Review of Decoupling of δ18O from surface temperature in Antarctica in an ensemble of Historical simulations

The manuscript employs simulations generated by an isotope-enabled General Circulation Model (GCM) to assess the temporal variations in isotopic composition, surface temperature, precipitation, and sea ice concentration within Antarctica over the past 200 years. Specifically, they suggest that differences between the simulated isotopic composition and temperature variations can be attributed to change of precipitation patterns due to the impact of sea ice concentration on the moisture pathways toward Antarctica.

The topic addressed by the manuscript is extremely important because the accuracy of the isotopic paleothermometer is directly affected by the link between isotopic composition in ice cores and temperature, which is only constrained empirically. Models are invaluable tools to study and explore the relationship between isotopic composition and temperature spatially, temporally, and with the different time scales. Here, the study based on the outputs of a single model (with occasional comparison with results from other studies using another one) lacks robustness to provide concrete evidence that can help strengthen our understanding of the isotopic paleothermometer. While the study suggested by the authors is worth pursuing, several shortcomings weaken what could otherwise be an important study for the field.

General comments:

The manuscript predominantly relies on the outcomes of multiple runs from the HadCM3 model. Although the authors note in the Method section that 'HadCM3 provides a reasonable representation of Antarctic Climate and δ18O,' there is a notable absence of a critical evaluation of the model's performance in comparison to available observations in Antarctica. The manuscript does not provide values indicating potential biases or errors in the model. It is challenging to assess the trustworthiness of the model outputs without a direct comparison with present-day observational data in HadCM3 iso, similar to the approach in Figure 1 of (Werner et al., 2018). This becomes more significant as the manuscript critiques observations without addressing potential biases in the studied model, even though the IPCC specifically relies on a diverse ensemble of models to balance potential biases. While a few articles are referenced (specifically (Holloway et al., 2016; Tindall et al., 2009)) that offer some comparisons with discrete values, the absence of confidence intervals makes it difficult to evaluate the robustness of the relationship between isotopic composition and temperature. This limitation affects the confidence in the authors' results, particularly given the extensive literature containing data that could be directly compared with the model outputs for temperature data (Jones et al., 2018)(Jones et al., 2018), isotopic composition (Dittmann et al., 2016; Ekaykin et al., 2002; Landais et al., 2017; Masson-Delmotte et al., 2008; Schlosser et al., 2004; Stenni et al., 2016; Touzeau et al., 2016), NSIDC products for the sea ice…

Some of the results are compared with outputs from another General Circulation Model (GCM), specifically ECHAM5wiso, to assess 'model dependency.' This comparison reveals substantial differences, with predicted slopes between isotopic composition and temperature regionally or across all of Antarctica showing almost a 100% disparity. These findings contribute to a diminishing confidence in the outputs of the HadCM3 model. It's worth noting that the comparison involves an older version of ECHAM with isotopes (ECHAM6wiso, released in 2021, has been demonstrated to be more accurate according to (Cauquoin and Werner, 2021)). Additionally, ECHAM5wiso was nudged with ERA40, a choice that may seem unconventional given the availability of more recent products like ERA-interim in 2006 (Dee et al., 2011) and ERA5 in 2018 (Hersbach et al., 2020).

Overall, the manuscript does not engage in a critical discussion regarding the capabilities of GCMs (CMIP5 or CMIP6) to accurately depict regional-scale climate dynamics. This is noteworthy given the existing body of literature highlighting that the simulated variability often underestimates (Laepple and Huybers, 2014; Shao and Ditlevsen, 2016; Zhu et al., 2019). It would be beneficial for the manuscript to address the impact of discrepancies between modelling outputs and observations within this context. A thoughtful consideration of this aspect would enhance the argument and bolster confidence in the assertion that the results from HadCM3 are reliable in this context.

