Interactive response to Reviewer #2 by Lilian Schuster et al.

We would like to thank the anonymous referee for taking the time to read our manuscript and provide constructive comments which helped us to improve our manuscript. We hope that our response is clarifying and we remain available for further questions.

Here we present a detailed point by point response (the reviewer's comments are given in italics, our answer in normal font). When appropriate, we indicate the text that has been added to the manuscript as a separate paragraph in quotation marks.

General comments:

RC (summary): *The author presents moisture source analysis with a focus on a specific site in Greenland. Using moisture source diagnostics and cluster analysis the sources are explored. The relationship to NAO and sea ice changes are explored. Temporal evolution is also addressed.The analysis of NAO and cluster analysis as well as the arguments for the validity of the PBL assumptions can be strengthened(see below). The paper is a good paper,well written, clear to understand and with sufficient references. There is some work to be done in terms of framing the introduction as a classical science paper but based on the quality of the rest of the paper, this should be no problem to change. I therefore suggest publication with major revision.*

AR: Thank your for your positive evaluation of our manuscript. We hope that our revisions to the manuscript have addressed your comments.

RC: *NAO/cluster analysis It is a nice idea to use cluster analysis for moisture sources. It is unclear whether it is reasonable to assume constant clusters throughout all seasons. It is ok to continue with clusters based on all months, but please document using relevant numbers, that the assumption holds. Since sea ice trends are also explored later, is it reasonable to assume that the clusters are constant in time? Please document why. Can you also add information regarding variability on the relevant timescales?*

AR: Thanks for this comment, which we believe also points out some weaknesses in the first manuscript version, mostly in the explanations of the clustering algorithm.

To find gridpoints that behave similarly, we used the K-means clustering algorithm. We estimated similarities between the gridpoints over the mean annual cycle of relative moisture source contributions, meaning that gridpoints of one cluster have a similar mean annual cycle of relative moisture source contributions. This rules out the possibility to build seasonal clusters (at least with this method), because the clusters are based on seasonality.

We did however try to find other ways to cluster the data. In Fig. RC2 1, we did the K-means clustering by using the features of relative NAO$-$, NAO neutral and NAO$+$. However, the regional clusters that arise are qualitatively harder to identify and interpret, so we preferred to stick to the current method.

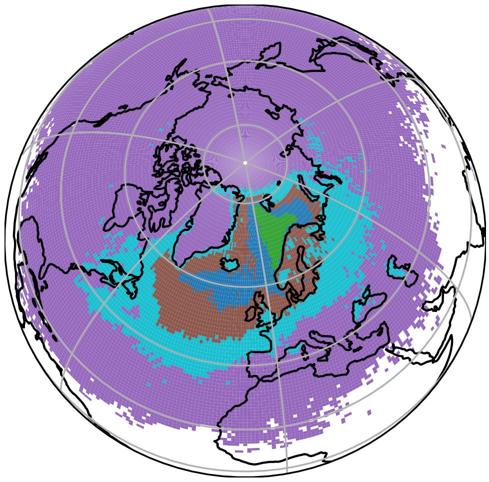

Figure RC2 1: K-means clustering of gridpoints into five clusters grouped after similarities in NAO−, NAO neutral and NAO+

In order to use a clustering algorithm we needed to assume constant clusters as we used the mean relative annual cycle as features. To analyse how the clusters change with time, we did a simple sensitivity check by repeating the clustering over the annual cycle of the relative moisture sources for the first half of the time series and for the second half of the time series. We found that the clustering is nearly identical for the two time series, as only a few of the gridpoints change their cluster between the 1st and 2nd half of the time series (Fig. RC2 2). The K-means clustering should not be over-

Figure RC2 2: The red gridpoints are clustered in a different cluster when comparing the clustering using the mean relative annual moisture source cycle of the first half of the time series against the second half of the time series (for each 230 months)

interpreted, as it was used as a help to construct different regions and to quantitatively describe the features we observed. To clarify the limitations of this clustering approach we added the following into Sect. 5.4:

> " The K-means clustering algorithm was used as a simple tool to construct different regions in which each gridpoint shares a common feature, i.e. the mean annual cycle of relative moisture source contributions. By dividing the time series

in half and repeating the K-means clustering approach for these two time series separately, we found almost no differences in the classification of the clusters (not shown) which gives a hint that the clusters are stable over time. Incorporating a more complex clustering approach that takes the annual cycle, NAO and sea ice into account could, however, give interesting new insights."

