Biogeosciences Discuss., 10, C4055–C4064, 2013
www.biogeosciences-discuss.net/10/C4055/2013/

Biogeosciences
Discussions

Interactive comment on "Dynamics of seawater carbonate chemistry, production, and calcification of a coral reef flat, Central Great Barrier Reef" *by* R. Albright et al.

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Received and published: 6 August 2013

1) How do water parcel trajectories measured using drogue and fluorosceine compare at your study site. Previous studies have shown that drogue and dye trajectories are affected differently by waves and wind as well as issues of dispersion with dye measurements.

The reviewer raises an important question, which we did account for in our analyses. In a pilot study, we compared the drogue and dye by employing both methods simultaneously (n=11). Our results indicate that at higher wind speeds, the drifter speeds exceed the dye speeds (please refer to the figure supplied in the supplement). In our

study, drogues were only employed during night transects, and night transects were conducted at an average wind speed of 5.6 ± 0.9 m s-1 (Mean ± 1SD), ranging from 4.0-7.2 m s-1. According to the relationship below, at the average wind speed of 5.6 m s-1, the drifter overestimated the current speed by less than 4%. One transect was conducted at 7.2 m s-1, for which the current speed would have been overestimated by approximately 14%. Our results are thus only partially consistent with Falter et al. (2008), who found that drifter speeds exceeded dye speeds by 30-100%. For the transect conducted at a wind speed of 7.2 m s-1, we corrected the transit time using the below relationship to obtain a speed which would more closely approximate the dye. Falter et al. (2008) reported that dye patch speeds were highly correlated with depth-averaged current speeds calculated from ADCP data, which is why the drogue speed from our study was corrected to the dye speed (as opposed to the other way around). We have now included the above paragraph in the methods section of revised manuscript.

2) There is a disagreement between the diurnal cycle data measured at the station near the lagoon and the rate measurements made by following parcels of water across the reef flat. The cycle data displays relatively coherent cycles of the different parameters measured suggesting that metabolic rates across the reef flat are relatively constant assuming that the open water values are constant and there was no significant variation in currents. In contrast, the rate measurements vary greatly for similar times during the day according to the composite 24 hr cycle in Fig. 5. The sinusoidal curve fit to the rate data in Fig. 5 implying that the authors believe that their measurements produce a coherent diurnal cycle fails to do so in my opinion. Previous studies have shown that diurnal cycles of rates on reef flats are quite coherent and consistent over a period of days to a month (Falter et al. 2012 and Silverman et al. 2012) using different methods. I see no reason why this reef should be any different.

As mentioned by Reviewer 2, the less clear diurnal signal from the rate data is likely due to lower sampling resolution for the Lagrangian transects compared to the autosampler measurements. We agree that the rate data are variable for similar times of day – this is likely due to fluctuations in light levels, flow, and slightly different trajectories between runs, thereby tracking over benthic communities with slightly different compositions. These sources of variation are now discussed in more detail in the discussion of the revised manuscript. To address the reviewer's point about comparisons of the day-to-day nec variation in this study and those of Falter et al. (2012) and Silverman et al. (2012), we here make that comparison explicitly. Our analysis (using the tabulated data in Falter et al. 2012) demonstrates that the day-to-day variation in their study is as variable as in our study. Please refer to the figure supplied in the supplement.

Figure 11 of Silverman et al. (2012) shows a stronger diurnal trend in calcification rates, which is likely a result of higher levels of replication. However, rates of nec are still highly variable for a given time of day. For example, at 0930h, Silverman et al. (2012) measured calcification rates (nec) ranging from 140-420 mmol m-2 day-1 (6-18 mmol m-2 hour-1); at ∼1200h, they report rates ranging from 170-470 mmol m-2 day-1 (7-20 mmol m-2 hour-1). (Data were extracted from Figure 11 of Silverman et al. (2012) using GraphClick Software). Similar to Falter et al. (2012) and the results of our study, midday values are highly variable. Please refer to the figure supplied in the supplement.

With respect to the sinusoidal curve in Figure 5, the curve is not a fit to the rate data. As stated in the Figure legend, the dashed sinusoidal curve line represents the diurnal trend in aragonite saturation state (plotted on the secondary y-axis). Because Reviewers 1 and 2 both misinterpreted this curve, we have removed the curve from the figure in the revised manuscript.

