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Interactive comment on "The importance of nitrogen fixation to a temperate, intertidal embayment determined using a stable isotope mass balance approach" *by* Douglas G. Russell et al.

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We thank the reviewer for the constructive comments.

Major comments:

1. This model (equations presented on page 5-6) is very trivial. Nitrogen cycle is very dynamic, many components of the cycle are missing in the model, such as anaerobic ammonium oxidation that occurs in sediments (Thamdrup & Dalsgaard, 2002) and flux of the dissolved organic nitrogen (Jickells et al., 2017) to name a few. Moreover,

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isotopic fractionation factors (ε) for these processes are different – not much is known about their precise values. None of the equations presented here have considered fractionation factors barring equation (3), and that too has inappropriately incorporated fractional factors. In addition, each term (expression) used in equations must be defined clearly, for example, what does mSources in equation (1) stand for? In a nutshell, equations need elaboration and model requires robustness.

*We agree that there are certainly other processes that contribute to the marine nitrogen cycle, and will include a more comprehensive discussion of these processes and the potential isotope effects associated. We note however that the net fractionation associated with these processes in sediment is small. Furthermore, all the processes discussed will result in an enrichment of the nitrogen isotope pool, and the most likely cause of the isotopically light signature of the nitrogen pool is due to nitrogen fixation, and this point will be reinforced.

2. These (model) equations are equations are time independent. We understand that biogeochemical processes are time dependent – for example N2 fixation is more in some season, while denitrification would dominate in some other season. So how good these isotope mass balance equation can represent such processes?

*It is true that biogeochemical processes are time dependent, however, using instantaneous rate measures to describe the cycling of nitrogen over the annual cycle is problematic i.e. how well will the data on any given day represent actual processing rates. The advantage of using stable isotopes in such a study is that they represent integrated measures of the accumulated pool and hence provide an insight into the longer-term behaviour of nitrogen cycling and the implications over the course of a year, which was the intention of this manuscript. Additional text will be added to emphasize this point

3. It is stated that equations (2) and (3) provide equation (4) (Page 5, line 30) but I guess equations (1) and (3) provide (4).

*Eq (4) is created by substituting Eqs. (2) and (3) into Eq. (1). This will be re-worded

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in the revised manuscript.

4. Section 2.2.1 on atmospheric deposition. Dissolved organic nitrogen in an important source of nitrogen that has been ignored.

*Dissolved organic nitrogen can comprise up to 50% of total atmospheric deposition in this region (Lansdown, 2009). Because of the unknown bioavailability of this fraction and also the fact that atmospheric deposition itself only contributes <5% to the total nitrogen input, we believe this will make little difference to the budget. This assumption will be stated in the revised version.

5. In the same section, how was atmospheric flux estimated from the concentration. Is it dry deposition or wet deposition or sum of the both? What was the deposition velocity and scavenging ratio? How much area was considered to estimate concentration into areal fluxes? All factors must be elaborated.

*This was estimated as bulk atmospheric deposition. We will provide a more comprehensive discussion of how the atmospheric flux was estimated in the methods section.

6. Are the measurements of atmospheric deposition, river inputs, N2 fixation, denitrification done simultaneously? If not, then how can one do mass balancing?

*Yes, with the exception of the concentrations that were used in calculating the input of DIN from atmospheric deposition (these were based on separate studies), all other measurements were undertaken concurrently. We will make further clarifying remarks explicitly dealing with this point in the methods section

7. Models (mathematics) are useful to understand processes but one cannot ignore experimental results just because there are methodological issues. Because of methodological issues, experimentalists provide errors associated with estimates. Models also need to be verified with observations. Therefore, the criticism of experiments presented on page number 14 (first paragraph) is a bit overdone.

*We agree this criticism is over emphasized and we will tone this back. We will em-

phasize this model matches closely with our previous experimental results (Russell et al., 2016) and also highlight how this approach integrates over time (and therefore time independence) of the mass-balance model.

Minor Comments:

All the minor comments will be addressed in the revised manuscript.

References Cited:

Lansdown, K. P.-M.: Biogeochemistry of nitrate in headwater streams and atmospheric deposition of the Dandenong Ranges, PhD thesis, Monash University, 2009.

Russell, D. G., Warry, F. Y., and Cook, P. L. M.: The balance between nitrogen fixation and denitrification on vegetated and non-vegetated intertidal sediments, Limnology and Oceanography, 61, 2058-2075, doi: 10.1002/lno.10353, 2016.

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