*We thank **Anonymous Referee #2** for his time and effort to review our manuscript and for his highly helpful comments and encouragement.*

General comments

In the manuscript "Influence of the North Atlantic subpolar gyre circulation on the 4.2 ka BP event" by B. Jalali et al. reconstruct the behavior of the North Atlantic subpolar gyre during the 4.2 event. The authors present new alkenone reconstructions obtained from two marine sediment cores west of Iceland as well as pollen data from a sediment core the Gulf of Lyon and compare it to a compilation of marine and terrestrial datasets from the North Atlantic region and to a modeling study. This new compilation adds vulnerable information on the 4.2 event and confirm an already postulated concept of a weakening of the STG during the 4.2 event and thereby fits well within the scope of CP.

The manuscript is well written, well structured and it is easy to follow the argumentation of the authors. Nevertheless, the report of the scientific methods is partially unprecise. Error estimates on the alkenone and pollen records are entirely missing, in the figures as well as in the methods chapter (also see detailed comments). Thus, it is difficult to evaluate of the significance of the presented changes in SST and pollen composition.

It is not usual to point the errors on the pollen data. Generally palynologists consider that the error on percentage derived from the pollen counting is on the order of 2%, although never provided in publications.

As the changes in SST during the 4.2 event of 1.5°C (MD99-2275) barely exceeds the error of the measurement, these definitely need to be addressed.

Internal precision on SST is estimated to be on the order of 0.5°C (Ternois et al., 1997); it is now added in the revised version. The SST changes in MD99-2275 are on the order of 1.5 to 2°C and thus significant.

The age models of the two different cores are based on two different methods and use different 14C calibration curves. As the discussion and conclusion of the manuscript severely rely on a robust stratigraphy, age models should be based on the same calibration and age model calculation method. For the latter I suggest to use a Bayesian age model approach, as already is done for core MD99-2275.

The age model of MD95-2015 and KSGC-31 cores are now based on the Bayesian method using Oxcal4.3, the MARINE13 calibration data set as well as local reservoir ages obtained from the Global Marine Reservoir Database using the eight nearest site reservoir ages (http://calib.org/marine/). This information is provided in the method sections in details.

Additionally, core KSGC-31 should be better included into the introduction and the method section. An age model description or reference to the age model as well as a short description of the coring position is entirely missing in the method section. Thus, it remains unclear which data is new and which data has already been published by Jalali et al. (2016).

Information about the KSGC-31 core has been added in the revised manuscript (introduction and method sections). More details can be found where these data have been already published.

Furthermore, references to already published alkenone data of core MD95-2015 (Marchal et al, 2002) and its discussion (e.g. Leduc et al., 2010) are missing.

The data presented in this paper are new and not those published in the above cited references.

The results and discussion chapter focus on the spatial extension strength of the SPG during the 4.2 event, including a short discussion on its driving mechanism. The authors describe a cold/warm dipole in the North Atlantic. They argue that this pattern originates from a wintertime atmospheric blocking that is induced by a weak subpolar gyre circulation. The description of the data is very short and oversimplified. Information about observed ranges of change as temperature amplitudes in the 4.2 event and % of AMOC reduction are mostly missing.

We have added additional description of range and SST values, but do not have such information on the AMOC. The discussion on the ISOW proxy strength in Figure 3 is qualitative and only one component of the AMOC.

This makes it difficult to assess the significance of the described changes and thus the reliability of the entire discussion. Regardless the uncertainty, the description of this circulation pattern matches with the dataset, yet seasonality of the different proxy records should be considered, as already pointed out by Eduardo Moreno-Chamarro. High solar activity and absence of major volcanic activity is assumed to be the driver of the observed climate patterns. Here the authors highly rely on the cited modeling studies. The connection between solar activity and changes in the atmospheric circulation need to be better explained to the reader.

According to reviewer 1 (Eduardo Moreno-Chamarro) model simulations with high solar and no volcanic activity do not reproduce the observed changes during the LIA, they just happen to be at high solar but cannot be attributed to this forcing. This has been revised based on the rev1 comments, but in any case our conclusion was to say that the same forcing as during the LIA were responsible for a weak SPG. We now use the recently accepted modeling data in this CP issue to argue on this aspect. The two papers (Yan and Lui 2019 and Ning et al. 2019) hypothesize that internal variability and insolation would have been key drivers of the 4.2 event.

Furthermore, the influence of volcanic activity is only mentioned in the conclusion. I suggest to partially rewrite the abstract, methods and results and discussion chapters under consideration of the points mentioned above as well as the specific comments.

In the revised version of the manuscript we provide the information requested. The role of external forcing has now been further developed taking into account reviewer comments considering the modeling studies performed on the LIA and what is known from the forcing around 4.2 ka (Kobashi et al. 2017). We also put more emphasis on sea ice conditions being different for the two RCCs and the results produced form transient modeling experiments of the 4.2 event (see answer to referee 1).

Specific comments

Please include core KSGC-31 into the introduction and add the missing information to the age methods section.

