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# *Interactive comment on* "Tidal and seasonal forcing of dissolved nutrient fluxes in reef communities" *by* Renee K. Gruber et al.

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MS No.: bg-2018-413, Anonymous Referee #2

Referee comments are included below denoted as "Ref2".

Author response to each referee comment is given below the comment denoted as "Authors". Please note that page/line numbers correspond to the revised version of the manuscript rather than the original version. Please also note that this version of the manuscript contains corrections from Referee #1.

Ref2: This MS reports fluxes of dissolved inorganic nitrogen and phosphorus and theoretical mass-transfer-limited uptake rates on a strongly tide-dominated reef platform. The amount of nutrients that is released in the water column is calculated from these



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Discussion paper

two data sets.

General evaluation: Overall, this is a very interesting paper, showing that mass-transfer-limited uptake rates may vary by an order of magnitude on the scale of minutes to hours on tide-dominated reefs, due to substantial variability in flow speeds and water depths over the tidal cycle. Differences between wave- and tide-dominated reef biogeochemistry that are due to the hydrodynamic regime are nicely highlighted. I have a number of relatively minor comments aimed at clarifying the methods and results (detailed below), which the authors should be able to answer easily. The main concern is that nutrient concentrations were not measured in Feb during the first 6 hours of the tidal cycle. Missing data were replaced by nutrient concentrations in offshore waters, which is maybe not perfectly supported by the data, particularly with regard to NOX (Figure 3). I would therefore recommend more caution in the conclusions regarding seasonal differences in JMTL (see below). The discussion about the implications in terms of nutrient limitation is worthwhile, however.

Authors: We thank the referee for their supportive and thorough comments on this manuscript (ms). We address the concerns about using offshore waters for nutrient concentrations in specific comments below.

Comments in detail: Introduction

Ref2: p. 2 Line 5: you could add a reference about corals: Grover, R., Maguer, J. F., Alle- mand, D., & Ferrier-Pagès, C. (2008). Uptake of dissolved free amino acids by the scleractinian coral Stylophora pistillata. Journal of Experimental Biology, 211(6), 860-865.

Authors: This reference has been added to p2 L21.

Methods

2.1 Field site

Ref2: p. 4 Lines 8-10: The hydrodynamic study of Lowe at al. (2015) was performed in

March-April 2014, while the MS reports nutrient concentrations from October 2013 and February 2014. Although this is justified later in the MS, the reason why hydrodynamic data collected at the same time as nutrients were not used to calculate mass-transfer-limited uptake rates is unclear at this point.

Authors: We moved an explanatory sentence from p6 to p4 L10-13 to clarify this point earlier in the Methods section for readers.

Ref2: p. 4 Lines 11-13: It is unclear if this concerns mass-transfer-limited uptake rates only, or nutrient fluxes as well.

Authors: We have added clarification to p4 L14 to indicate that this applies to all data.

2.3 Control volume approach

Ref2: Line 8: “Depth-averaged flow speeds were bin-averaged”: do you mean that you first averaged flow speeds in each bin (at 5 min intervals) before depth averaging?

Authors: We think the word “bin-averaged” may have been confusing, so we have clarified the wording on p5 L11.

Ref2: Lines 13-21: I think this paragraph needs some clarification. You are using the mean of concentrations at both stations to calculate the local benthic flux, but you explain then that this local term represents nutrient uptake or release occurring at a sampling station (in my mind, CR or SG, while you are calculating Jnet on the transect). The advective flux is described as nutrient uptake or release during transit between sampling stations, which I also find very confusing. Should not it be what is added or subtracted to the transect due to water transport?

Authors: A control volume approach comes from fluid dynamics and is using both Eulerian (the “local” flux term) and Lagrangian (the “advective” flux term) frames of reference. Basically, when you take samples at fixed positions in a moving fluid, the changes that you see are always a balance of changes in situ (i.e., nutrient uptake at your sampling station) and changes due to water masses advecting into your sampling station. Depending on the flow speed, one term may dominate over another (e.g., in fast flow, the "advective" term will dominate, while in slow flow the "local" term will dominate). Control volumes are used to estimate net fluxes over the entire volume (so a mix of coral and seagrass on the reef flat). We have clarified this on p5 L22-23.

Ref2: The minus sign in front of Jnet is also surprising at first glance. It might be more understandable to state that the sign of Jnet was reversed so that uptake is positive and release negative.

Authors: The minus sign is typical of benthic flux studies, as it is used to define the frame of reference. There is an explanation of the sign convention on p5 L21-22.