RC: *Analysis of NAO variations are also done in this manuscript. It has been documented that NAO is important for southern/central Greenland (Vinther2010, Sodemann 2008). But is NAO really the dominant driver for this given location? Fig 9b is not strongly convincing regarding this. There are a few ways to approach this. Either explore other weather patterns(ScB, EAtl, NAO+/-) (see e.g. Ortega et al 2014) and GBI for summer (Hanna et al. 2015) or use the already analyzed clusters and connecting circulations to document variability. Or come up with a third alternative. No matter what, it is important to argue that the chosen index (e.g. NAO) is indeed a driving circulation pattern for the location.*

AR: Thanks for pointing this out.

The Greenland Blocking Index (GBI) is freely available and allowed us to repeat the NAO analysis with the GBI, see Fig. RC2 3 and Fig. RC2 4. We added the insights about the GBI into Sect. 5.1:

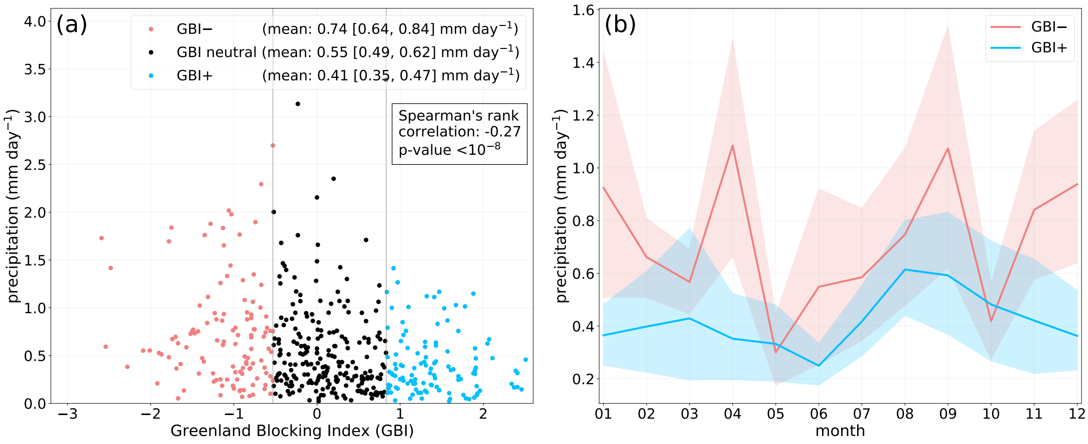

Figure RC2 3: Same as Fig. 9 from the manuscript but with GBI instead of NAO-index: **(a)** Scatterplot of precipitation in study region against the GBI for in total 460 months (February 1979–May 2017). Months were separated into months with GBI below or equal to the 25 $\%$ percentile (GBI$-$), above or equal to the 75 $\%$ percentile (GBI$+$), or in between the 25 $\%$ and 75 $\%$ percentile (GBI neutral). **(b)** Mean annual cycle of precipitation in study region for months with GBI being below or equal to the 25 $\%$ percentile (GBI$-$) and above or equal to the 75 $\%$ percentile (GBI$+$). For each month of the year, month-specific 25 $\%$ and 75 $\%$ percentile thresholds were computed. The shaded areas represent the 95 $\%$ confidence interval of the mean.