The comparison with observational products seems to selectively draw from publications that align with the authors' findings, potentially overlooking contributions that present contrasting results (Clem et al., 2020; Jones et al., 2019) or dismissing them without explicit justification ((Casado et al., 2023), in which I was involved as a lead author). There are several instances detailed below in the specific comments when the authors obtained different results using the same calculation (trends from linear regression) and presumably the same datasets (PAGES 2k) and obtain different results without providing an explanation. Notably, in (Casado et al., 2023), considerable effort was invested in validating trend calculations using the products from (Stenni et al., 2017). It would be valuable for the manuscript to articulate why different trends are identified here, especially as the current narrative suggests a potential error in (Casado et al., 2023). While the scientific method encourages revisiting and improving upon previous results, the manuscript should provide evidence-based arguments when challenging peer-reviewed scientific findings, moving beyond statements like 'It is unclear why the trend in (Casado et al., 2023) is higher."

In Section 5 (*Drivers of $\delta^{18}O$ changes*), there appears to be a lack of evidence supporting the hypothesis regarding the mechanisms behind $\delta^{18}O$ variations. It is commonly acknowledged that correlation does not necessarily imply causation. In this context, the connections between $\delta^{18}O$ and other climatic parameters are not based on correlation but on the representation of end-members (warmest and coldest conditions) on a map. The linkage between $\delta^{18}O$ and temperature, specifically attributing the decoupling to sea ice concentration as depicted in Figures 4 and 5, is unclear. Notably, the authors seem to have overlooked the potential impact of the SAM, which has been suggested to influence both isotopic composition (Kino et al., 2021) and sea ice concentration around Antarctica (Eayrs et al., 2021).

Specific comments:

Lines 5 to 6: "Our ensemble captures observed historical SAT and precipitation trends, and weak δ18O trends." Currently, this statement is not supported within this manuscript. It does conflict with recent publications on the topic (Casado et al., 2023; Clem et al., 2020; Jones et al., 2019) without clear justifications for considering this manuscript's results as more valid than those published in peer-reviewed journals

Lines 6 to 7: "The weak δ18O trends mean there is no significant relationship between SAT and δ18O over one third of Antarctica, and also half of our considered ice core sites, though relationships are stronger when using regional averages."
While it can be debated, it's important to note that in Antarctica, every site where precipitation isotopic composition has been sampled demonstrates a remarkably robust correlation with temperature (Dittmann et al., 2016; Fujita and Abe, 2006; Landais et al., 2012; Schlosser et al., 2004; Stenni et al., 2016; Touzeau et al., 2016). Any counterargument should be accompanied by evidence explaining why the findings in this manuscript should be considered more valid than those published in peer-reviewed journals.

Lines 19 to 21: "The collapse of Antarctic ice shelves have similarly increased in frequency (Graham et al., 2022; Milillo et al., 2022; Wille et al., 2022), with a 1,600 square kilometers iceberg breaking away from the Brunt ice shelf on January 22nd, 2023."
This seems anecdotal and unrelated to the topic of the manuscript.

Lines 26 to 27: "The remote nature of this vast continent means that observational data covering Antarctica are sparse in both space and time (Turner et al., 2004)."
Rewrite, too vague and imprecise. Satellite data are observational, and they are regularly gridded and relatively dense. Also, most of the observations are in the last 50 years, so they are not "sparse in time", but rather extremely concentrated on a specific period. I understand what you meant, but the sentence could be improved.

Lines 27 to 28: "The reconstruction of past temperature change is thus of paramount importance for understanding natural variability, versus effects in response to anthropogenic climate change" "paramount importance" does not seem appropriate here. "versus effects in response…" is nonsensical.

Lines 34 to 35: "it is sometimes referred as the 'ice core paleothermometer' (Lorius and Merlivat, 1977; Masson et al., 2000)."
It is referred as the "Isotopic paleothermometer" in the specific references that are cited here. Also Lorius and Merlivat 1977 does not qualify as a peer-reviewed publication, consider replacing by (Lorius et al., 1969). Other proxies from ice cores can be used for past temperature reconstructions, for instance the borehole thermometry.

Line 36: "The paleothermometer" is not sufficient, should be describe as "isotopic paleothermometer".