3) Assuming that there are no analytical problems with the measurements themselves, perhaps the large variation in rate calculated rates could be the result of the different transect lengths (Table 2). While I agree that the lateral mixing, if any is negligible because the community structure is zonally distributed, i.e. different zones and cover types on the reef flat are parallel to the reef front/crest. Therefore, water parcels cross-

ing the reef travel over different cover types and the rates may vary from cover type to cover type as well. These issues are not discussed by the authors or taken into account in their rate calculation and may be crucial in better understanding and utilizing their results. I don't see how this is possible though as some of the floating water transects were >200 m long (up to ca. 400 m) while the community structure transects were fixed at 200 m length. Perhaps some sort of normalization to transect length can be used to adjust the calculated NEC and NEP rates.

The reviewer raises a valid point, which we address in our response to #2 regarding variability in the data.

To address the reviewer's comment regarding transect length and the potential for measuring metabolic activity in1-2 biological zones, we have conducted a sensitivity analysis by removing the two shorter transects (2 only in summer, that were 85 and 88 m respectively); doing this did not reduce the variability. We have, therefore, chosen to leave these transects in, but we have now included in the discussion a comment on differences in transect trajectory as a potential source of variation. Another source of variation is the ocean source water – as Davies Reef experiences tidal reversals, transects ran from crest to lagoon during outgoing tides and from lagoon to crest during incoming times. The majority of transects were conducted during outgoing tides, i.e. from lagoon to crest. Again, we ran a sensitivity analysis for these transects by removing the transects that ran during incoming tides, but this did not significantly reduce the overall variance in our measurements. This point has also been included in the revised discussion.

4) There is a heavy reliance on the Shamberger et al. paper, which is my opinion is not merited. The authors should consider that the Shamberger paper is very problematic and its results should be considered equally so or the following reasons: a) Rates of calcification are based on measurements of alkalinity on the reef flat and a constant open water alkalinity value taken from a monitoring station two weeks prior to the diurnal cycle measurements at a monitoring station far removed from the reef flat (this

is not mentioned in the paper, but the author conceded this when asked); b) Transit times of water across the reef flat are derived from wave height measured in front of the reef flat using a model. While I don't see any special reason not to rely on the model results I know that models are fallible and should be verified with corresponding measurements. Shamberger et al. did not or could not verify the wave/current model results using direct current measurements on the reef flat as they didn't make any as far as I know.

We are unaware of the problems that the reviewer raises for the Shamberger et al paper. To remedy what the reviewer perceives as a 'heavy reliance' on Shamberger et al. 2012, we have removed some of the citations of Shamberger and expanded our citation range (e.g., including Falter et al. 2012).

5) Fig. 7 displays the relationship between NEC and NEP. My main gripe with this figure is that the correlation line goes through two clusters of data probably representing nighttime (lower values of NEC and NEP) and daytime (higher values of NEC and NEP), which essentially displays a straight line going through two points and not points distributed along a straight line. This expresses in my opinion the problematic variation in the calculated NEC and NEP due to the issues raised in comments 2 and 3. In addition, as the authors so rightly state there should be a relationship between NEC and NEP considering the well known and documented phenomenon of light enhanced calcification, however not as it is displayed in this figure. Light enhanced calcification is a well known and documented phenomenon so there is nothing new here. However, the authors seem to think that the relation between NEP and NEC should extend to nighttime measurements as well, which in my opinion just doesn't make sense. If they would have done the regression analysis for daytime measurements as originally propose by Barnes and Chalker they would get a very low correlation coefficient judging by the clusters of data in this Figure.

We have now removed this figure from the manuscript. We believe that the low correlation coefficient of the daytime ncp and nec alone is likely due to the narrow ranges

in ncp and nec observed during the summer and winter studies and not because the coupling does not exist.

6) The rest of the discussion derived from the poor NEC and NEP data should be adjusted and there just doesn't seem to be much point in continuing consideration of the other results.

