

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

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The main comments of the reviewer are that 1. Our study did not quantify ecosystem metabolism in absolute units and 2. The relationship between maximum omega aragonite and leaf CaCO₃ (Figure 6) is not statistically significant.

1. Comment: Resolving ecosystem metabolism in seagrass meadows is not straightforward, as approaches involving chambers, the most common approach in the literature (Duarte et al. 2010), interferes with the effect of hydrodynamics and turbulent mixing in buffering effects on pH and omega and would have, therefore, have inflated the fluctuations in omega relative to those actually occurring in the meadow. The alternative of using eddy correlation approaches to resolve unconfined metabolic rates was not possible because, beyond the technical difficulty of this approach, eddy correlation

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approaches work best when there is a unidirectional flow, such as in tidal dominated systems, and is less reliable in wave dominated deep meadows. This makes it difficult to design a set-up with upstream and downstream sensors to properly resolve advection processes. Hence, we argue that a relationship between changes in omega and pH and structural parameters is far more useful than a relationship with metabolic rates, and LAI is easily measured and can be resolved at large scales, thereby providing options to map where conditions may be most suitable for calcifiers, where metabolic rates would be cumbersome to derive. Nevertheless, we deployed ADVs in our study, which allowed us to evaluate water movements, TKE and roughly calculating a residence time for the water masses. This is valuable information, rarely reported in studies from a biological vantage point, while more oceanographic oriented studies usually refrain from detailed biological measurements (at appropriate time scales) at their reference sites for long-term monitoring, like the LTER sites (Hofmann et al. 2013).

Action: We now improved the specification of the goal of the study and the reason why relationships with structural parameters, rather than metabolic rates, were examined. Part of the introduction now reads: “Focussing on the effect of structural traits on pH instead of direct measurements of metabolism has an advantage as approaches involving chambers, the most common approach in the literature to determine metabolism (Duarte et al. 2010), interferes with the effect of hydrodynamics and turbulent mixing in buffering effects on pH and omega and therefore might inflate the fluctuations in saturation states relative to those actually occurring in the meadow. Open water determinations of metabolism work best when there is a unidirectional flow, such as in tidal dominated systems, and a set-up with upstream and downstream sensors to properly resolve advection processes is used. Another advantage is that LAI is easily measured and can be resolved at large scales, thereby providing options to map where conditions may be most suitable for calcifiers, where metabolic rates would be cumbersome to derive.”

2. Comment: We realize that the relationship between maximum omega aragonite

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and leaf CaCO₃ should have been described more clearly and that it may have come across sounding more significant than the statistics actually demonstrated. In lines 21-25 on page 12319 we described our statistical analysis as:

“We tested (1) the effect of oxygen production and structural parameters of the meadow and (2) the effect of hydrodynamics on the carbonate system (pHNBS and Ω Ar) using univariate ANOVA in separate Models in JMP (SAS) and (3) the effect of Ω Ar (min, max, mean) on the carbonate load of the leaves with an ANOVA considering all carbonate parameters (min, mean, max, range).”

In this paragraph it is clearly stated (under 3) that the analysis was different from the others where we used univariate analysis. In the Result section (page 12322) we state:

“Even though the full model for Ω Ar (max, min and mean) vs CaCO₃ was not significant ($r^2 = 0.56$, $p = 0.05$), the maximum Ω Ar within the canopy was correlated with the calcium carbonate load of the leaves ($F = 5.73$, $p < 0.05$, Fig. 6), thereby providing a direct link between Ω Ar and net carbonate deposition on the leaf surfaces.”

This is not equal to a significant linear relationship between maximum Ω Ar and CaCO₃. We do see how our figure 6; highlighting only the relationship between max Ω Ar and CaCO₃, with the linear regression formula in the legend, could be confusing and lead the reader to believe we pictured a significant linear regression.

