Authors' response to Editor and Reviewer comments

The authors would like to sincerely thank both reviewers as well as the editor for their very thoughtful comments that have hopefully improved the manuscript to the standard of GMD. All feedback is reproduced in the following in black font, with our responses added in blue font. Short, specific changes to the manuscript text are reproduced in red.

Comments from the Editor

Comments to the Author:
The manuscript is structured well and describes the model and its assessment satisfactorily. Were I a referee, I might make some suggestions concerning certain aspects of content and analysis [*], but these would reflect my own particular biases of the science and I would rather leave such considerations to the formal review process. As such, I am satisfied that this manuscript can be published as is. The only technical issue that I spotted was the occurrence of anomalous curly brackets ("{}") around DOIs in the references section.

Thank you to the Editor for noticing the problem with how DOIs were listed; the brackets have been removed.

[*] For instance: the manuscript does not give a good overview of the alternative approaches to calcification used in other models; the manuscript could include an examination of the model's CaCO3 production relative to that of other models (e.g. the CMIP5 archive); the intercomparison of both models to various properties (nutrients, DIC, alkalinity, etc.) could possibly be summarised by means of Taylor diagrams (or better).

The absence of an overview of other model parameterisations of calcification is noted by both reviewers as well. A paragraph has been added to the introduction giving an overview of calcification in climate models of varying biological complexity (page 4, line 14 - page 5, line 14):

The simplest model representations of ocean biological calcification modify DIC and alkalinity tracers using implicit carbonate production and fixed, instantaneous dissolution. These models utilise spatially and temporally uniform $CaCO_3$:POC (rain ratio) production and export parameterisations that are tuned to modern ocean carbon profiles (e.g., Yamanaka and Tajika 1996, Dutay et al. 2002, Najjar et al. 2007). They do not contain the requisite mechanistic flexibility needed to model significantly different or transitioning biogeochemical climates. Some attempt to circumvent this limitation by adding parameterisations that adjust $CaCO_3$ production and/or POC export according to changes in a state variable (e.g. depth or pCO_2 , Schneider et al. 2004, Ridgwell et al. 2007a). Other models introduce greater complexity and calculate $CaCO_3$ export production using a rain ratio, carbon and nutrient tracers, and an explicit phytoplankton PFT (e.g., Six and Meier-Reimer 1996, Palmer and Totterdell 2001, Heinze 2004, Schmittner et al. 2005, Popova et al. 2006a, Hofmann and Schellnhuber 2009). These models (often abbreviated as NPZD, for nutrients-phytoplankton-zooplankton-detritus) might also include adjustments to $CaCO_3$ and POC export from changes in state variables or carbonate chemistry (e.g., Six and Meier-Reimer 1996, Heinze 2004, Hofmann and Schellnhuber 2009) but they do not capture shifts in ecosystem structure (because they only contain one phytoplankton PFT) and also cannot produce strong $CaCO_3$ production seasonality when applied globally. A variety of approaches are taken in representing calcification in multi-PFT models. Calcification of one or more PFTs

with spatially and temporally uniform rain ratios are common (Le Quere et al. 2005, Schmittner et al. 2008, Aumont and Bopp 2006); less common approaches include co-limitation of calcification by light and temperature (Moore et al 2002), with a coccolith shedding parameterisation (Tyrrell and Taylor 1996), or adjustment of the rain ratio with changes in carbonate chemistry (Gehlen et al. 2007, Yool et al. 2013) or latitude (Yool et al. 2011). Other models simply ignore $CaCO_3$ altogether (Aumont et al. 2003, Gregg et al. 2003, Litchman et al. 2006). Dissolution of $CaCO_3$ in the above models typically follows the method of Dutay et al. (2002), assuming that sinking speed and dissolution rate are static (e.g., Le Quere et al. 2005, Aumont and Bopp 2006, Schmittner et al. 2005, 2008), but can also be parameterised as dependent on carbonate chemistry (Gehlen et al. 2007). Of all the models mentioned above, only Hofmann and Schellnhuber (2009) contains a prognostic $CaCO_3$ tracer.
Comparison of the UVic model's $CaCO_3$ concentration with the CMIP5 archive and the Lam et al (2011) dataset has now been included in the form of a Taylor diagram (page 47), with associated text in Section 4 (page 18, line 26).

