- 1 Author Response:
- 2

3 The authors would like to thank the anonymous reviewer #1 for the careful consideration and

- 4 recommendations for our manuscript. Below, we addressed the individual comments in detail.
- 5 Our responses are in indented using a blue color to help the review process.
- 6
- 7 Interactive comment on "Seasonal Net Ecosystem Metabolism of the Near-Shore Reef System in
- 8 La Parguera, Puerto Rico" by Melissa Meléndez et al.
- 9 Anonymous Referee #1
- 10 Received and published: 18 December 2018
- 11

12 The presented study has a beautiful dataset of time series measurements over a heterogeneous

13 nearshore environment in Puerto Rico. The manuscript introduction and discussion are clear and

14 well-written and the data produce visually convincing and yearly integrated rates that are rare for

15 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
- 17 and its results difficult.
- 18

19 A major issue that I have with this study is the focus on coral reefs and whether the presented 1-20 D mass balance is reflective of coral reef processes. The monitoring location is on the ocean side 21 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 22 processes and benthic communities while the results are extrapolated to the downstream fore reef 23 which occupies a relatively small area. Without data on the footprint of the 1D mass balance, 24 25 current directions, and flow rates it is challenging to generalize these results to a very 26 heterogeneous area that has shallow fringe reefs, sand flats, seagrass beds, mangroves, and 27 deeper environments all adjacent over ~0.5km distance, especially considering the authors 28 estimate of concentration contributions from up to 6km away (Ln 498). Daily and seasonal 29 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 30 currents/winds come from the North, a large portion of the signal will come from the large 31 32 upstream seagrass bed (Figure 2c).

33 34

35 We appreciate your comments. We agree the observations are not solely reflecting Enrique coral 36 reef metabolism and that attributing the estimated processes to "Cayo Enrique mid shelf coral 37 reef" could be misleading. Our study does not attempt to evaluate the role of any particular 38 benthic community on NEC or NEP variability. The primary objective of this study is to 39 characterize the temporal carbonate chemistry changes observed by the MapCO2 buoy and to 40 discern the predominant biogeochemical and physical processes that drive said variability. One 41 caveat of this study that should have been stated more clearly, is that it does not provide the relative contributions of different benthic community types to NEC or NEP. We agree that this 42 43 needs to be clarified in the introduction to clearly state that the waters the buoy observes are 44 affected by coastal physical and biological processes associated to the shelf ecosystems of La 45 Parguera, that is indeed comprised of mangrove forests, seagrass beds, unconsolidated 46 sediments, coral reefs, hard bottom carbonate substrates, and phytoplankton communities. This 47 will be clearly stated in the introduction of the revised manuscript. However, although the

48 primary objective of the Atlantic Ocean Acidification test-bed is to monitor near-reef carbonate

