Reviewer 2

The sequence of warmings and coolings associated with the last deglaciation and the Holocene has shown contrasting patterns between southern and northern high latitudes. Global-scale processes such as variations of AMOC strength and the alteration of atmospheric circulation seem responsible for this contrast. Most of the high resolution paleorecords studied so far were gained from Antarctic ice cores. Therefore, acquiring high-resolution proxy records of past sea surface temperature is relevant for finding out how spatio-temporal patterns of water temperature evolved in the Southern Ocean and whether any links with Antarctic temperature variability are recognized. Orme and co-authors present a high-resolution, diatom-based record of sea-surface temperature gained in the western Indian sector of the Southern Ocean (Core KH-107 COR1GC, ca. 54.27◦S, 39.77◦E, WD 2834 m). Their sediment record spans the past 14.2 Ka BP. The age model bases on fifteen AMS radiocarbon dates obtained on mono-specific samples of the planktic foraminifera Neogloboquadrina pachyderma sin. The average temporal resolution of their diatom counts is ca. 60 years (diatom analysis was conducted every cm in the 2.48 m gravity core). By using the Modern Analogue Technique applied to diatom assemblages, Orme and colleagues estimate the summer SST and the winter sea ice concentration. They describe and discuss patterns, timing and magnitude of sea-surface temperature variability through the late deglaciation and the Holocene in the western Indian sector of the Southern Ocean and possible links to global and regional forcings and mechanisms. The Introduction presents basic information on (1) deglacial events and Holocene intervals, and (2) main mechanisms/forcings behind temperature in the Southern Ocean; it reads well and helps the reader less familiar with issues addressed later in the MS. The Methodology is clearly written. Results are concisely presented; the results representation however can improve (see suggestions below). Figures are self-explanatory and necessary in number. References are satisfactory.

A major concern is how the Discussion is organized. Throughout the Discussion, there are several inconsistencies and several vague statements that lack scientific support. Some ideas are shortly presented, without any further and deeper discussion. Too many forcings and mechanisms are offered as possible explanations for the SST variations (solar forcing, internal climate variability, sea-ice and productivity changes, ocean-atmosphere coupling, rapid climate change events globally, establishment of modem ENSO amplitude and frequency), without clearly distinguishing which mechanism/forcing/s was/were more important when, and the reader gets lost. Please consider: (1) shortening and focusing the discussion, and (2) adding a Table summarizing with main mechanism/forcing/s for each for each of the discussed intervals.

We would like to thank reviewer 2 for the very helpful comments provided, which we feel have improved the manuscript.

In the discussion, particularly the first two paragraphs of section 5.2.1., the text has been shortened to make the discussion more focused. See lines 270 to 290.

In line with a similar comment from reviewer 1 (number 3e) and the comment here labelled L326-340 the paragraph about cycles/variability at the end of the discussion (lines 351-374) has been reorganized and re-written. We start by presenting the evidence for 200-300 year cycles in both Southern Ocean records and those reflecting westerly winds/SAM and then use this as a basis for stating two hypotheses 1) that there was shared internal variability and ocean-atmosphere coupling or 2) a shared external forcing (solar). In support for the first point we describe the model findings of Latif et al (2013) about centennial internal variability in the Southern Ocean and atmospheric circulation triggered by changes in deepwater formation. We have added more detail about this. In support of the second point, that solar forcing may have caused the changes, we have linked to modern evidence for the effect of solar activity on the SAM during the instrumental period. We have removed the parts of the discussion about ENSO and rapid climate events which were more speculative, and instead focused on the two causes we consider most likely.

As suggested we have now added a table to summarise the main causes suggested for the Younger Dryas, millennial changes and mid-late Holocene variability (lines 730, table 2) and referred to this in the text (line 268)

Below I list several minor comments and give some suggestions which might be helpful to improve your MSL.

66-67: this is repeated several times throughout the Intro. Please revise. This sentence has been shortened and merged with the previous sentence to reduce the repetition (lines 66-68).