Action: We have revised all our statistics, updated the paragraph on statistics and we clarified the text and updated Figure 6 as a panelled figure with mean, max, min and range Ω Ar. The section on statistical analysis now reads:

“2.4 Statistical analyses We tested the effect of structural parameters of the meadow on oxygen concentrations (mg L⁻¹) and the carbonate system (pHNBS and Δ Ar) with a Generalized Linear Mixed Model (GLMM) in R (lme4 package) using site (variation of daily mean, max, min and range per day) as a random factor. We tested the effects of hydrodynamics on Δ Ar using Generalized Linear Models (GLM) in R since the

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distinction between seasons meant we were left with too few data points to evaluate a random effect. We used the same approach (GLM) for the analysis of the effect of Δ Ar (max, min, mean, range) on the carbonate load of the leaves. We evaluated which set of parameters (structural, hydrodynamic, metabolism) was the best predictor for Δ Ar in the meadow by model selection using Akaike’s Information Criterion (AIC). As our several structural and hydrodynamic parameters are auto-correlated, we performed a Principal Component Analysis (PCA) in R to obtain 1 principal component for the structural and 1 for the hydrodynamic set explaining most of the variation which we used as input, together with the range of oxygen concentrations for our GLMM (prediction Δ Ar) or GLM (prediction CaCO₃). In figures portraying correlations with max, min, and range pH and Δ Ar, linear regressions are based on repetitive measurements and significance should be inferred from the GLMM models.”

Another main comment is the fact that the discussion is very general, and covers well travelled ground.

Comment: We agree that a more balanced discussion is needed. This paper is by no means the first to demonstrate the buffering effect of metabolically active coastal vegetation on the carbonate system parameters. However, most of the literature on this subject is descriptive (i.e. Hoffman et al 2011) and this study adds to previous ones in examining the relationship between fluctuations in omega and pH and structural descriptors of seagrass meadows.

Due to the seasonal measurements collecting the data was spread out over 2011 and 2012; these kinds of campaigns take time and do not allow for rushed publishing. Even though we are not the first to point out the fact that pH fluctuates in these areas, and not even the first to measure these fluctuations in seagrasses (see Invers et al in 1997), we believe we make a strong case evaluating various structural parameters including photosynthetic area (LAI) to these fluctuations, and while LAI is signalled as the best predictor, this is not a standard measurement as now generally density and biomass are assessed.

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Action: We have improved the specification of the contribution of this study and give due credit to previous studies that have demonstrated broad pH fluctuations in metabolic-intense communities. The discussion section now reads: “The capacity to modify coastal pH in shallow near-shore water with submerged vegetation is widespread, in areas with seagrass (Buapet et al. 2013; Hofmann et al. 2011; Invers et al. 1997; Semesi 2009; Schmalz & Swanson 1969) and as well as in macrophyte habitats in general, such as kelps (Delille et al. 2000; Frieder et al. 2012; Hofmann et al. 2011; Menendez et al. 2001; Middelboe & Hansen 2007), but the magnitude of buffering will depend on both structural and metabolic parameters and hydrodynamic processes of each system.”

The reviewer has pointed out some confusing sentences and weak presentations of the data; we are very grateful for such detailed evaluation of the manuscript and have addressed the comments point by point below, greatly improving the revised version of the manuscript.

Abstract: Pg 2 Line 2: “.diel pH in shallow:.” should read “.diel pH change in Shallow. . .”

Action: Corrected

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.

Comment: We agree there is a general consensus about the fact that metabolic rate, biomass and water residence time affect pH variability in seagrass meadows. However, the fact that a relationship can be postulated does not mean that this relationship actually exist or that the strength of the relationship can be anticipated. Indeed, Duarte et al. (2010) found only a weak relationship between seagrass biomass and metabolic rates across seagrass meadows. Hence, the test and description of relationships between fluctuations in omega and pH and structural parameters of seagrass meadows

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is indeed a novel contribution to our understanding on the effect of these habitats on omega and pH conditions.

Action: Rephrased to “Whereas the capacity of Mediterranean *P. oceanica* meadows to affect pH is well characterised, and the relationship between pH variability, metabolic activity and water residence times in seagrass meadows have been described, detailed and simultaneous evaluation of several structural traits believed to drive these changes to assess which of those is the most important is still lacking.”

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

Action: Corrected, throughout the manuscript.

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.

