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Interactive comment on "Sources and cycling of nitrogen in a New England river discerned from nitrate isotope ratios" *by* Veronica R. Rollinson et al.

Anonymous Referee #1

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Review of " Sources and Cycling of nitrogen in a New England river discerned from nitrate isotope ratios" by Rollinson et al.

Summary

Rollinson and colleagues present a comprehensive examination of nitrogen (N) loading dynamics in a New England watershed. The analysis includes measurements of DIN including nitrate (NO3-), nitrite (NO2-) and ammonium (NH4+) as we all as dissolved organic (DON) and particulate (PN) forms. In contrast to previous studies of N loading in this watershed, the authors also leverage the use of nitrate N and O isotopes for constraining confounding influences of source mixing and cycling mechanisms. Together

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Discussion paper

with various temporal and spatial perspectives (seasonal transects of the whole river, weekly site sampling, river discharge measures and point source characterization), they stitch together a comprehensive picture of N sources and controls on loading from the watershed into the estuary. The study indicates very little uncycled atmospheric N loading and identifies underlying drivers of N loading that stem from differing hydrologic regimes (e.g., base flow conditions vs. shallow flow influences). They authors also outline how nitrate N and O isotopes are expected to behave in the framework of 'nutrient spiraling' – and use the differential behaviors of N and O isotopes to constrain cycling and source partitioning.

Major comments

Overall, the manuscript is very well-written and covers a lot of ground. The data are of high quality and the analysis and interpretation of the data is sound.

One criticism I have is that the manuscript is probably overly long-winded in some aspects and might benefit from some trimming and tightening to make it more approachable to a broader audience. I did appreciate the application of the data to the broader understanding of sources and cycling phenomena in the watershed and the thoroughness of this discussion (sections 4.2), but thought that the discussion of loading (4.3), for example, could be condensed.

My only other critique is that there were times when I was left wondering about the error on some of the endmember estimates and flux terms. My guess is that the small distinctions in average endmember isotopic compositions might be overwhelmed by natural variability in sample population (and/or in the intercept on the modified Keeling plot)?

Also, for example, it is not clear how 'close' the flux comparison between the 2018 data in this study may compare the historical 2002 data from Fulweiler and Nixon (Lines 680 to 689). While it seems clear that the drastically disparate hydrologic regimes of the two years underlie the major changes in N fluxes, having a better understanding of the magnitude of the error associated with these watershed scale flux estimates would help readers put the assessment in to a clearer context. Similarly, the estimates of endmember concentrations (L692) should have some indication of confidence. By some measures, 50 and 65 are not all that different, for example. Finally, the same can be said for the estimates of endmember δ15N and δ18O values (for example, L457; L490 to L500).

To be clear I am not trying to insinuate that the data and findings are suspect in any way – just that more attention could be given to presenting the error intrinsic to such endmember estimation.

Minor comments L99: Sigman et al 2019 reference – missing?

L101: what is meant by 'inherent' cycling?

L265: 'barring a single outlying value. . .'

L303: were highly anti-correlated (?)

It might be useful to understand whether the WWTFs are of the combined-sewer overflow type or not, which plays into the residence time of waste in the facilities – and hence N speciation. There is a clear seasonality in the WWTF speciation data that is not really highlighted or addressed. So, while the DIN and TON fluxes from the WWTF are remarkably constant, the NH4+ and NO3- fluxes would not necessarily be constant. It isn't clear whether this really plays into any of the overall findings.

It appears that the WWTF samples were not analyzed for nitrate isotopes? This would be a unique dataset and could offer some interesting insights. Given the large shifts in total N speciation in the WWTF – I suspect there would be some substantial variation in the N and O isotopes of WW effluent as well. While these data are not necessarily paramount to the conclusions presented in the paper, knowing more about isotopic variability associated with annual WWTF operations and effluent would be valuable to the riverine N biogeochemistry community in general. Side question – was nitrite measured or reported from WWTF samples?

L377:. . . would admittedly arise from loading by point sources to the degree that a point source has elevated conductivity.

L456: Refer to Figure 8.

L498: “. . . values of NO3- in rainwater.”

L500: . . . with higher discharge can thus be partially explained. . .

L507: Kendall is misspelled.

L516 to L522. Consider splitting this sentence into two sentences.

L529-531: Please include reference here.

L542: which limited light penetration.

L620: to primarily reflect

L640: there was little to no accumulated snow in March 2019

L645: I think it would be good to state that no samples were taken from Kenyon Industries much earlier – when it is introduced as a potential point source.

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