L. 93-94: since the authors state that 'Topography has a strong influence on the position and form of the ACC in this region', they should provide a more detailed figure of the study area in Fig. 1, including bathymetric information.

We have now included in figure 1 a bathymetric map of the Conrad Rise adapted from Ansorge et al (2008) including the current strength (shown by absolute geostrophic velocity).

L: 108: Neogloboquadrina pachyderma should be in italics. Corrected.

L. 115-125: Using reservoir age from a core gained in a mid-latitude coastal upwelling system is -at the least-risky and caution is advised. Oceanographical and nutrient conditions in the SE Pacific Ocean are quite different from those in the Conrad Rise and can hardly be straightforwardly applied. The high-resolution sampling (every cm) make the age model uncertainties even larger. Additionally, reworking should be considered/discussed.

The paragraph as a whole has been rephrased to explain the different evidence for reservoir changes in different locations (lines 105-119), which justifies our decision for choosing to use a consistent reservoir age through the record. We hope that this balanced assessment of the limited available evidence shows the reader that there are age uncertainties associated with the selection of reservoir ages.

We have included the evidence of reservoir ages from the Siani et al (2013) study mentioned here in this paragraph for couple of reasons. The first is that this site carries a Southern Ocean signal, as it is bathed in waters from the Southern Ocean that are deflected northward by the South American continent, and is outside of the Peruvian upwelling system, with little evidence for local upwelling (e.g. low primary productivity; Abrantes et al., 2007). The second reason is that this is the only core to have reservoir age estimates for both the last deglacial and the Holocene. Therefore while we did not base our chosen reservoir ages on this study, we feel that the consideration of different evidence from the Southern Ocean does help show the reader the current limited and challenging nature of reservoir age estimates.

Though there might be some discrepancies in reservoir age changes through time between basins, the good correspondence between COR1GC SST and ice core records (figure 7) suggests that the corrections made in the present study are adequate.

L. 145-46: these three processes strongly impact your diatom signal (the basis of your SST reconstructions), but it is hardly discussed in 5. Discussion.

This answer also covers the correction for line 241-243 below.

Many studies have shown that diatom sedimentary signals, though imperfect, preserve the main features of diatom productivity/assemblages in surface water (Zielinski and Gersonde, 1998; Gersonde and Zielinski, 2000; Armand et al., 2005; Crosta et al., 2005; Romero et al., 2005). As such, diatoms have been robustly used to quantitatively infer past surface conditions through several statistical techniques (Gersonde and Zielinski, 2000; Crosta et al., 2004, 2020; Nielsen et al., 2004; Esper et al., 2014; Esper and Gersonde, 2014; Ferry et al., 2015; Benz et al., 2016; Xiao et al., 2016), generally in agreement with other techniques based on other micro-fossils or geochemical proxies (Becquey et al., 2002,2003; Panhke et al., 2005; Ho et al., 2016).

We have added a paragraph to the methods section 3.3 about the dissolution of diatoms (lines 135-146). We feel that this fits better at this point in the paper rather than in the discussion. We observe that the dissolution of poorly silicified diatoms, often those from cold waters, could result in reconstructed warmer temperatures and less sea ice (Xiao et al 2016). However we note also that temperature is the dominant factor effecting species assemblages, and that diatom dissolution is at a minima at 50-55°S (Pichon et al., 1992; Esper et al., 2010). This means that while dissolution may have altered the assemblages to some degree, the effect on the reconstructed temperature should not be strong particularly at the latitude of the Conrad Rise. During analysis we observed good preservation through the core, with (1) well preserved diatom valves in which the fine ornamentation was still visible, (2) diverse diatom assemblages containing diatoms along the whole size spectrum and (3) little fragmentation. Finally, the good general agreement of the new SST record with previous records from similar realms and with the ice core temperature records, gives confidence in that the assemblages in COR1GC are reflecting climate and that our diatom-based reconstruction are therefore robust.