Comment: The examination of temporal patterns was not a goal of the study, which focussed on the period of peak metabolic rate and LAI (June to September). Our first goal was to evaluate the carbonate system in sites with a wide range of structural parameters. Indeed, seasonal patterns of metabolism (Barron et al. 2006) have been already reported for *Posidonia oceanica* meadows, including in the same area. However, the strong emerging relationship with LAI (leaf area changes from June to September) suggests a strong seasonal component, which we feel confident about.

Action: We now better specify that our goal was not to resolve temporal dynamics and that our study was conducted in the period of peak metabolism and LAI, therefore, focussing on the period when effects are likely to be most important. The objectives now read: “Here, we evaluate the effect of structural parameters (shoot density, leaf area index, biomass) of the meadow and the interaction with physical forcing (hydrodynamics) on the resulting carbonate system in the meadow during the period where these effects are likely to be more important, i.e. between June and September.”

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

Comment: Bare sand and seagrass. The seagrass meadows at shallow depths around the island of Mallorca where we sampled cover most of the available surface with bare sand patches and some times loose rocks mixed with the dominant cover of seagrass. Sites were clearly different, with visually distinguishable vegetation differences (this is subjective, prone to observer bias). "tufts" of seagrass can be observed, while the minimal distance between sites was 20 m., the area in between the sites being open sandy areas as well as seagrass. The maximum distance between sites was dictated by logistics as we moored the boat in a sand patch and distributed material from a central point (a sandy patch, normally where the anchor was deposited) depending on the terrain.

Action: Clarified the text, and removed the wording "patches" as this suggests an isolated vegetated area while our discrete sites were not completely surrounded and isolated by sand.

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?

Comment: Water residence time is also dependent on current velocities, which we had no control over. As currents in our area are hardly directional and have a wave component it is hard to estimate a net effect of a particular water body passing through the sandy patch and the meadow. We chose the biggest available patch in our research area, in some areas no big patches are present. As we found the influence of the meadow was big (resulting in similar values and patterns) even in the largest sandy patch, we discontinued these measurements during the subsequent campaign. However we think it is important background information pointing to the overwhelming effect of the meadow on the surrounding water masses.

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 deter-

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mine 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.

Comment: We agree our approach does not allow for evaluation of community metabolism. As stated above, the suggested approaches for evaluating community metabolism have limitations – incubation chambers can significantly affect carbonate chemistry and Eddy correlation techniques do not work well where there is no unidirectional flow. There is no clear upstream and downstream in our system, as currents are dependent on waves; wind direction & speed. It would be possible with many concentrated deployments in 1 region but this would not allow for an evaluation of structural parameters of the meadow. The main goal of the current study was to evaluate the effect of seagrass LAI, and we therefore chose to sample across a range of meadows. The reviewer claims that our approach is "not state of art", but simultaneous measurements with ADVs and multiparametric sensors in seagrass systems are not common. For example, the recent publication by Buapet et al 2013 only calculated effective fetch and no other hydrodynamic parameters. We feel that the strength in our data set lies in the combination of ADV data and the simultaneous measurement of water chemistry across meadows that differed in the parameter of interest – structure (LAI) – while keeping other variable as constant as possible (light, temperature, hydrodynamics).

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.

Comment: We had problems with our equipment, but never with two separate devices at the same site. We lack data on hydrodynamics for 2 sites, leaving 6 and 8 sites (Sep, June resp) with data. We lack pH data only for one site (Magalluf June, leaving 6 and 7 measurements) while our oxygen sensor failed 3 times, leaving 4 and 7 measurements respectively. We do not have trustworthy CaCO₃ data for 1 site, summing a total of 6

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and 7 measurements. As we have pH data for all but 1 site and for that site we do have hydrodynamic measurements and CaCO₃ data we did represent all sites in Table 1 and 2. Although it would have, of course, been nice not to lose any data due to technical problems, in field studies problems can be expected, and these contingencies did not affect the results presented in a substantial form.

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

Comment: We agree with the reviewer the appearance of this piece of information that is later not used is awkward. The HOBO sensors are accurate, and measure in situ, but give a value (lux) that is not easy to convert to PAR, or to a value that can be compared among systems.