- 49 chemistry and explicitly account for the effects of OA and determine its impact on coral reefs,
- 50 this study offers new possibilities to gain meaningful insight into the biogeochemical processes
- 51 occurring in coastal marine environments and which can significantly modulate said impact.
- 52 Furthermore, we believe users of the existing observational OA assets data will benefit from
- application of methods presented to develop further understanding of ecosystem metabolic
- 54 processes.
- Efforts to better understand the hydrodynamics in the area and the extent of the buoy's footprintshould be an essential component of the buoy's observations. A better understanding of how the
- 57 hydrodynamics (e.g., currents/winds) change the footprint and how different functional groups
- 58 affect the disequilibrium between coastal and open ocean waters are essential questions raised
- 59 from this study. However, further observations on the hydrodynamics, residence times, organic
- 60 carbon sources, benthic and fish communities are needed to fully answer these questions.
- 61 We agree that this issue requires attention. For this revision we will provide a conservative
- 62 footprint estimation and re-write the section 2.11 that explains the area over which our
- 63 measurements are influenced (see below). We will also provide a table that shows % cover of the
- 64 different benthic communities and the scaled NEC and NEP presumably attributable to each
- 65 benthic type.
- 66
- 67 Preliminary results on the footprint using Acoustic Doppler Current Profiler (ADCP) located
- about 0.20 km south of the buoy shows that the line of the extent of the footprint is
- approximately 2.63 km from the North East and 1.43 km from the South East (Fig.1). The two
- 70 major current components are 3.38 cm/s, 290° and 6.13 cm/s, 140°. We scaled up to the tidal
- 71 period of 12 hr according to the methodology described by Courtney et al. (2016). This method
- 72 assumes the flow is tidally driven. The primary author did a spectral analysis to check the period
- of the winds and the currents, and the dominant period is coherent with the tides, which gives us
- a good measure of the timescales over which the footprint would be defined. The orthogonal (orside) components of these currents it is challenging to determine due to the weak eastward flow
- 75 side) components of these currents it is channeliging to determine due to the weak eastward now 76 and the "channel" (between two reefs) nature of the location where the ADCP was positioned
- and the challer (between two reels) nature of the location where the ADCI was positioned and where the buoy is located (Fig. 1). Additional evidence of this weak eastward flow from
- 78 hydrodynamic observations in La Parguera (date from 1997) showed that occasionally, the
- reastward tidal component could not overcome the mean westward flow resulting in
- 80 acceleration/deceleration of the westward flow rather than causing east-west reversals
- 81 (unpublished observations). We note that the bathymetric features relative to our buoy asset
- 82 does not support the use of the Principal Component analysis (the method used in Courtney et al.
- 83 (2016)) to describe the footprint.
- 84
- 85 In the revised manuscript we will provide an estimate of the extent of the footprint using
- 86 available ADCP current velocity measurements adjacent to the buoy (March 2017, November
- 87 2016, and from February June 2009). The benthic data will be analyzed to show the % cover of
- the different benthic communities. This data is available through the NOAA Biogeography
- 89 Branch (Bauer et al. 2012).





90 91

92 Figure 1: Mean surface currents at Enrique during November 2016 and the corresponding lines
93 of the extent of the footprint. The white dot indicates the buoy's location. The width of the lobe
94 is the unknown (yellow color).

I am particularly concerned with how appropriate the chemical assumptions are (2:1 TA: DIC

97 calcification and 1:1 DIC: O2 metabolic ratios) when the footprint is situated over non-reef98 environments? How applicable are these to water column processes (see later comments)?

98 99

We will add a sentence on the discussion about this caveat and the assumptions made in thisstudy. It might be important to note that recent work has begun to demonstrate that Redfield may

102 not hold, and in fact may vary, within coral reef ecosystems (e.g., Rosset et al., 2017). For

103 Enrique forereef and Enrique seagrass relative to the

104 offshore station, we observed a mean $\Delta TA/\Delta TCO_2$ ratio of

105 0.7 and 0.4, respectively (Fig.2). The depletion of TA was

- 106 calculated as the difference between reef and offshore TA
- 107 values. It is important to note that this offshore station is 10
- 108 km away from Enrique. In coral reef environments, where
- 109 calcification is dominant (but not the only) process
- 110 affecting seawater chemistry, $\Delta TA/\Delta TCO2$ is near 0.5
- (e.g., Cyronak et al., 2018). Our observations suggest thatthe TA and DIC behavior in the forereef of Enrique is
- 112 the TA and DIC behavior in the forefeet of Enrique 1

indicative of a system where calcium carbonate

calcification/dissolution processes dominate. Figure 2shows that major metabolic and biogeochemical processes

are shown with the calcification path represented on the

- $A_{\rm T}$ -DIC diagram as a slope of 2. While calcification is an
- 118 important process throughout much of the year, respiration

119 particularly in the late spring appears be an additional

120 source of DIC to the system.

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Figure 2: Changes in TA and DIC concentration (normalized to S=35) between Enrique reef, seagrasses relative to offshore waters.