L. 172: please give age ranges for the Holocene (which age definition of Holocene did you follow?) I suggest adding a box in the upper part of Fig 4, indicating the main intervals of the last deglaciation (YD, ACR, etc.) and Holocene (early/middle/late). This is presented later in Fig 7, but it should be earlier when Results are described.

We have included reference to the formal defined boundaries of the early Holocene of 11.7-8.2 ka BP (Walker et al., 2018) in the introduction at line 51. We do not separate between the mid and late Holocene in the discussion and results, therefore the age boundaries of these have not been specified or separated in the figures.

We have added the lines and labels to figure 4 as suggested, and also added the time periods to figure 3.

L. 173-183: References for the paleoecological information of the diatom species should be provided here. The reader does not know where the species ecology does come from

This has been corrected (line 182-195)

L. 184: 'The estimated total diatom abundance shows a decreasing trend through the record', please revise this statement. It is not quite correct to state that the total diatom abundance (TDA) shows an overall decreasing between last deglaciation and the latest Holocene. Indeed, TDA varies strongly up to 5.5 Ka BP and experienced afterward a two-step decrease, first around 5.5 Ka BP and later between 4 and 1 ka BP.

We decided to change this to the diatom flux rather than the diatom abundance. Diatom flux is a closer proxy for productivity and upwelling than diatom abundance because it accounts for changes in sedimentation rate and sediment density. We used the approach of Romero et al. (2015). We had the required data on diatom abundance and sedimentation rate for COR1GC however not the density data for this core. We therefore used the density data for COR1bPC, which was taken from the same location during the same cruise. As both cores have good chronological constraints (COR1bPC has 13 radiocarbon dates for the last 14.2 ka BP) it was possible to transfer the density values from COR1bPC to COR1GC by identifying the depths that were closest in age. The largest age difference between samples that were transferred was 20-30 years.

The results now show a decrease in diatom fluxes at 14.2-12 ka BP, low fluxes from 12-10.3 ka BP, higher fluxes between 10.3 and 5.2 ka BP and lower fluxes after 5.2 ka BP.

We have altered the methods (lines 147-154), results (lines 195-198) and discussion at lines 294 and 347 (see also our response to corrections below).

L. 190-198: all short intervals mentioned here should be easily recognizable in Fig 4. Please add some arrows to help the reader to better understand what you are trying to communicate. Moreover, add marks between millennial ages in Fig 4: your Results description goes into centennial-scale description (e.g., Between 11.6 and 8.7 ka BBT, etc). Without these centennial-scale marks is even more difficult to recognize whichever trends and shifts occurred.

We have added tick points every 200 years to Figure 4 so the reader can assess the centennial timings as suggested. We have added arrows to highlight the two significant centennial events at c.8.2 and c.2.2 ka BP as identified by SiZer and explained in this paragraph. We have also added a line to show the increasing trend during the Younger Dryas. These changes are in addition to the alterations made for the comment below labelled Figure 4.

L. 210: 'high temperatures between 11.6 and 8.7 ka BP during the Early Holocene, followed by a cooling trend thereafter': this is a matter of interpretation. The range of SST variability is larger (larger amplitude) between 8 and 1 ka BP than earlier between 8.2 and 11.8 ka BP. However, is it correct to state that a cooling occurred during the middle to late Holocene? I am not able to recognize a clear decreasing trend in your data depicted in Fig. 7d.

This sentence is referring to the findings in other records rather than the COR1GC record, although we are stating that these other records show similar findings to the COR1GC record. There is evidence for a slight cooling trend in that the mean decreases from 4.3-3.9°C between the early to mid-late Holocene, there were lower minimum temperatures and reconstructed sea ice increased in the mid-late Holocene but was absent in the early Holocene. However this cooling is shown by the SiZer analysis to not be statistically significant.

