This paper presents a new MOT reconstruction from noble gas measurements between 74 and 59.5 ka. These new data are combined to other MOT data obtained over the last climatic cycle. They are compared to benthic d18O and Antarctic dD and used to discuss the effect of solubility pump.

As all previous studies showing MOT reconstructions based on noble gas measurements, these data represent a huge analytical effort and they are worth to be published because they will be very useful. The discussion of the data could however be improved in a future version of the manuscript. The conclusions conveyed by the abstract are fine and the discussion and conclusion of the manuscript should thus be reorganized to be in line with the abstract.

List of comments:

What is the exact scientific aim of the paper ?

- Make the link between CO2 atmospheric concentration and MOT ?
- Discuss the link between MOT and Antarctic temperature ?
- Compare the MOT between MIS 2 and MIS 4 ?
- Discuss the MOT dynamic over millennial-scale DO events ?
- Separate sea level and deep ocean temperature contribution in benthic d18O stack ?

We appreciate the anonymous reviewer's overall positive comments on the manuscript and their clear and constructive criticism on how to better convey the main arguments of the paper. The scientific aim of the paper is to present new MOT reconstruction for MIS 4 and the MIS5-4 transition and discuss the climatic implications of these data for our understanding of atmospheric CO2 and climate change. Based on the reviewer's comments we have substantially restructured the discussion and conclusion sections and address specific comments below.

Figure 1: I find it confusing to have identification of MIS4 and MIS2 through intervals between vertical dashed line and black bars of different width to define MIS 4 and MIS 2 MOT -> Better find another definition for the black bars like "cold MIS 4 MOT" + better explain how these black bars were defined.

In this first draft of the manuscript we did not adequately explain the reasoning behind the different intervals for MIS4/MIS2 comparison. The MIS 2 (or LGM) interval is from the previously published Bereiter et al study, which did not extend through all of MIS 2 (as defined by benthic d18O). For our MIS 4 record, we were concerned about misalignment between ice core and sediment records to define MIS 4, so chose the interval of low atmospheric CO_2 / EDC dD to define MIS 4 in this manuscript. We have specified this in the updated manuscript:

Line 140-147: Here, we do not use the intervals identified and defined by benthic $\delta^{18}O$ to compare MOT in MIS 4 and MIS 2, as the alignment of ice core and sediment records is uncertain, particularly in MIS 4. Instead, we define MIS 4 as the interval in which CO_2 and Antarctic temperature remain low and stable (70.3-63.7 ka, or Greenland Stadial 19 and Interstadial 18). For Taylor Glacier samples, we compare MIS 4 samples to five replicate MOT samples from MIS 2 (19.9 ka). For WAIS Divide samples, we compare the measured MIS 4 samples to all available, previously *published (Bereiter et al., 2018a) MOT data from MIS 2 (24 – 18 ka), but applying the fractionation correction used in this study. The difference in WAIS Divide MOT results for the full MIS 2 interval (n=11) versus 20-19 ka (n=4) differ by less than 0.01°C, so the difference in the selected intervals to define MIS 2 for each core should not affect the MIS 4-2 comparison.*

Figure 2: The MOT temperature increase between 64 and 60 ka is not discussed in the manuscript while it seems that a strong MOT increases occurs between 62 and 60 ka while the EDC dD increase is less marked than between 64 and 62 ka when the MOT is stable. It could be argued that there are not enough MOT points and some scattering but this is equivalent to the period between 70 and 68 ka which is discussed in the text as the second phase of MOT decrease during MIS 4.

This is a fair point, and a related question about the MOT trend between 70-68 ka was raised by reviewer 1. To evaluate whether the apparent decoupling between dD and MOT during GS18 is statistically significant (or, instead, may be attributed to scatter) we compare the correlation between dD that has been smoothed to remove high frequency variability (see figure 4 caption for details) to contemporaneous MOT data (r^2=0.57). This correlation with dD is lower than what is found when comparing all available MOT data to contemporaneous dD (r^2=0.94, figure 4a). However, the MOT range for this subset of the data is relatively narrow, so the lower signal to noise ratio may reduce the expected correlation. Based on the estimated uncertainty of individual MOT data from the pooled standard deviation of replicate samples in this record (0.34°C), we can predict the expected correlation between MOT and contemporaneous dD if we assume that the true dD and MOT signals are perfectly correlated (r^2=1), and that the lower correlation is due entirely to noise in the MOT data. Based on this assumption, we would predict an r^2 of 0.44±0.20, compared to the actual correlation r^2=0.57. This would suggest that the apparent decoupling between MOT and dD during this interval may be due to random noise.

How robust is the MOT increase during GS 20 ? If we consider only the GS 20 data points (I.e. do not consider the two GI 20 data points), there is no MOT tendency over GS 20.

Using a one tailed z-test on our Monte Carlo simulations of the data and including the two MOT data points that mark the low at the end of GI20, we find a statistically significant (p=0.03) increase in MOT at the onset of the record (during GS20). However, without the two low points at the end of GI20, the MOT increase during GS20 is not robust (p=0.48). While we acknowledge that the analytical uncertainties of our record present a challenge in detecting the finer scale variability shown in our record, we respectfully push back on the reviewer's comment here and argue that the noteworthy aspect of this early part of our MOT reconstruction is that MOT is increasing at all, given that this interval is widely regarded as a period of long-term cooling.

