Journal: Limnology and Oceanography

Manuscript ID: LO-24-0740

Title: Spatial dynamics of animal-mediated nutrients in temperate waters

# Dear Dr. Mullarney,

We have revised our manuscript, LO-24-0740, entitled " Spatial dynamics of animal-mediated nutrients in temperate waters." We have addressed all the reviewer’s suggestions. A detailed outline of these revisions is below. All line numbers are in reference to the clean version of the manuscript. We are grateful to you and the reviewer for thoroughly examining our manuscript, providing constructive guidance, and recommending it for publication in *Limnology and Oceanography*.

We found the referee’s suggestions insightful, especially those regarding the clarification of our methods. Addressing these comments allowed us to clarify the variability and drivers of marine animal-mediated nutrients in nearshore coastal ecosystems. Having revised our manuscript per the referee’s suggestions, we hope our manuscript is now suitable for publication in *Limnology and Oceanography*.

Thank you for your continued consideration of this manuscript.

Em Lim

**COMMENTS TO THE AUTHORS**  
  
**Editor-in-Chief:**  
**Comments to the Authors:**  
In addition to addressing all reviewer and editor comments in your revision, **please also make sure that your revised manuscript conforms to the style and formatting requirements for**Limnology and Oceanography, as specified in the online instructions for authors (https://aslopubs.onlinelibrary.wiley.com/hub/journal/19395590/about/author-guidelines).

**RESPONSE**: We have taken care to follow all formatting requirements.  
  
**Deputy Editor:**  
**Comments to the Authors:**  
Dear authors,  
Firstly - please accept my sincere apologies for the lengthy review process - it took quite a while to find reviewers and some didn't complete their reviews as they had promised, and we let things linger too long (I will do everything I can to ensure this doesn't happen with a revised version).  
  
The good news is that the reviewer viewed the manuscript favourably. However, they nonetheless had a few suggestions for improvements and requests for clarifications which should be addressed in a revised version. While the topic is outside of my area of expertise, their opinion nonetheless coincides with my own reading and judgement of the manuscript - that the text describes a lot of work, and the analysis and writing are in very good shape.  
  
I had just a few small comments:  
L60 - I suggest 'tidal' rather than 'tide' exchange.

**RESPONSE**: Done.

L7, L417, - specify what 'it' refers to.

**RESPONSE**: Done.

L104-L106 - 'open nature of nearshore environments' - this is perhaps not entirely clear, as in the following sentence you refer to intertidal and shallow subtidal ecosystems, which could be inside estuaries (and may not then be open per se). Also I would use 'fast' and 'slow' rather than high and low when referring to flow speeds.

**RESPONSE**: We have clarified that not all nearshore ecosystems are open, and changed “high” to “fast” when referring to flow.

L170 - Similarly, at a few points throughout the manuscript (also L254, L452) there's a slight ambiguity with 'higher' and 'lower' when referring to concentrations (as physical processes researchers often associate 'higher' with height in the water column) - you could consider using greater or larger instead.  
**RESPONSE**: We have changed higher to greater when referring to concentrations.

L171 - hyphenate fine-scale.

**RESPONSE**: Done.

L239 - clarify 'they'-> this species.

**RESPONSE**: Done.

L426 - 'These sources?'

**RESPONSE**: Correct, we have made this edit.

L444 - I suggest 'enhances, rather than masks, NH4+ variability'

**RESPONSE**: Done.

L511 - I suggest ' is advected' rather than 'flows'

**RESPONSE**: We prefer “flows”

L514 - clarify - this process?

**RESPONSE**: Done.

Reviewer: 1  
  
Comments to the Authors  
Overall Comments  
  
I really enjoyed reading this paper. I thought the questions were interesting, timely, and cool and the science was nicely done and described. I liked their approach to think about how ambient concentrations of ammonium could reflect animal biomass and also how the flow environment could modulate these relationships. The data are of good quality and I think the Discussion pretty accurately reflects their data with nice context from the wider literature (with a couple of small places where I disagree outlined below).