- The slope of the corrected O2 measurements against DIC at the buoy site shows a slope 1.1 126
- (Fig.3) with a weak linear correlation coefficient of $r^2 = 0.35$, but significant (p-value<0.0001, n 127
- = 28340). Variation in this molar stoichiometry (i.e. the P/O ratio) can arise in certain 128
- environments if organic carbon production is coupled to significant uptake of NO_3^- or NH_4^+ , but 129
- 130 this ratio is typical of many other reef ecosystems (e.g., Crossland et al., 1991).
- 131
- 132 Odum et al., (1959) measured the metabolism in Enrique reef using upstream and downstream
- methods and found that the photosynthesis to respiration ratio was 1.15. The concentrations of 133
- dissolved inorganic nutrients based on the concentration of nitrate and phosphate (< 0.03 uM) in 134 the area suggest that the impact of skewed stoichiometry ratios is less pronounced. Potential

- 135
- 136 deviations of these stoichiometries can
- change the NEC and NEP absolute rates, 137 but not the major seasonal dynamics. We 138
- 139 agree these ratios can vary in daily time
- scales, depending on how long a single 140
- 141 community within the footprint affect the
- 142 buoy measurements. However, this doesn't
- 143 change our model's results or conclusions
- but would change the numbers slightly. 144
- 145
- 146 The stoichiometry assumptions may
- 147 produce errors that are negligible because of
- 148 the large natural variability of CO₂ system
- parameters. However, we will add the NEC 149
- and NEP changes associated to these 150
- 151 stoichiometry assumptions on the
- 152 discussion of the revised manuscript.





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161 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 162 advective or "HOR MIX" term is applied for O2. How can horizontal mixing be ignored for O2 163

- and be treated differently than CO2? Certainly this requires some discussion, explanation, and 164
- 165 validation. Also, see other comments on bubble corrections being applied to only O2.
- 166

We did not treat O₂ equally to CO₂ because CO₂ is more soluble than O₂, and therefore less 167

- sensitive to bubble exchange. The O₂ has lower solubility, and thus a larger portion of the total 168
- gas exchange flux is driven by bubbles ($\sim 2.8\%$). In addition to the timescales, both gases are 169
- different. It takes 20 times longer for the CO₂ to equilibrate with the atmosphere than if it 170
- 171 behaved like oxygen. The average CO₂ and O₂ equilibration time are 1.43 and 0.15 months,
- 172 respectively. These timescales for equilibration were calculated according to Sarmiento and
- Gruber (2006) following equation 8.3.11. Therefore, we assumed the O2 horizontal advection 173

- 174 gradients are small, relative to the biological and air-sea exchange processes, because of the
- relatively rapid exchange with the atmosphere (e.g., Emerson et al., 1995). We note the average
- equilibration time of oxygen in at the site is far less than the average residence time as calculatedby Venti (~0.3 months).
- 178

179 Further, much of the results are obtained from an oxygen sensor that has limited marine 180 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 181 500 times larger and is presented in the supplement (n = 21456). It is not clear which calibration 182 is used (Figure S1 or S2). It seems an overall calibration should be reported, not the "best" one. 183 184 Further, an R2 of 0.78 is poor for a calibration curve with so many data points. I also wonder if a 185 salinity/temperature correction would improve these results? At times the variability around the fit curve is 145-195 umol kg-1 (Figure S2) while the total variability is from 145-215 umol kg-1. 186 187 Unfortunately, it may not be possible to determine which sensor or data are correct given the 188 variability and issues brought up with the sensors used (Ln 272-298). Further, it is not clear why 189 RMSE are reported in Figure S1 and S2 calibrations and p values are reported for calibrations in 190 S3. Overall, this greatly reduces my confidence in the presented O2 values and fluxes and much 191 more work is needed here to validate these numbers, especially since the authors are using a non-192 standard O2 sensor that is not designed for seawater measurements. Finally, the referenced study 193 that previously used this sensor (Xue et al. 2016) only state that it is used in conjunction with Chl 194 A data and "can be used to roughly reflect biological activity in combination with DO% data", 195 which is not a strong endorsement for the use of this sensor.