We have added to the results the word ‘slight’ in places to show the cooling wasn’t large (between lines 202-205) and added a sentence to state that the cooling is not significant in the SiZer analysis section of the results (line 208). In the discussion section 5.1 we have added a sentence to acknowledge that unlike some other records the COR1GC cooling in the Holocene was slight (lines 230) but acknowledge here that there were cool events (low SST excursions, sea ice species). We have also adjusted the wording in the discussion (line 348) and conclusion (lines 379) to acknowledge that the reconstructed temperature difference is minimal.

L. 215: ‘Although most records, including COR1GC, show a long-term cooling over the Holocene (Xiao et al., 2016)‘. Please revise: the PS2606-6 SST record shows similar values during YD and the entire Holocene. SO, where is the Holocene cooling?

This sentence is about the Holocene, not the temperature difference between the YD and late Holocene. During the Holocene the PS2606-6 record is showing a cooling, whereby temperatures at c.12-9 ka BP in this record were warmer than the period after 9 ka BP (Xiao et al., 2016). This is in support of other records which generally also show cooling (e.g. Bianchi and Gersonde, 2004; Anderson et al., 2009).

This PS2606-6 record shows an abrupt change at 9 ka BP, rather than a gradual cooling, therefore to acknowledge that there is a difference between the gradual cooling shown in some records and the rapid cooling in others, we have added:

‘Furthermore, while most records show cooling over the Holocene, either gradually or as an abrupt cooling at the end of the early Holocene (Xiao et al., 2016), there are some differences between records’ at line 228.

L. 218: ‘The records from Bouvet Islands’, where is this? Which sector of the SO? Please provide more accurate information.

We have added ‘in the eastern Atlantic sector of the Southern Ocean’ after this statement at line 237.

L. 222-23: ‘Our new record from core COR1GC conversely shows SSTs were 1◦C lower during the ACR compared to the mid-late Holocene‘, Is this 1◦C difference statistically significant? 1◦C of SST difference lays surely within the range of variability of your SST reconstruction.

Given that the temperature difference between the ACR and late Holocene is close to being significant (given the RMSEP of 1°C) and there is a good similarity between the magnitude and patterns of temperature change between the COR1GC SST and ice core records, we feel confident that the cooler temperatures during the ACR are real.

However we have added a sentence to acknowledge that transfer function prediction error is a possible cause of the differences between records (such as other records showing no difference in temperature between the ACR and mid-late Holocene), as this is a factor potentially effecting other transfer function based records as well. Lines 243-245:

‘The contrasting findings between SST reconstructions may be explained by the reconstructed temperatures being close to the prediction error of SST transfer functions, which is ~1°C in this study and 0.86°C in Xiao et al. (2016).’

L. 233: can a 2-3◦C rise of SST during the Holocene -compared to last deglaciation- as WARM conditions? I understand that it was warmer, but it is not a warm environment per se, mainly when your SST reconstructions is compared with records from mid and low latitudes.

We did not mean to imply that conditions were warm, rather that they were warmer compared to the rest of the record. We have changed the wording from ‘relatively warm conditions through the Holocene’ to ‘leading to slightly warmer conditions through the Holocene’ (line 257)and also later in the section referred to the ‘marginally warmer early Holocene’ (line 260). In the conclusions we have also adjusted the sentence at line 378 to clarify that these temperature changes were slight.

L. 241-43: I agree with these mechanisms and forcings impacting the reconstructed SST record at your core site. However, since diatoms experience dissolution between sea surface and the bottom of the ocean, I can assume that your reconstructed SST values vary depending on which species did it to the sediment. There is, however, no discussion on the possible role of preferential dissolution/preservation of diatoms (see also l. 145-46).

Please see our response for point L145-146 which addresses this. The discussion of this has been incorporated into the methods section rather than here (line 135-146). We consider that the oceanographic conditions at the site have not changed enough through time to alter the amount of diatom dissolution and therefore the SST signal.

L. 245: ‘Southern high latitude warming (Termination 1b) during the Younger Dryas‘, this is given as one unique interval before (see l. 209-10). Please revise and rephrase correspondingly.

