We would like to thank the Anonymous Reviewer #1 and the Anonymous Reviewer #2 for their time and constructive comments. Their comments are reproduced in green below. We reply in black.

Anonymous Referee #1

General Comments

This paper measures $CaCO_3$ production rates by rhodolith-forming coralline red algal communities at higher latitudes. The authors report annual $CaCO_3$ production of 100.9 to 200.3 g ($CaCO_3$) m^{-2} yr^{-1} across a latitudinal gradient in Svalbard. Comparisons of $CaCO_3$ production with physical parameters indicates that geographical latitude, duration of the polar night, and duration of sea ice cover correlate with $CaCO_3$ production. The authors conclude that light is the primary driver of coralline algal growth. I think the paper should be published however I have a few serious concerns about the statistical analyses describe below, in addition to a few other minor comments.

Specific Comments

The authors should include literature on coralline red algal growth rates by the Kamenos group and the Halfar group.

We added a section on the utilization of coralline red algal growth increments as climate recorders in the introduction chapter:
"Additionally, growth-rates without quantification of spatial $CaCO_3$ production rates have been calculated for e.g. subarctic (Halfar et al. 2011) and temperate (Kamenos et al. 2008) coralline red algae in order to use the algae as high-resolution climate recorders."

If any of the studies listed in Table 1 used methods similar to those in the current study, then comparisons among results should be made. To permit comparisons going forward, the authors could consider recommendations for a unified method of measuring annual $CaCO_3$ production.

The only study using a similar method is the one by Freiwald and Henrich (1994), because it also utilizes fuchsine to stain the growth increments of the coralline red algae. However, there is a significant difference in the counting of protuberances per square metre. While we used only collected specimens to count the number of protuberances and to extrapolate our results on the square metre scale, Freiwald & Henrich (1994) counted the number of protuberances using underwater video footage. These approaches involve the possibility of different biased errors, which are negligible in the particular approach, but prohibit a comparison of the results in terms of absolute $CaCO_3$ production rates.
Regarding the recommendation of a unified method to quantify $CaCO_3$ production rates by coralline red algae, we do not feel that our method is generally suitable. For our method, it is mandatory that the coralline red algae develop protuberances and that these protuberances are the main spots of $CaCO_3$ production. This is not the case for many species, as for example *Clathromorphum nereostratum* that has been used by Halfar et al. (2011) to reveal 225 years of Bering Sea climate.

The authors should consider fresh river discharge when examining environmental parameters that may influence $CaCO_3$ production.

We appreciate that recommendation, but as we have shown in a previous study (Teichert et al., 2013), that melt water discharge, which is the only significant source of fresh water in the surrounding area of our study sites, does not affect the water depths in which the coralline red algal communities prevail. To indicate that, we added a paragraph to the discussion stating the circumstance in short and referencing to the correspondent article:

"Annual melt water discharge has no impact on the coralline red algal communities, which prevail in at least 38 m water depth, while melt water does not fully intermix with the water body and only affects the uppermost ~20 m of the water column (Teichert et al., 2012; 2013)."

For the multiple linear regression, the authors need to adjust for the multiple comparisons and overfitting otherwise they are reporting inflated R^2 values and potentially falsely rejecting the null hypothesis of no significance. If the adjustments have been made, they need to be reported in the methods. Also, Table 4 says that multiple linear regression was used while Figure 5 says one-way ANOVA was used to test for correlations between $CaCO_3$ production rates and environmental variables.