**RESPONSE**: Thank you for your kind words and careful review.  
  
The one major (though not really that major) quibble I have with the data is the individual models they use to look for effects of individual animal taxa on concentrations of ammonium. I just don’t think this is appropriate and looks like data dredging to me to do individual models for each taxa. They show that limpets are correlated with overall ammonium which seems unlikely to be a real biological effect unless these reefs are stacked with limpets in a way I’ve never seen in a kelp forest. I don’t think these analyses are appropriate and should be removed. A potential alternative way to address the question though would be to do some sort of ordination on the animal biomass data (e.g., NMDS) with site as replicate and then test how ammonium concentrations correlate with the ordination axes. You could potentially break this up by tidal cycle if you have enough data. This would give you some more powerful inference about how ammonium concentrations varied with animal community composition.

**RESPONSE**: We have removed the individual family models since these results are not central to the questions we ask.  
  
I think the science is good and L&O would make a good audience for this paper. I had a few comments and concerns that should help a revision become publication worthy.  
  
Specific Comments  
  
Lines 206-212 - So fishes and inverts weren’t counted outside of the kelp forest? I was expecting that to be part of the methods to make a more robust comparison of ammonium inside vs. outside the forest. I found this part of the methods a little hard to understand. I had to reread this section a couple of times to make sure I got it.

**RESPONSE**: That is correct: fishes and inverts were *not* counted outside kelp forests. We agree that surveys entirely inside kelp forests paired with surveys entirely outside forests would be more robust, but unfortunately due to visual obstruction and concern with diver entanglement by the kelp, surveys entirely inside the kelp forests were not possible. Instead, as shown in Figure S1, we laid the Reef Life Survey transects right along the edge of the kelp forest to best (and safely) characterize the biological communities *associated* with each forest. We have clarified this section in the methods (lines 199-203). We still feel that with our sampling design and analysis, we can explore the role of animal biomass in modulating ammonium retention by kelp forests, albeit with more noise than if we had data on animal biomass inside vs outside kelp forests. We have also added a sentence to our discussion to address this (lines 432-434).  
  
Lines 226-27 - 0-2m above the benthos seems quite variable honestly and I expect this generated some heterogeneity in the data that might not have existed had they been consistent about the depth off the bottom as these samples would have had more/less influence from the benthic organisms excreting ammonium. Nothing to be done now but some unfortunate variability here.

**RESPONSE**: We agree, and tried to minimize variation in the field, but cannot correct this now.  
  
Lines 229-30 - Not sure I understand the ‘standard’ creation here vs simply the samples taken outside the kelp forest. How were these ‘standards’ used?

**RESPONSE**: The standard-additions protocol II cited in the methods uses seawater taken from the same site as the samples to generate a standard curve for the fluorometric detection of NH₄⁺. We have clarified this in the text (lines 226-228).   
  
Line 302 - is animal abundance in biomass or density?

**RESPONSE**: This is density. We have clarified this in the text (lines 291-294 and line 301).

Line 303 - Not sure why diversity was a predictor here. That wasn’t well set up in the Intro of why they were interested in diversity as being important here.

**RESPONSE**: Body size and diversity are two predictors of NH₄⁺ supply rate (Allgeier et al., 2014). We have added a sentence to the introduction to bridge this gap (lines 94-95).  
  
Lines 306-7 - biomass is a measure of abundance. do you mean ‘biomass as a predictor instead of density’ which is another measure of abundance.

**RESPONSE**: We use abundance to mean the number of animals/m2 and biomass to mean the wet weight of all the animals/m2. We have clarified this on lines 291-294 and keep this terminology consistent through the text. Our use of “abundance” is consistent with terminology used by other papers using Reef Life Survey data (e.g. Waldock et al., 2019, The shape of abundance distributions across temperature gradients in reef fishes).