Done

Introduction Line 55ff. Please include a reconstructed change in the position of the Subtropical Gyre and the westerly wind belt, at the onset of the 4.2 event (Repschläger et al. 2017). The North Atlantic oscillation (NAO) severely influences the modern climate variability in the North Atlantic region. Though NAO typically is changing on interannual time scales which is not preserved in the presented datasets, previous research also refers to more NAO+ and NAO- like phases during the Holocene with a relative NAO- mode during the 4.2 event (Wassenburg et al., 2016). The relation of these modes to the strength of the SPG might add an important point to the discussion.

We did not add this reference as the relation between the NAO mode and the SPG shift described by Repschläger et al. (2017) is not consistent with the reconstruction of Olsen et al. (2012). According to these authors, the southward shift of the STG at the 4.2 ka reflects a strengthening of the westerly wind belt. However, the reconstruction of Olsen et al., 2012 indicate positive NAO till 4500 yr BP and its decrease to a minimum at 4200 yrs BP and sustained weak NAO till 2500 PB, which is not consistent with Repschläger et al. (2017).

Wassenburg et al. (2016) is essentially about early Holocene, thus not relevant of this study.

Methods section Line 80-90: Please add the core position and age model of core KSGC-31 to the description.

Done

Line 97: At the latitude of the coring positions (58 and 66 N), a contribution of the C37:4 component on the alkenones is likely. Thus, the assumption that the proportion of the C37:4 component is neglectable and can be excluded from the temperature calibrations might be violated. Therefore, the use of the Uk'37 index might lead to an overestimation of the reconstructed temperatures. This becomes evident, when comparing the modern SST of 5°C (annual mean temperature) to 8°C (august SST) at the position of core MD99-2275 with core reconstructed top data that indicate temperatures of 9.5°C. I suggest to provide information on the percentage of the C37:4 component and discuss the potential use of the Uk37 index. A comparison of the core top data with the modern hydrography under consideration of the blooming season of the coccolithophores could help on this discussion. This discussion also can partially be put in the supplementary information.

There is no C37:4 in MD95 2015 (no surprise considering the SST values) and trace amounts in some horizons of the MD99-2275 core. Yet, we are not aware of any publication that provided evidence that using Uk'37 leads to an overestimation of SSTs or even improve estimates based on Uk37. Instead Sikes and Sicre (2002) have shown that including C37:4 does not improve SST estimates.

Comparison between MD99-2275 SSTs and instrumental data (Hadley SSTs) was performed and published by Sicre et al. (2011) on this core and shown good agreement between the two dataset for summer SSTs, the season of production of alkenones at high-latitudes. They are also coherent with seawater column data (Sicre et al. G-cubed 2002) and diatom-based reconstructions (Jiang et al., 2015). The same cross-comparison has been conducted off Newfoundland and given consistent results with those at MD99-2275 (Sicre et al., 2014). We do not see the kind of improvement that could be expected from core top comparison, given the match we have already seen between time series and our reconstructions.

Line 98: Please state how the data of core MD95-2015 relate to the published record in Marchal et al., (2002).

The data we present are a completely new record where alkenones have been measured continuously at a 1cm sampling step to resolve decadal time scale.

Line 202 to 205 Changes of 1°C at 4100 and 4300 hardly exceeds error of measurement.

It exceeds internal precision of the method (0.5°C) as stated above. This has been added in the revised version of the manuscript.

Line 221 to 249 Add Wassenburg et al., 2016 to discussion.

Wassenburg et al. (2016) is essentially about early Holocene, thus not relevant of this study.

259 to 260 Though solar activity was discussed in the manuscript, volcanic activities are so far not included into the manuscript.

The issues have been re-discussed in the manuscript.

Technical corrections

Line 14/15: Add resolution of records (e.g. years/centennial scale)

Done

Line 73: “unprecedented high-resolution” Please add resolution of records (e.g. years/centennial scale).

Done

Line 104 to 108: Please revise, parts of the information seem to be duplicated

Done

Line 110: “Populus, Salix... In marine” replace ": : :"

Line 111ff: Please be more precise about the use of the different Pollen as environmental indicators.

We now show the *Fagus*/*Quercus* ratio that better expresses vegetation changes in relation with precipitation and provide a description of this index in the last paragraph of the Material and Method section.

Line 120-121: Please add a more details to the SST data description including SST values in °C.

Done

Line 124 Replace “several” by number of cores and refer to figure 1.

Done

Line 138: “Similar temperature pattern” not clear similar or the same pattern please rephrase sentence.

Done

Line 137-138: “As earlier stressed....” here the reference in you text is not clear, maybe replace by “As emphasized in the introduction”

Done

Line 208: “Pollen data at this site also indicate: : :” Please replace at this site by site number.

Done

Figure 1 Line 528/29 “marine core KSGC-31 (Jalali et al., 2016; this study)” please provide additional information about the core throughout the manuscript

Done

Figure 2 add error bars Line 539 “1σ uncertainty for the 14C dates” Do you mean calibrated 14C ages?

Yes, we mean calibrated 14C dates. Triangles without error bars in Figure 2 indicate the tephras.