Our nec and ncp data show variability but does not necessarily make them 'poor' – please see our response to Comment #2. We have demonstrated that the variability in our data is comparable to that reported in other studies, including the two that the reviewer cites as showing 'highly consistent' data (we disagree that the data shown in Falter et al. 2012 is 'highly consistent'). Without specific, constructive suggestions from the reviewer, it is difficult to understand how he/she would like the discussion to be 'adjusted'. Please see our response to #9 for modifications to the interpretation/discussion that we have made. We hope these changes satisfy the reviewer's concerns.

7) Regarding the Barnes measurements on Davies reef cited and compared to the results of this study. One should consider that the calcification rates were derived from pH and Dissolved Oxygen measurements and the necessity to have a PQ and RQ values that can vary considerably from reef to reef (Kinsey) and over a diurnal cycle on the same reef (Silverman et al., 2012). It is interesting that Barnes who used PQ and RQ values of 1 arrived at the same rates measured by Kinsey and others on other reef (ca. 4 kg CaCO3 yr-1) using the Lagrangian method. In conclusion I think that the results of Barnes should be considered carefully and one should highlight the drawbacks of his methods.

We feel that we have adequately noted the caveats in comparing our data to D. Barnes' work. Detailed critique of Barnes' methods is beyond the scope of this paper. We have noted that a direct comparison is difficult due to differences in methodologies and technologies employed.

8) I don't agree with the arbitrary use of daytime and nighttime lengths for summer

and winter of 12 hours and 12 hours. Why not use the actual daytime and nighttime lengths?

Estimates for daily and annual net calcification rates have been adjusted using actual local day lengths. In summer (January), sunrise was at 0600h, and sunset occurred at 1900 h, corresponding to a day length of 13 hours (night length of 11 hours). In winter (July/August), sunrise was at 0640h, and sunset was at 1800 h, corresponding to a day length of 11.33 hours and a night length of 12.67 hours. Using these adjusted daytime and nighttime lengths, the average net daily calcification in summer has been adjusted from 6.5 to 6.9 mmol CaCO3 m-2 h-1. And winter rates have been recalculated and adjusted from 3.5 to 3.2 mmol CaCO3 m-2 h-1. When scaled up, these values yield a net daily calcification rate of 166 mmol (16.6 g) CaCO3 m-2 d-1 in summer and 77 mmol (7.7 g) CaCO3 m-2 d-1 in winter and an annual calcification rate of 4.4 kg CaCO3 m-2 y-1 (assuming 0.5 years or 182.5 days at each net rate).

9) Sections 3.3.2 L. 18 – The authors state that there was a strong positive correlation (r = 0.497) between NEC and ïAËŻU◦ arag.. The r value obtained is not strong and the correlation can barely be considered positive. Perhaps in economics studies.

For logistical reasons (inability to access the reef flat at extreme low tides), we were restricted to working on the reef flat in water depths of > 1 m. Consequently, rate data were collected over a relatively small range of omega values in comparison to other studies (e.g., Shamberger et al. 2011, Shaw et al. 2012, Falter et al. 2012, Silverman et al. 2007, Yates and Halley 2006). In summer, the range of saturation states during which Lagrangian transects were conducted was 3.42-3.94 (0.52 unit change), and in winter it was 3.29-3.69 (0.4 unit change). The variability in our rate data over a 0.4-0.5 unit omega change is comparable to that reported in similar studies over a similar range (please refer to the figures provided in the supplement; 0.4-0.5 unit omega ranges and the corresponding ranges in nec are indicated by red boxes).

The relatively narrow range of aragonite saturation state values for which we were able

to collect rate data precludes a robust analysis of the relationship between saturation state and nec. For this reason, the estimate of thresholds (i.e., extrapolation beyond the sampled range of omega values) has been removed from the paper, and the discussion has been modified accordingly. We have retained the Pearson's correlations between saturation state and nec (Results Section). Although we only have rate data for a narrow range of omega values, there are significant correlations between nec and omega for both seasons. The term 'strong' has been removed.

