Author Response: We thank Matthieu for a thorough and thoughtful review of the manuscript. In response to the suggestions from the reviewer, we propose several key changes to the manuscript that will improve the final version while still retaining much of the work already presented here:

1. We will streamline the analyses included in sections 3.1-3.5 to ensure that:
    a. A single model output is utilized for all calculations (ERA-5);
    b. We will remove the discussion around condensation temperature and focus on the combined effects of precipitation intermittency and surface temperature;
2. We will expand on section 3.6 (Mount Brown South water isotope record) in order to dig more deeply into the impacts of precipitation intermittency on the isotope record at this site
    a. We will remove a discussion around d_{ln}, as this does not enhance the text or help to understand the impacts of precipitation intermittency on the stable isotope record;
    b. We will include a discussion on stratigraphic noise, including estimations of the impact of stratigraphic noise at this location based on the two water isotope records available (MBS-Main and MBS-Charlie);
    c. We will better interrogate the relationship between precipitation intermittency and $\delta^{18}O$ using the methods suggested by the reviewer (compare the temperature record to the temperature record with precipitation intermittency, and also with precipitation intermittency but excluding the EPE events);
    d. If possible, we will support the results with an investigation into modelled results where precipitation intermittency is both included and excluded from modelled isotopic values.

We feel that these changes will address the reviewers primary concern (that the manuscript illustrates the impacts of precipitation intermittency without fully understanding it) and result in a much stronger final study.

Review of Climatology of the Mount Brown South ice core site in East Antarctica: implications for the interpretation of a water isotope record

The manuscript describes the climatic conditions at the Mount Brown South ice core drilling site in Antarctica, and the implications for the interpretation of the isotopic composition as a paleoclimatic record. The manuscript combines observations, reanalysis, and back-trajectories analyses to evaluate the contribution of warm synoptic events to the signal. Specifically, the authors address the global contribution of extreme precipitation events, a subset of events which accounts for the largest total precipitation, which includes atmospheric rivers. The results are compared to the upper part of the main ice core from Mount Brown South, as well as from a short replicate core.

Overall, such studies evaluating the impact of the local climatic conditions to the isotopic signal stored in the ice core should be necessary preamble approaches to the interpretation of the isotopic paleothermometer. The authors combine a large range of polar meteorology tools to constrain the impact of precipitation intermittency and seasonality which are two limiting factors to the interpretation of the water isotopes at high resolution. The methodology is appropriate; the figures convey the necessary information. Overall, the manuscript is well written but I believe the following points should be taken into account before it is considered for publication.

General comments:
I acknowledge that this is already a very dense manuscript, which includes results ranging across disciplines and provide an in-depth study of the impact of the signal of the largest precipitation events imprinted in the ice core. In my opinion, the manuscript presently falls just short of achieving its goal, i.e. “understand how precipitation intermittency impacts the temperature records preserved in an East Antarctic ice core” (first sentence of the conclusion), because the pertinence of the results for signal analysis is not sufficiently discussed. To move

beyond *illustrating* the impacts of precipitation intermittency on the signal, and really *understand* them, I would suggest including the following discussion elements:

- While it's true that precipitation intermittency, and in particular the fact that synoptic events are associated with warmer than conditions (and thus a warm offset), for the interpretation of the isotopic records, which are always given as an anomaly, it does not matter. The most important question is for each time scale, if the amplitude of the variations of isotopic composition and of temperature can be scaled by the same factor. Section 3.6.2 which evaluates the correlation between isotopic composition and temperature is disconnected from the rest of the studies which address the EPE influence. At this point, new calculations evaluating the modelled isotopic signal with and without the contribution of the EPEs might be out of the framework of the manuscript, but discussing the influence of these large events, building up on the results from (Sime et al., 2009), (Casado et al., 2020), and (Münch et al., 2021) would support the main goal of the manuscript,