Comparison of the CAL (formerly COCCS) model performance with respect to the NOCAL (formerly NOCOCCS) model and observations is now included using Taylor diagrams for various properties as a new Figure 3 (page 40) in Section 4, and is also discussed in the main text (page 17, line 16).

Lastly, in response to reviewer comments, references to model "coccolithophores" have now been replaced by the term "calcifiers".

Comments from Reviewer 1

This paper reports development of the University of Victoria intermediate complexity Earth system model to include calcification. A fair case is made for why it would be valuable to include planktic calcifiers in such models, and their approach to doing so is described. However, it is not obvious that this attempt to include planktic calcifiers is an improvement on previous attempts.
The reviewer makes a fair point that the first version of this manuscript did not emphasise how the model is an obvious improvement beyond other attempts. The motivation for this project is hopefully now made clearer with additions to the abstract (page 2, line 8):
The UVic ESCM now fills a niche in Earth System modelling that was previously unoccupied in that it is relatively inexpensive to run, yet resolves the complete earth system carbon cycle including prognostic calcium carbonate and a separate calcifier PFT. The model is now ideally suited to test the parameter space of feedbacks between the carbonate and carbon cycles and the climate system as transient simulations.
and conclusions (page 23, line 1):
The UVic ESCM now fills a niche in Earth System modelling that was previously unoccupied in that it is relatively inexpensive to run, yet resolves the complete earth system carbon cycle including prognostic calcium carbonate and a separate calcifier PFT. Since the UVic ESCM includes ocean sediments and calcite compensation (something none of the CMIP5 models do), it is now a model that is particularly well suited to reducing the uncertainty of the fate of emissions

over the long term. It is now also ideally suited to test the parameter space of feedbacks between the carbonate and carbon cycles and the climate system as transient simulations.

Lastly, a comparison Taylor diagram of calcite concentration across similar models has been added to the manuscript which adds context to this model over others (new Figure 10).

If anything, this attempt seems to include a large number of arbitrary steps that, although they could be reasonable, are not justified, and hence a compelling case is not made for why we should believe this scheme, either on its own merits or in comparison to previous attempts.
It is assumed by the authors that the reviewer is referring to the Model Tuning section of the manuscript. It was decided to include this section because previously published model descriptions so rarely mention specifics regarding how major changes to code are implemented in practice. After speaking with another individual who had tried (and failed) to implement a prognostic calcite tracer in HAMOCC, the first author realised the absence of prognostic calcite tracers in climate models might have a simple explanation: it is very challenging to keep alkalinity stable. Of course this kind of revelation is never mentioned in published model descriptions and so it would be impossible to know how our study compares to previous attempts. It is hoped that publishing our method will help others facing similar challenges. Since GMD is a model development journal, it seems to be an appropriate forum.

The motivation for the tuning steps is justified in the Model Tuning section in terms of keeping the model "stable" with respect to DIC and alkalinity. Why each step is taken is described throughout the section.

In addition, the model output in terms of where planktic calcifiers are successful and abundant in the model ocean does not seem to agree particularly well with the real world situation or to be an obvious improvement over previous methods. Thus there is also no justification from model results suggesting that we should prefer this model formulation.
This study's motivation was poorly communicated in the first version of the manuscript. The motivation was not to create the best model of coccolithophores currently available (UVic is not a logical choice for such an exercise), but to create a climate model that resolves the calcium carbonate cycle, including a separate biological actor. Hopefully the changes mentioned earlier now make the motivation for the model enhancements clear. In addition, references to model "coccolithophores" have now been replaced with "calcifiers", and model simulations are now referred to as CAL (for the model with calcifiers) and NOCAL (for the model without calcifiers). Where UVic model results are presented, they are now described in terms of calcifiers. They are only described as "coccolithophores" when compared to other models that explicitly include "coccolithophores". Furthermore, a sentence of caution is added to the third paragraph of the Model Description (page 7, line 24):
Just as the general phytoplankton PFT cannot perfectly describe the physiology or ecology of any of the individual classifications of phytoplankton it represents,

the calcifying PFT represents a group of phytoplankton with a common role in the carbonate cycle (calcification) and a few generalised shared physiological traits.
Hopefully this moves the manuscript away from describing an attempt to model coccolithophores, towards an attempt to model calcification and calcite cycling.