196

197 The Max-O2 sensor provides a correlated, but inaccurate estimate of O2. This inaccuracy could arise from contamination with atmospheric O2 concentrations and the time of equilibration in the 198 199 headspace that was initially developed to be used to measure the CO2 gas and not O2. These 200 interferences do not allow the sensor to capture the extreme (low or high) diurnal seawater O2 201 changes. We think the sources of errors from these sensors are not a significant issue over 202 timescales of greater than daily timescales. However, most importantly, a post-deployment correction was possible using high-accuracy optode O2 and Winkler data over the seasonal 203 cycle. We created a composite diurnal cycle each month with all the measurements available. We 204 found that the MAX-250 sensor daily values > 205 umol kg⁻¹ and < 185 umol kg⁻¹ were not 205 captured. However, most of the MAX-250 O₂ measurements were negatively correlated to 206 207 $pCO_{2,sw}$ and supported our idea of using a post-correction. The best fits were found during early deployments, which supported our hypothesis that the Optode drifted after a couple of months 208 209 after deployment. This is why we corrected the MAX-250+ measurements using the offset and slope of Fig.S1 (L296-297). Still, we wanted to show that the annual cycle agrees favorably with 210 211 the observed Optode seasonal variability (Figure S2). The p-value for both correlations (Figure S1 and S2) is significant with a p-value <0.0001. To clarify the confusion, we will only show the 212 correlation used for the post-correction (Figure S1) and add the p-value of the correlation to the 213 figure caption information. 214

215

216 Xue et al., 2016 used the O_2 saturation level (O%), calculated from the ratio of surface seawater

to atmospheric oxygen partial pressures. However, no post-correction or comparison with an

optode or Winkler data was made. We will add a sentence in Ln-298 to specified that the use of

the Max-O₂ measurements cannot be used alone to calculate the dissolved O₂ content in surface

220 water and that a post-correction should be made to use this as a proxy for NEP. Also, we intend

to describe the seasonal variation, and further investigation should be made to accuratelyestimate daily NEP rates.

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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.

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229 Continental riverine discharges play a very minor role in this site, excepting the seasonality of 230 salinity, which is remotely influenced by large South American rivers. The seasonal evolution of salinity distributions as influenced by these rivers and their potential impact on carbonate 231 232 chemistry of Caribbean reef environments is a topic ripe for future research (e.g., Fournier et al, 2017). We assumed the DIC and TA-S slopes are minimally affected by local river inputs. See 233 234 the next comment. We did not explicitly consider the slope-discharge relationships of Xue et al., 235 (2016), but instead used this reference to highlight the high uncertainty encountered when 236 estimating the horizontal mixing term. We will revise the text to make the clarification.

237

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

243 differences? Therefore, is this even a good indicator of horizontal transport at this site?

244

The average salinity offshore is 35.96. We observe a reasonable relationship and a clear signal 245 between TA and DIC with salinity (e.g., Fig. 4-A). Therefore, we assumed that any TA and DIC 246 changes from the ocean-end member to the buoy site are express albeit attenuated in the coastal 247 248 region by the local nearshore processes. Changes in TA and DIC in the oceanic end member 249 could arise from biological nutrient uptake from phytoplankton communities. However, pelagic net primary production (and more importantly net community production) in these oligotrophic 250 waters is considered to exert a minor influence on DIC and TA variation (see author comment to 251 referee #2). Associated errors to this assumption are considered on our Monte Carlo simulations, 252 253 and further modeling efforts could provide a better understanding of the advective processes in 254 this area. Figure 4-B also shows the different end-members in the area and their relative TA to

- 255 Salinity signature.
- 256

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Figure 4: A) The DIC and Salinity correlation for the oceanic and reef water. B) Linear
correlations of Salinity with bottle TA for the Enrique reef, Enrique seagrass, the bio Bay and
inner mangrove channels.

