We thank anonymous Referee #2 for his/her constructive criticism and valuable comments. In the following we address the points brought up, with referee comments in boldface and author responses in normal typeface.

A main critical question that the authors should clarify is about the need and innovative aspects of this work since there are other meta-analyses ex- ercises performed and published on this topic; for examples see Findlay et al. (2011). The authors mentioned the accordance with the Findlay et al., 2011 meta-analyses results. The general negative effects of OA on Ehux calcification and PIC/POC has already been shown. What can we learn more on the coccolithophore response to OA mainly from the meta-analyses of monoclonal culture experimental results that haven't already been published? In addition, the large majority of data available and presented here are from Ehux and for all other tested species there are not enough data to do a meaningful meta-analysis.

As mentioned in our manuscript, other meta-analyses did not specifically focus on coccolithophores, except for the work by Findlay et al. (2011). However, these authors only analyzed how ocean acidification influences the ratio of PIC to POC in E. huxleyi and only included 15 single experiments in their meta-analysis. Besides PIC/POC, we also included calcification and photosynthesis responses in our analysis and used 48 single experiments to do so. The meta-analyses by Hendriks et al. (2010) and Kroeker et al. (2010, 2013) also analyzed calcification and photosynthesis responses of coccolithophores but included only 2-19 single experiments in their analyses (see manuscript for a detailed listing of experiments used in their studies). We are confident that the much larger dataset used in our meta-analysis justifies the publication of our work.

Although the majority of data dealing with coccolithophore responses to ocean acidification examined E. huxleyi, we found it important to make a distinction between the single species, as their responses to OA is quite diverse. This difference can clearly be identified with the help of our analysis, although the available datasets for C. braarudii and G. oceanica are rather limited.

Since a main justification for having this article published is to use a larger set of ex- periments to allow a more robust prediction of the impacts of OA on coccolithophores, it is key that the authors clearly make a comparison with the number of data previously used and the benefit of having this new meta-analysis.

This has been done in the last paragraph of the introduction, where the number of data used in the meta-analyses by Hendriks et al. (2010) and Kroeker et al. (2010, 2013) are listed. The number of data used in the meta-analysis by Findlay et al. (2011) and differences between their and our analysis will also be added.

I found the title -Responses of coccolithophores to ocean acidification: a meta-analysis- misleading, since the number of living coccolithophore species is >200 and having the responses of 4 tested heterococcolithophore species mainly from culture experiments doesn't resolve the response of coccolithophores to OA.

We believe that using the plural „coccolithophores" does not imply that we are resolving responses of all living coccolithophore species to OA. It rather refers to coccolithophores that have been the subject of ocean acidification research, which also becomes apparent in the abstract. If possible we would prefer not to change the title.

It is mentioned that "The perturbation method appears to affect photosynthesis, as responses varied significantly between total alkalinity (TA) and dissolved inorganic carbon (DIC) manipulations". This needs to be clarified since it is hard to conclude this on the basis of a meta-analysis. This hypothesis should be probably tested in a controlled experiment for example as, shown in Hoppe et al. (2011).
We agree that it is prematurely to firmly conclude that the perturbation method affects photosynthesis. That is why we discuss the topic with great care and conclude that the subject needs to be further clarified. However, we will revise the paragraph and highlight the points mentioned by the referee.

The results shown in Table 2 should be checked carefully since it is suggested that the C. braarudii results in Krug et al. (2011) and Langer et al. (2006) are completely different when instead they are very similar.
It is not clear to us why the reviewer comes to the conclusion that responses of C. braarudii are very similar in the studies mentioned. Responses depicted in table 2 are directly taken from the respective papers.
Langer et al. (2006) state: "In the Coccolithus pelagicus [n.b. presently referred to as Coccolithus braarudii] cultures neither PIC nor POC content per cell changes significantly over the CO_2 range tested […], yielding a stable PIC/POC ratio."

Krug et a. (2011) state: "POC production rates of Coccolithus braarudii were highest at intermediate *p*CO2 [...] and declined towards lower and higher levels [...], although more pronounced in case of the latter [...]. Calcification rates, although quite noisy, clearly decreased towards higher *p*CO2 levels [...]. The considerably stronger decrease in PIC compared to POC production led to a pronounced drop in PIC/POC […]."

It would be important to add the Conclusions section to summarize the main findings and the differences with previous similar meta-analysis exercises.
We feel that this is dealt with in the discussion section and that it is redundant to add a conclusion.

Findlay et al., 2011 is not listed in the references.
The reference his will be added.

'PIC/POC ratio' should be changed to 'PIC/POC'; 'ratio' is redundant.
This will be changed.

In the introduction when mentioning the ballasting properties of coccolithophores the paper by Ziveri et al., 2007 could be mentioned.
Good remark, this will be added.