Author response: *While in principle we agree that a bias does not matter to an anomaly measurement, we find that the magnitude of this bias is dependent on the proportion of accumulation derived from EPEs, in which case we find that the warm bias can vary substantially from year-to-year. However, as highlighted by the reviewer, we have not considered the influence of stratigraphic noise, which would act as an additional filter, and reduce the inter-annual variability the magnitude of the warm bias from precipitation intermittency. In the revised manuscript, we will address the impacts of stratigraphic noise, which will result in a re-assessment of the conclusions of section 3.6.2 and build upon the previous results (Sime et al., 2009; Casado et al., 2020; Münch et al., 2021);*

- The impact of stratigraphic noise is completely ignored (Ekaykin et al., 2004; Fisher et al., 1985; Münch et al., 2016; Petit et al., 1982), and since it is similar than precipitation intermittency, it needs to be at least discussed. Specifically, here, the distance between the cores will be key to evaluate if the stratigraphic noise will affect the two cores the same way, or not, both in term of dating uncertainty, and of noise added to the isotopic signal.

Author response: *We acknowledge that stratigraphic noise is an important contributor to the water isotope record which can further mask the underlying climate signal preserved in the ice core record. As we have two closely-situated cores, it is possible to estimate the impact of stratigraphic noise at our site and we will add a section discussing this to the manuscript. We similarly will include an estimation of the signal-to-noise ratio based on the two isotope records we have available, and a discussion around how this impacts the temporal resolution of a climatic signal resolvable within the core.*

- Overall, the use of the condensation temperature here is not based on physics, since the temperature time series are simply compared with the isotopic records. Since the condensation temperature is an artificial temperature retrieved from the model at a variable location, which is solely based on the pressure level, and not on where the model is predicting that the precipitation takes place, it does not bring any added value to you. How are the results of Section 3.6 for precipitation weighted surface temperature? While I agree that in principle, the fractionation takes place way above the surface when the moisture condensates, the signal is actually only weakly affected by the local fractionation coefficient sensitivity to temperature when you actually calculate a Rayleigh distillation, and the link between temperature and isotopic composition is acquired by integrating the whole distillation (Rayleigh, 1902).

Author response: *We agree that the discussion of condensation temperature within the text is not supported by a clear physical mechanism. Instead we propose to re-do calculations for surface temperatures, and present only a discussion on surface temperatures within the text.*

- At the end, section 3.6 is disconnected from the section 3.1 to 3.5, even though, this is the important aspect. How much of the results of section 3.6 can you explain from the EPE? Can you compare the temperature record to the temperature record with precipitation intermittency, and also with precipitation intermittency but excluding the EPE events? What does it teach you for the interpretation of the d18O of the MBS core?

Author response: *We agree that section 3.6 is somewhat disconnected from the previous sections, and we will include the suggested comparisons in order to further interrogate the data in order to better understand the true impacts of precipitation intermittency at this site (rather than simply illustrating them, as highlighted by the reviewer at the beginning).*

Specific comments:
Line 59: The following studies should be cited here (Sime et al., 2009), (Casado et al., 2020), and (Münch et al., 2021).
Author response: *We will include a discussion and citation of the listed studies here.*

Lines 64-65, and then later on, 83-84: the percentiles at which the EPE are defined should be consistent throughout the manuscript. Either, it needs to be based on other studies (such as the Wille et al, 2021), and then defined just once in the introduction, or, if you chose to apply your own threshold, it needs to be defined in the methods, sensitivity tests need to be applied (more cumbersome).
Author response: *We will use a clear and consistent definition of EPEs through the study (which we will update as the 95th percentile to retain consistency with previous studies).*

Lines 162 – 164: Is $_{17}$O-excess actually used in this paper? If it isn't this is unnecessary information to add here.
Author response: *This will be removed from the manuscript as 17O-excess is not discussed further.*