Although a large amount of model development work has clearly taken place, I do not recommend this paper for publication in anything like its current form because it is not clear to me that it constitutes any advance in our understanding. I think the paper would need to contain either (1) justification from (new?) observations or experiments for the choices made in this model setup, and why these choices are better (more realistic) than the choices made in other models, and/or (2) justification from the improved quality (realism) of the model outputs as to why to prefer this scheme..
The reviewer is kind to recognise the large amount of work that has gone into the development of this model. Of the two options listed, the second one is more aligned with the motivation of the model (to improve the mechanistic realism of the ocean carbon cycle in an intermediate-complexity climate model). In pre-industrial equilibrated form, justification of one scheme over another is difficult to argue because, as has been shown by others, schemes may perform similarly for the modern ocean but have very different responses to changes in forcing (Taucher and Oschlies, GRL, 2011 is one example of this). The manuscript does show some improvement in simulated nutrient and carbon tracers compared to the original scheme, as well as broad agreement with observation-derived global flux estimates, and calcite flux estimates in line with similar models (all of which use schemes more like the NOCAL version).

A manuscript is currently in preparation that employs the CAL, NOCAL, and an intermediate model under RCP forcing, which demonstrates very different responses across configurations, in NPP and carbon export, because of the treatment of calcite and the presence or absence of calcifiers. It will challenge the common OCMIP-style treatment of calcite used by all of the other biogeochemical models. That is not to say that this other manuscript will prove the greater realism of CAL model output, but it will demonstrate the large sensitivity in ocean biogeochemistry to the calcite parameterisation.

1712/L2: not so, coccolithophores are not particularly successful at low phosphate concentrations (Lessard et al, 2005).
The text in question reads: "Compared with other phytoplankton, *E. huxleyi* have a greater tolerance for high irradiance, an enhanced ability to utilise phosphate and non-nitrate nitrogen, a lesser susceptibility to iron limitation and a greater susceptibility to zinc limitation (Zondervan, 2007)."

That nutrients are important to coccolithophore dynamics, but not the only limiting factor, is described in the sentences above 1712/L2 (1711-1712: "Blooms of coccolithophores tend to follow blooms of diatoms, which explains why they correlate with nutrient-depleted conditions, but coccolithophores have shown an ability to grow in nutrient-rich substrate, suggesting limited nutrients alone is not a necessary condition for blooms (Zondervan, 2007).")

According to the Zondervan (2007) review, which includes the Lessard et al (2005) study, field studies of coccolithophores (biased towards studies of E hux) tend to bloom when concentrations of nitrate and/or phosphate are very low. Quoting the Zondervan (2007) review, the Lessard et al. (2005) study "... suggest[s this phenomenon is due to] the combined abilities of *E huxleyi* to use non-nitrate N (Palenik and Henson, 1997) in addition to its exceptional P acquisition capacity (Riegman et al., 2000) that provide it a competitive edge in nutrient-depleted waters with shallow mixed layers and high light."

The choice to include a lower half saturation constant for calcifiers is motivated by the Zondervan (2007) review, as well as the Le Quere et al. (2005) summary that lists a lower half saturation constant for phosphate as appropriate for modelling "calcifiers".

