BG Discussion: Reply to all reviewers

We would like to thank all reviewers for their constructive feedback.
Based on their suggestions we will make the following changes:

- Update the niche expansion noted in the abstract and conclusion from 17% to 18.8% (the mean for paired species) as well as the range of values observed for individual species (3-76%).
- Update the axis labels of latitudinal plots in Fig. 3.
- Update Fig. 7 to include both time and depth.
- Include PCA plots of the BATS, Med, and AMT data sets within temperature, salinity, and nitrogen as supplementary figures.
- Include niche expansion analysis for the BATS station.
- Clarify that holococcolithophores generally constitute a minor component of the total coccolithophore abundance but that holococcolithophores dominate under certain environments and are furthermore important in terms of niche space.
- Clarify the hypervolume metrics with visual representation.
- Discuss why contribution of holococcolithophores is minor.
- Discuss importance of time and depth in structuring niche space.
- Discuss positive Spearman correlations with silicate.
- Discuss uncertainties and biases of the SEM compilation.
- Discuss impact of physical processes.
- Discuss gaps of knowledge in dedicated section.

We provide a response to the reviewers and a detailed explanation of our changes below.

BG Discussion: Reply to RC1

We would like to thank the first anonymous reviewer for the kind and positive feedback. A detailed response to their comments is found below.

This is a beautifully crafted review drawing attention to the overlooked global importance of coccolithophore species for which heterococcolithophore (diploid)-holococcolithophore (haploid) pairing has been identified. As stated, roughly these paired species account for 18% of total coccolithophore abundance, but because the diploid phase tends to be more heavily calcified than the lighter, or non-calcified, haploid phase, they are likely to occupy different

ecological niches, as exemplified by different biogeography (Fig.3; eg. holococcolithophores rare in Southern Ocean>50S), different depth profiles (Fig.4; holococcolithophores in upper 50 m, heterococcolithophores evenly distributed with depth), different seasonality (Fig.7; but apparently different niches off Bermuda and in Mediterranean; this needs to be better explained).

The apparent different niches off Bermuda and in the Mediterranean, as observed in Fig. 7, were the result of averaged depth profiles, which squeezed together the temporal and spatial pattern. We have now updated the original figure (which was plotted to time) to include both time and depth (see Figure 1 attached). This figure shows depth to be an important variable and that holococcolithophores are predominantly present in low-nutrient regions, which is consistent with our Spearman correlations (Table 6 and Table 7 of the original manuscript) and observed PCA niche space (Fig. 6 of the original manuscript). We will replace Fig. 7 with this new figure in the manuscript.

The coloured SEM plate in Fig. 2 is spectacular and I applaud how the red (diploid) and blue (haploid) colour coding is consistently adopted throughout all graphs.

What knowledge is missing, albeit repeatedly admitted, is how this haplo-diplontic life cycle works to expand the niche for the globally most abundant (59.2%) coccolithophore *Emiliania huxleyi*, because its non-calcified haploid stage cannot currently be identified by regular LM and which calls for new molecular techniques [1].

A number of these knowledge gaps where future work should focus, best should be summarised in the abstract. This also includes [2] more SEM observations in the Pacific; [3] role of carbonate chemistry within the haploid-diploid niche space; and [4]resolution of the fact that grouped hetero-holococcolithophore abundances may not always be the best representation for individual species.

We will update our discussion to include a section to specifically highlight the current knowledge gaps mentioned above, as well as some gaps raised by the second reviewer below [line 195]. This discussion will cover mainly aspects on the impact on the carbon pump and the limited descriptions of the life cycle pairs.

In detail: How was the estimate made that the haplo-diplontic life cycle expands the coccolithophore niche space by 17%??

Thank you for pointing out the lack of description for the 17% estimate of niche space expansion. This number was from an older version of the manuscript (which used a different statistical analysis). We will correct this number with the current analysis including the range observed in the Mediterranean, BATS, and AMT data sets (e.g. 3-78%), as well as the average NE observed for paired species (18.8%).

BG Discussion: Reply to RC2

We would like to thank the second anonymous reviewer for the in-depth and constructive feedback. A detailed response to questions raised are provided

below.

The study by de Vries et al. is a broad synthesis studies, which a main focus on describing environmental portioning and drivers behind differential haplo-diplontic stages of the coccolithophores. As the haploid stage is often overlooked, yet it is ecologically and biogeochemically important, this is an important review studies of the inclusion of the life stages towards an improved understanding of the coccolithophore ecology.