Section 2.2: It is not clear to me why the EPEs are characterised with RACMO and the temperatures, geopotential,… are extracted from ERA5. The dataset (RACMO, ERA5, or other) should here be only a tool used to apply the method (such as the routine from Turner et al, 2019 to identify EPEs), and ideally, it should be reproducible with both tools. Using both datasets as a gridded meteorological data input is not necessarily bad, if both are used completely for all diagnoses, and if both datasets are used for sensitivity tests, but here, it raises the question whether the results can be reproduced using one single dataset only. I believe that a coherent ensemble is more important than "direct comparisons with previous work".
Author response: *We agree that the use of combined datasets is an ineffective, and potentially misleading, way to do these analyses. We will re-do the analyses using a single gridded product (ERA-5)*

Lines 259 to 261: "Here, we use HySPLIT to generate 5-day back-trajectories (120 hours), originating at the MBS site at a height of 1500 m above ground level, which is equivalent to approximately 3500 m above sea level."
Here, you evaluate the formation of precipitation at 1500 m. agl. Later on, the condensation temperature is defined as the 650mbar pressure level (line 554). Is this coherent?
Line 317 (and several times later on): if the error bar is larger than one, the '.2' is not a significant figure.
Author response: *Thank you, we will update errors to reflect this throughout.*

Line 549 to 555: "In Antarctica, a strong inversion layer is present over much of the ice sheet meaning that surface temperature and condensation temperature (above the inversion layer) are not always directly related (Jouzel and Merlivat, 1984). Water isotope records are directly dependent on condensation temperatures rather than surface air temperature. We therefore calculated the temperature anomaly relative to the 30-day seasonal mean temperature for all EPE days identified by RACMO2.3p2 using both surface (t2m) and condensation temperature (approximated at the 650 hPa pressure level) data from ERA-5 (see section 2.2.2).".
This is not exactly true. The local fractionation coefficient is related to the local condensation temperature (Jouzel and Merlivat, 1984), not the water isotopic composition which integrates all the successive fractionation coefficients throughout the Rayleigh distillation (Ciais and Jouzel, 1994; Rayleigh, 1902; Schoenemann et al., 2014). As a result, the most important factor is the rainout fraction (which follows Clausius Clapeyron's law, and thus, is a logarithmic ratio of the local to source temperature). Overall, the fractionation coefficient variation with temperature is only a second order parameter (Bailey et al., 2019). Taking into account the surface temperature or the condensation temperature is then just a convention at this point.
Author response: *We thank the reviewer for this clarification and discussion. As previously discussed, we agree that the use of condensation temperature throughout this manuscript has been poorly handled and we will instead discuss only the relationship of the isotopic record to surface temperature throughout. While this again oversimplifies the controls on the isotopic composition, we feel (as highlighted by the reviewer) that this will be a clearer way to improve understanding of the isotopic record, and in particular provide a consistent approach to understanding how precipitation intermittency and local temperature influence the isotopic record.*

Section 3.5.1: The warm bias induced by the correlation between strong precipitation events and temperature was already used in (Sime et al., 2009), (Persson et al., 2011), and (Casado et al., 2018). The proposed strategy could help enhanced the discussion. Since the purpose is to apply these results to the interpretation of the isotopic signal, it would make sense to evaluate if this is only going to be a bias without any frequency dependency or if this create a signal that affects particularly the high frequency variability of the signal. A bias does not matter for the interpretation of the paleothermometer because only the anomalies are studied.

Author response: *Given that this study forms the basis of understanding a 1200-year climate record, we agree understanding the frequency-dependence of the bias is an important aspect of this study which has been ignored. We will include a discussion around the frequency-dependence of the bias, with consideration to the results presented in the aforementioned studies.*