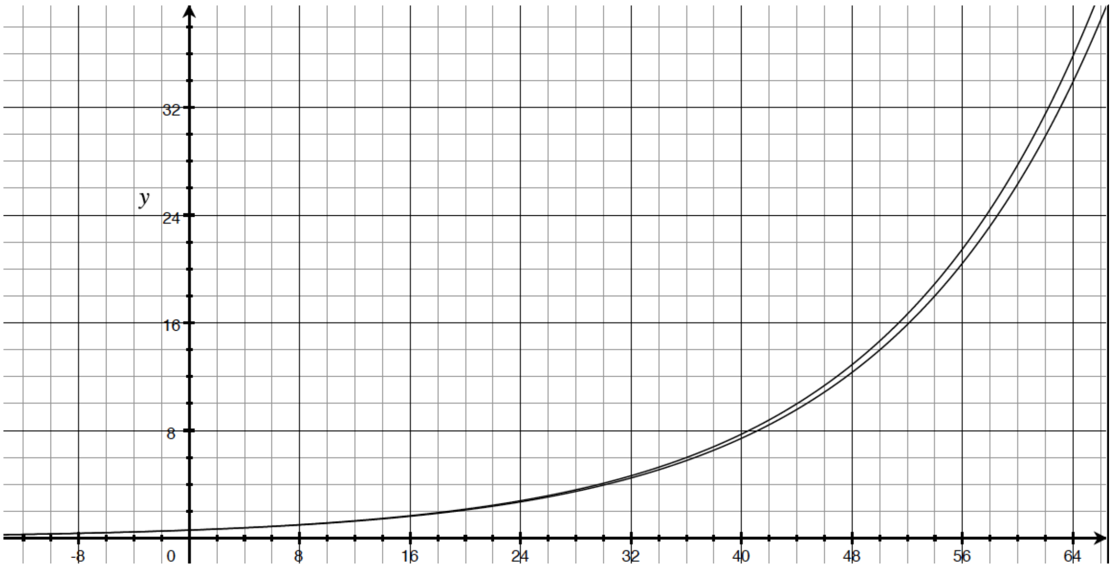
1713/13: Diatoms, including mat-forming diatoms, are an important part of the shade flora.
The authors welcome a reference to this subject if the reviewer could provide one. In this particular case the manuscript is referring to the combination of data analysis and modelling in the paper of Cermeno et al. (2008) that demonstrates the relative dominance of diatoms and coccolithophores in the Atlantic correlates with nutricline depth. This suggests that shifts in phytoplankton community composition can occur as a result of changes in nutrient availability. "In general" has been added to the sentence (new page 6, line 8).

1713/24: This gives a false impression; coccolithophores or planktic calcifiers have been included previously in several global scale models (whether or not they are fully coupled climate models), with varying degrees of success (Gregg & Casey; LeQuere et al., 2005; Moore et al., 2002).
The sentence has been deleted.

1719/1720: several aspects of the representation of phytoplankton growth and competition are unconventional in this model, and are not fully explained or justified. For instance, it is of concern that an arbitrary factor is included in the growth equation of all phytoplankton groups. A tuneable iron half saturation constant, which is allocated a different value for each PFT, without any reference to values obtained in experiments, is tuned in order to obtain more satisfactory results,. This has the side-effect of reducing the value of any similarity that is obtained between results and reality, because it could all be down to tuning of this parameter rather than constituting evidence that the model representation as a whole is along the right lines. The representation of light limitation is also highly unusual and is neither explained nor justified, likewise the use of a new method (rather than the standard Q10) for the effects of temperature on growth. The effect of light scattering by coccoliths is highly complex and neither a derivation nor a reason is given for using this formulation. Taken together, this all gives an impression of an excess of arbitrariness in terms of model setup, and a lack of confidence that what is going on in the model bears relation to what is happening in reality.
None of the phytoplankton equations are new formulations, but they are now

also applied to a new PFT (the calcifiers). References have been added where appropriate.

Regarding concerns about the growth rate parameterisation:
The effect of temperature on phytoplankton growth used here (and previously published versions of the UVic ESCM) is a slightly modified form of the classic and commonly used Eppley curve (Eppley, R.W., 1972. Temperature and phytoplankton growth in the sea. Fisheries Bulletin 70, 1063–1085). A plot of both the Eppley curve and the UVic ESCM formulation (growth rate in per day units on the y-axis, temperature in deg C on the x-axis) is included below. The first sentence in the Phytoplankton section is now revised so it is clear this is not a new parameterisation (page 12, line 10):
As in Keller et al. (2012), the maximum possible growth rate of phytoplankton and calcifiers is a modified Eppley curve (Eppley 1972), and is a function of seawater temperature, an e-folding temperature parameter, and iron availability.



