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Biogeosciences Discussions

Interactive comment on "Sources, fate and geochemical dynamics of nitrate in an oligotrophic lake" *by* U. Tsunogai et al.

Anonymous Referee #4

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This study relies on natural abundance NO3 stable isotopes, including O17, to trace atmospheric NO3, and isotope data over a summer period to estimate fluxes between pools. In this system, N and P are present at very low concentrations and productivity is low. Despite this, nitrate isotopes and concentration profiles suggest active cycling of nitrate. The study attempts to take a new and important step with respect to using multiple NO3 isotopes to understand lake N cycling. The value in this approach is clearly evident, and has much potential to advance our understanding of NO3 cycling. The manuscript could be improved in two areas however. First, it is in places difficult to follow, and needs to be substantially revised for clarity, organization and length. Second, there are assumptions made with respect to estimates of NO3 cycling that are not discussed in enough detail or incompletely addressed. Comments below address some of the specific issues.

-It would be very useful to provide data for NH4 and TP data in the introduction. TP gives a better indication of phosphorus availability than SRP, and the size of the NH4 pool is critically important toward assessing some of the later assumptions. Additionally, while nitrogen limitation seems plausible it would be useful to provide further support for this, given the possibility of P limitation in a lake with such a small catchment and large water volume. NO3:TP ratios would be useful in this regard too.

-The trends in nitrate isotope values presented in Fig. 4 are a little puzzling. The 15N and 18O data suggest strong effects of assimilatory processes through most depths, consistent with the NO3 drawdown observed. However, the deepwater 17O data indicates substantial inputs from nitrification, despite the drop in concentration, which suggests assimilation.

-Given the extremely low concentrations of NO3 in the system, what is the sensitivity of the natural abundance isotopic estimate of N cycling to error? Some additional error analyses or explanation is necessary for estimates of NO3 removal and nitrification. It would be very useful to support these estimate with some independent measurements of nitrification and/or uptake, which could be done with a tracer addition, even with the logistical constraints mentioned earlier. Despite the objections raised about tracer additions (pg 7253), many of them valid, the tracer approach would provide some very useful data. In addition, while the authors have clearly pushed the O17 method forward in this manuscript, this type of application is novel, with many untested or poorly known assumptions. Therefore, verification by other means will be very useful.

-I do not completely understand the calculations and use of fractionation data (pg 7240, line 1-5, and previous page); this section should be clarified and there is quite a bit of text that could be removed or moved to the discussion. Also, it's unclear to me how is the PON data is being used. N uptake is typically dominated by ammonium uptake, so the PON data cannot be to assess N sources without additional information. Finally, looking at Table 1, NO3 15N and 18 isotopes don't appear to vary linearly- does this contradict statements on pg 7239?

-For Fig. 7, why is so little of the data used in this plot?

-Pg 7244, line 25-26, the term “reducing” is confusing here. Perhaps for example for #1, “subtracting the contribution of NH4 to total N uptake” or something similar.

-Pg 7244-45. The corrections described here should act to lower the estimates of nitrate uptake based on primary production substantially. Application of a 10x correction factor seems a little arbitrary here without some consideration of the similarities or differences between the lake mentioned to support it (i.e. Castle Lake). How much periphyton production can occur in this system? From the description (steep walled, deep, soft bottomed), I’m guessing much less than Castle Lake. Also NH4 uptake is likely to contribute greater than 50% of uptake. This assumption needs to be better referenced. Without further justification, I don’t agree with the statement that the primary production data support the reliability of the isotope based measurements to a very large degree (pg 7245, line 14)

-Is the mean presented in Table 1 based on the water volume or a simple average?

Some examples of text that should be revised for clarity or removed:

-Pg 7232, line 25 pg 7233 line 4. Awkward phrasing, and an example of text that should be removed unless there is a specific point to be made.

-Pg 7236, line 23-24. Meaning is unclear here.

-I am also confused by the statement that the lake is a closed system “from internal/external sources”. If I understand this correctly, it seems that the authors own measurement contradict this statement. Please clarify.

Interactive comment on Biogeosciences Discuss., 7, 7227, 2010.