While the paper joins various components of coccolithophores biology/ecology, my major questions are focus on the methodological approach, which in many ways is insufficiently described/explained, with some of the resulting conclusions then also not supported. I would like the authors to address the following methodological approaches.

There are several caveats behind such synthesis approach that need to be highlighted and further elaborated.

The first and most important is compilation of SEM dataset, which strikes me as uncertain (what is the images were not taken) and difficult to present in the quantitative terms. Were both phases in the initial SEM dataset presented quantitatively or where there also just studies that took qualitative SEM approach? How did this impact how the authors proceed with the study?

Where are potential biases? How can you estimate uncertainty in the quantified approach?

We agree that the SEM compilation represents a certain degree of uncertainty, and as such the resulting analysis primarily serves as a first order estimate of global coccolithophore abundance. The degree of uncertainty is noted in the large standard deviations observed in both the abundance and HOLP-index estimates. We will further clarify and discuss this in the manuscript.

We have limited uncertainty by focusing our analysis only on only one technique (SEM), which reduces uncertainties due to method comparison. Furthermore, SEM is more accurate in distinguishing life stages of coccolithophores than other microscopy techniques. (Bollmann et al., 2002; Cerino et al., 2017; Godrijan et al., 2018).

In addition, to account for identification uncertainty, we limited our HOLP-index analysis to studies that identified holococcolithophores to a species level, and limited our in depth analysis (e.g. Mediterranean and Atlantic) to studies which we were confident accurately identified the samples.

There is likely a bias towards the Mediterranean Sea and the Atlantic Ocean due to the large number of samples in these regions. There is potentially also a temporal bias to bloom seasons. We will further develop these aspects on the level of uncertainties of our data set in the Methods and Discussion sections.

Second, I do follow the nice overlap and nice expansions in the hyperspace. It is not clear to me how the authors transitioned from hypervolume to the nice space and how are the two haplo-diplontic stages represented in the Eq 2 (line 15) based on the similarity metrics? How was the intersection or the union between two hypervolumes determined?

Hypervolume and niche space are interchangeable terms. We will update the manuscript to reflect this.

We utilized the R package described in Blonder et al. (2014) and already referred in our manuscript to the original publication for the mathematical description of the hypervolume algorithms. The package calculates the 'niche space' hypervolume using kernel density estimations, and provides shape, volume (|A|and |B|), intersection, union and set difference of hypervolumes as well as similarity metrics. Since the method also resulted in some confusion for RC3 we will include Figure 3 (attached at end) in the hypervolume methods section.

Why was NE not calculated for BATS?

We focused our analysis of environmental drivers on the AMT and Med data sets, and for consistency decided to do the same for NE. Furthermore, we have limited data to perform a NE at BATS, because BATS presents only two paired species for which both life cycle phases to be present. We will however include NE analysis for BATS in our updated version of the manuscript which is provided as an attachment (Table 1).

We find that the NE analysis of BATS has more similar values to the Mediterranean than the AMT data set, although one would expect them to be more similar to the latter based on hydro-graphic similarity. This indicates the potential importance of the nature of the data set, here a transect vs a time series, which focus on spatial versus temporal correlations.

Third, in the section of seasonality (line 135), only a handful of environmental parameters are missing and there could be other important physical-chemical drivers. For example, In Figure 8, you also include turbulence in there, why was such parameter not included in the PCA analyses? What about pH, for example?

Turbulence was not measured in any of the original publications of our data set, and pH was only measured in some of the data sets. We have thus not included these variables in our analysis. We included turbulence in Figure 8 because it is commonly included in the Margalef niche space model (Margalef, 1978; Houdan et al., 2006; Frada et al., 2018). Although, as already argued in the manuscript, the relationship to turbulence broadly holds in terms of general patterns (MLD and seasonality), we agree that our data set does not explicitly support inclusion of turbulence of the model and we will update the discussion to better reflect this. pH (and more specifically carbonate chemistry) is potentially a key factor on coccolithophore ecology as we discuss in lines 396-407. This discussion will be expanded to highlight the contradictory nature of pH effects as noted by reviewer 3 [line 366].

Also, as described are large-scale patterns, what about mesoscale type events, advection and other physical parameters?

This is a good point. The effects of physical processes are difficult to constrain but this certainly warrants discussion. Effects of mesoscale processes should be partly negated since we averaged our data over several years. However, other physical processes may be important to consider. Godrijan et al. (2018), for instance, noted that the East Adriatic Current (EAC) may be partly responsible for holococcolithophore abundance during winter and spring. We will add this to our discussion of the manuscript.

