

Interactive comment on “Seasonal Net Ecosystem Metabolism of the Near-Shore Reef System in La Parguera, Puerto Rico” by Melissa Meléndez et al.

Anonymous Referee #1

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The presented study has a beautiful dataset of time series measurements over a heterogeneous nearshore environment in Puerto Rico. The manuscript introduction and discussion are clear and well-written and the data produce visually convincing and yearly integrated rates that are rare for these environments. However, I have a number of methodological and technical concerns about the way the data was analyzed and applied, which make the validation of the presented 1D model and its results difficult.

A major issue that I have with this study is the focus on coral reefs and whether the presented 1-D mass balance is reflective of coral reef processes. The monitoring location is on the ocean side of a fringe reef with the mean current coming from offshore and what appears to be a relatively steep reef slope. Thus, concentration changes are indicative of the upstream water column processes and benthic communities while the

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results are extrapolated to the downstream fore reef which occupies a relatively small area. Without data on the footprint of the 1D mass balance, current directions, and flow rates it is challenging to generalize these results to a very heterogeneous area that has shallow fringe reefs, sand flats, seagrass beds, mangroves, and deeper environments all adjacent over ~0.5km distance, especially considering the authors estimate of concentration contributions from up to 6km away (Ln 498). Daily and seasonal changes in current and wind direction (which are present according to Ln 498-499) could significantly bias results that are largely extrapolated to reef processes. For example, if the currents/winds come from the North, a large portion of the signal will come from the large upstream seagrass bed (Figure 2c).

I am particularly concerned with how appropriate the chemical assumptions are (2:1 TA:DIC calcification and 1:1 DIC:O₂ metabolic ratios) when the footprint is situated over non-reef environments? How applicable are these to water column processes (see later comments)?

Another major concern is that the main tracers in the study (pCO₂ and O₂) are treated differently. For example, in Ln 357 and Ln 438, pCO₂ and O₂ are treated differently in that no advective or “HOR MIX” term is applied for O₂. How can horizontal mixing be ignored for O₂ and be treated differently than CO₂? Certainly this requires some discussion, explanation, and validation. Also, see other comments on bubble corrections being applied to only O₂.

Further, much of the results are obtained from an oxygen sensor that has limited marine application and has not been validated sufficiently (Ln 293-298). This section is a bit misleading, as it presents the “best fit” for calibration of the sensors (n=40) whereas a dataset exists that is 500 times larger and is presented in the supplement (n = 21456). It is not clear which calibration is used (Figure S1 or S2). It seems an overall calibration should be reported, not the “best” one. Further, an R² of 0.78 is poor for a calibration curve with so many data points. I also wonder if a salinity/temperature correction would improve these results? At times the variability around the fit curve is 145-195 $\mu\text{mol kg}^{-1}$

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1 (Figure S2) while the total variability is from 145-215 $\mu\text{mol kg}^{-1}$. Unfortunately, it may not be possible to determine which sensor or data are correct given the variability and issues brought up with the sensors used (Ln 272-298). Further, it is not clear why RMSE are reported in Figure S1 and S2 calibrations and p values are reported for calibrations in S3. Overall, this greatly reduces my confidence in the presented O₂ values and fluxes and much more work is needed here to validate these numbers, especially since the authors are using a non-standard O₂ sensor that is not designed for seawater measurements. Finally, the referenced study that previously used this sensor (Xue et al. 2016) only state that it is used in conjunction with Chl A data and “can be used to roughly reflect biological activity in combination with DO% data”, which is not a strong endorsement for the use of this sensor.

Ln 382-397. This section on physical transport leans fairly heavily on the assumptions of Xue et al. 2016 and much more details (at least in the supplement) are needed such as the DIC-S slope vs. discharge relationships from Xue et al., 2016. Seeing that the authors note that there is limited freshwater input (Ln 503) I wonder how the assumptions from Xue et al. fit here, which assume a freshwater end-member? The salinity at the buoy averages 35.3 (Table 2); I wonder what the offshore salinity was and if this method can even be applied / is appropriate for in this environment? It would seem that this physical transport term is negligible from Figure 5; is this due to the fact that there are negligible salinity differences? Therefore, is this even a good indicator of horizontal transport at this site?