Action: Therefore we added the data from the meteorological station at the beginning of the results section so the reader can appreciate and compare the incoming ambient light at the surface. Cumulative light (W m⁻² day⁻¹) was 54622 ± 2691 (SE) in September and 74000 ± 1615 (SE) in June. Like the daily average light intensity, 402 ± 21 and 477 ± 10 respectively this represents a significant difference (Students t-test, p<0.01) between seasons.

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.

Comment: Our first goal was to evaluate the carbonate system in sites with a wide range of structural parameters, as the focus of the study was on providing information on how (pH) conditions within the meadow vary as a function of structural parameters.

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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.

Comment: Correct, our wording has not been adequate. What we meant with non-directional is the fact that we calculated the resulting velocity using the ADV's output for velocity components x and y (in 1 horizontal plane) but ignored compass bearings of this velocity so did not take into account the directionality of the current.

Action: We removed the addition "non-directional" as this is indeed misleading and clarified the procedure.

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?

Comment: We had neglected to include these in our table, our apologies.

Action: We added error estimates to our density estimate. For biomass and LAI we used the whole sample that was collected. As *P. oceanica* is an extremely slow growing plant and a protected species, we aim to keep intrusive sampling limited.

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

Comment/Action: Correct. We actually do not use organic carbon values in our analyses so we rephrased leaving out the method to obtain organic carbon as this is not relevant in this paper.

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.

Comment: In September, oxygen concentrations were overall lower compared to those

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in June. Action: we clarified the text. We agree with the reviewer it would be useful to report the max and min values for oxygen (as well as for pH). However a table with additional information will occupy a lot of space. We have therefore added a supplementary table S1 containing this information.

Reviewer: 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?

Comment: We agree that the range of pH change within the seagrass studied is comparable to that expected from a doubling of CO₂, and hence within the range of ocean acidification predicted by year 2100, which is considered to be sufficient to compromise calcifiers (e.g. Doney et al. 2009, Kroeker et al. 2013). However, this would suggest that, rather than this range of pH being small, that this range of pH is significant when considering the scenarios of ocean acidification and responses of calcifiers. Moreover, whereas more extreme pH ranges have been reported in the literature in association with dense marine macrophyte stands in shallow waters (e.g. Kerrison et al. 2011) these may be misleading as they may refer to the exception rather than the norm. Whereas we could have possibly located seagrass meadows in the Mediterranean that would exceed this range, by sampling in shallow, dense meadows in sheltered areas, we opted to sample in relatively open and moderately deep meadows, which are more representative of general conditions in *Posidonia oceanica* meadows.

The question the reviewer poses "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?" is a pertinent one, and invites a more robust argument for the significance of this study. Whereas the question holds value, it is not exactly what the paper quoted (Duarte et al. 2013) concluded. Duarte et al. (2013) - which is not the paper reviewed here - concluded that the variability

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ity in pH in coastal waters depends on the balance between three forces, or end members, an oceanic driver, delivering the impact of ocean acidification by anthropogenic CO₂, watershed processes, and metabolic processes within coastal ecosystems. The balance between these three forces varies across coastal ecosystems. Indeed, Duarte et al. (2013) signaled at islands, where watershed effects will be limited, as sites where an open-ocean signal could deliver OA.

The relevance of the study presented here is that in the seagrass meadows studied, which are characteristic of the conditions in Mediterranean islands, generally lacking significant rivers, watershed processes are relatively negligible as coastal waters usually have the same salinity as open-Mediterranean waters (Basterretxea et al. 2010). Hence, the only buffer for ocean acidification for vulnerable organisms in *Posidonia oceanica* meadows is the possible metabolic regulation of pH. Our study shows that the range of diel variability in pH is comparable to that expected from OA along the 21st Century, hence, sufficient to provide refugia for calcifying organisms.