Lines 38 to 41: "This ice core based record was then used to show that simulated temperatures from the Atmospheric General Circulation (AGCM) models run in the frame of the Coupled Model Intercomparison Project Phases 5 (Taylor et al., 2012) and 6 (Eyring et al., 2016, CMIP6) are too low. However, the paleothermometer relationship has been shown to vary spatially over the Antarctic continent (e.g. Sime et al., 2008, 2009a)."
(Casado et al., 2023) does not state that the temperatures are not too low, but that the variability (natural and forced) is too low. In addition, variable isotope-temperature conversions are used for the different regions of Antarctica in Casado et al, 2023, so it seems that the use of "however" here suggests an opposition that is not based on actual opposite point of views.

Line 49: "The geographical variability in the 'paleothermometer' is due to controls on d18O other than SAT."
This statement appears slightly misleading in the sense that it doesn't reflect that Rayleigh distillation (Dansgaard et al, 1964) suggests already that the signal acquired across the distillation pathway, and not a pure local surface temperature signal.

Lines 59 to 62: "AGCM isotopic studies have focused on the effects of external forcing on the SAT–δ18O relationship, including elevation and greenhouse gases across a range of timescales (e.g. Sime et al., 2009b; Werner et al., 2018; Goursaud et al., 2021). A major result is that, for differing time-scales and driving mechanisms, different SAT–δ18O relationships can be obtained."
This sentence suggests that this result was only found using isotope enabled GCM studies, while several proxy based studies have also shown this, including relatively old ones, see for instance (Guillevic et al., 2013; Jouzel, 1999).

Lines 78 to 83: "The Historical simulation protocol was defined in the frame of the CMIP Phase 6 (Eyring et al., 2016), with an express purpose to investigate the anthropogenic forcing on climate (Johns et al., 2003) and serve as a benchmark to evaluate model performance (Andrews et al., 2020; Miller et al., 2021; Parsons et al., 2020; Rong et al., 2021; Roach et al., 2020). There have been few examples of studies using Historical simulations focused over Antarctica (Gao et al., 2021; Purich and England, 2021; Raphael et al., 2020; Roach et al., 2020).
Here, we use the Hadley Center general circulation model (HadCM3; GCM), to run six transient Historical simulations."
This is technically correct, it also implies that HadCM3 is a CMIP6 model, although it is actually a CMIP5 model. The description should first be the model. Then be the transient historical simulations that were done. At this point, for non-modeller specialist such as myself, it is not possible to know what the *historical simulation protocol* entails, so rather than giving examples of studies, the key important points that a reader should know should be described here.
For instance, it is not clear if/how the surface conditions were prescribed since only the atmospheric component was used. Was the sea ice concentration simulated or prescribed? If it was prescribed, since HadGEM performs relatively poorly for sea ice concentration, surface ocean temperature, and to some extent for the 850hPa temperature (Agosta et al., 2015), how is that affecting the results?

Lines 89 to 90: “HadCM3 provides a reasonable representation of Antarctic climate and δ18O (Turner et al., 2006; Tindall et al., 2009; Holloway et al., 2016).”

Tindall et al, 2009 provides a comparison of d18O with observations from mostly tropical and temperate regions, with only 2 data points in Antarctica. Holloway et al, 2016 provides a comparison of the outputs of the model and 4 ice core records during the last glacial maximum. To my knowledge, no comparison between modern δ^{18}O from observations and model outputs has been published. Considering the large warm bias that most of the isotope enabled CMIP5 models suffered which have been fixed in CMIP6 versions (Cauquoin and Werner, 2021; Werner et al., 2018), it feels like this statement does not provide the necessary information to know if we can trust the outputs during the historical periods. Please consider reproducing Figure 1 of (Werner et al., 2018).

Lines 96 to 97: “Where we regress climate variables against δ18O, the linear regressions are computed using the stacked individual ensemble members, rather than using the ensemble mean.”

Unclear to me. Are you computing the linear regression on a stack of all the individual members ? Or are you computing it against individual members and then stacking the linear regression ? The former seems fairly similar than using the ensemble mean.