2.4 Uptake rates at the limits of mass-transfer

Ref2: p.5 Lines 25-30: In the results, JMTL is first calculated for both CR and SG (Figure 6), not along the study transect. Maybe the reasoning would be easier to follow if there was first a paragraph about the calculation of mass-transfer velocity and JMTL at each one of the two stations, and then, a new paragraph explaining the calculation of Jrelease.

Authors: In Section 2.4, JMTL is first calculated along the study transect (p6 L4) similarly to how Jnet integrates fluxes along the study transect. Addition of text to p5 L22-23 should clarify that Jnet is an integrated measurement over the transect, which should alleviate confusion. On p6 L23-26, Jrelease is calculated using Jnet and JMTL. At the end of this section (p6 L28-p7 L6), we calculate JMTL over the full tidal cycle at both stations individually; we have added some text to this section to clarify these calculations.

Ref2: p.5 Line 30 to p.6 Line 9: This, although necessary, is really hard to follow. There are very nice explanations given in the results (p. 7 Lines 23-27) that might help the reader to go through the equations. Would it be possible to integrate these explanations into this paragraph, specifying the parameters whose variability has the most influence?

Authors: We have moved some text from the Results to p6 L9-12 to give a simpler explanation of mass transfer velocity.

Ref2: Maybe, in a second step, you could simplify equation 4, as some parameters are constants (or nearly constant). This would highlight the influence of flow speed (and possibly water depth: the drag coefficient CD was taken as 0.02 in Gruber et al. (2017), but I didn't understand if this is the case here) and nutrient diffusivity?

Authors: We prefer to leave Eq. 4 as it is; constant variables are defined in the text below each equation, so it should be clear which variables are constant (very few in Section 2.4 – just density, kinematic viscosity, and diffusivity). We have added the constant values for diffusivity to p6 L13 to make this easier for readers to use.

Ref2: In Gruber et al. (2017), u* is a function of ux, not u (this MS). Is there an explanation? I would also suggest that you explain why you are using u in S calculation, and not ux.

Authors: Ux is used when we make estimates (such as with the CoVo) during the ∼unidirectional ebb tide period (since ux is flow speed along the axis of the transect). When we calculate JMTL over the full tidal cycle (no longer ∼unidirectional, but rather with large changes in direction), we use u (non-rotated flow speed). We clarified this by using ux in Eqn 4,7 and then explaining the difference in text p7 L1-3.

Ref2: Lines 16-28: I would suggest to give these informations before Line 10 (calculation of Jrelease). Could you please clarify which instruments were deployed exactly? On Figure 1, a velocimeter is shown at CR, but you are not using the data, right? Jnet was calculated from the ADCP data at SG, are you using the same current speed data for the calculation of JMTL and then Jrelease, or the data from Lowe et al. (2015)?

Authors: We think this should now be clarified due to clarification of JMTL calculation at the beginning of Section 2.4 (see responses above). ADP data were used to calculate

Jnet, JMTL, and Jrelease as this instrument was deployed in Oct, Feb, and Apr. ADV data were only available from April, so were used only to calculate the full tidal cycle version of JMTL (last part of Section 2.4). We have added explanation of which velocity data were used on p6 L31-p7 L1 and added a comment about ADV deployment time into Figure 1.

Ref2: Lines 29-30: This deserves more explanations. Do you mean, for example, that error bars on Figure 4 are uncertainties in estimates of each one of the calculated fluxes? Please add this information in the caption.

Authors: Indeed, the error bars in Figure 4 are standard deviations of each calculated flux. We have added this information to the caption. Good catch!

Results 3.1 Nutrient concentrations and measured fluxes

Ref2: Line 4: From Table 2, water temperature is about 2âŮęC warmer in Feb (not 3âŮęC). Some observations could be supported by statistics: line 6 (DIP and DON are slightly lower in Feb); lines 6-7 (concentrations are similar on the reef and offshore until 6 hours after flooding); lines 18-19 (no difference between Oct and Feb for NH4?).

Authors: Good observation about temperature, we have corrected this. We don't think statistics on nutrient concentrations or fluxes would add very much to this ms. The purpose of this ms is not to determine whether seasons are significantly different from one another. If that were our purpose, we would need to replicate by season (at least 3 dry seasons and at least 3 wet seasons, which we did not do). Doing statistics would give you some p-values, but it wouldn't actually give you any truth about seasonality.