Action: We have now added text in the introduction and discussion to discuss the significance of this study and address the question posed by the reviewer. The text now reads:

Introduction: "...Whereas watershed effects can be a significant source of pH regulation and variability in coastal, estuarine waters, these are restricted in islands, which vulnerability to ocean acidification can only be offset by metabolic-intense ecosystems able to remove CO₂ (Duarte et al. 2013). This is the case of Mediterranean islands, which have small watersheds and little or no runoff to the coast, but where seagrass, *Posidonia oceanica*, meadows support intense metabolism (Duarte and Chiscano 1999; Duarte et al. 2010), possibly contributing to alleviate the expected impacts of ocean acidification."

Discussion: "The island of Mallorca lacks rivers and surface runoff, although CO₂ from soil respiration can be delivered to coastal waters through groundwater inputs (Baster-

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retxea et al. 2010). However, salinity maintains open-sea Mediterranean water properties, thereby pointing to a clear dominance of oceanic forcing on biogeochemical properties, including pH. Island environments with small watershed have been suggested to be particularly vulnerable to ocean acidification, unless they contain metabolically-intense ecosystems (Duarte et al. 2013), such as *Posidonia oceanica* meadows in Mediterranean islands. The results presented here show that the metabolism of *Posidonia oceanica* meadows, which are autotrophic ecosystems, can affect pH imposing a range of pH daily comparable to the predicted range due to ocean acidification over the 21st Century (Doney et al. 2009).”

Pg. 9, line 3: Capitalize Bay

Action: Corrected

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 when the seagrass data were not shown?

Comment: The seagrass data are shown (Table 2), the data we do not show (in a figure or table) in the article are from the bare sandy sites. We offer the oxygen range and mean pH_{NBS} and range in the text with the results from a Students t-test showing the mean pH in the bare sites was lower than in the vegetated sites. We cannot adequately resolve the advection term, but we do have measurements of average flow velocity just over the canopy sampled with the ADVs. A quick calculation from the averaged data on local flow velocity (Table 1) shows that between every sampling point (15 min = 900 seconds), the particular parcel of water measured by our ADV travelled on average 13 ± 3.2 meter. Since the bare sites were between 2 and 20 m in diameter, and surrounded by the meadow, this means that a particular parcel of water came from the meadow, as the sensors were deployed in the middle of the bare patch, at max 10 m. from the meadow edge. Of course this is a crude calculation not taking into account the logarithmic boundary layer slowing down water parcels close to the bottom.

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

Comment: The relationship between metabolic activity of the meadow, for which we use measured oxygen concentration as a proxy and pH suggest seagrass metabolic activity is the main driver for the observed pH changes. Unfortunately we were not able to obtain offshore data covering the right time frame or even the right measurement frequency to compare to our data, furthermore this would not have been comparable to our measurements in shallow sites as an offshore location is located over greater depths. We argue that probably the best estimate for pH values in a hypothetical unvegetated system is the intercept of the relationships between LAI (LAI=0) and pH/omega aragonite. The observed changes are whole system observations, originating from the seagrass + epiphyte + sediment community (i.e. the whole seagrass meadow ecosystem). The Bay of Palma is oligotrophic and plankton metabolic rates are too low to generate oscillations such as those observed here (see Navarro et al. 2004 for planktonic metabolic rates). In contrast, metabolic rates have been reported for meadows in Mallorca (Barrón et al. 2006), and are intense. The effect of seagrass on pCO₂ in this region has been already documented (Gazeau et al. 2005), where the metabolism of seagrass meadows down to 20 m depth propagated into major changes in pCO₂ in the surface.

Action: 1) pH values in unvegetated systems: We added the linear regression equations to the Figure legends, with the intercept (LAI=0) as a proxy for an unvegetated system. We calculated how much time the meadow elevated the carbonate system over this hypothetical value, on average $88 \pm 16\%$ of our measurements for the evaluated meadows. We added these values to Table 2. 2) Link observed changes to seagrass: We will add a supplementary figure demonstrating the relationship between pH and oxygen (as the number of figures is already quite extensive) S2 to supplement the text on page 9 “We observed clear diurnal patterns in pH_{NBS}, following those of oxygen (Fig. 2a and b, Table 2), and strongly correlated (Figure S2). Oxygen pro-

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duction by photosynthesis, or metabolic activity of the plants during the day directly influenced the carbonate system in the meadow, as there was a strong correlation between O₂ (in $\mu\text{mol kg}^{-1}$) and DIC (in $\mu\text{mol kg}^{-1}$) in the canopy with an average daytime relationship for all the experimental sites of $-0.96 \mu\text{mol DIC} / \mu\text{mol O}_2$ in June and $-0.97 \mu\text{mol DIC} / \mu\text{mol O}_2$ in September (average mean r^2 of 0.90)."