"We repeated the analysis of the NAO-index for the Greenland Blocking Index (GBI, dataset from NOAA, 2020 based on Hanna et al. (2016)) that is defined by the mean 500 hPa geopotential height for the 60° N–80° N, 20° W–80° W region (e.g., Hanna et al., 2016). The higher the GBI, the weaker and less variable is the precipitation, specifically in January and April (not shown). Due to the strong negative correlations between the NAO-index and the GBI with Pearson correlation

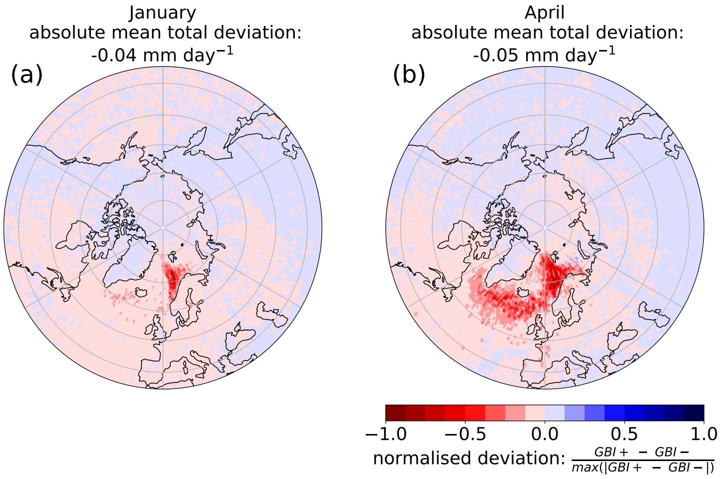

Figure RC2 4: Same as Fig. 11 from the manuscript but with GBI instead of NAO-index: Normalised moisture source deviation between months with GBI being above or equal the 75 % percentile (GBI$+$) and months being below or equal the 75 % percentile (GBI$-$). The same thresholds as in Fig. RC2 3b were chosen. To better compare the moisture source deviations they were normalised by dividing each grid point by the maximum difference between the months with GBI$+$ and the months with GBI$-$, which gives e.g. the gridpoint with the largest positive difference a normalised deviation of 1.

> coefficients of minimum -0.96 in June and maximum -0.74 in December, we get relations between GBI and precipitation or moisture sources that are very similar but reversed to those from the NAO-index, which is in line with Hanna et al. (2016) and Nusbaumer et al. (2019). ”

To our knowledge, there exists no freely available time series dataset of e.g. Scandinavian Blocking/Atlantic Ridge/NAO-/NAO+. Therefore, we would need to classify the weather patterns ourselves, and use daily instead of monthly aggregated data. This would substantially increase the length and complexity of an already long manuscript, and we prefer to argue for a subsequent study (ideally with ERA5 data as pointed out by Reviewer #1). We added the following into Sect. 5.4 to add this limitation of our study and open the way for further research:

> ”Instead of using only the NAO-index or GBI, a more sophisticated classification into 4–7 weather patterns (e.g., Ortega et al., 2014; Grams et al., 2017; Falkena et al., 2020) together with a case study analysis of the pathway of moisture source transport for each weather pattern could be done in a subsequent study to better understand the dominant drivers and sources of precipitation in our study region.”

RC: Introduction: *The introduction is interesting reading, yet not optimal in terms of structure and content for a journal paper introduction. The manuscript is about moisture sources for Greenland, with a specific focus on a single location. Please spend less text on Arctic amplification and instead use the introduction to introduce relevant literature related to this topic. Especially highlight the current knowledge gap and motivate for why your analysis is relevant and explain the regional focus of Greenland. And address why this region of interest for our science community.*

AR: Thanks for this suggestions. We removed some parts of the Arctic amplification and we reformulated other parts to explain better the knowledge gap and to justify further why we chose the specific study region in Northeast Greenland (see Sect. 1).

RC:Uniqueness of the location -state differences and highlight benefits. *The study focuses on a rather specific site on the north eastern margins of the Greenland ice Sheet. The authors refer sufficient to other studies that have explored Greenland moisture sources, but these studies have also addressed that moisture sources are not uniform over Greenland. How this location differs from other findings due to location is under explored in this study. It is encouraged that the authors throughout the text further motivate that their site is beneficial for Greenland studies since it is has a unique location in close proximity to areas of recent strong sea ice decline(as shown in fig 11)*

AR: We added some additional text into the introduction (Sect. 1):

> "The study region in northeast Greenland has also a unique location as it is near to areas in the Greenland Sea of recently strong sea ice loss (specifically in winter, Onarheim et al., 2018; Bliss et al., 2019). "

We also added some text into the discussion part (Sect. 5.2):

> " The study region in northeast Greenland is in close proximity of the Greenland Sea that has lost sea ice rapidly in the last decades, specifically in winter (Onarheim et al., 2018; Bliss et al., 2019). While the Greenland Sea has lost around a third of its initial winter sea ice extent, the Barents Sea has even lost half of their winter sea ice extent (compared to ice conditions of 1979–1989, Stroeve and Notz, 2018). "

It must also be mentioned that the presence of the caves (and their potential paleoclimate archive) still is a strong driver for our study.