Another reason for not including a plot of nec vs aragonite saturation state is that we believe we are less able to detect the effects of saturation state on nec when diurnal fluctuations in light are expected to have a much stronger impact on nec. Light is known to cause a 3-5 fold effect on nec for the range in light experienced between dawn/dusk and midday (Langdon et al 2005 and many others). Unless the nec observations are carefully confined to times when light is saturating to ncp and nec, variations in light will heavily influence nec and mask the effect of saturation state. According to lab studies, the sensitivity of nec to omega is on the order of 20% per unit change in omega. Assuming a diurnal swing of 0.4 units (comparable to what was observed at Davies), the expected maximum effect of omega is roughly +/- 8%, which is small relative to the effect of light, which can be 300-500% for the full range in light during the day. At Davies in the summer, PAR was 170-232 at 0900 and 1500 on many days. It peaked at midday anywhere between 350 and 800 (if you include the back deck light measurements). If you use the relationship from Langdon & Atkinson (2005), Fig. 2, nec=0.01*PAR+6.83, you can compute that nec might vary 20-74% between 9a and noon or noon and 3p which will be large compared to the expected OA signal. Very shallow reef sites (or sites with large tidal fluctuations) such as Lady Elliot Island yield a good nec-omega relationship because the omega swings between 1 and 6 and the OA effect can be seen even in the presence of a strong light effect.

10) While I don't think that nutrients and salinity should be a problem in estimating changes in alkalinity in coral reefs as shown in previous studies, the way that changes

in salinity are presented (Table 1, 0.1 PSU) could be considered to have on effect assuming that changes in alkalinity were not very large. In any case, the authors should at least site Kinsey (1978) who showed that the effect of changes in salinity and nutrients has a negligible effect on changes in alkalinity in coral reefs.

The changes in salinity reported in Table 1 are for the entire course of the study, not during a given transect. Changes in TA were measured throughout a given transect, during which the salinity did not change. Therefore, we have no reason to believe that salinity introduced an error to TA values and nec calculations. As suggested by the reviewer, we now cite Kinsey (1978) here.

11) Section 4.2 L. 9 – The average net daily should be the daily average NEC.

This wording has been changed.

12) Section 4.4 L. 25 and onwards – You can already tell that a reef is bleached or has undergone a trophic phase shift without having to wait around for alkalinity and DIC measurements to come out of the lab. I also think that slopes of AT to CT measurements in coral reefs are not very useful as a monitoring tool because of unknown sensitivity to analytical problems between measurement sets, sampling times (sample at constant times or constant tide phase or constant zenith angle?).

We do not suggest that the slope of an AT-CT relationship should be used as an indicator of ecological processes that are more easily detected using visual surveys. However, we do suggest that the AT-CT relationship provides an indicator of benthic carbon flux processes, which reflects community composition and potentially ecosystem state and performance (Anthony et al. 2013), not easily picked up by visual monitoring. For example, conventional reef monitoring does not provide information regarding shifts in reef function (e.g., metabolism), which may be of interest for benthic carbon fluxes and biogeochemical cycling. We have, therefore, chosen to retain this part of the discussion, but we have included a comment that analytical problems between measurement sets, sampling times, etc. should be considered as sources of variation.

AT-CT diagrams can be very powerful tools for summarizing how a reef is altering the carbonate chemistry of the water. They can show at a glance whether the system is in net carbonate deposition and net primary production or net dissolution and net organic matter decomposition. They can also show the balance between ncp and nec. If the slope is shallow ncp dominates and if the slope is approaching 2 nec is dominating. If the data points tend to fall along a line as many reef systems do you can see if the community is offsetting the effects of OA by elevating omega relative to offshore source water or aggravating it by decreasing the omega. All of this can be interpreted by collecting water samples. No knowledge of the current is required as would be needed to derive ncp and nec rates. Of course if you have the additional information, both approaches have merit. We acknowledge that the AT-CT method does not work in all situations, specifically in situations where the chemistry of the source water is highly variable. However, after studying several reef systems, AT-CT diagrams have been informative in many more cases than when they have not.

13) Section 4.5 – This conclusion assumes that the decrease in NEC with ïAËŻU◦ arag. based on daytime and nighttime measurements will hold for daytime measurements alone when all the calcification is going on. In my opinion if the authors want to see how calcification will be effected by future ocean acidification they should use daytime measurements alone.

Please refer to our response to Comment #9; we have removed the discussion section on estimating thresholds due to a small range of omega values during which lagrangian transects were able to be conducted.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C4055/2013/bgd-10-C4055-2013-supplement.pdf

Interactive comment on Biogeosciences Discuss., 10, 7641, 2013.