Additionally, (Ln 382-386) when were the cruise data from that parameterized the offshore end-member and how were they distributed through the year? How were the concentrations interpolated through the year?

Ln 444 The O₂:DIC ratio is stated as 1.1, but I cannot find this data. Applying this ratio from simple time-series measurements is not straightforward as calcification influences the DIC. More discussion / data is need to validate this, especially considering that any variability in this ratio will affect the NEC rates as defined in Eq. 13. Much more

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discussion and validation seems needed to apply this stoichiometry, especially in this heterogeneous environment where some communities can have DIC:O₂ ratios that vary significantly from 1 (seagrass, sands, water column, etc.).

Ln 621-630. It is finally addressed here that these measurements are not benthic fluxes (i.e. coral reef fluxes) but are integrated across the water column and benthos (over a large, undefined heterogeneous area). Yet, most of the generalization in the paper are for coral reefs. This discussion / caveat should come much earlier and be further explored. For example, assuming the 6km foot print, how does water depth vary over this area (e.g. the relative importance of water vs. benthic), how does the benthic community vary over this area (e.g. sand, grass, reef?), what is the magnitude of the advective term through time, are there any seasonal changes in tides or wind that may cause a different model footprint?

Ln 621-630. The presented 1-D mass balance is very different from incubations (very small area), mesocosms (ex situ), gradient flux (benthic only), Eulerian (defined control volume), Lagrangian (follows a water parcel) – I am not sure the point trying to be made here is but this over simplifies many important differences between these methods. I suggest to delete this whole section.

Ln 646-659. Related to a comment above, this section suggests that water column DOM and POM may be driving the observed changes. I would interpret this as a potential significant influence of water column processes. This should be explored more and discussed more explicitly as differences between benthic and water column processes. See earlier comments.

Overall, many of these issues have been identified and addressed through contrasting “open-water” or 1D-type mass balances to the more advanced techniques cited in this study (Ln 174-177). These techniques (incubations, gradient flux, Eulerian, Lagrangian, etc.) were developed due to the limitations of inferring benthic or water column processes from a 1D balance of time rate of change measurements, especially

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in coastal heterogeneous environments. Unfortunately, with simple time-series measurements and 1D mass balances these methodological biases and caveats remain, and must be addressed and reflected in the presented results and conclusions.

Addressing these concerns will likely lead to a re-analysis of stoichiometry, concentrations and fluxes in the manuscript, a significant re-focusing of the intro and discussion away from reef-specific processes, and a detailed introduction and discussion on the limitation of the use of a 1D model in this environment.

Detailed comments:

Ln 153 – The TA to DIC ratio is reversed here; it should be 2(TA):1(DIC). Considering this, I find that it would be clearer to write Eq. 2 as: $\text{Ca}^{2+} + 2\text{HCO}_3^- \rightarrow \text{CaCO}_3 + \text{CO}_2 + \text{H}_2\text{O}$ to provide the complete stoichiometry and highlight the origin of this 2:1 ratio that is a central tenet to the presented chemistry.

Ln 154 – “DIC:TA ratio” is inconsistent with previous sentence (but consistent with the ratio). I find that the TA:DIC ratio is most appropriate considering the suggested re-write of Eq. 2.

Ln 294. The presented r^2 is different than shown in Fig S1. The units are also different ($\mu\text{mol kg}^{-1}$ vs mmol m^3). It is not clear what calibration is used for what periods. Figure S2 shows a reduced correlation coefficient, and significant variability that greatly reduces the confidence of the O_2 calibration and measurements.

Ln 346-348. This sentence is very awkward.

Ln 376-378. It would be interesting to see the data from the temperature loggers. I find it unlikely that stratification never occurred, but some simple metrics between the top and bottom sensors could easily demonstrate this and possibly it's insignificance (e.g. <1% of time).