In line 210, why were not the same approaches used for the Med and ATM?

By using water column vs niche space approach, this excludes the possibility of comparing two regions.

We used different approaches for the two different data sets due to the different nature of the two data sets (AMT is spatial and Med is temporal). Although the two data sets can be compared by limiting the principal components of the PCA to salinity, temperature and salinity, the structure of the PCA plot is strongly influenced by both depth and time (time is represented by day length in the Med). Not including these variable results in lack of separation between the two life cycle phases (see Fig. 4, Fig. 5, and Fig. 6 attached).

This in itself is interesting and we will discuss this in the Discussion as the importance of depth is also apparent in Fig. 7 (see discussion above [line 40]). We will additionally include (Fig. 4, Fig. 5, and Fig. 6 attached as supplemental figures).

Forth, where is 17% of expanding niche space coming from?

Thank you for pointing out the lack of description for the 17% estimate of niche space expansion. Similarly to the comments from reviewer 1, this number was from an older version of the manuscript (which used a different statistical analysis). We will correct this number with the current analysis including the range observed in the Mediterranean, BATS, and AMT data sets (e.g. 3-78%), as well as the average NE observed for paired species (18.8%).

Fifth, the authors report 7.3 to 18% of the species abundance, which is a relatively wide range and needs to be better quantified with the uncertainty.

These are two different numbers. Calcifying haploid coccolithophores account for 7.3% of the total abundance, and 18% of the paired-species abundance. We understand the confusion and will clarify this in the manuscript.

Also, given the quantitative estimates presented, I wish the authors to have better addressed some of the knowledge gaps, the impact on the pump of the haploid-diploid stage, standardizing the approaches to represent different species (paired- non paired, etc),.

We are happy to discuss current knowledge gaps in further detail in the manuscript. The impact of the life cycle phases on the biological pump is currently not mentioned but critical. We will include a discussion in the knowledge gaps section. Standardization of paired and non-paired species is currently not possible as new HET-HOL pairs are still being described (discussed in page 4, line 110). We have followed the most up-to-date understanding of coccolithophore life cycle pairs from Frada et al. (2018), however this will change in the future as more pairs are described. We will include this point in the knowledge gaps section.

Based in Figure 3, one could conclude that the relative abundances (f,g,h) of holococcos are only slightly lower compared to the heterococcos (c,d,e) and these figures need changes.

This is a good point. We have updated the axis to absolute abundance and will provide the updated Fig. 3 below in the revised manuscript (see Fig. 2 attached).

What is the difference in shading?

The light shading on the latitudinal plots is log transformed (as noted in the caption). We have updated the axis to better reflect this (see Fig. 2 attached).

BG Discussion: Reply to RC3

We would like to thank the third reviewer for their positive and constructive feedback. Our in-depth response can be found below.

General comments This is an interesting paper, timely, and relevant to the field of physiological ecology of phytoplankton. It deserves to be published but needs some minor revision. The paper was a bit sloppy in spots, with a number of typos. The paper should be checked over carefully prior to final submission.There are some terms that really need to be clarified in the revision to avoid confusion and to sharpen their points. First, when discussing nitrogenous nutrients they refer to "fixed nitrogen" as the sum of nitrate and nitrite (line 90-91). This reviewer has no idea why they are using the adjective "fixed" for the sum of these molecules (and they do not include ammonium or urea in that sum, for example). Typically, the fixation of nitrogen by phytoplankton is describing the uptake and assimilation of N2 gas into organic nitrogen fractions, which is not what they are describing. I would advocate that they globally scrub the term "fixed nitrogen" and replace it with something like dissolved inorganic nitrogen (DIN, here defined as nitrate + nitrite only).

We will replace 'Fixed nitrogen' with dissolved inorganic nitrogen (DIN) as suggested.

Second, in their equation about niche expansion (line 150) they refer to terms describing the "intersection of hypervolumes" and the "union of hypervolumes". If there is a union of hypervolumes, then they also intersect, right? The authors must very carefully define the difference between these. As long as there are ambiguities in the definition of those terms, then the entire niche expansion argument won't have much relevance.

In set theory the union includes all data points. While the difference is only the overlapping set of data points. We will include Figure 3 (attached) to the manuscript to clarify.