3.2 Mass-transfer velocity and nutrient uptake

Ref2: It is unclear from p. 6 lines 3-5 that the drag coefficient was calculated as a function of water depth to draw Figure 5 (the drag coefficient was taken as 0.02 in Gruber et al., 2017). Please clarify in the Methods.

Authors: We actually used a variable form of CD (from McDonald et al. 2006), which was an accidental omission from the ms. We have added an equation and explanation for calculating CD (p6 L14-18). Between the publication of Gruber et al. 2017 and the submission of this ms, we found a more realistic (we believe) way of estimating the drag coefficient. Drag in shallow reef environments is very much an ongoing field of research!

Ref2: p. 7 Lines 26-27: Can you roughly quantify the effect of temperature on S?

Authors: Temperature changes alter S by <0.01%, and we have added this value to p8 L12.

Ref2: p. 7 Line 28: Could you add on Figure 5 the drag coefficient as a function of hours after reef flooding (and maybe flow speed as well, to avoid having to go back to Figure 2). Also state in the caption that hydrodynamic data (and presumably water depth?) are from March-April 2014 (Lowe et al., 2015), while temperature and salinity from Oct 2013 and Feb 2014 (is that right?).

Authors: We have added 4 more panels to Figure 5, which now shows tidal phase-averages of CD and u for both communities. We have also added the clarification that hydrodynamic data are from April 2014 to the caption. Temperature is from April as well, but as discussed above, temperature affects S by a negligible amount (<0.01%); salinity is not part of these calculations, except in the viscosity of seawater, which effectively a constant over the range of salinities measured in the coastal ocean.

Ref2: p. 7 Line 32: Figure 2 doesn't show water depth at SG and CR. Could you add tidal phase-averaged water depth at each site on Figure 3?

Authors: Figure 2 shows water depth at SG, and we have added this clarification to the caption of this figure. Water depth at CR is indistinguishable from SG as the reef platform is basically flat (there is a 10 cm vertical difference between SG and CR, which would not be visible in these figures). Tidal phase-averaged water depth is already shown in Figure 3 as a black line.

Ref2: p. 8 Line 4: Could you explain "which differ by a factor of <4"? Is it the ratio of the flow speeds?

Authors: This statement refers to S, and we have added this clarification to p8 L21.

Ref2: Figure 6: There are missing nutrient data, especially in Feb (0-6 hours after reef flooding; Figure 3). I understand from p. 6 Lines 24-26 that missing data were replaced by nutrient concentrations in offshore waters. From Figure 3, this looks acceptable in Oct, but maybe less in Feb, especially for NOx. Could you show on Figure 6 the time periods during which nutrients were actually measured?

Authors: As the referee points out, we did not collect water samples during flood/high tide in Feb. However, from the samples collected in Oct, it can be seen that water flooding the reef (i.e. at 0-1 hours after reef flooding in Figure 3) has nutrient concentrations very close to offshore waters. This is because this flooding water is offshore water – flow is occurring across the entire reef flat, which is why seagrass and coral nutrient concentrations are similar during this time. As can be seen in Oct, sometime around high tide ($\sim$3 h after reef flooding) concentrations begin slowly diverging from those in offshore waters. This same pattern of reef concentrations roughly matching offshore concentrations from 0-3 h would almost certainly occur in Feb as well, since the hydrodynamics of the reef remain the same. We agree that there is a period of 2.5 hours (3 – 5.5 hours after reef flooding) when concentrations were not measured but are likely diverging from those offshore. Rather than make assumptions about what nutrient concentrations were during those 2.5 hours, we applied rather large error terms to tidal phase-averaged concentrations of NOx and DIP (standard deviations of 0.5 uM and 0.05 uM, respectively). These values are given on p7 L17-18. This error was then propagated (via Monte-Carlo) through all estimates of JMTL.

Ref2: p. 8 Line 10: JMTL is not shown for ammonia. Why?

Authors: JMTL isn't shown in the large multi-panel figures (Figs 4, 5, and 6) because it looks very similar to NOx (being concentration and flow-dependent) and we were trying to minimize the size and complexity of figures (there are quite a few figures already). The reference to NH4 on p8 L27 has been removed to clarify this sentence.

Ref2: p. 8 Lines 11-13: This looks speculative, as nutrient concentrations were not measured in Feb during the first 6 hours of the tidal cycle (see previous comment on Figure 6). Please state clearly that you are assuming that NOx concentrations are similar on the reef and offshore during this period and add some comment in the Discussion.

Authors: We are confident that for the period from flood to high tide (0-3 h after reef flooding in Fig 6), offshore nutrient concentrations are representative of concentrations on the reef for reasons discussed above. We have made the wording more cautious on p8 L28-30.