Regarding concerns about the parameterisation of iron limitation:
Iron limitation is justified for inclusion as part of the equations that determine the maximum potential growth rate in Keller et al (2012) as "Although the maximum potential growth rate of phytoplankton in models is often calculated using temperature, we include dissolved iron in our calculations because iron is necessary for photosynthesis, the reduction of nitrate to ammonium, and a number of other key cellular processes (Galbraith et al., 2010) (i.e., we assume that iron must be available before photosynthesis or the uptake and utilisation of nitrogen and phosphate is possible)". The half saturation constants were tuned because the model uses a mask for iron limitation and thus, does not resolve the iron cycle, so there are no corresponding half saturation constants in the literature. Amendments have been made to the text to make it more clear what the motivation is for using an iron mask, and why the masking approach is taken over the inclusion of a full iron cycle (page 12, line 18):

Iron limitation is calculated from the concentration of iron that is prescribed in interpolated monthly-mean fields using an iron half saturation approximation constant ($k_{\_Fe}$) (Keller et al. 2012, Galbraith et al. 2010). Inclusion of a prognostic iron cycle in the UVic ESCM would increase the computational cost of running the model and is outside of the realm of this model development project, though is currently underway (L. Nickelsen, personal communication). However, accounting for iron limitation on growth rates by means of a limitation mask improves phytoplankton biogeography without additional computational cost (Keller et al., 2012). Calcifiers and mixed phytoplankton are assigned different $k_{\_Fe}$ values that vary the degree of iron limitation, and that are tuned to produce the best possible PFT distributions, not actual iron affinities. Calcifiers are assigned a lower $k_{\_Fe}$ value than mixed phytoplankton to simulate the relatively low iron half saturation constant for calcifiers recommended by Le Quere et al. (2005).

Regarding concerns over light limitation/light scattering parameterisation (page 13, line 12):
The light limitation equations are also not new; "As in Schmittner et al. (2005)," has been added to the sentence before the first light limitation equation.
Light scattering of coccolithophores is parameterised following Balch and Utgoff (2009). A sentence is now added at the end of the description of light limitation (page 14, line 4):
Values for k_c and k_{CaCO3} come from Balch and Utgoff (2009).

To be fair to the authors, the underlying problem here is that there is just no solid basis on which to build such a model. Despite decades of efforts, the nature of the ecological niche is simply not well known, whether for coccolithophores, foraminifera or pteropods. Although several hypotheses have been advanced, there is no strong body of evidence behind any of them, and therefore no scientific consensus as to the mechanism which should underpin a model of this sort. This study falls foul of the problem of "trying to run before we can walk" (Anderson 2005).
Anderson, Thomas R. "Plankton functional type modelling: running before we can walk?." Journal of Plankton Research 27.11 (2005): 1073-1081.
The authors sympathise with the Anderson (2005) argument, however given the rapidly changing state of the oceans, simply waiting for a better understanding of phytoplankton physiology before attempting global-scale modelling is not prudent; nor happening. Models can be built to make predictions; a task where accurate representation is critical and higher resolution, more complex models are a better choice for this line of research. Models are also built to understand the solution space of projections, to understand how mechanisms interact, and to understand what processes are important to resolve. That is, they help us understand how to build those models that can make better predictions. Simpler, lower resolution models such as the UVic ESCM are ideal for this type of research because they can test the solution space of biology. High-CO_2 transient simulations are not included here, but will provide additional insight into the importance of how models are constructed for projecting future climate change, or running paleo simulations.

We would like to thank the reviewer for their critical and thorough review. It helped us to improve our manuscript.