279

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

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284

The cruises include data from three cruises to the CaTS station on 2011 and 2012. The A22

CLIVAR and WOCE transect, the Global Ocean Data Analysis Project, the Carbon Dioxide in
 the Atlantic Ocean (CARINA) and other cruises around the area in different years from 1997 to

288 2008. The dates are Nov- 2005; Dec-2008; Feb-March 2008; August 1997; Feb-March 1982;

289 Oct-2003. Around 237 samples were used for TA and 220 for DIC. The data seems not to show

seasonal or interannual variability (Fig.5). Please note, that the data points represent different

291 locations in the Caribbean Sea and that there are no long-term measurements of TA and DIC

from a single location. These data were only used to determine the TA and DIC to salinity

293 relationship for the ocean-end member. Part of this information is in the supplemental material

- 294 **(S3)**.
- 295



Figure 5. Seasonal and interannual TA measurements in the Caribbean ocean. This data set was
used to establish the TA and DIC salinity correlations for our ocean end-member.

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297

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 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.).

309

310 See the previous comment for more details.

311

312 Ln 621-630. It is finally addressed here that these measurements are not benthic fluxes (i.e. coral 313 reef fluxes) but are integrated across the water column and benthos (over a large, undefined 314 heterogeneous area). Yet, most of the generalization in the paper are for coral reefs. This 315 discussion/caveat should come much earlier and be further explored. For example, assuming the 316 6km footprint, 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 317 the magnitude of the advective term through time, are there any seasonal changes in tides or 318 319 wind that may cause a different model footprint?

320

The authors agree, and this issue will be addressed in the abstract and introduction of the revised
manuscript. We will be careful with the generalization of coral reef benthic fluxes in the revised
document. Additionally we will enhance this section to strengthen the concept. There are no
significant seasonal wind changes. For more information about the benthic community's changes

- 326 in La Parguera, please refer to Referee #1's author comments.
- 327

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
- 331 over simplifies many important differences between these methods. I suggest to delete this whole
- 332 section.

- 333
- We consider this section is relevant because it provides information for future comparison to be made. For example, Courtney et al. (2016) assessment of various methods show where his NEC
- 336 chemistry points lie. See Referee #1's author comments for details. We will add Figure 3 in the
- **337** Referee #1's author comments to the supplemental information to offer a better perspective on
- where our results lie in comparison to other methods used to estimate NEC in reef environments.
- 340

Ln 646-659. Related to a comment above, this section suggests that water column DOM andPOM may be driving the observed changes. I would interpret this as a potential significant

343 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.
- 345

Agreed and author will add discussion on this in the introduction of the revised manuscript. See
Referee #1's author comments for more details on the DOM and POM fluxes.

348

349 Overall, many of these issues have been identified and addressed through contrasting "open-350 water" or 1D-type mass balances to the more advanced techniques cited in this study (Ln 174-351 177). These techniques (incubations, gradient flux, Eulerian, Lagrangian, etc.) were developed 352 due to the limitations of inferring benthic or water column processes from a 1D balance of time 353 rate of change measurements, especially in coastal heterogeneous environments. Unfortunately, 354 with simple time-series measurements and 1D mass balances, these methodological biases and 355 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 356 fluxes in the manuscript, a significant re-focusing of the intro and discussion away from reef-357

- 358 specific processes, and a detailed introduction and discussion on the limitation of the use of a 1D 359 model in this environment.
- 360

361 The authors will: 1) remove the section on "free energy", 2) summarized the trivial information on carbonate chemistry in the introduction, 3) provide an early introduction to the ecosystem 362 metabolism, 4) addressed the caveats and assumptions of the stoichiometry, benthic fluxes used 363 in the 1-D model to make sure that the caveats and uncertainties are clear for the reader in the 364 365 introduction, 5) analyze the ADCP data available for the site from 2009-2017 and calculate an effective footprint area (according to Courtney et al., 2016) for the two seasons (summer and 366 367 winter), and 6) assess the benthic communities (Pittman et al. 2010) within this effective 368 footprint.

