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Interactive comment on "Photosynthetic activity buffers ocean acidification in seagrass meadows" *by* I. E. Hendriks et al.

Anonymous Referee #2

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The manuscript by Hendricks et al. described measures of water column [O2], pH and calculated carbonate system parameters over five Posidonia meadows and some bare patches in shallow water off the island of Mallorca. The primary objective of this paper was to evaluate the effects of seagrass abundance and hydrodynamics on the on chemistry of the overlying water column, and determine if they were large enough to offset the impacts of future acidification on calcifying organisms (leaf epiphytes) within the meadow. Results indicate clear diurnal patterns in [O2] and pH that most likely resulted from photosynthesis and respiration of the seagrass meadows. Further, they showed that photosynthetic activity could raise pH sufficiently to increase aragonite saturation (Omega) that might have significance for calcareous organisms.

Unfortunately, the use of a single sensor unit at a fixed location did not allow the authors

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to resolve spatial variability or advection processes sufficiently to quantify ecosystem metabolism in absolute units. As such, the results must be viewed as being semi-quantitative at best since the relationships (i.e., slopes) are not readily generalizable to other seagrass ecosystems. Furthermore, the statement claiming statistical significance of the relationship between maximum OmegaAr and leaf CaCO3 (Fig. 6, Results pg 11, lines 22-23) is not consistent with my re-analysis of the data plotted in Fig. 6 (re-digitized from Fig. 6 using Data Thief), which produced the following:

Regression Slope = 22.6+/20.5 Intercept = -25.9+/- 93.1 r2 = 0.11 Regression F = 1.22 df =10

A statistically significant linear relationship between maximum ïĄŮAr and leaf CaCO3would require an F > 4.1, even a significant correlation would require an r > 0.57 (equivalent to r2 > 0.32).

The Discussion is very general, covers well traveled ground, and is largely a mass balance argument. Although generally accurate, there is very little new here.

Consequently, this study did not extend our quantitative understanding of the effects of seagrass abundance and hydrodynamics on water column chemistry beyond that which has already been published more than once in the literature, and several of those papers were cited in this Ms. In sum, this is not the first paper to demonstrate that, given sufficient biomass, residence time for the water and sunlight to drive photosynthesis, seagrass metabolism may be able to at least transiently buffer the effects of ocean acidification on carbonate system parameters.

The paper also suffers from some confusing passages, weak presentation and interpretation of the data (e.g. analysis of Fig. 6). At this point, I am not convinced that it represents a significant new contribution to our understanding of seagrass impacts on ocean biogeochemistry to warrant its publication as a full length paper. Perhaps a much abbreviated note, providing the most salient (and significant) results, with a clear/concise summary of their significance would be publishable.

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A revised manuscript should also clearly address the following specific comments:

Abstract: Pg 2 Line 2: ". . ...diel pH in shallow. . ..." should read ". . ...diel pH change in shallow. . ."

Introduction: Pg 4 Lines 20-22 overstate the degree to which "metabolic and structural traits believed to drive these changes have not yet been resolved". It is well known that density (biomass), metabolic rate and water residence time are the key drivers.

Methods: Pg 5, last line & pg 6 first line: Measurements should be presented in chronological, not seasonal order – September 2011 first, then June 2012.

Pg 6, line 3: So with the exception of Magalluf, sites were only visited once? This does not allow you to make any significant inferences regarding temporal patterns.

Pg 6, line 6: What separated the patches? Bare sand or rocky reef?

Pg 6, line 7: Bare patches ranging from 2 to 20 m represents a considerable range in size and water residence time. How did you control for that?

Pg 6 line 11: A single sensor system in the middle of the "patch" is hardly state-of-the art and not sufficient to determine community metabolism, because you can't determine the integration scale of water upstream. This fundamentally limits the ability to make conclusions from these data. It would have been more appropriate to employ upstream-downstream, control volume and/or eddy correlation for these objectives.

Pg. 6 line 17: For which sites were the data lost? How does this affect the final distribution of samples across sites and dates? If you don't have data from certain sites, then you didn't really sample them, and the other data (shoot density, etc.) should not be presented here.

Pg 6 lines 22-26: The Methods describes time series of light measurement collected with the marginally accurate HOBO sensors, and additional data from a meteorological station at Ses Salines, but only the HOBO data were incorporated in the analysis (Table

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2, Fig, 2). If the met data from Ses Salienes were not presented, or even used in these analyses, their existence is irrelevant should not bementioned in the Methods.

Pg 7 lines 11-12: The reluctance to calculate metabolic rates is understandable, however it also undermines the value/novelty of the information presented here relative to prior existing knowledge.

Pg 7 lines 19-20: Velocity is, by definition a vector (directional) quantity. If directionality was ignored, the resulting values should be called “speed” or something other than velocity.

Pg 8, lines 8-9: If seagrass structural parameters were measured with replication (6 -8 quadrats at each site), why were no error estimates provided in Table 1?

Pg 8 line 15: One cannot determine organic carbon content from simple loss-on-ignition. This needs to be corrected.