Comments from Reviewer 2

GENERAL COMMENTS

The manuscript by Kvale et al. describes in detail an attempt to better represent pelagic calcite production in the University of Victoria Earth System Climate Model through the inclusion of calcifying Plankton Functional Types (PFT) - specifically coccolithophores and Foraminifera. Considerable uncertainty remains over the ecophysiology of both of these plankton groups, including their biogeography, contribution to biogeochemical cycles, and fundamentally, the drivers of cellular calcification. Hence, attempting to represent them in Earth System Models is bound to be difficult and fraught with problems. Although the authors extensively state the rationale for their inclusion in biogeochemical models, what is not clear from the paper is whether this study represents a better representation than other modelling studies and what new insights are provided by the inclusion of calcifying PFT in the UVic model. Does the model perform better than similar models?

The authors would like to thank the second reviewer for their very careful reading of the manuscript and helpful feedback. This point was also made by the first reviewer. The motivation for this project is hopefully now made clearer with additions to the abstract (page 2, line 8):

The UVic ESCM now fills a niche in earth system modelling that was previously unoccupied in that it is relatively inexpensive to run, yet resolves the complete earth system carbon cycle including prognostic calcium carbonate and a separate calcifier PFT. The model is now ideally suited to test the parameter space of feedbacks between the carbonate and carbon cycles and the climate system as transient simulations.

and conclusions (page 23, line 1):

The UVic ESCM now fills a niche in Earth System modelling that was previously unoccupied in that it is relatively inexpensive to run, yet resolves the complete earth system carbon cycle including prognostic calcium carbonate and a separate calcifier PFT. Since the UVic ESCM includes ocean sediments and calcite compensation (something none of the CMIP5 models do), it is now a model that is particularly well suited to reducing the uncertainty of the fate of emissions over the long term. It is now also ideally suited to test the parameter space of feedbacks between the carbonate and carbon cycles and the climate system as transient simulations.

Lastly, a comparison Taylor diagram of calcite concentration across similar models has been added to the manuscript that adds context to this model over others (new Figure 10).

Whether the authors succeed in better representing pelagic calcification in the Uvic model is unclear from either the abstract or conclusions of the paper - the abstract lacks any clear results to support the statement that "improvements to the representation of zooplankton calcification and carbon export therein" have been made.

The motivation of the research was poorly communicated in the first version of the manuscript. The motivation was not to improve the representation of calcification (the model uses only the standard fixed $CaCO_3$: POC rain ratio for

this, though with the additional pressure of inter-PFT competition). The primary motivation is to improve the representation of the carbonate cycle by including the prognostic calcite tracer, variable dissolution rates, and the ballast model. This has hopefully become more clear through edits regarding earlier criticisms made by the first reviewer. Additionally, the abstract text has been modified to read (page 2, line 5):
This paper describes the implementation of a calcifying phytoplankton PFT in the University of Victoria Earth System Climate Model (UVic ESCM), and mechanistic improvements to the representation of model carbon export (a full calcite tracer, carbonate chemistry dependent calcite dissolution rates, and a ballasting scheme).
The second sentence in the Conclusions has been modified to read (page 22, line 25):
This model is a unique attempt to include calcifiers as an explicit phytoplankton PFT alongside a general phytoplankton and diazotroph PFT in an intermediate complexity model, and to make the calcifiers and zooplankton responsible $CaCO_3$ production and prognostic export, and detrital ballasting.

The modifications improve model performance with respect to carbon and nutrient fluxes, but with respect to what exactly - the model with/without calcifiers or the model compared to data or other similar models?
The last 2 sentences of the abstract are modified to read (page 2, line 13):
The described modifications improve UVic ESCM performance with respect to observed carbon and nutrient fluxes. Primary production, export production, particulate organic carbon and calcite fluxes all fall within independently observed estimates.

As well as the abstract lacking any obvious quantification of the improvements that the model represents with regards to other models lacking explicit representation of pelagic calcifiers or compared with field data, the conclusion brings in further elements that complicate how calcifiers should be better represented in models. A revised paper needs clearer statements of the results and insights gained in both the abstract and conclusions.
This manuscript is meant to be a model development paper that presents a new tool, comprised of components that have not been assembled in this particular configuration previously. The motivation and context for the study have now been expanded in the abstract and conclusions (please see earlier responses and edits).