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- 372 Detailed comments:
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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: Ca2+ + 2HCO3- > CaCO3 + CO2 + H20 to
provide the complete stoichiometry and highlight the origin of this 2:1 ratio that is a central tenet
to the presented chemistry.

- 378
- 379 Yes. This is clarified in the revised paper.
- 380

- 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 rewrite of Eq. 2.
- 383

388

384 Yes. This is clarified in the revised paper.

Ln 294. The presented r2 is different than shown in Fig S1. The units are also different (umol kg1 vs mmol m3). It is not clear what calibration is used for what periods.

The r2 in Ln 294 is the rounded r2 show in Fig.S1. The authors will be limited to the exact value
shown in Fig. S1 for clarification. The units will be modified to show consistency. Fig. S1 is the
calibration used (Ln 296-297). We will only provide the calibration curve used to avoid
confusion. See previous comments.

Figure S2 shows a reduced correlation coefficient, and significant variability that greatly reducesthe confidence of the O2 calibration and measurements.

The correlation coefficient is significant with a p-value <0.0001 for n=21456. The modeled was verified with both corrections, and no significant difference was observed on NEP and NEC.

- 400 Ln 346-348. This sentence is very awkward.
- 402 The sentence was rewritten.
- 403

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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 the time).

407

The surface average was 28.63°C, and the middle and bottom averages were 28.63°C and
28.58°C (Fig.6). A two-sample t-test was performed for the surface and bottom results. The test
shows that the two means are not significantly different at the 5% significance with a p-value =
0.56 showing that a significant difference does not exist. We will add this information to the
supplemental material. The change in density between the surface and bottom measurements is <

- 413 0.09kg/m) and the mean Brunt–Väisälä frequency is 1.1149e-04 with not significant changes (p-
- 414 value <0.001).









- 420
- 421 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 assume that some 422 of the horizontal advection is due to tides. Is the horizontal advection assumed to be only due to 423 424 wind-driven currents? 425 426 The SSS changes due to tides are assumed to be small here because the semidiurnal tidal currents 427 exhibited amplitudes of approximately 10-20 cm/s and oscillated about the mean and lowfrequency spectrum. These high frequencies (<24-33 hr) represent 6% of the total variance on 428 429 pCO₂. 6 % of the total may be important, but this term also includes noise, tides, and biological 430 processes occurring on a daily scale. This correlation is likely tidal aliasing of diurnal (photoperiod) processes. The average salinity changes throughout a tide (12 hr period) is -431 7.3461e-05 with a standard deviation of 0.0217 units. This change does not alter our TA and 432 433 DIC. Most all of the correlation with tides is eliminated using a 24 hr averages. 434 435 Ln 403 (Eq9) and ln 410. The use of "mixed layer water depth" seems odd here considering the 436 water is only 3m deep. It would appear that Eq. 9 needs to be reparameterized for shallow water 437 environments as it was previously stated that there was no stratification. 438 439 We will change the term mixed layer to the water column. Equation 9 specifies that the 440 measurements should be integrated through the water column or mixed layer depth. 441 442 Ln 440 –O2 units in mmol m-3 while DIC units are umol kg-1, Please be consistent. 443 444 We agree that the use of different units can be confusing and will change the O2 units to umol 445 kg-1 accordingly. 446 447 Ln 441-442 "and corrected by the bubble flux injection and the bubble flux exchange (Manning 448 and Nicholson, 2016)" This reference is a bit obscure because it is just a link to MATLAB code 449 without further method explanation. Was this correction done for CO2 in section 2.6.3? I find it 450 odd that CO2 and O2 are note treated the same in terms of air-sea flux. These should be 451 condensed into a single section on air-sea exchange and treated the same. 452 453 See previous comments. We will provide the reference the Matlab code is based on (Manning et 454 al., 2016). 455 456 Ln 490-506. I am confused by this section. In the case of a 1D balance the footprint is simply a 457 function of water residence time, which the authors calculate from the flow velocity. The part 458 that is confusing is starting with a discussion on spatial variability with a unit of change per time 459 (and not space!). Spatial variability should be examined at the same time, or time-corrected for 460 diel variability, and reported as +/- pCO2 over the 2km transects. This section should be re-461 written or deleted. 462 463 See the previous comments. 464 465 Ln 491-492. What was the variability in the other parameters (SSS, SST, O2)? Was it consistent
- 466 with the CO2 variability? How did the O2 measurements conducted at this time compare to the
- 467 MaxTec O2 gas sensor?