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 r^2 are insufficient, particularly given that the statistical significance claimed for maximum Ω Ar and leaf CaCO₃ (Fig. 6) cannot be reproduced. Furthermore, the relation between LAI and pH seems tenuous at best – the necessity to rely on mean/min/max values, rather than metabolic fluxes really hurts the paper.

Comment: See explanation about the difference in statistical evaluation between Ω Ar & CaCO₃ and structural parameters & the carbonate system leading to this discrepancy.

Action: We have now clarified this section as stated above. We propose to illustrate the relationship between LAI & O₂ with Supplementary Figure 4.

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 $\text{LAI} = 0.0034 \cdot \text{Biomass} - 0.37$, $r^2 = 0.64$), it is surprising that the relation between maximum Ω Ar 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).

Comment: We agree, that is why we created "categories" for the model grouping related parameters. However that might not have been the optimal solution.

Action: We think it is important to explore all structural parameters separately. However, since they are auto-correlated, we have performed a PCA analyses to distil a

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principal component for structural (explaining 73% of variation) and hydrodynamical (explaining 99% of variation) parameters, which we use for the model selection, to evaluate the relative importance of metabolism, structural parameters and hydrodynamics.

Pg 10, line 26: Statistical significance of correlations (r) is not determined by an F test. Are these regression results? If so, please provide r^2 values, in addition to F. We need to know if the relationship has any predictive power, not just whether it is significant.

Comment: Our GLMM model now includes a random factor to account for the several measurements per site. This type of model does not give an r^2 value, it does not even give a probability value in R, which we solved by testing against a no slope model to obtain Chi² and p. We think this model, and the GLM model for the mean, and hydrodynamics where we do not have repetitive values) is preferable over single regression analyses providing r^2 values. The danger in providing r^2 values for regressions is in the fact that this type of analysis considers the repetitive measurements as independent.

Action: We added r^2 values to the figure legends. We alerted the reader to the fact that significance of the correlations should be inferred from the table values of the GLMM analyses in the paragraph describing statistical analyses.

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 r^2 ; "influences" are predictable from mass balance and simple biogeochemistry: $\text{CO}_2 + \text{H}_2\text{O} = \text{CH}_2\text{O} + \text{O}_2$.

Action: We rephrased the paragraph and added r^2 values and slopes as suggested (to the figure legends).

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

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depths were provided that would support these estimates. Furthermore, the times 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.

Comment: We estimated the residence times (paragraph 2.2, page 7) for a normalised volume of 1 m³. We used an approximation where we used our point measurement of flow velocity as input for a BL profile based on (averaged, modelled) measurements in seagrass from the same bay in a flume tank years before. The weakness of this approximation is that we did not have flow profiles at all shoot densities and therefore had to use the same transformation for all meadows, not taking into account shoot density. However, using a logarithmic velocity profile is preferable over calculations without taking into account the BL. Our measurements were taken close to the bottom, where flow speeds are lowest, while the meadows where we measured had a much higher density as our modelled data. Therefore we believe the actual residence times to be shorter, and the local environment does have a dominant impact (although the effect of the surrounding meadow should be taken into account); our calculations are relative and only serve as a comparison between sites. Ideally we should have measured a full profile with the ADV but that proved to be logistically challenging with maximum bottom time for divers.

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.

Action: Eliminated the 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

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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).