On a more technical note, the authors must recognise that all of their coccolithophore parameterisations from the literature represent just one coccolithophore species, Emiliania huxleyi. Hence the true diversity of the PFT they are trying to represent in poorly examined or included in the model. Such a limitation needs to be thought about carefully when comparing model output to field observations (e.g., global coccolithophore biomass).
A sentence is added to the last paragraph of the Model Assessment section (page 22, line 18):
More specifically, biological parameter choice for the calcifying PFT is biased towards *E. huxleyi,* which necessarily biases model results.

The first sentence of the third paragraph in the Model Description section is also modified to read (page 7, line 20):
In this latest version the general phytoplankton PFT is exactly replicated, but given new parameter values to reflect key physiological characteristics of calcifiers, albeit biased towards *E. huxleyi*.
Recent work on calcifiers has revealed their rapid adaptability to acidification (e.g., Lohbeck et al., Nature, 2012) so assigning any kind of static characteristics to PFTs is probably a questionable approach that will eventually be removed from models in favour of various methods for resolving evolution. There remains a lot of uncertainty in the solution space for phytoplankton response to changing environmental conditions and every approach towards parameterising PFT characteristics has limitations. The main goal of this particular modelling project is to improve the mechanistic realism of the carbon cycle in the UVic ESCM.

SPECIFIC COMMENTS
Pg 1711, Lns 13-16: Balch et al. (2005) refers only to surface concentrations of calcite derived from satellite estimates, not production rates (ln 13!) or export fluxes (ln 16!).
The text is modified to read (page 3, line 12):
Satellite reconstructions of $CaCO_3$ concentration in the euphotic zone show strong seasonal and regional variability (Balch et al. 2005). Much of this variability comes in the form of high latitude summer blooms, where about 69% of the October to March global $CaCO_3$ concentration occurs in the Southern Hemisphere (40% south of 30S) and 59% of the April through September global $CaCO_3$ concentration occurs in the Northern Hemisphere (29% north of 30N; Balch et al. 2005).

Pg 1712, lns 2-4: No evidence that coccolithophores as a PFT have any of these characteristics - they all refer to Emiliania huxleyi. Also the greater/enhanced/lesser susceptibility are relative to which other PFTs? Hence (pg 1714, ln 27) the new parameter values really reflect key physiological characteristics of E. huxleyi and not coccolithophores as a whole.
In the sentence in question, "coccolithophores" is replaced with "E. huxleyi". The unique characteristics are relative to other generalised phytoplankton- since Zondervan (2007) is a review this is a very general summary statement. "Generally" is added to the sentence (page 3, line 19).

The Le Quere et al. (2005) paper was used as the basis for parameter assignment, which lists appropriate parameter values for a "phytoplankton calcifier" PFT and does not specify these are only limited to *E hux.* The sentence has been rephrased as (page 7, line 20):
In this latest version the general phytoplankton PFT is exactly replicated, but given new parameter values to reflect key physiological characteristics of calcifiers, albeit biased towards *E. huxleyi*.

Pg 1716, ln 18: What is the justification for a fixed production ratio of calcite relative to POC production. How well does this value fit with measured production ratios (and not export rain ratios)? How do the authors justify a shared CaCO3:POC production ratio for both coccolithophores and foraminera?