Lines 314-15 - So was animal biomass measured outside of the kelp forest in the within-site comparisons? I don’t think it was from reading the methods. So was this animal biomass from the kelp forest as the predictor here? Seems like the biomass of animals outside of the kelp forest would have influenced this difference so it feels like they are missing some important information with which to interpret their patterns.

**RESPONSE**: Correct: animal biomass from the edge of the kelp forest (our proxy for kelp-associated biological communities, as clarified above) is the predictor here. We agree that it might have reduced noise to consider how the difference in animal biomass between in and out of kelp forests influences the difference in ammonium concentration and have added a sentence to our discussion to address this (lines 432-434).

Lines 329-31 - How were these data included in the model? Or were they? Not clear how these data were used

**RESPONSE**: In the results we state the estimated total NH₄⁺ excretion rate by the sea cucumbers for comparison to the total excretion rate of the crabs used in the other experiment (lines 355 and 361). These rates were not included in any model. We have attempted to clarify this in the text (lines 329-331).  
  
Lines 336-37 - What was this relationship used for? I’m not clear why they tell me this.

**RESPONSE**: See above response.   
  
Line 345 - and by abundance you mean density of animals? Or biomass of animals?

**RESPONSE**: By abundance we mean the total number of animals/m2 as defined in the methods.  
  
Lines 346 – 350 - This model exploration looking for the influence of individual families of animals seems like data dredging to me looking at these relationships for all families. Clearly limpets are not driving this overall relationship. I would argue strongly that this part of the analysis be removed. Its not appropriate.

**RESPONSE**: We have removed the single-family models.  
  
Fig 3b - Not sure I understand what the ‘None’ comparison is on Fig 3b. Where did these None surveys happen? I don’t think I got that from the Methods.

**RESPONSE**: “None” here refers to our no-kelp control sites, as explained in the Methods (lines 224-226). We have updated this figure to use language consistent with our methods.  
  
Line 365 – ‘We found limited evidence for an effect of animal biomass…” - Is that because you also didn’t account for animal biomass outside of the kelp forest? Or at least I can’t see that you did so.

**RESPONSE**: Yes, that is possible, as acknowledged earlier and now added to the discussion.  
  
Lines 372 – 374 - Again I don’t think these single-family models are appropriate. I think they should be removed

**RESPONSE**: They have been removed.  
  
Lines 389-391 - But I’m guessing the two crab treatments weren’t different from each other?

**RESPONSE**: This is correct.  
  
Lines 441-442 - I don’t see this effect of tide in the data in Fig 3e. I don’t see any consistent effect of tide (but clearly the interaction with animal biomass). I don’t think you can say that there was a consistent effect of tide given the interaction and flood tide is definitely not consistently higher than slack tide here.

**RESPONSE**: This sentence refers to the interaction shown in Fig 3c, which shows the relationship between kelp biomass and NH₄⁺ retention is more dramatic or pronounced (steeper line) during flood vs slack tide.

Lines 447-451 – It’s also likely that they have differential uptake rates of ammonium. I would guess that *Macrocystis* has higher uptake rates than *Nereocystis* so the uptake dynamics between the two species likely contributes in that the giant kelp is simply using the nutrients faster not necessarily retaining fewer nutrients in the water column.

**RESPONSE**: We have added this to the discussion (lines 415-416).  
  
Lines 452-455 - This pattern is just simply confusing honestly.

**RESPONSE**: We added a sentence to the Methods to describe ammonium sampling at the no-kelp sites (lines 224-226), and we have rewritten this part of the paragraph to increase clarity (lines 416-422).

I was surprised not to see [other relevant papers about spatiotemporal dynamics of CND in kelp forests in the lit cited.]

**RESPONSE**: We feel that we have been quite thorough in our survey and use of the relevant literature, but we would be happy to incorporate specific papers that the reviewer feels are needed.