Comment: It is counter intuitive, however understandable if taken into account that the ADV is located at 1.2 m above the bottom while the multiparametric sensor is in fact lower within the canopy. The measured TKE represents vertical mixing, probably caused by the flapping motion of the leaves, which can efficiently propel water located at the top of the canopy into the lower canopy region. Preliminary measurements (Moore et al. in progress) of our lab with detailed pH sensors indicated a higher pH on the upper canopy region (where irradiance is high and a lot of leaf surface is exposed) compared to below (where sediment processes affect the resulting pH). Therefore more mixing causes a higher saturation state at our sensor location.

Action: We added this argument to the discussion. It now reads: "Vertical mixing, related to LAI by its influence on leaf movement in flow, enhances the mean and minimum ΔpH by mixing water from the top of the canopy, where irradiation is at its peak and high productivity is expected, with water from within the canopy. The near-bottom water has a longer residence time, and heterotrophic sediment processes influence the final measured pH as well as the autotrophic meadow. Therefore enhanced vertical mixing positively influences the carbonate system, measured by the multiparametric sensor near the sediment."

We do not understand the second remark about the single data point and negative relationship as we never claimed a significant relationship for maximum saturation state & TKE and certainly not negative. We have updated Figure 5, and split the graphs per season, improving the presentation of the results.

Pg 11, lines 22-24: I get very different statistical results when I perform a regression

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analysis on the data in Fig. 6 (see general comments above). This needs to be sorted out.

Comment: See explanation about the difference in statistical evaluation between Ω_{ara} & CaCO_3 and structural parameters & the carbonate system leading to this discrepancy.

Action: We revised all our statistics throughout the paper, and clarified the section describing statistics as well as the text in the results section. We have re-done figure 6 and rephrased the figure header.

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.

Comment: With important we meant that these parameters needed to be included to obtain the best model score. The reviewer is right about the autocorrelation between some parameters.

Action: We have conducted a principal component analysis (PCA, in R) to reduce the various auto-correlated parameters to 1 value (component) explaining most of the data; 73% for the structural parameters LAI, density and biomass and 99% for the hydrodynamic parameters u_{velocity} , TKE (average, max, min) and Reynolds stress. With this single input parameter we re-performed a mixed model (lme4) in R for the various scenarios, with site as a random factor (to account for repetitive measurements during the week at each site, see explanation of the completely revised statistical section below) for max, min and range aragonite. For the mean omega aragonite we used a more simple GLM analysis as only one mean per site was calculated. We re-did Table 5. We do feel that the Aikaike index is the right criterion as the likelihood ratio (Chi square)

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only compares a model against the intercept-only model and we aim to evaluate what the best model is among our several options. We corrected the redundancy in the discussion p.12, line 13.

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

Comment: We think it is important to point out that coastal organisms are exposed to these short-term oscillations (as well as the long term trend). Predictions on the effect of OA for these organisms are often evaluated with laboratory set-ups mimicking the changes in the long-term trend while no fluctuating component is introduced at relevant timescales, as these organisms would encounter in their natural environment. This demonstrates this argument still needs to be pointed out.

Action: We assume our wording has been confusing and as this argument has been made elsewhere we removed the particular sentence.

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

Comment: The information is not new but we think the article benefits from a comparison with other studies. We have shortened the paragraph.

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

Comment: This is not new but certainly relevant so we have chosen to let it stand

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.

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Comment: In these sections we tried to tie our results to predictions of future trajectories of pH modifications. We do refer a lot to the literature but our opinion is this serves to describe the broader impact and place of our work within the current state-of-the-art.

Action: We shortened this section

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.

Action: We revised tables and legends and the reference list.

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.

Action: We clarified the legend. It now reads: "Diurnal profiles of light levels (lux) and of oxygen concentration (mg l⁻¹; upper panels) and pH (lower panels) in the canopy during the June (left panels) and September campaigns (right panels) in Magalluf."

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

Action: Added. We decided to split Figure 5 a and b into separate graphs for September and June. The panel for June is on the left (contrary to remarks to present data consecutively) because we think the augmenting scale (lower flowspeeds/TKE in June compared to September) makes the graph more logical and easier to interpret.

â€œ Citations used in this response

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C6425/2013/bgd-10-C6425-2013-supplement.pdf>

Interactive comment on *Biogeosciences Discuss.*, 10, 12313, 2013.

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