Do these two groups have similar cellular levels of inorganic and organic carbon?
The UVic model, like most other Earth System models, lumps production together with export, hence there is only a rain ratio that represents both. A fixed rain ratio is a common way of representing calcification in earth system models and is the way it has historically been done in UVic. It is mentioned briefly in the Conclusions that a variable ratio is desirable but for structural reasons not practical. The detailed explanation is that because calcite is now a full tracer, any variation in the rain ratio would now have to be traced and accounted for in 4 dimensions, which makes carbon conservation difficult. It would require a major re-write of the way the calcite tracer is implemented, and add substantial computational expense to the model. This aspect of the model is expanded on in Section 2.2.2, where it is explained why the model cannot use 2 different rain ratios for phytoplankton and foraminifera. A couple of sentences are added to this section to expand the context of the parameter value choice (page 11, line 7):
In earlier versions of the UVic ESCM, a $R_{CaCO3:POC}$ value of 0.03 was used. In this version, it is increased to 0.04, which places it closer to (but still outside of) the low end of the 0.05-0.25 range estimated by others and summarised by Fujii et al. (2005). A $CaCO_3$:POC production ratio for *E. huxleyi* is summarised by Paasche (2001) to vary between 0.51-2.30. A lower rain ratio for the model therefore indicates the calcifier PFT cannot be considered to represent calcifiers exclusively, with other non-calcifying phytoplankton sharing the physiological traits also represented by the PFT.
If total model $CaCO_3$ export-production to total model POC export-production is considered, this latest model version is now more variable than earlier versions since the calcite:POC ratio applies to calcifiers only, while the POC produced by mixed phytoplankton and diazotrophs only affect the global ratio through competition.

Pg 1719, ln 22: Why are coccolithophores assigned a lower maximum growth rate than mixed phytoplankton - other than reading Le Quere et al., 2005, what is the justification- it would be useful here. What is the implication that in the end the Uvic model actually requires them to have similar growth rates to maintain a population (pg 1723, ln 24-27)?
The sentence in question is expanded (page 12, line 12):
Calcifiers are assigned a lower maximum growth rate (a) than mixed phytoplankton, an assumption used previously by Le Quere et al. (2005) but also justified by comparing measured growth rates for a selection of four coccolithophores by Buitenhuis et al. (2008) (0.3-1.0 day^{-1} at 15C) with the general range for phytoplankton by Eppley (1972) (a maximum rate of about 2.2 day^{-1} at 15C).
The implication of similar growth rates is now made clearer with the following additions to the text (page 20, line 26):
Calcifiers, however, are overrepresented by a factor of ten. This overestimate is primarily due to the low number of PFTs in the model, which requires that the calcifier PFT use parameter values (i.e., the growth rate factor) more similar to the general phytoplankton PFT than data support, if it is to avoid extinction.

Also (page 23, line 27):
There are four potential improvements to the CAL model that have not yet been

addressed. Simulated calcifiers are wholly dependent on relative competitive advantage, and can easily go extinct or cause the general phytoplankton PFT to go extinct with only small adjustments to production parameter values, especially the growth rate.

Pg 1727, lns 25-27: How do Beaufort et al. (2011) show no significant correlation between calcification and coccolithophore biomass on a global scale? What evidence do they present for this?
The sentence is rephrased as (page 20, line 12):
While increasing calcification correlates with increasing CO_3^{2-} concentrations, no significant correlation between coccolith mass and chlorophyll or cell abundance is apparent in global sampling of surface water and sediment core samples (Beaufort et al. 2011).

Pg 1728-1729: What are the references to support the statement that coccolithophores only contribute 2% of NPP? What satellite and sample data are the authors referring to?
The sentence is rephrased as (page 21, line 13):
Jin et al. (2006) estimate coccolithophores contribute only 2% of NPP, which is in better agreement with the MAREDAT relative abundance estimate for coccolithophores (Buitenhuis et al 2013a).

Pg 1731, lns 17-21: Have the authors tested their assumption that inclusion of other PFT would improve the coccolithophore performance? Have the authors run a coccolithophore and diatom version?
(although this assumption has not been tested) has been appended to this sentence (page 24, line 4).

Pg 1731, lns 29: What is the influence of pH on calcification?
The sentence is modified and expanded to read (page 24, line 12):
Lastly, the model does not account for decreasing calcification with increasing CO_2 concentration (Riebesell et al. 2000), which would doubtless affect simulated tracer distributions and biogeography. Sustained declines in calcification are questionable (Lohbeck et al. 2012) and were therefore omitted.

The author would like to thank the reviewer for their time and expertise which helped us to improve the manuscript significantly.