Finally, they talk about a 7% contribution of holococcolithophore abundance to the total coccolithophore abundance as being significant (abstract and line 331). It may be statistically significant, but it seems to this reviewer to be a little overblown. I would suggest that holococcolithophores more appropriately would be considered a minor constituent of the total coccolithophore assemblage. For holo/heterococcolith paired species, the holococcolithophore abundance represents 18% of the paired species abundance, only about a fifth, definitely still a minor fraction, at best. This doesn't detract from the results. It is still a fascinating observation and the question that arises to this reviewer is why is that fraction so small?

We agree that 7% is a minor fraction of total coccolithophore abundance. It is however not an insignificant fraction. Our argument is mainly that holococcolithophores are not insignificant rather than contributing significantly to total

coccolithophore abundance. We furthermore show that under certain circumstances holococcolithophores do become the dominant fraction. We will update our manuscript to reflect this.

As part of our discussion, we will comment on the reasons why the haploid fraction is much smaller than for the diploid life cycle phases:

- Strong dominance of *E. huxleyi*, which has a haploid naked phase
- Abundance of haploids in low nutrient regions which sustain a smaller total biomass compared to high nutrient regions (provided there are no other limiting factors).
- Potential sampling bias, specifically to bloom seasons when haploid abundance is low.

We will also highlight the implications towards calcium carbonate production (haploid tends to have a lower PIC:POC ratio) and how this will change under future climate (potentially more haploid relative to diploid in stratified regions with reduced nutrient supply). A scenario which is projected to occur in each major ocean basin under Representative Concentration Pathway (RCP) 8.5 (Fu et al., 2016).

This paper requires some revision but it provides new insights to a very real problem in coccolithophore ecology. It deserves to be published and will be cited well. The authors simply need to clean it up a bit.

Specific comments

Line 4 after "diploid life cycle phase" are they referring to coccolithophores only or other organisms. Please clarify.

Line 13 "ballast" not "ballasts"

Line 13-15 They are describing the biological carbon pump, not the carbonate pump(aka alkalinity pump). The linkage of calcite production to the biological carbon pump is a strong one via ballasting of organic carbon to the sea floor. This is not the alkalinity pump however. Klass and Archer (2002) were looking at the impact of ballasting of sinking POC and the effect on the rain ratio.

That is a good point. We will further clarify the distinction between the carbonate and organic carbon pump and include a reference for the former (i.e. Zeebe (2012))

Line 16 Globally, about one quarter of all marine sediments are calcium carbonate. Citing the 30-90% value presents a skewed view of the importance of calcite sediments on Earth.

Line 18- Given that this sentence is going back to the biological carbon pump, you might move it up in the paragraph where you are first mentioning the biological carbon pump.

Line 30 add an "s"..."A few organism"Line 91 Reference to "fixed nitrogen" and all subsequent uses of that term in the paper...see general comments above.

Lines 151 and 152- Must describe how the "union" and "intersection" of hypervolumes are being distinguished. See general comments above.

Line 162- Again, they are describing the function in R to calculate the "intersection"of hypervolumes when the reader may not be clear about the difference between this calculation and that of the union of the hypervolumes! This is a really important distinction.

Line 185 As they state, on a regional basis, holococcolithophores generally contributed <6% of total coccolithophore abundance. This seems pretty minor to be honest!

Line 190- change to..."where a HOLP-index"...not "an Holp-index"

Line 193- add comma, "In the global data set, heterococcolithophore..."

Line 218- change to..."high nutrient concentrations, cold water temperatures at depth or other factors not addressed in this study".

Line 253 They show significant positive correlations with silicate. This is a very interesting observation. The Discussion section should have a few sentences explaining how this could be!

This is a good point and we will include this in the discussion. The trends are potentially explained by different silicate requirements of coccolithophores noted in Durak et al. (2016). Who found evidence for silicate requirement of the heterococcolith life cycle phases of *S. apsteinii*, *C. braarudii* and *C. leptoporus* but not for *E. huxleyi* or *G. oceanica*. Follow up experiments have furthermore found holococcolith life cycle phases of *C. braarudii* and *C. leptoporus* do not require silicate (manuscript by Langer et al., currently in prep).

Line 273- add "s" to heterococcolithophore to make it plural.

Line 285 add comma, "overlap metrics, respectively"Line 331 "Our meta-analysis shows that holococcolithophores are important contributors to coccolithophore abundance and ecology contributing 7.3% to total coccolithophore abundance" This observation doesn't match the data. 7.3% is a small number. Call it like it is!

Line 336- re-word this so that it agrees with the minor contribution..."past and future oceans could have other biogeochemical effects. A shift towards"...

Line 363- Remove "?"

Missing BibTeX entry, now fixed.