Yes, the coefficient of determination has been adjusted. We added the formula to the methods, with *n* being the number of points and *k* being the number of independent variables:

$$R_{adj}^2 = 1 - \frac{(1 - R^2)(n - 1)}{n - k - 1}$$

Regarding the issue with the reduced major axis algorithm and Levene's test for homogeneity of variance based on means (One-way ANOVA) used in Figure 5 in contrast to the multiple linear regression used in Table 4, we would like to state that the One-way ANOVA was used to indicate if the $CaCO_3$ production rates and a particular abiotic factor show a correlation at all, while the multiple linear regression was used to assess the relative influence of the obviously controlling factors. From that point of view, we feel that our calculations have been done in a proper way. This is also indicated in the methods:
"The resulting $CaCO_3$ production rates (excluding the shallow water control group from Nordkappbukta) were plotted against the physical parameters water depth, seawater calcite saturation, annual mean temperature, duration of sea ice cover, geographical latitude, and duration of the polar night at each site. The results were checked for correlations using reduced major axis algorithm and Levene's test for homogeneity of variance based on means (One-way ANOVA). Correlating factors were checked for their relative influence on $CaCO_3$ production rates using multiple linear regression analysis with an adjusted coefficient of determination…"

Were the calculated $CaCO_3$ production rates averaged over the same period of time for each specimen and the environmental data? Otherwise, the authors are aliasing their results.

Yes, the $CaCO_3$ production rates were averaged for one year regarding the coralline red algal specimens and the environmental data, respectively. The only exception is the seawater calcite saturation, which is a snapshot record from the MSM 02/03 expedition of RV Maria S. Merian in 2006. This was only indicated in the referenced studies by Teichert et al. (2012; 2013), so we now added that indication in the methods chapter:
"… seawater calcite saturation (snapshot conditions recorded during MSM 02/03 expedition of RV Maria S. Merian …"
We also added an indication for the snapshot character of our records in Table 3.

The authors should consider including the source of environmental parameters in the methods, not just in Table 3. They should also include errors on the environmental parameters. The errors should also be included when presenting any of the algal data as well (e.g., Figure 5).

We agreed with the suggestion to include the source of environmental parameters in the method chapter and updated the correspondent passage:
"The resulting $CaCO_3$ production rates (excluding the shallow water control group from Nordkappbukta) were plotted against the physical parameters water depth, seawater calcite saturation (snapshot conditions recorded during MSM 02/03 expedition of RV Maria S. Merian, see

also Teichert et al. (2012; 2013)), annual mean temperature (data from LEVITUS 94), duration of sea ice cover (data from Svendsen et al., 2002; Nilsen et al., 2008; Spreen et al., 2008; AMSR-E Sea Ice Maps), geographical latitude, and duration of the polar night (data from USNO Sun Rise Tables) at each site."

We also appreciate the recommendation to include the errors on the environmental data. We would have done that during our study, if such errors would exist, which is not the case for most of the data. For some of the data, errors do exist but are negligible small, so we did not include them to allow for an unbiased comparability with the other environmental data. Detailed concerns regarding the particular parameters are stated here:

- Water depth:	Water depth was directly measured during the Maria S. Merian expedition with an accurateness of ±0.5 metres. This error is negligible.
- Geographical latitude:	Geographical latitude was measured by GPS with an accurateness of ±5 metres. This is negligible regarding the distances between the study sites.
- Polar night:	Data on the duration of the polar night have been retrieved from USNO Sun Rise Tables (http://aa.usno.navy.mil/data/docs/RS_OneYear.php). This data base does not quote an error.
- Sea ice cover:	Data on the sea ice cover have been retrieved from Svendsen et al. (2002), Nilsen et al. (2008), Spreen et al. (2008), and AMSR-E Sea Ice Maps (http://www.iup.uni-bremen.de:8084/amsr/). There are no errors included in these sources.
- Annual mean temperature:	Data on the annual mean temperature have been retrieved from the LEVITUS 94 database (http://iridl.ldeo.columbia.edu/SOURCES/.LEVITUS94/). There are no errors included in this source.
- Calcite saturation:	The measures on the calcite saturation of the seawater reflect only snapshot conditions, so the statement of an error in the variation of the saturation over time is not possible.

I appreciate that the authors did not include data from the shallower site 714 in the statistical analyses, however they may want to include it in Figure 5 for reference.