Ln 380. Mixing due to a 0.25m tide in a 3m deep water column may not be insignificant (8%) especially when compared with the previous sentence (<0.1%). Also, I would

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assume that some of the horizontal advection is due to tides. Is the horizontal advection assumed to be only due to wind-driven currents?

Ln 403 (Eq9) and Ln 410. The use of “mixed layer water depth” seems odd here considering the water is only 3m deep. It would appear that Eq. 9 needs to be re-parameterized for shallow water environments as it was previously stated that there was no stratification.

Ln 440 – O_2 units in mmol m^{-3} while DIC units are $\mu\text{mol kg}^{-1}$, Please be consistent.

Ln 441-442 “and corrected by the bubble flux injection and the bubble flux exchange (Manning and Nicholson, 2016)” This reference is a bit obscure because it is just a link to MATLAB code without further method explanation. Was this correction done for CO_2 in section 2.6.3? I find it odd that CO_2 and O_2 are not treated the same in terms of air-sea flux. These should be condensed into a single section on air-sea exchange and treated the same.

Ln 490-506. I am confused by this section. In the case of a 1D balance the footprint is simply a function of water residence time, which the authors calculate from the flow velocity. The part that is confusing is starting with a discussion on spatial variability with a unit of change per time (and not space!). Spatial variability should be examined at the same time, or time-corrected for diel variability, and reported as $\pm p\text{CO}_2$ over the 2km transects. This section should be re-written or deleted.

Ln 491-492. What was the variability in the other parameters (SSS, SST, O_2)? Was it consistent with the CO_2 variability? How did the O_2 measurements conducted at this time compare to the MaxTec O_2 gas sensor?

Ln 491-496. Is this the only time period when currents were measured? Did the measurements capture a full tidal cycle? How indicative of yearly flow rates is this? What about seasonal changes in wind direction?

How and from what data was the mean current direction in Figure 2 calculated? Did

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flow always come from offshore? Were there changes in the current direction over a tidal cycle?

Ln 494 “averaged column velocity profiles” it is not clear what this is.

Ln 501-502. “Enrique’s physicochemical” to Enrique Reefs physical-chemical

Ln 509 “On an annual scale, the Enrique reef experiences a seasonal SST daily average variations” These 3 conflicting time-scale adjectives are very confusing.

Ln 521 “Enrique forereef, like many other reefs, is a persistent source of CO₂ to the atmosphere (2.04 ± 2.13 mmol CO₂ m⁻² day⁻¹)” Based simply on the presented SD, I would argue it is balanced and/or not different from zero. Since many of these net rates are around zero with high SD I would suggest some additional statistics to support these conclusions.

Ln 527-528 “The injection of bubbles represents <2 % of the total O₂ flux variation at the site.” I would move this up to the methods and say it is not important and remove it from analysis. Since O₂ is much less soluble than CO₂, the same conclusion can be assumed for CO₂. See earlier comments.

Ln 559 nTA and nDIC are not defined.

Ln 577 see earlier comments on mixed layer

Ln 589 “of the method is that is based on” add it

Ln 690-691. Applying rates determined over a 6km footprint to such a small (and benthic) area is an invalid comparison. It is likely that the forereef rates in this small area (0.07km²) are very different. See earlier comments.

Ln 695-704. Water column processes are ignored in this discussion on benthic reef processes and this section does not address the limits of the presented dataset.

Ln 838-839. “for providing the Be-7 sampling and residence time analyses.” Where is

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this data? It could help explain some of the issues with physical transport and ecosystem attribution (see earlier comments).

Figure 2 – please add scale bars to (and letters) to figures. Lajas is not shown.

Fig S1 and S2 – Please provide p values as is done in S3.

Please add plots of the measured/modeled parameters (SST, SSS, TA, DIC, pH) vs the in situ bottle samples so the reader can evaluate how accurate the modeled and bottle samples are. This could go in the supplement or at least report correlations and p values.

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