Specific comments:

RC: L85 *"...a model without spatial or temporal gaps..."-unclear. Just delete this sentence.*

AR: Agreed. We removed the entire sentence.

RC: L88 *It is ok to use ERA-Interim but try to address this point and potential implications briefly in the discussion rather than here.*

AR: Yes, we added the following into the limitation section (Sect. 5.4):

> "In the moisture source diagnostic either evaporation or precipitation can occur in each time step of 6 h. Therefore, using shorter time steps and a finer grid resolution (e.g. using the ERA5 reanalysis dataset instead of ERA-Interim) could influence the diagnostic."

RC: Sec 2. *The methods described are partly unclear. As a reader without prior knowledge of* *back trajectories, the description of how ERA-Interim and moisture source diagnostics works together is unclear. Please clearly state this and maybe consider reorganization of this section.*

AR: Thanks for pointing this out. The 15 days backward trajectory calculations are based on the 3D windfield of ERA-Interim. Additionally, fields such as the specific humidity, 2m-Temp and the PBL height are needed from ERA-Interim as well to apply the moisture source detection method. In our manuscript we mention this in Sect. 2.1. We have also added the following to the manuscript to clarify the link between ERA-Interim and the moisture source diagnostic (Sect. 2.2):

> ... the trajectory calculations are ... "based on the ERA-Interim dataset".

RC: L110 *"In the next step..."The next step of what? Are you here referring to the moisture source diagnostics?*

AR: Yes, this was a bit confusing. We meant with "in the next step" just the working step that is done afterwards and did not mean the 'time step' from the sentence beforehand. We replaced it by "Afterwards, " to clarify that.

RC: L115 *–Moisture uptakes above PBL. Be clear about why this is neglected–does Sodemann2008 argue for this?*

AR: Yes, Sodemann et al. (2008a) argue for using only the below PBL height moisture uptake as evaporation source. We have clarified more in the text that we follow the Sodemann et al. (2008a) approach (see Sect. 2.2):

> "According to Sodemann et al. (2008a), ..."

Above the PBL one can not assume anymore that the air is well-mixed, so the moisture uptake there can not be directly assumed to be a result of evaporation occurring at the surface (see Sect. 2.2). There are studies which apply the same methodology and consider moisture uptake in the free atmosphere as well (e.g., Baker et al., 2015; Fremme and Sodemann, 2019; Hu et al., 2020), however it is unclear how they justify this approach (see Sect. 5.4). An other study uses the same approach but does not mention the detection efficiency (Bohlinger et al., 2017).

RC: L121-122 *The wording of why to choose Lagrangian rather than Eulerian methods is not optimal hereafter you have already described that you use Lagrangian methods. Please rewrite or replace.*

AR: Thanks. We removed this sentence from the methods into the limitation part (see Sect. 5.4):

> "For this climatological study, we had do use a lagrangian approach instead of a more comprehensive Eulerian tagging approach to keep the computational costs low. Although the case study of Winschall et al. (2014) found similar moisture

> source regions for both approaches," a direct comparison to other models would be necessary ...

RC: L123 -126 *Please clearly describe, wherein your methods do you lift the PBL height by 1.5? Is this done on ERA-interim data or in the moisture diagnostic? To the best of my knowledge this is have not been done for the North Atlantic before. Is the PBL height also underestimated in this region, please argue with references and please argue with numbers, what difference this have for the amount of moisture uptake in this study? As a reader I would like to be convinced that this approach is better than the existing ones, and also clearly know what differences this makes compared earlier studies such as Sodemann 2008. Please clearly state this.*

AR: The PBL scaling approach is suggested by Sodemann et al. (2008a), maybe the text was not clear on this point. They also lifted the PBL by 1.5 to account for the underestimation of the marine PBL height and its small-scale variability. So, instead of using the PBL as it comes out of ERA-Interim, we use instead 1.5xPBL as threshold for moisture uptake. The exact same PBL scaling procedure has also been done e.g. in Sodemann and Zubler (2010); Winschall et al. (2014); Langhamer et al. (2018). We added some explanations of Sodemann et al. (2008a) into the manuscript at Sect. 2.2:

> "marine PBL height can vary on small scales"

and clarified that the scaling was done

> "same as in Sodemann et al. (2008a)" .