Lines 99 to 101: “Our Historical SAT–d18O linear relationship at the regional scale, as well as at the nearest model grid-cell to each ice core location, are compared with the ECHAM5-wiso slopes and correlation coefficients provided in Stenni et al. (2017).”

Why would you use ECHAM5-wiso here when your manuscript is about HadCM3 ? If you're using an isotope enabled version of ECHAM, why not use ECHAM6 which has been released in 2021. And if the goal is to provide a reference that is published, why is the comparison with observations not included, i.e. (Casado et al., 2017; Fujita and Abe, 2006; Masson-Delmotte et al., 2008; Schlosser et al., 2004; Stenni et al., 2016; Touzeau et al., 2016).

Line 114: “3 Trends in Antarctic SAT, precipitation, sea ice and δ18O”

Is this a result section ? it seems to include results and discussion, but then section 4 and 5 as well. While I don’t think Climate of the Past has a strict rule on which structure to use for manuscript, I am not convinced that the classical structure wouldn’t help the readability of the manuscript.

Lines 122 to 123: “This is consistent with observations of 0.12±0.07 °C per decade over 1957-2006 (Steig et al., 2009) and 0.11±0.08 °C per decade over 1959-2012 (Nicolas and Bromwich, 2014).”

This is only partially true. Jones et al, 2019 reports larger warming across Antarctica when the SAM-congruent trend has been taken into account. Clem et al, 2019 reports temperature increase of 0.6°C per decades at the south pole station, where the map in Figure 1E. reports actually a cooling between 1850-1900 and 1950-2000. ERA5 reports as well a large warming across Antarctica, which is indeed a reanalysis based on satellite observations.

Also, is the comparison really accurate if you compare on the one hand model outputs over all of Antarctica and meteorological observations from N&B which are clearly biased toward coastal regions?

Lines 129 to 130: “Forecast System Reanalysis (CFSR), and 7.1±1.5 mm/y per decade from the National Centers for Environmental Prediction reanalyses 2 (NCEP-2) over 1979-2009.”

It seems that different sources of data (observations, reanalyses, or other type of models) are used for different variables. Wouldn’t it be valuable to compare all of your variables with one systematic source of data, may it be direct observations, satellite observations, reanalyses…

Lines 166 to 168: “Despite the simulated increases in SAT and precipitation, d18O shows a very weak trend of 0.04 ±0.003‰per decade (r=0.21) over the last 50 years. Interestingly, (Casado et al., 2023) provide a higher trend from 1950–2005 of 0.11± 0.02‰per decade. It is unclear why the trend in (Casado et al., 2023) is higher.”

The response provided seems insufficient, particularly in light of the extensive sensitivity tests conducted in (Casado et al., 2023)to elucidate the disparities with the trends observed in S(Stenni et al., 2017). It would be helpful to have clarification on the handling of isotopic data from the PAGES2k network in this context, such as the methodology for averaging monthly isotopic data with annual and interannual data. Given that a trend is essentially a mathematical representation, and both this manuscript and Casado et al., 2023 utilise the same dataset, it raises concerns about the disparity in values. Moreover, the methods employed in Casado et al., 2023 were replicated using the outputs generated in Stenni et al., 2017, resulting in a slope of 0.10 permil per decade. Additionally, an alternative method based on dynamical system theory yielded an even larger value.

Lines 169 to 170: “Before this, we provide a brief overview of the regional picture. At the regional scale, over the Historical period, trends are small (Figure 2).”

The regions in Figure 2 seem to be different than the ones in Stenni et al 2017 beyond the impact that the model grid would do to the attribution. For instance, none of the coastal grid points are included in your analysis, which differs strongly from Stenni et al. Victoria Land region extend further east near the coast (with the strong consequence of adding an additionnal core in this grid point compared to Stenni et al). Another notable difference comes from the lack of the coast part in DML coast region, which means that almost none of the ice core available for this region are represented in your average. As this seems to be a significant difference compared to Stenni et al, 2017, where several pages were included to explain the choice of the region, it needs to be justified.