From our point of view, it is problematic to include the data from the shallower site 714 in Figure 5. This figure depicts the results of our statistical analyses, excluding site 714, which is also appreciated by the Reviewer. It is our concern that the insertion of "data" points without impact on the result of the calculation, including the trend lines and the coefficients of determination, would puzzle the reader. We think that our motivation to exclude site 714 is clearly stated in the discussion chapter: *"The other constant for all sites is water depth, except for station 714 in 27 m water depth, with a relatively high* $CaCO_3$ *production due to increased irradiance levels. Because of that, site 714 was excluded from the coherence plots in Fig. 5."*
To give full reference on the possibly influencing factors on site 714, we include these data into Table 3, which also depicts that water depth is the only control that differs for that site at the same locality.

Table 2, Table 4, and Figure 5: exact p-values should be listed, unless $p<0.0001$.

We agreed with the recommendation and inserted the exact p-values (unless $p<0.0001$) into Table 2, Table 4, and Figure 5. Additionally, we inserted the exact p-values (unless $p<0.0001$) into Figure 4.

Text in Figure 5 is nearly illegible in the pdf produced online.

We reviewed Figure 5 in the online in the pdf and are of the mind that the text is well readable, at the latest after zooming in a little. Compared to many Figures in articles already published in Biogeosciences, the text in Figure 5 is relatively big and we consider that enlarging the font size would rather lead to increased complexity than to improved legibility. For that reason, we would like to keep Figure 5 as it is (except for the updated p-values as described above).

Anonymous Referee #2

General Comments

The manuscript describes an original investigation on the carbonate production of polar corallines, based on a previously unexplored method of growth zones coloration and calculation of incremental weight of produced algal carbonate. The paper is clearly written and the interesting data on these extraordinary rhodolith beds deserve publication after moderate revision. I've remarked some points of weakness that can be summarized here.

Specific Comments

The age model for the algal growth is based on the idea that each zone corresponds to one year as the conceptacle production. However a large degree of uncertainty exists about the matter, and the manuscript fails to clarify the matter. In particular, the interpretation of Fig. 3 is weakly supported by the evidence (low resolution picture, and possible misinterpretation of the microscopic anatomy of the algal zonation?) and literature data are inconclusive (see annotated manuscript). I suggest to clarify in the manuscript what is really known and accepted and what still remains in the field of hypotheses.

We agreed with the recommendation to better point out why we assume an annual growth pattern for our samples. We also appreciate that this annual pattern is not finally proven, but that comparability between our study sites, as it is necessary for the analysis of the potentially controlling factors, is given in any case. However, we extensively updated our discussion in terms of the interpretation of the coralline red algal growth increments:
"To gain data on the annual rhodolith $CaCO_3$ production, a method was used that enabled the analysis of large sample sizes. This analysis bases on the assumptions that (1) the growth increments counted in the protuberances of L. glaciale are annual and that (2) the increase in weight with age is linear, so specimens of different ages can be compared. Assumption (2) is confirmed by the findings shown in Fig. 4, indicating a significantly linear weight increase with age, but there has been discussion on the nature of coralline red algal banding patterns, ranging from daily over lunar to annual cycles (Bosence, 1980; Freiwald and Henrich, 1994). The coralline red algal calcification process involves high magnesium calcite precipitation within most cell walls (Kamenos et al., 2009). Early electron microscope work showed that the calcified cell walls of coralline algae have a two-layered structure, an inner layer of acicular calcite parallel to the cell wall, succeeded by radial, inward growing calcite crystals (Alexandersson, 1977; Garbary, 1978; Cabioch and Giraud, 1986). Because the underlying biomineralisation process takes place only during the growth period (Bosence, 1991), the result is a growth pattern starting with heavily calcified cell rows at the beginning of the growth period and grading into less calcified cell rows towards the end of the growth period (see also Fig 3C). In our experiments, the visibility of that pattern has been amplified using fuchsine staining, resulting in bright, heavily calcified summer bands (i.e. there is little cell lumen/pore cavity to bind the fuchsine staining) and dark, less calcified winter bands (i.e. there is more cell lumen/pore cavity to bind the fuchsine staining).