RC: L127 *–"A measure for performance of the method" Is fig 3 a measure for performance? Do we trust that moisture sources and their variability are adequate after?*

AR: Yes, it is true that Fig. 3 is no real measure for performance (there is no way to truly verify the methodology) and we removed this sentence (see Sect. 2.2). Fig. 3 rather describes the detection efficiency variability. There is no dependence of this efficiency with the amount of monthly mean precipitation visible. We added the following into Sect. 2.2 to clarify this:

> ... "with the detection efficiency being roughly independent of the monthly precipitation" ...

The performance analysis of the lagrangian moisture source diagnostic in general is given by Winschall et al. (2014) (see Sect. 5.4).

RC: L129 *52% is moisture uptake that is not accounted for? This seems like a lot. Great that this is also treated later.But what effects have 1.5 PBL on this fraction? What is the argument for only including moisture uptake below PBL rather than all moisture uptake? Please demonstrate with relevant numbers that this assumption does not strongly influence the results.*

AR: Yes, only accounting for 48% of moisture uptake is weak but, as discussed in the paper, Sodemann and Zubler (2010) had similar numbers (50%). We still believe that using the moisture uptake above the scaled PBL of a grid point would lead to "better results for the wrong reasons", and does not represent well the underlying evaporation at the surface as the atmosphere can not be assumed to be mixed above the PBL (see Sect. 2.2 and Sect. 5.4). In the case of the southern Patagonian ice field, 15 % of moisture sources are betweeen 1xPBL and 1.5xPBL (personal communication with Lukas Langhamer). A comparison study that analyses the pros and cons of this threshold and the relevance of scaling the PBL height would be necessary but we argue that this should to be done in a subsequent study (described in Sect. 5.4). Furthermore, the fact that we observe a high correlation between precipitation amounts and accounted precipitation is an indicator that our variability analyses are robust.

***RC: L144** Generally,there is an issue here that (at least the DJF) NAO index values for the ERA-interim are not evenly distributed. This is just nature, but please add numbers to describe the ranges for NAO+, NAO-and NAO neutral.*

AR: The way we defined the monthly thresholds to distinguish between NAO$+/-$ was such that for each month of the year, we have the same amount of entries for NAO$+$ as for NAO$-$ (see Sect. 2.3). Therefore, we used a classification that differs between years below the NAO 25% percentile and above the 75% percentile. So, the NAO-index threshold is different for each month. While the NAO 25%/75% threshold is [-0.67, 0.77] for all months together, it is e.g. for August [-1.17,0.67], for December [-0.46, 0.84], for April [-0.63, 1.01] and for May [-0.92, 0.57]. We included these numbers into the text in Sect. 2.3 to clarify that.

***RC: L163-168** If you mention this –please relate to your findings and the site of this study. The site is so north that it is only partly close to the North Atlantic storm track. But what does fig 4 show?*

AR: It is correct that our study region is not so affected by the cyclone track, also reflected by the annual cycle of precipitation in ERA-Interim. Therefore, we deleted these two sentences.

***RC: L167-168** Is this expected to be the case for your site? What role does increased moisture content in the air due to warmer temperatures play relative to this?*

AR: Yes, it is true that in our study region the contrast between snow-free land and snow areas might not be the reason for more precipitation occurring in summer, so we removed this sentence.

***RC: L170** Is precipitation really constant during the year? The accounted precipitation is, but not the ERA-interim precipitation. Spring/summer is clearly lower than the rest of the year. E.g. July median is roughly 3 times lower than September*

AR: Yes, you are right. We removed the first part of the sentence (...While precipitation amounts are relatively constant during the year...) that was in Sect. 3.2 to clarify that.