Line 366- improper hyphenation of wrap-around word "coccolithophore"

Line 370- I disagree with this statement. Calcification measurements are including the calcite production of holo- and heterococcoliths. However, the standing stock of calcite is being underestimated by not including the holococcolithophore abundance. Also, leave off the last words of the sentence, "or activity".

This is a good point and we will update the manuscript to reflect this.

Line 380- Sentence "Overall observations in the haploid stage of E. huxleyi are...".There is some classic literature that the authors should cite from the mid 1990's: Campbell, L., et al. (1994). "Immunochemical characterization for eukaryotic ultraplankton from the Atlantic and Pacific oceans." Journal of Plankton Research 16(1): 35-51. They used immunochemical antisera to identify haploid stages of E. huxleyi.

We would like to thank the reviewer for this interesting paper and will include it in our discussion of haploid *E. huxleyi* quantification.

Line 385;Again, there were a number of classic SEM studies from the Pacific Ocean. One by Reid (1980). Reid, F. (1980). "Coccolithophorids of the North Pacific Central Gyre with notes on their vertical and seasonal distribution." Micropaleontology 26: 151-176. The SEM plates in the paper are meticulous and it might be worth a look before you discount all Pacific SEM observations. See also previous work of Honjo and Okada from the Pacific.

Again, thank you for these citations. We have previously contacted Honjo and Okada on multiple occasions in regards to their SEM measurements in the Pacific with no success. Unfortunately this data is not available as part of the original publications and does not appear available elsewhere.

We were unaware of the publication by Reid, F. but unfortunately she appears deceased and the data is again not retrievable from the original publication (the depths are averaged in Table 1.).

We will update the discussion to better reflect this, and clarify that although SEM measurements were made in the Pacific in the 1970s and 1980s this data is not retrievable. We will furthermore discuss the trends noted in these studies. Mainly, absence (or comparatively low abundance) of holococcolithophores in Okada and Honjo (1973) and Honjo and Okada (1974), and low abundance (500-5,000 cells per L) of holococcolithophores in Reid (1980). Which supports trends observed for Silver (2009) included in our analysis.

Line 404- There is contrary evidence you should cite to be balance, though: Rivero-Calle, S., et al. (2015). "Multidecadal increase in North Atlantic coccolithophores and the potential role of rising CO2." Science 350(6267): 1533-1537.

We will include this reference and note contradicting evidence.

Line 407- eliminate the "s" from "compositions"

Line 414- Reword, "Our analysis shows that holococcolithophores constitute about one fifth of total paired coccolithophore abundance..."

Figures:

Figures 3 and 4- The font on all the axes is way to small to be readable. These must be increased in size.

We have increased the size of our axis labels.

Figure 4- Legend is reversed for red and blue colors...Heterococcos are plotted in red(not blue) and holococcos are plotted in blue (not red).

Thank you for pointing this out. Heterococcolithophores are plotted in red and holococcolithophores in blue throughout the manuscript. However in the caption of Fig. 4 this is reversed. We will update the caption to correct this.

Figures 5 and 6- No units are provide in this figure or the legend for the color bars!

We included the units in the caption of the figure, but have now included units within the figure instead.

Fig. 6 change "fixed nitrogen" to DIN (see also Fig. 7)

Table 2 is excellent and a great reference. Should you state the names for the holoforms of R. clavigera and R. xiphos since you have left them blank?

They are blank because *A. robusta*, *R. clavigera*, and *R. xiphos* are all associated with *S. quadridentata*. The same is true for *S. pulchra* and *S. protrudens*

which are both associated with *S. pulchra* HOL. We will update the table to reflect this.

Tables 6 and 7- You never discuss the significant relationships with Silicate (not "Si" as you say in the table!) This really deserves some discussion. See our response above [line 315].

Tables 8 and 9 The legends are very minimalistic. Please move your definition of NE1and NE2 to the legend from the footnotes. This needs to be more obvious to the reader. Also, maybe specify in the table legend what the Jaccard and Sorensen columns refer to (and units?) or refer the reader to the text.

We will expand the legends for both tables and move our NE definitions.