The assumption of an annual banding pattern is hardened by the distribution of the reproductive conceptacles in the protuberances of L. glaciale. These conceptacles contain the spores of the plants, form annually (Jackson, 2003), and clearly parallel the longitudinal, fuchsine-stained growth increments in the protuberance sections (Fig. 3C). Due to their spatial distribution in the protuberances, conceptacles are not visible on the cut face at all increments, but their thickness corresponds to the growth increments.
For those reasons, we assume an annual banding pattern exhibited by our specimens. On the other hand, we appreciate that the nature of the growth pattern still has to be proven by growth experiments under laboratory conditions. However, because we applied the same presuppositions for all sites, comparability of the potentially controlling factors is given."

The multiple regression gives a very high r value due to the redundancy of variables. Some of them should be eliminated from the discussion, since one mirrors the other (for example the duration of the polar night and the latitude).

We appreciate the concerns about the multiple regression analysis, but we have a different point of view, because the variables are not redundant. Of course, the duration of the polar night is controlled by the geographical latitude, but the duration of the polar night only regulates the *time*, in which light is available at all. Because the geographical latitude also controls the *intensity* of the light, as the irradiation intensity decreases with increasing latitude, its control on the $CaCO_3$ production rates of the coralline red algae has to be assessed independently. Duration of the sea ice cover and annual mean temperature are also not redundant as sea ice does not only depend on temperature, but also on the prevailing oceanography, such as currents and coast line properties. We thus are concerned that all the variables in the multiple regression analysis are necessary for a thorough analysis of the influence of the particular controls. However, we tested a multiple regression excluding the duration of the polar night and the adjusted coefficient of determination was still highly significant (R^2_{adj}=0.9699).

The inverse correlation of carbonate production and water saturation is unexpected but apparently significative. It deserves discussion, or removal for further exploration.

We agreed with that recommendation and added a paragraph in the discussion chapter, which discusses this issue in greater detail (page 9, line 10ff.):
"The apparent correlation with the carbonate saturation is rather confusing. Regarding the regression line in Fig. 5B, $CaCO_3$ production rates seem to increase with decreasing calcite saturation, which is not meaningful because coralline red algae are heavily calcifying organisms. Nevertheless, Alexandersson (1977) and Okazaki et al. (1982) state that coralline red algae induce a microenvironment suitable for $CaCO_3$ precipitation by metabolic excretion of alginic acid, so the degree of carbonate saturation is of minor importance as long as Ω_{Cal} ≥1. Hence, as for water depth, the effect of the calcite saturation may be superimposed by the other parameters. Another possibility might be that the concomitant grazers are more affected by less saturated seawater and the coralline red algae are less affected by abrasion, but this is very speculative and not provable within the present study. Additionally, one has to consider that the seawater calcite saturation data only depict snapshot conditions recorded during MSM 02/03 expedition of RV Maria S. Merian, (see also Teichert et al. (2012; 2013)), while continuous records over several years would be necessary for more confident conclusions."

References could be improved (see annotated manuscript), in particular the data contained in table 1 has been dealt with in a recent review (Geodiversitas special volume) and the table could be easily substituted by reference to that paper.

We updated the particular references annotated in the manuscript and completed Table 1 with data from Basso (2012) in the Geodiversitas special volume. We only included data that contain specimen identification at least to genus level, but we refer to the Basso (2012) paper for further information. We did not agree with recommendation to remove Table 1 from our manuscript and to only refer to the Basso (2012) paper for two reasons: First, our table contains data not listed in the Basso (2012) paper and, secondly, because access to the journal Geodiversitas is restricted. Accordingly, publication of Table 1 in the open access journal Biogeosciences will provide a much broader readership with this important information.