It is not clear what is meant here, but for clarity we have removed the term ‘termination 1b’ and instead used the Younger Dryas throughout the paper.

L.266-67: ‘Greater upwelling has been shown by higher opal deposition to the south of the Polar Front in the Southern Ocean (Atlantic, Indian and Pacific sectors) through the period 12.7- 11.5 ka BP‘, this is true. However, your TDA data do not show any significant difference among ACR, YD, and early Holocene. Therefore, your data offer no convincing evidence of an intensification of upwelling following the last deglaciation.

This paragraph (now lines 277-290) is about the explaining the identified sequence of events and evidence for this based on previous studies, rather than linking with our evidence which comes in the following paragraph. Therefore we have not changed this statement but have adjusted the part where we discuss our results (see next comment).

L.275-76: ‘as tentatively inferred from the slightly increased diatom abundances at 12.7-12 ka BP’. This is hardly recognizable in your COR1GC record. Your TDA does not actually differ from earlier and later values. Please revise.

We have changed the diatom abundance to diatom fluxes, as explained above in response to the correction L184. The new diatom flux record however also doesn't support that there was higher diatom productivity or upwelling at this time, as the values decrease from 14.2 to 12 ka BP. Therefore we have altered this sentence to read: 'However there is no evidence of increased productivity and therefore upwelling at the Conrad Rise, as diatom fluxes instead decreased through this period (Figure 8E)' (lines 294-295)

L. 283-84: 'Indeed a southward shift is indicated by an increase in Polar Front species at c. 12 ka BP', you mean Thalassionema nitzschioides var. nitzschioides? The increase is not that clear in F. kerguelensis.

Yes this was in reference to the increase in *Thalassionema nitzschioides* var. *lanceolata.* We have specified this now at line 302

L. 296: 'has been attributed to high annual, winter and spring insolation levels', please clarify: do you mean average annual insolation or winter and spring insolation?

We have changed this to spring insolation and now cite the papers Shevenell et al. (2011) and Etourneau et al (2013) who also concluded that spring insolation caused early Holocene warming due to a longer summer season. Lines 315-318.

L. 315: SSW's, misspelling. See also below l. 317.

This has been corrected

L. 316: 'together can explain the gradual cooling in the COR1GC record', please provide SST range, average and 1 STD for your mid and late Holocene SST record.

We have now ensured that the mean, standard deviation and range is now provided for the three key periods (ACR, early Holocene and mid-late Holocene) in the results section, lines ~200. These show that the mean decreased from 4.3 to 3.9 °C and there were more frequent low temperature excursions in the mid-late Holocene, with the minima changing from 3.3 to 2.2°.

We have adjusted the wording as follows at line 342:

'which together can explain the <u>slight</u> cooling in the COR1GC record, <u>the increased frequency of cold events and increase in sea ice</u>'

Which we feel provides the reader with clarity about the evidence for the mid-late Holocene cooling in this record.

L. 320: 'however it is not clear if this occurred as although there was a gradual increase in sedimentation rate, potentially reflecting an increased deposition of diatoms'. Be cautious with this: according to your data, no increase in total diatom abundance occurred at this time.

We have re-written this sentence to show that the evidence of a decreasing diatom flux does not support increasing productivity, and removed the sentence about the sedimentation rate increasing, which has a less direct link with productivity. At the recommendation of reviewer 1 we have also added a sentence about the reason for this decrease. (lines 346-350)

L. 326-340: this is a quite different story from all the above discussion and confuses the reader. Several forcings are mentioned/shortly discussed in 14 lines (solar forcing, internal climate variability, sea-ice and productivity changes, ocean-atmosphere coupling, rapid climate change events globally, establishment of modem ENSO amplitude and frequency). Presenting an alternative climate scenario at the very end of the manuscript (rapid climate change events globally and establishment of modem ENSO amplitude and frequency), without any further discussion makes this subsection even more confusing and does not add anything valuable to your overall Discussion.