- 468
- 469 This data is under analysis, and it is part of the biogeochemical component of Sea Grant project
- 470 "Natural Coastal Barriers at Risk: A First Assessment of Biogeochemical and Physical
- 471 Stressors." We haven't analyzed the oxygen measurements of these spatial surveys. Results are
- 472 in the process of rigorous QA/QC. Table 1 shows a summary of the results for pCO2, Temp and
- 473 Salinity and Figure 7 shows the area covered.
- 474

475 We also carried a 16-hour sampling near the MapCo2 buoy in where we took TA discrete

476 measurements. Below the NEC and NEP results using the TA-anomaly technique and the 1-d

477 model (Table 2, Fig. 8). Both methods show the increase in TA from dissolution processes

478 during the night coupled with an increase in respiration. Note that our model has not been479 parametrized for daily processes. These results are preliminary and need further verification.

480

481 Table 1: These statistics are from a spatial sampling carried out the 30^{th} - March - 2017. See 482 Figure 7.

	Sal (psu)	Temp (°C)	pCO ₂ (uatm)
Median	36.18	28.43	406.97
Mean	36.18	28.37	407.02
Std	0.0091	0.13	7.18
Max	36.21	28.56	461.00
Min	36.10	28.15	381.66

483

484

485 Table 2: NEC and NEP from the diurnal sampling at Enrique on March 2-3, 2018. The first NEC

486 is calculated with the 1-d model using pCO2 and O2 as proxies, the second NEC uses the

487 discrete measurements and the TA-anomaly technique, and NEP is computed with an Aanderaa

488 Optode sensor. These statistics are from the cumulative sum of each parameter. See Figure 8.

489

	NEC (mmol m^{-2} day ⁻¹) –	NEC (mmol m ⁻² day ⁻	NEP (mmol C m ⁻²
	1-d model	$^{1}) - TA$ anomaly	day ⁻¹)
Median	6.92	3.83	36.44
Mean	6.94	3.46	35.21
Std	6.73	2.88	15.54
Max	23.30	9.47	67.96
Min	-6.54	-2.69	1.00

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496 Figure 7. Sea surface pCO2 salinity, and temperature spatial maps for La Parguera on March

497 30th, 2018. The star represents the buoy location. The maximum distance traveled from the buoy
498 to offshore was around 1.5 km. Currents near the surface are predominantly directed inshore

- 499 from the southeast. On average, the buoy is predominantly seeing shelf waters from the southeast
- 500 areas.
- 501



503Figure 8. Cumulative sum of NEC and NEP (mmol $m^{-2} day^{-1}$) from a 16-hour sampling at504Enrique forereef on March 2-3, 2018. The black color represents the NEC calculated using the 1-505d model, the red color represents the NEC calculated from the discrete measurements using the506TA-anomaly technique, and the blue color represents the NEP from an Aanderaa Optode sensor.507Positive values are dissolution and respiration and negative calcification and photosynthesis.508LCT = local time.

509 510

Ln 491-496. Is this the only 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 flow always come from offshore? Were there changes in the current direction over a tidal
cycle?

516

517 There are ADCP field observations from approximately one month in March 2017 and

518 November 2016 and a 5-month time series from February – June 2009. We only have used the

519 deployment of November 2016. This data is under analysis, and it is part of the hydrodynamic

component of Sea Grant project "Natural Coastal Barriers at Risk: A First Assessment of
 Biogeochemical and Physical Stressors."