Lines 622: The use of deuterium excess here appears superfluous and raises more questions than answers: (i) why is the dexcess introduced here, near the end of the Results and Discussion sections, and not in the introduction or in the methods ? (ii) why is the dexcess introduced at all since it's not used in the manuscript ? (the correlation will temperature will be discussed later on) (iii) the non-linearity of the relationship between d18O and dD will not appear on a single site with small variations of temperature, so why is the manuscript using a non-classical definition of dexcess ? (iv) and, in turn, what is the classical d-excess definition looking like? Both definitions have positive and negative aspects, but here, it is not clear what is the benefit of using this definition. Overall, I would recommend to remove the dexcess completely from the manuscript, and focus more on the link between d18O and accumulation, but if you decide to keep the dexcess, it would be necessary to explain why this specific definition is used here, even though, the classical definition has been shown "be more problematic at sites further inland than MBS", and also (and maybe more importantly), at much larger time scales which the variations of temperature were much larger.
Author response: *We agree that a discussion on d-excess/dln does not provide any additional understanding of the relationship between precipitation intermittency and the water isotope record, and will be removed from the manuscript.*

Lines 640: There is no discussion on the stratigraphic noise, and if this could explain why the two cores have weak correlation.
Author response: *A discussion on stratigraphic noise will be included in the revised manuscript.*

Lines 665: Dating ice cores using the water isotopes has been already proven to lead to high errors when the signal to noise ratio is low (Laepple et al., 2018), which could be discussed here.
Author response: *Dating of the core was done primarily using species with clear seasonality (nssSO_4^{2-}, Na^+, SO_4^{2-}/Cl^-, with the Pinatuba eruption (identified as a peak in nssSO_4^{2-}) used as a tie point between records (section 2.1.3), with water isotopes used as a secondary dating tool. However, we realise that this sentence (line 665) suggest that $\delta^{18}O$ was the primary dating tool. We will re-write this to clarify that there is weak cyclicity across all of the species used for dating during this period, including$\delta^{18}O$.*

Lines 671 to 677: What is the conclusion from the piece of information? The unexpected correlation between dexcess and temperature could be link with three possible explanations: (i) the definition of dexcess is not adapted and a lot of the 1st order (d18O) signal remains in the dataset, (ii) it is actually a source temperature signal, but condensation and source temperature are correlated and this explains this link, or (iii), this is a complete random correlation (according to the pvalue, there is 0.7% chance of this happening).
Since there is a strong positive correlation between the dln and d18O in figure 7, it seems likely that the use of dexcess here (using either the classical or log definition) is not bringing additional information, but this should be tested if the dexcess is kept in the manuscript.
Author response: *We agree that here (and throughout the manuscript) the inclusion of dln/dxs is superfluous and does not enhance the text – nor is there a physical reason that we might expect to see a relationship between dln and the local temperature. As such, we believe that the text will be enhanced and streamlined by removal of discussions surrounding dln and will focus solely on the relationship between d18O and site temperature.*

Line 696 to 697: The error bars appear very small considering the relatively low coefficient correlation (Higbie, 1991), I would suggest rather 0.2 and 0.3 respectively. A simple test can be to compare the slope of d18O with the temperature to the inverse of the slope of the temperature to d18O (which should be equal normally), but sometimes offer difference which can be used to evaluate the error bars.
Author response: *We will re-asses the error bars associated with the calculated slopes and perform the suggested test to ensure these are presented accurately.*

Lines 696 compared to the previous paragraph: it seems here that you are comparing the condensation temperature to isotope slopes from your results to mostly surface temperature datasets. How is this affecting your results? Does using the condensation temperature really help the study? Overall, the compared data should be coherent, especially because variations of surface temperature are not scaled one to one with condensation temperature.

Author response: *Again, we agree that the use of condensation temperatures here (and throughout the text) is not beneficial to the study and we will re-do calculations to consider surface temperatures instead.*

Bibliography

Bailey, A., Singh, H. K. A. and Nusbaumer, J.: Evaluating a Moist Isentropic Framework for Poleward Moisture Transport: Implications for Water Isotopes Over Antarctica, Geophys. Res. Lett., 46(13), 7819–7827, doi:https://doi.org/10.1029/2019GL082965, 2019.