Ref2: Figure 4: I assume that JMTL is the mean of the values shown on Figure 6 for SG and CR?

Authors: JMTL shown there was calculated along the transect (SG to CR). We have added a note in Figure 4 caption to clarify.

Ref2: p. 8 Line 17: Is "NOx release" "net NOx release"? Seasonal differences: could you add your stats as a column in Table 3?

Authors: We have added "net" to NOx release as suggested.

p. 8 Line 19: This comment refers to Lines 11-13 (see above). I would suggest to simply state that, contrary to DIP, DIN concentrations are similar at both seasons during the part of the tidal cycle studied.

Ref2: P 8 from Line 21, and Figures 7 and 8: I understand that you averaged S and JMTL over each one of the 12-hours period available, and then averaged these averages. If that's right, first you should explain why, and then, I don't think averaging averages is the best way to assess standard deviations (they also appear very small in Figure 7, given the range in Figure 5).

Authors: That is correct. We thought this would be an interesting way to conceptualise the data in a 'bigger picture' sense given that tide-dominated systems are so physically controlled by the tide. We could have just averaged all the data together, but we were trying to be creative (and more physically-focused) since tide-dominated systems have never really been studied before! We have added explanation to p7 L8-9. Standard deviations are not averaged, but are generated by running the entire calculation (as shown in the Methods) with 10,000 sets of noise-corrupted data using Monte-Carlo simulations. The range you see in Figure 5 is not standard deviations, but are values from the spring vs neap cycle (see Fig 5 caption). Standard deviations are relatively small for our estimates because S is so closely controlled by flow speed (as you can see in Fig 5); as a result, JMTL is also closely controlled by flow speed. Flow speed has a small standard deviation thanks to the accuracy and precision of ADPs/ADVs, which is then propagated through our estimates.

Ref2: Lines 21-24 and Figure 7: I'm not sure this is very useful. The two points about S are: (1) the small difference between SG and CR, which is already described p. 7 Lines 31-32 and p. 8 Line 3, and (2) the difference between DIN and DIP which you can easily talk about after p. 7 Lines 25-26.

Authors: Figure 7 does add to the story because it is showing the 'bigger picture' of mass transfer velocity. The first discussion of S (p7 L31-32 in the referree's comment) talks about how it varies within a tidal cycle (at times 30% greater at CR vs SG). Figure 7 (and Lines 21-24 in the referee's comment) show the 'bigger picture' that when averaged over longer time periods, differences between SG and CR are actually fairly small. We prefer to leave this as is.

Ref2: From Line 24 and Figure 8: Again, I would not recommend using all data from Figure 6 due to missing nutrient values. Differences between seasons and sites are already described (p. 8 Lines 6-14).

Authors: We believe we are representing missing nutrient values in a reasonable way through: assumptions based on hydrodynamics (described previously) and relatively large error terms (standard deviation of 0.5 uM for NOx), which are propagated through these calculations. We would prefer to leave this figure as is.

Discussion

Ref2: p. 9 Line 17 (DIP and DON): DIP and DON are “slightly lower (. . .) in Feb” p. 7 Line 6 and “similar between seasons” p. 9 Line 17. One of these two sentences needs to be re-written after stats are performed.

Authors: We have changed p7 L24 to read “similar between seasons”. Our response to stats is discussed previously.

Ref2: Line 28 to p.10 Line 2: Do you mean that mass-transfer-limited uptake was demonstrated in controlled environments because nutrient release was negligible compared to uptake?

Authors: That is the most likely explanation why flume experiments show uptake near mass-transfer limits. Whereas, in natural reef environments, a host of other processes (detrital remineralisation, phytoplankton grazing, etc) are occurring that confound uptake measurements (as discussed in the following paragraph in Section 4.2). We have added text (p10 L12) to clarify this.

Ref2: p. 10 Line 4: overestimation of DIN release on Tallon: I don’t understand your point. Whatever the source, I think that your calculation of DIN release is fine. Could you clarify?

Authors: This line is simply addressing the question of other DIN inputs to the CoVo that may not be accounted for in our calculations (like N2-fixation and porewater advection, on which there is some existing literature). We think this text is fine as written.

Ref2: References There are two papers by Lowe et al., 2015. Please use 2015a and 2015b throughout the text.





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Authors: There is only one Lowe et al., 2015 paper. The other paper is Lowe and Falter 2015.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-413/bg-2018-413-AC2-supplement.pdf

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