RC: L173. *How do you define "local sources" -please describe?*

AR: We meant with local sources the moisture source from the study region or its direct surrounding and added that into the text in Sect. 3.2:

> ... local moisture sources "(from the study region or its direct surrounding)"

RC: L170-179 *Avoid the use of the word "seem". Either the sources are there or they are not.*

AR: Thanks, we changed this as suggested.

RC: Fig 5 *There is too large of a region on the globe which is white. This makes it more difficult to look at the plot. Consider a different projection(e.g. polar stereographic) where North Atlantic/Arctic is emphasized (and apply to other relevant plots such as fig. 6). Also, fig 5 contours of z500 could be closer so it is easy to distinguish differences from month to month.*

AR: Thanks for pointing this out. We switched to the North polar stereographic projection (on Fig. 5 and Fig. 11). We chose a minimum latitude threshold of 35° N as there are less than 3% of the moisture sources further southward. For the legends of Fig. 7 and Fig. 8, we preferred the orthographic projections to show the full extent of the clusters. As suggested, we increased the amount of contour lines (every 50 m, but labelling only every 100 m). We hope that this makes it easier to distinguish the differences in the geopotential height from month to month.

RC:Fig. 6 *–The colors on this plot looks very similar blue-toned with little contrast on my(and maybe others) printer. Please change to a color scale with stronger contrasts.The signs and meaning on the legend for 15 and 30 IVT are unclear in figure and caption, please improve this. Please add a comment on an uneven color scale on this.*

AR: Thanks for recognizing that. We increased the contrast by using a colormap that also changes colors ('viridis'- colormap of matplotlib). In addition, we improved the color scale, noted the uneven color scale in the caption and increased a bit the arrow length and width.

RC: Fig 6 *Is figure of anomalies a better way to display this?*

AR: We have thought about this as well. However, in this case, the reader has to be familiar with the mean IVT to understand their anomalies. Therefore we thought it would be misleading the readers' interpretation and decided to not plot the anomalies.

RC: L211-217 *This section here is unclear. Please reformulate. Are sea ice areas defined as* *constants throughout the year or are they changing months by months?*

AR: Thanks for pointing this out. We used the sea ice concentration time series of each gridpoint from February 1979 – May 2017. We clarified this by adding the following sentence into the caption of Fig. 7d:

> ”The values for the sea ice concentration and hence the sea ice areas change throughout the months and years.”

We also added a sentence into the text in Sect. 3.2.1:

> Note that, sea ice areas as defined in Fig. 7d (gridpoints with sea ice concentration$\geq$0.5) change from month to month.

***RC: L278ff** This analysis is interesting and relevant, but the method and text are a bit unclear. Please reformulate to enhance clarity and clearly state relevant findings.*

AR: Thanks for pointing this out. We hope that Sect. 4.2 got clearer by reshaping the text, adding the R^2-values of the found correlations

> ”increasing precipitation in autumn and winter for decreasing maximum sea ice (R^2-values of 0.14 and 0.13)”

and adding the following to the manuscript:

> ”The moisture uptake region *3O* (mainly Norwegian Sea) is one of the major moisture source contributors, however it is also mostly sea ice-free which might explain why we could not find strong relations between precipitation in the study region and Arctic sea ice area.”

***RC: Sec 6 (Conclusion)** The content of the conclusion is unclear and unprecise and does currently not let the key method sand findings of the manuscript stand out. This will improve the many “skim-readers”understanding of the paper strongly. Please improve and address all key components of the manuscript*

AR: We added some additional information about the used methods (lagrangian moisture source diagnostic and K-means clustering approach) into the conclusions (Sect. 6):

> ” We used the lagrangian moisture source diagnostic of Sodemann et al. (2008a) to estimate the origin of water vapour for precipitation over the study region. We applied a classification algorithm (K-means clustering) to group gridpoints into clusters after their similarities based on the annual cycle of relative moisture source contributions. ”

We also reference to the corresponding figures and added some additional quantities:

> maximum over the Norwegian Sea ”(30 % in the mean in January ”...
> ... ”with the exception of autumn where precipitation increases by 8.2 [0.8, 15.5] mm dec^{-1} over the period” ...

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