

Comment on bg-2021-86

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Referee comment on "Towards a history of Holocene P dynamics for the Northern Hemisphere using lake sediment geochemical records" by Madeleine Moyle et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-86-RC1>, 2021

Studies of variations in the terrestrial phosphorus cycle are few and far between—particularly those from lake sediments that stretch back into the Pre-Anthropogenic disturbance intervals. What continues to surprise me, at least, is the significant impact that even subsistence-level agriculture had on phosphorus weathering. This compendium of a broad array of lake sediment phosphorus records further illustrates this point, and highlights that most of what we think of as the “natural” phosphorus weathering rate is likely incorrect. This paper builds on another one by some of the same authors that documents that total (sedimentary) phosphorus flux is strongly related to lake water phosphorus content based on ~historical records, and hence can be used to assess and predict water quality. Specifically, the current manuscript (1) uses sedimentary phosphorus concentrations determined by a host of other studies to (2) infer sediment inferred lake-wide sediment phosphorus loading in those catchments, which in turn (3) is used to calculate sediment-inferred catchment phosphorus yields, which further (4) is used to model variations in lake dissolved phosphorus concentrations via various runoff, elevation, and temperature scenarios. An ambitious work, and a very valuable contribution to the field.

Challenges exist with this kind of approach, which the authors have attempted to constrain to the extent possible. Multiple assumptions are included in their calculations approaches for (1) and (2) above, not least of which is the some of the data that drive their calculation model is simply not available, or the lakes themselves have changed so much over time that they are not applicable to the paleo-record. For example, a fixed sediment focusing factor is included for many (most) of the records—not a particularly problematic work-around, but one which might “make” the various records look more alike. Second, there are assumptions based on the details of the studies included about the degree of diagenetic alteration in the sediments. The authors clearly lay this out, and I

know that for the lakes used that I am familiar with, this is a fair enough assumption. The reason why this is important, however, is that the sediment column can lose phosphorus to the overlying water column in some situations (anoxicity being one of them), and that phosphorus could then be flushed out the outlet. Of course, in low throughput lakes, the released phosphorus tends to just drive biological processes that result in the phosphorus returning to the sediment column. Overall, this paper manages the model input assumptions in a reasonable and well-documented way.

Several aspects of this study seem a bit more conjectural and/or less convincing, including (3) and (4). For the issue of determining sediment-inferred catchment phosphorus yields (3), this depends strongly on runoff into the lakes, which seems to be inferred by modern records it seems—is that correct, because it was not obvious in the current manuscript? I can see no way around this, but runoff certainly has varied through the Holocene, and at some settings perhaps by a lot. Additionally, one might guess that watershed modifications could have occurred such that inflow rates might have varied—I realize that this is likely a minor factor, but could any of the lakes have been affected by impoundments being built on tributaries? Overall, more discussion of the potential ways that variable runoff or effective runoff could impact the catchment phosphorus yields would be helpful.

For the environmental factor analysis (4), the paper analyzes temperature, runoff and elevation as potential variables affecting lake phosphorus concentrations through time (displayed in Figs. 8 and 9). Although I understand the motivation of this analysis, the results were inconclusive. Figure 9 for example breaks the lakes into temporal windows and then environmental factors, and by my read at least I see no statistically significant trends. By having this rather inconclusive environmental factor analysis toward the end of the manuscript, where you expect to find some of the take-home messages, it diminishes the power and readability a bit. I suggest that most of this be move to a Supplementary file with brief mention in the main manuscript about the general findings for this.

Finally, one central correlation found by the authors which seems relatively weak is displayed in Fig. 7b. This is a critical aspect of the story (sed P loading correlates to current water TP), which is compromised if just one of the lakes (Hatchmere) is removed from the analysis. I suggest that the authors consider the implications of this very weak, if any, correlation for the modern benchmarking analysis presented in this figure. I am frankly not terribly surprised for this to be the case, as modern cultural eutrophication of lake systems might blow up a relationship that is present in the longer record, but it should certainly be argued more from the weak data rather than pinning a trend line on one outlier.