This point was shared by reviewer 1 and to address it we have re-written this final paragraph. Please see our response to reviewer 1 and the above comment at the start of this response.

Figures

Fig 1: please consider (1) zooming into the closest area to core COR1GC (include bathymetry), and (2) identifying the Atlantic, Indian and Pacific sectors of the Southern Ocean. In the caption the references for cores TN057-13-PC4, TN057-17PC1, MD07-3088, EDML, and EDC should be presented.

We have now included in figure 1 a bathymetric map of the Conrad Rise adapted from Ansorge et al (2008) including the current strength (shown by absolute geostrophic velocity). We have added labels for the Atlantic, Indian and Pacific sectors to panel (a) and referenced each of these records as recommended.

Fig. 3: note that you name core COR1GC differently depending on the figures. Please revise. var. in Thalassionema nitzschioides var. nitzschioides should not be italics. Thalassiosira oestrupii has been renamed for years already, the current name is Shionodiscus oestrupii. Consider using exponential nomenclature for x-axis of TDA. Consider adding some arrows to lead the reader in better understanding major shifts/changes in (1) the species composition of the diatom assemblage, and (2) total diatom abundance.

We have changed all the figure captions so they are COR1GC rather than KH-10-7 COR1GC. We have changed Thalassiosira oestrupii to Shionodiscus oestrupii, both here and in the manuscript. We have corrected the labelling so var. is not in italics. We have added arrows to show the major trends in the diatom species abundances and the diatom flux data. The axis label is now in exponential nomenclature.

Fig. 4: Please consider adding a box in the upper panel indicating YD, early/mid/late Holocene, etc. (see Fig 7) Consider also adding the (1) average and 1STD of your own data, and (2) present-day summer and winter SSTs., and (3) present-day mean winter sea ice concentration.

We have added and separated the sections in the COR1GC record as advised and highlighted the modern summer SST and sea ice concentration. As diatom records reflect summer SST rather than winter SST, we have not included the winter SST as this would have no relevance to the record presented. We also decided not to present the average and standard deviation on the plot, as it was our view that this could make the graph look crowded. The average and standard deviation for the different sections are included in section 4 as advised previously.

Fig. 7: the long-term pattern of your SST data is pretty similar to that of East Antarctica cores: low ACR values, increase during the YD, and warmer Holocene SST. A simple statistical analysis should help you to better understand the trends. Your SST record shows ten SST minima (cooling) during the Holocene: it seems to me that most of these minima are made by only ONE sample. This can be part of regular variability of the diatom assemblage and not at all related with actual SST variations. Caution is advised in the interpretation of these minima!

The SiZer analysis already represents a step forward to statistically understand the significance of the short-term variability. We feel that additional statistical tests, like providing a correlation matrix, is hampered by the age uncertainty and low resolution of many records. In this vein, a direct correlation test between COR1GC SST record and ice core records, though possible, will imply important resampling and standardization steps that may alter the results and may not bring many new information. However to show more clearly the similar timing of changes between the COR1GC SST record and those from ice cores, we have added a supplementary information file including the COR1GC SST record and EDML temperature (δ18O) and sea ice extent (ssNa) records. These have been normalised and smoothed using SiZer (using the local linear kernel estimator) and a bandwidth of 400 years. The results highlight the close association between the SST, atmospheric temperatures and sea ice extent in the region. The Supplementary Information has been referred to in the paper at lines 251 and 293.

Although some of the minima are single data points they represent temperature excursions of 1-2°C which is above the prediction error for the record. Despite the same methods, material and location, the early Holocene does not have these large oscillations in temperature, supporting that the occurrence of these minima in the mid-late Holocene reflect real climate changes rather than just diatom assemblage variability, which would be present through the whole period. We discuss the most persistent cold excursion at 8.2 ka BP but not any of the later minima specifically, other than in relation to the increasing variability, so feel that these are not over interpreted in the manuscript. As such we have not altered the text.