Results

Pg 9 lines 7- 13: September 2011 O2 concentrations were lower than what? June 2012? Do these limits represent max and min diurnal values? Mean and range O2 values presented in Table 2 (not Table 1 as indicated in the text) are not terribly useful without temporal context.

Pg 9 last line, pg 10 lines 1-3: This pH range is pretty small, and similar to what one would expect from a doubling of CO2 in the atmosphere (CO2SYS predictions). So, if coastal/estuarine dynamics are already so large as to make ocean acidification unimportant (Duarte 2013), why should the range reported here (similar to expectations from OA) be important?

Pg. 9, line 3: Capitalize Bay

Pg 9 line 6: Since the data were insufficient to resolve the advection term adequately, how do you know that the patterns were caused by the seagrasses meadow, especially

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when the seagrass data were not shown?

Pg 9, lines10-12: How does pH vary further offshore, and how do you know the observed changes were due to seagrass?

Pg 10, lines 11-20: Presentation of the relationships between LAI, O2 and pH is very confusing. The significant positive relationship between LAI and [O2] needs to be illustrated with at least one figure, as no data are provided. Parenthetically indicated values of F and r2 are insufficient, particularly given that the statistical significance claimed for maximum ïĄŮAr and leaf CaCO3 (Fig. 6) cannot be reproduced. Furthermore, the relation between LAI and pH seems tenuious at best – the necessity to rely on mean/min/max values, rather than metabolic fluxes really hurts the paper.

Pg 10, lines 21-22: Given that biomass and LAI are strongly correlated in your data set (linear regression of data in Table 1 reveal LAI = 0.0034*Biomass – 0.37, r2 = 0.64), it is surprising that the relation between maximum OmegaAr and biomass was not also significant. In any event, one needs to be extremely careful constructing GLM models from variables (LAI & Biomass) that are not independent (Table 5).

Pg 10, line 26: Statistical significance of correlations (r) is not determined by an F test. Are these regression results? If so, please provide r2 values, in addition to F. We need to know if the relationship has any predictive power, not just whether it is significant.

Pg 11, lines 1-6: O2 doesn't influence pH. You're using it as a proxy for photosynthesis. Metabolism is the driver here. Further, less important than identification of "influences" at this stage would be getting at predictive power, i.e., slopes and r2; "influences" are predictable from mass balance and simple biogeochemistry: CO2 + H2O = CH2O + O2.

Pg. 11 lines 7 – 8: Exactly how were residence times determined? And resident over what? Patches of undefined dimension? No data on patch dimensions and water depths were provided that would support these estimates. Furthermore, the times

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seem rather short if the changes in water chemistry parameters are simply local. For example, a residence time of 0.05 h is equivalent to 3 minutes, during which time it is difficult to get an accurate estimate of O2 flux using a leaf segment enclosed in a laboratory O2 electrode, much less an open system such as this. Clearly, the water is being influenced by more area than the small patches that are only partially described here.

Pg 11 lines 9 – 13: So, you really have no way to constrain any confidence estimate on residence time. In which case, I suggest eliminating the entire section.

Pg 11 lines 17 – 20: This is a little surprising; one would expect mixing to increase air-sea exchange, thereby keeping the pH, and OmegaAr, high. Or were they out of atmospheric equilibrium because of CO2 depletion? In any event, an explanation is necessary, esp. since you don't really know the source of the water being measured. Further, I don't place much confidence in the regression of TKE vs max OmegaAr, as statistical significance, and the negative relationship, appear to rest on a single data point (0.00025, 4.2).

Pg 11, lines 22-24: I get very different statistical results when I perform a regression analysis on the data in Fig. 6 (see general comments above). This needs to be sorted out.

Pg 12, lines 1-9: Poor sentence structure here makes the paragraph hard to understand. In what way were they "important"? Simply by the minimum TKE? Since many of these parameters are correlated (LAI, O2 range, TKE etc), how can you load them into a GLM model as independent predictors? And why are you using the Aikaike index, relative to other least squares approaches?

Discussion Pg 12 line 13: . "Change" is, by definition "dynamic", which makes "dynamic changes" a redundant passage.

Pg 12 lines 15-17: This is a poor argument as it confuses large-scale means with

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local oscillations that lie on top of the means. The global temperature is rising, but not everywhere equally, and not at the same rate. Further, we still get cold weather. Another example – Keeling's CO2 curve shows clear seasonal oscillation (winter CO2 is higher than summer). But the mean CO2 keeps rising. In any event, none of the short term oscillations describe here have anything to do with the long term trend.

Pg 12, lines 21 – 26, pg 13 lines 1-4: This passage is largely correct, but contains no new information relevant to this study.

Pg 13, lines 8 -9: This is simply a mass balance argument; again nothing new here.

Remainder of Pg 13 – 16: much of this, esp Sec 4.2, is general literature review, and covered extensively in other publications. I don't disagree with it, but it's hardly new and barely mentions any of the results presented here.

Tables were inadequately prepared and described, and several references in the text appear incorrect. Description of data in headers and presentation in tables were not sequentially consistent.

Figure 2 provides a representative plot of oxygen concentration. Evolution of O2, as stated in the legend, represents a change or flux, and must, by definition include a time component.

Figure 5: a) and b) sections should be identified on the figures.

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