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BG Discussion: updated Figures and Tables

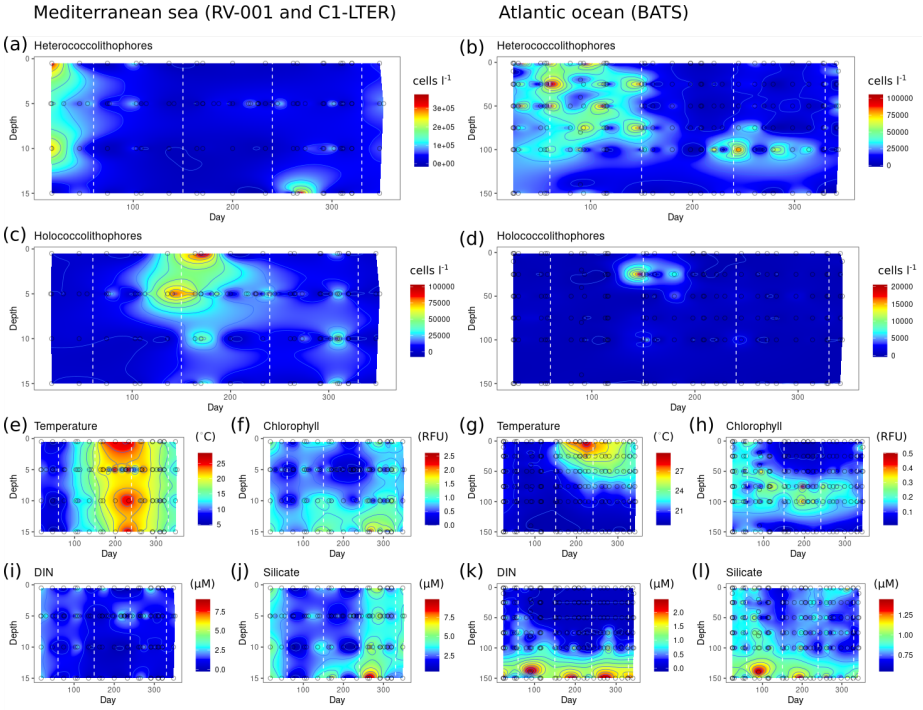

Figure 1: Seasonality of hetero- and holococcolithophores at the BATS station in Bermuda (left column) and the RV-001 and LTER-1 stations in the Mediterranean Sea (right column). Note heterococcolithophores are most abundant in the winter followed by a high abundance of holococcolithophores in the late spring and early summer. **(a-b)** Heterococcolithophore abundance; **(b-c)** Holococcolithophore abundance; **(e, g)** Temperature; **(f, h)** Chlorophyll; **(i, k)** DIN (nitrite + nitrate); **(j, l)** Silicate.