522

523 Different monitoring events have been taken place over La Parguera shelf in support of

524 dispersion/recruitments of fish larvae and other projects. In summary, all the field observations

525 near Enrique's west-end indicate that surface current is predominantly directed to the west-

northwest in alignment with the channel orientation. While these currents can reach up to 10

527 cm/s depending on the tide phase and wind forcing. The wind direction doesn't show seasonal
 528 changes, and it is predominately coming from the south-east (Fig.10). Other observations from

changes, and it is predominately coming from the south-east (Fig.10). Other observations from
McGillis et al., 2011 reported a predominant current in the directed towards the west, parallel to

the axis of the reef at a speed of 2-10 cm s-1. Drogues released in the fore-forereef of Enrique

and the ADCP time series indicated that the circulation of coastal waters at La Parguera is

towards the west-north-west with an overall velocity of 7 cm s -1, with no changes in the

- direction (details on Esteves, 2005; Williams et al., 2009, McGillis et al., 2011).
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We changed the text accordingly.

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.

We changed the text accordingly. The part "from daily averages" will be removed from this
sentence since it is specified in the methods that the composite year is the result of daily averages
(Ln 463-469).

556 Ln 521 "Enrique forereef, like many other reefs, is a persistent source of CO2 to the atmosphere 557 $(2.04 \pm 2.13 \text{ mmol CO2 m-2 day-1})$ " Based simply on the presented SD, I would argue it is 558 balanced and/or not different from zero. Since many of these net rates are around zero with high 559 SD I would suggest some additional statistics to support these conclusions.

- 561 We will add the corresponding statistical result to this sentence.
- 562 563 Ln 527-528 "The injection of bubbles represents <2 % of the total O
 - 563 Ln 527-528 "The injection of bubbles represents < 2 % of the total O2 flux variation at the site." I 564 would move this up to the methods and say it is not important and remove it from analysis. Since
 - 565 O2 is much less soluble than CO2, the same conclusion can be assumed for CO2. See earlier
 - 566 comments.

567	
568	Agree. See earlier comment's response.
569	
570	Ln 559 nTA and nDIC are not defined.
571	
572	It is defined in the caption of figure 6. We will add this to the main Ln 559 accordingly.
5/3	
574	Ln 5// see earner comments on the mixed layer
575	The mixed layer is changed to water column denth accordingly
577	The mixed layer is changed to water column depth accordingly.
578	Ln 589 "of the method is that is based on" add it
579	
580	We will modify it accordingly.
581	
582	Ln 690-691. Applying rates determined over a 6km footprint to such a small (and benthic) area is
583	an invalid comparison. It is likely that the forereef rates in this small area (0.07km2) are very
584	different. See earlier comments.
585	
586	We will modify it accordingly.
587	
588	Ln 695-/04. Water column processes are ignored in this discussion on benthic reef processes and
589	this section does not address the limits of the presented dataset.
590	We will modify accordingly to the footprint estimated and the limit of the observations. See
592	earlier response's comments
593	
594	
595	Ln 838-839. "for providing the Be-7 sampling and residence time analyses." Where is this data?
596	It could help explain some of the issues with physical transport and ecosystem attribution (see
597	earlier comments).
598	
599	These data are presented in Ln 257-261. We will add a couple of sentences explaining the
600	ecosystem the physical transport and ecosystem attribution in the discussion section.
601	
602	Figure 2 – please add scale bars to (and letters) to figures. Lajas is not shown.
604	We will modify the figure accordingly
605	we will modify the figure accordingry.
606	Fig S1 and S2 $-$ Please provide p values as is done in S3. Please add plots of the
607	measured/modeled parameters (SST, SSS, TA, DIC, pH) vs the in situ bottle samples so the
608	reader can evaluate how accurate the modeled and bottle samples are. This could go in the
609	supplement or at least report correlations and p values.
610	
611	We add the p-values for Figs S1 and S2. The measured, bottle and modeled observations are
612	presented in Fig 1. We will add a table to the supplemental information with the corresponding
613	statistical results between the measured, bottle and modeled data.
614	

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