Casado, M., Landais, A., Picard, G., Münch, T., Laepple, T., Stenni, B., Dreossi, G., Ekaykin, A., Arnaud, L., Genthon, C., Touzeau, A., Masson-Delmotte, V. and Jouzel, J.: Archival processes of the water stable isotope signal in East Antarctic ice cores, Cryosph., 12(5), 1745–1766, doi:10.5194/tc-12-1745-2018, 2018.

Casado, M., Münch, T. and Laepple, T.: Climatic information archived in ice cores: impact of intermittency and diffusion on the recorded isotopic signal in Antarctica, Clim. Past, 16(4), 1581–1598, doi:10.5194/cp-16-1581-2020, 2020.

Ciais, P. and Jouzel, J.: Deuterium and oxygen 18 in precipitation: Isotopic model, including mixed cloud processes, J. Geophys. Res. Atmos., 99(D8), 16793–16803, doi:10.1029/94JD00412, 1994.

Ekaykin, A. A., Lipenkov, V. Y., Kuzmina, I. N., Petit, J. R., Masson-Delmotte, V. and Johnsen, S. J.: The changes in isotope composition and accumulation of snow at Vostok station, East Antarctica, over the past 200 years, Ann. Glaciol., 39(1), 569–575, doi:10.3189/172756404781814348, 2004.

Fisher, D. A., Reeh, N. and Clausen, H. B.: Stratigraphic noise in time series derived from ice cores, Ann. Glaciol, 7, 76–83, 1985.

Higbie, J.: Uncertainty in the linear regression slope, Am. J. Phys., 59(2), 184–185, doi:10.1119/1.16607, 1991.

Jouzel, J. and Merlivat, L.: Deuterium and oxygen 18 in precipitation: Modeling of the isotopic effects during snow formation, J. Geophys. Res. Atmos., 89(D7), 11749–11757, doi:10.1029/JD089iD07p11749, 1984.

Laepple, T., Münch, T., Casado, M., Hoerhold, M., Landais, A. and Kipfstuhl, S.: On the similarity and apparent cycles of isotopic variations in East Antarctic snow pits, Cryosph., 12(1), 169–187, doi:10.5194/tc-12-169-2018, 2018.

Münch, T., Kipfstuhl, S., Freitag, J., Meyer, H. and Laepple, T.: Regional climate signal vs. local noise: a two-dimensional view of water isotopes in Antarctic firn at Kohnen Station, Dronning Maud Land, Clim. Past, 12(7), 1565–1581, doi:10.5194/cp-12-1565-2016, 2016.

Münch, T., Werner, M. and Laepple, T.: How precipitation intermittency sets an optimal sampling distance for temperature reconstructions from Antarctic ice cores, Clim. Past, 17(4), 1587–1605, doi:10.5194/cp-17-1587-2021, 2021.

Persson, A., Langen, P. L., Ditlevsen, P. and Vinther, B. M.: The influence of precipitation weighting on interannual variability of stable water isotopes in Greenland, J. Geophys. Res. Atmos., 116(D20), 2011.

Petit, R., Jouzel, J., Pourchet, M. and Merlivat, L.: A detailed study of snow accumulation and stable isotope content in Dome C (Antarctica), J. Geophys. Res. Ocean., 87(C6), 4301–4308, doi:10.1029/JC087iC06p04301, 1982.

Rayleigh, Lord: LIX. On the distillation of binary mixtures, Philos. Mag. Ser. 6, 4(23), 521–537, doi:10.1080/14786440209462876, 1902.

Schoenemann, S. W., Steig, E. J., Ding, Q., Markle, B. R. and Schauer, A. J.: Triple water-isotopologue record from WAIS Divide, Antarctica: Controls on glacial-interglacial changes in 17Oexcess of precipitation, J. Geophys. Res. Atmos., 119(14), 8741–8763, doi:10.1002/2014JD021770, 2014.

Sime, L. C., Marshall, G. J., Mulvaney, R. and Thomas, E. R.: Interpreting temperature information from ice cores along the Antarctic Peninsula: ERA40 analysis, Geophys. Res. Lett., 36(18), 2009.