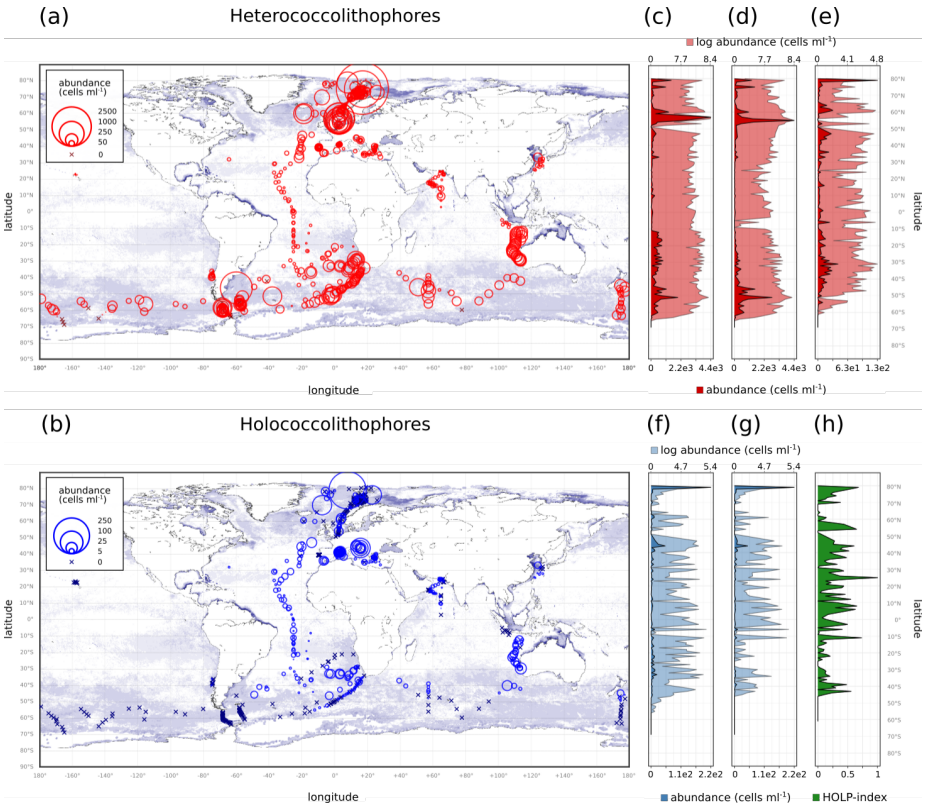

Figure 2: **(a-b)** Global coccolithophore distribution; **(c-h)** latitudinal coccolithophore distribution. **(a)** Heterococolithophores; **(b)** Holococcolithophores; **(c)** Heterococcolithophores; **(d)** *E. huxleyi*; **(e)** Paired heterococolithophores; **(f)** Holococcolithophores; **(g)** Paired holococcolithophores; **(h)** HOLP-index.

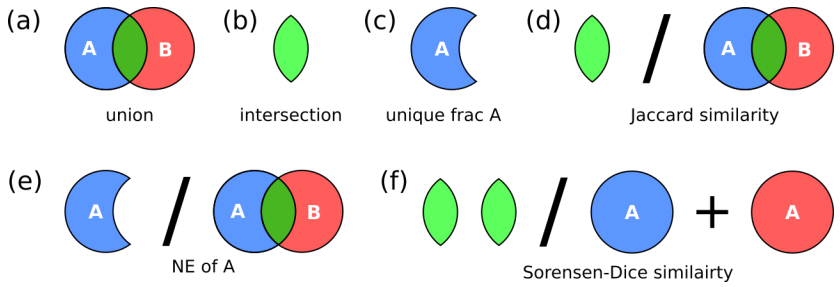

Figure 3: Hyper volume metrics utilized in this study. **a)** union; **b)** intersection; **c)** unique fraction A; **d)** Jaccard similarity metric; **e)** niche expansion of A: **f)** Sorensen-Dice similarity

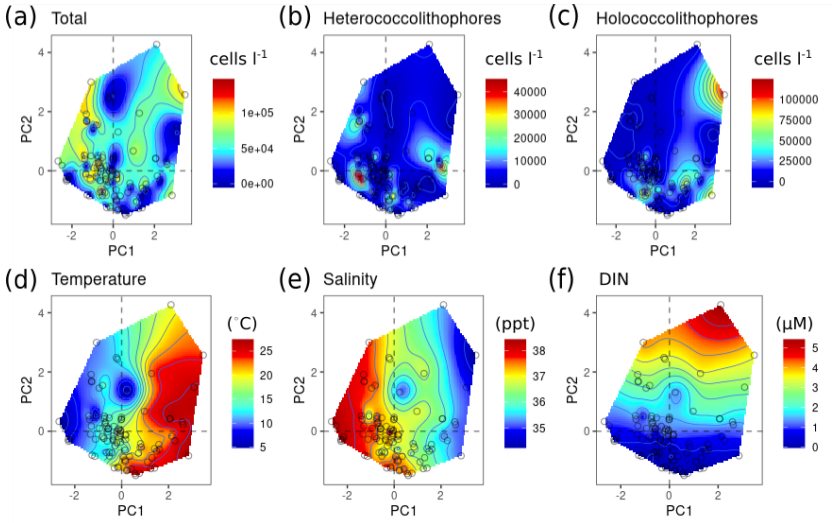

Figure 4: PCA niche space in the Mediterranean data set. Temperature, Salinity and DIN are included as principal components. Note lack of clear separation between the two life cycle phases when day length is not included. **(a)** Total coccolithophore abundance;**(b)** Heterococcolithophore abundance; **(c)** Holococcolithophore abundance; **(d)** Temperature; **(e)** Salinity; **(d)** DIN (nitrite + nitrate)

