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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.

Anonymous Referee #1

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Dear Editor The manuscript (bg-2013-170) entitled "Carbonate chemistry dynamics of the Davies Reef flat, Central Great Barrier Reef" submitted by Albright et al describes recent measurements of net ecosystem calcification and production made on the Davies Reef flat during the summer and winter seasons. Considering the increasing interest in the potential impacts that ocean acidification may have on coral reefs I find this manuscript very timely and relevant. However, even though the manuscript is well written, I have a few reservations regarding the quality of the data and its interpretation, which in my view prevent it from being accepted for publication in its current state. Please see my detailed comments below.

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. 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. 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 crossing 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. 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 for 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. 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. 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. 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. 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? 9) Sections 3.3.2 L. 18 – The authors state that there was a strong positive correlation (r = 0.497) between NEC and ïĄŮarag.. The r value obtained is not strong and the correlation can barely be considered positive. Perhaps in economics studies. 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. 11) Section 4.2 L. 9 – The average net daily should be the daily average NEC. 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?). 13) Section 4.5 – This conclusion assumes that the decrease in NEC with ïĄŮarag. 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.

In conclusion, I think that the data requires additional detailed analysis for it to become worthy of publication. I look forward to see a revised version of the manuscript.

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