Lines 171 to 172: “and is the highest for the Weddell coast with a trend of 0.05 ‰ per decade (r=0.39), and the strongest for the peninsula with a trend of 0.04 ‰ per decade (r=0.57).”

Which one is it ? The "highest" and the "strongest" should be the same.

Lines 183 to 185: “These disparities could be explained by the different time windows, the different methodologies or the lack of ice core data to make representative regional reconstructions.”

All of these hypotheses can be readily examined to ensure the robustness of the arguments detailed here. For the first hypothesis, it might be beneficial to incorporate a table in the supplementary materials, offering a comparison for the same time windows. Adaptations in methodologies can be explored, and Supplementary Table S3 in Casado et al., 2023, already presents trends using both their approach and the one from Stenni et al., 2017. Additionally, the absence of ice cores to establish representative regional reconstructions can be tested by focusing solely on specific grid points of the model corresponding to the ice core locations, comparing them to the regional average encompassing all grid points in the region. A fourth option, which the author does not explicitly address, is the potential bias or insufficient representation of variability in HadCM3, as suggested by Casado et al., 2023, particularly for most CMIP models.

Lines 194 to 206: This is an interesting discussion, but again, it fails to address the elephant in the room which is the model biases. The maps show areas with non-significant link between isotopic composition and temperature in the model, which seems sound and robust, but how does it compared to observations in the field? For instance, no correlation is found at the site of Vostok, where precipitation isotopic composition shows a significant correlation (R = 0.63, slope of 0.35 permil per degree) (Touzeau et al., 2016), while the map suggest a slope of 0 with non-significant correlation. The slope and correlation at Dome C also seems lower (below 0.3 with a r <0.3) than in observations (R^2 = 0.63 and slope of 0.49) (Stenni et al., 2016). In general, any discussions which could support the validity of the model outputs would strengthen the manuscript, or at least provide confidence interval on the range of values that can actually be interpreted.

Lines 211: “Here, and also for other warm climate results”

Is it actually relevant? The manuscript is about historical reconstructions. This could potentially be discussed in the end of the discussion, but this subsection feels like results.

Lines 219 to 220: "Interestingly, however, this is not the case when comparing between the last 50 years of our HadCM3 simulation and the ECHAM5-wiso simulation."

It is obsolete to compare your result with yet another CMIP5 model, when the isotope enabled version ECHAM6 wiso is available for more than 3 years.

Lines 225 to 227: "all the historical SAT-d18O relationships are different from the LGM-PI ECHAM5, and LIG-PI HadCM3 relationships: Werner et al. (2018) report LGM-PI regional gradients in ECHAM5 that are 17-26% lower, while (Sime et al., 2009b) and (Holloway et al., 2016) present LIG-PI regional gradients that are ~50% lower for HadCM3."

Nobody would expect the historical and the LGM-PI relationships to be the same, considering the difference of time scales and underlying mechanisms driving the temperature changes. Is this really necessary in this manuscript which is about historical changes in the isotope-temperature relationship?

Lines 228: "ECHAM5 towards ERA-40 reanalysis,"

There are two generations of newer ERA products. It is unclear why the authors did not use either of these products. If the nudging is not conducted with the newer products in a revised version of the manuscript, which is what is really needed here, a clear justification for this omission will be required.

Lines 238 to 239: "The primary mechanism driving continental-scale SAT-d18O decoupling is the simulated loss of sea ice during the historical period (Figure 5DH)."

The rationale that could explain how to make this assessment is not supported by Figure 4D to H. The patterns of sea ice concentration anomalies does not explain any link with the variations of temperature and isotopic composition inside Antarctica by itself. There is no correlation provided, no mechanism, no simulations in which the sea ice concentration is artificially varied to support this assessment. Overall, this entire section falls short in establishing any form of causality and warrants a comprehensive revision.

The conclusion should be revised once the rest of the manuscript has been reassessed.

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