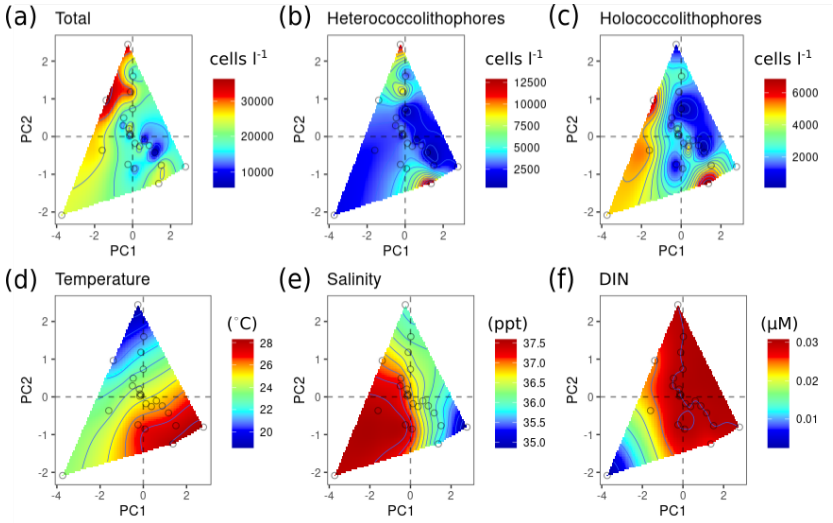

Figure 5: PCA niche space in the AMT data set. Temperature, Salinity and DIN are included as principal components. Note lack of clear separation between the two life cycle phases when depth is not included. **(a)** Total coccolithophore abundance;**(b)** Heterococcolithophore abundance; **(c)** Holococcolithophore abundance; **(d)** Temperature; **(e)** Salinity; **(d)** DIN (nitrite + nitrate)

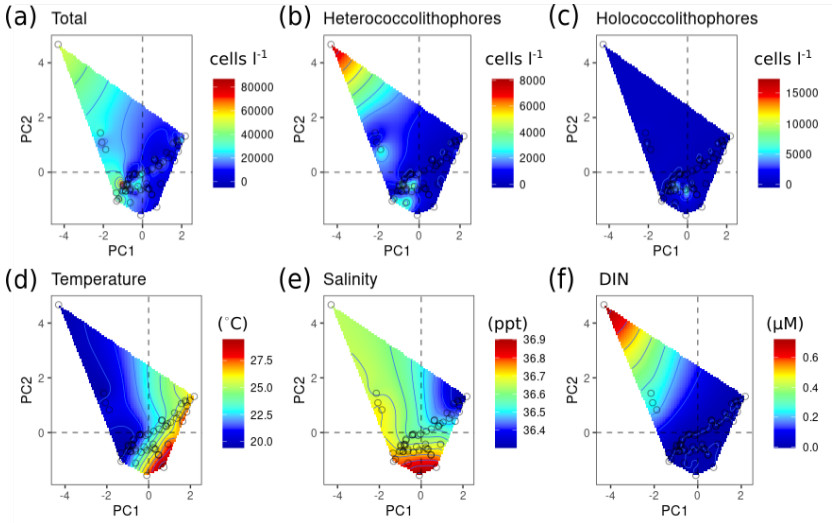

Figure 6: PCA niche space in the BATS data set. Temperature, Salinity and DIN are included as principal components. Note lack of clear separation between the two life cycle phases when depth and date are not included. **(a)** Total coccolithophore abundance;**(b)** Heterococcolithophore abundance; **(c)** Holococcolithophore abundance; **(d)** Temperature; **(e)** Salinity; **(d)** DIN (nitrite + nitrate)

Table 1: Niche expansion and niche overlap.

Species	Study	NE1	NE2	Jaccard	Sørensen
Paired species	AMT	0.11	0.05	0.84	0.91
	BATS	0.49	0.02	0.50	0.66
	Med	0.31	0.15	0.54	0.70
A. quattrospina	AMT	0.50	0.05	0.45	0.62
	Med	0.47	0.18	0.35	0.52
C. leptoporus	AMT	0.21	0.45	0.34	0.51
	Med	0.26	0.61	0.13	0.23
C. mediterranea	AMT	0.06	0.46	0.48	0.65
	Med	0.22	0.42	0.37	0.54
H. carteri	AMT	0.41	0.30	0.29	0.45
H. wallichii	AMT	0.42	0.47	0.11	0.20
S. anthos	AMT	0.69	0.04	0.27	0.42
	BATS	0.41	0.28	0.32	0.48
S. arethusa	Med	0.26	0.29	0.45	0.62
S. bannockii	AMT	0.17	0.19	0.63	0.77
S. halldalii	AMT	0.17	0.08	0.74	0.85
S. histrica	AMT	0.36	0.17	0.47	0.64
	Med	0.03	0.78	0.19	0.32
S. molischii	AMT	0.44	0.32	0.24	0.39
	Med	0.47	0.53	0.00	0.00
S. pulchra	AMT	0.18	0.14	0.68	0.81
	BATS	0.49	0.21	0.30	0.46
	Med	0.51	0.16	0.33	0.49
S. nana	AMT	0.50	0.06	0.44	0.62
S. strigilis	Med	0.12	0.53	0.35	0.52

NE1 = Heterococcolithophore niche expansion,
NE2 = Holococcolithophore niche expansion