from acidification. Three aspects of this are particularly valuable, the region modeled is one of those most sensitive to acid deposition, the model was calibrated to a number of separate years, and catchment specific data on soils was used. Neither of the last two conditions are common in regional modeling applications. The paper is also well written. There are just two points of clarification and a few possibilities for “amplification” that I would recommend.

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Specific Comments First - I am confused about the target loads prediction summarized in the conclusion. As I read it, a 50% reduction from current deposition levels is expected, and this will leave just for lakes below critical thresholds (not counting four lakes below these thresholds in the pre-industrial period). But the paper goes on to state that 30% reductions from present deposition levels will help 3 of these lakes below the critical threshold. I presume this more “stringent” reduction is a 30% reduction IN ADDITION to the already agreed 50% reduction. But that is not how I read the text. Please clarify this.

The second point of clarification concerns weathering rates. The modeled rates for the catchment range between 38 and 219 mmol/m<sup>2</sup>/yr (Section 3.1). But in section 3.2 it is stated that soil profiles from some of the study sites had base cation weathering rates of 3–13 mmol/m<sup>2</sup>/yr. This sounds like a large discrepancy - but perhaps it is inappropriate to compare the catchment rate with soil profile rates. Please clarify this potential discrepancy.

There are some interesting aspects of the calibration and model simulation that I think deserve more mention. First, the dry deposition rate appears to be a way to get the assumption of sulfur steady state to balance. Please present the dry deposition factors, and discuss if they indicate the degree to which the sulfur steady state is or is not fulfilled.

Another aspect of the calibration is optimization of the organic acid charge density. Hruska, et al. 2003, which is the reference for the organic acid dissociation, defined this density range for a number of lakes. It would be good to see how the calibrated densities compared to the narrow, well-defined range of densities reported by Hruska. Any discrepancy here may be of relevance to the role of organic acids in delaying alkalinity recovery that is brought up in the discussion.

Finally, in Figure 4, which shows the ability of the model to capture the observed lake chemistry variations for a site, a lake is chosen that has observations back to 1990.

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There are a number of lakes with observation back to 1983, if I understood the data correctly. Please use one of these longer time series to give a better sense of how the model is capturing the annual variability.

Beyond that, there are some points that looked to be of interest in the modeling that should be

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3595, 2006.

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