Except for the GI 19 evolution, there is not so strong evidence for a fine scale correlation between dD and MOT on this figure.

We respectfully disagree with the reviewer's comment here. There is a statistically significant correlation between dD and MOT for this record (r^2=0.59), which is lower than the correlation of all available MOT data versus dD (r^2=0.94). However, as mentioned above, the signal to noise ratio for this record should be lower than that of the (previously published) terminations. Using the pooled standard deviation of replicate samples from this study as a predictor of random noise (as above), we would predict an r^2 of 0.58±0.09 for the record published here and r^2 = 0.93±0.01 for all available

MOT data versus dD if MOT and dD were perfectly correlated and any lower correlation is due to random noise. We have added this point to section 4.1.3:

Lines 241-249: As highlighted in this, and several other MOT studies (Bereiter et al., 2018a; Shackleton et al., 2019, 2020), one of the most striking features of MOT records is their strong correlation to Antarctic water isotope records (Fig. 4a). For the MOT data from this study, we find a lower correlation between MOT and EDC δ^2*H (n=56,* $r^2 = 0.59$*) than between all available MOT records (n=243,* $r^2 = 0.94$*). However, MOT and* δ^2*H data for this interval cover a relatively narrow range compared to other records, resulting in a lower signal to noise ratio, and thus may explain the lower correlation. To test this hypothesis, we use the pooled standard deviation of replicate MOT samples (0.3°C) as a predictor of random noise in the MOT record to estimate the expected correlation between* δ^2*H and MOT if we assume they are perfectly correlated (*$r^2=1$*). Under these assumptions, we would predict* r^2 *values of 0.58±0.09 and 0.93±0.01 for the MIS 4 subsample and all MOT samples respectively, which is consistent with the observed values.*

Figure 3: Following last comment, I am not confident that the Model MOT can be drawn as shown on the bottom panel with details at a scale of a few ka. Without more MOT data between 120 and 75 ka, and especially over the 120 – 110 ka strong modelled MOT decrease and large MOT increase and decrease between 88 and 78 ka, the modelled evolution is not robust which casts doubt on the interpretation in term of CO2 solubility pump between MIS 5d and MIS 5a.

We agree with the reviewer that the modelled results within the region of ~120-75 ka (where there is a gap in MOT data) should not be overinterpreted. The purpose of the carbon box model was to demonstrate that the change in CO2 across (but not within) MIS5 could be mostly explained by ocean cooling. We attempted to show this with the arrows pointing to the start and end of MIS5 (where we do have MOT data) and text in the figure with the net change in MOT and modelled CO2 over MIS5. However, we did include some speculation about the CO_2 *variability within the gap in MOT data at the end of the figure 3 caption, which we have removed and replaced with the statement 'model results within 120-74 ka should be interpreted with caution, as MOT data do not exist for validation'. We have also included a caution about this in the main text:*

Lines 201-203: We emphasize that the available MOT data spans 9 kyr at the onset and 2 kyr at the end of the long (~57 kyr) MIS 5 interval, so our insight into the role of the solubility pump on CO_2 *variations within MIS 5 is limited.*

p.5, l. 142-146: it is difficult to understand what is described here. It would help to clearly give the period (with dates) that you are discussing here. + the evolution after 70.5 ka is not very clear due to the lack of MOT data and scattering.

This is a fair point, which was also brought up by Reviewer 1. We have added the specific periods with dates that to the manuscript, which are defined by GI19 (72.1-70.3 ka) and the second from the onset of GS19 to 67.5 ka, where the MOT record reaches a minimum. To test if the rates of MOT decrease are robustly different, we can estimate the rate of MOT decrease and its uncertainty for each of these intervals from the Monte Carlo simulations of the MOT data. We find a cooling of -0.41±0.09°C/kyr in the first stage of the 5a-4 transition, and -0.19±0.07°C/kyr in the second stage. A two-tailed z-test shows that the difference between these rates of MOT decrease is statistically significant (p=0.05).

Section 4.2 is confusing while it is a good idea to use MOT to decipher sea level contributions from deep ocean temperature on benthic d18O. What is the purpose of this section? Quantify the uncertainties in the reconstruction of MOT through sensibility to sea level value? If so, it is probably better in the annex or in the result section? then you again discuss the link with benthic d18O and sea level in section 4.4. The flow of ideas is difficult to follow.

We appreciate the reviewer's comments/suggestion. In the updated manuscript, we have removed most of this section and combined it with the discussion on trends in coeval MOT and d18O.

Sections 4.3 and 4.4 present the link between Antarctic temperature and MOT and invoke change in AMOC. The discussion on the link between Antarctic temperature and MOT should be gathered in a unique section for an easier reading of the manuscript.

Thanks for this suggestion – we have followed it and combined these sections.

The end of section 4.3 focuses on the temperature and ice volume of MIS2 vs MIS 4 which is quite disconnected from the beginning of the section. Try to reorganize the full discussion to convey clear conclusions and messages.

We have moved the end of this section into a relatively brief separate section (now section 4.2: The cold and stable MIS 4 interval), which also includes a discussion on why temperatures in MIS 2 and 4 may be comparable. This new section is admittedly speculative, but we label it as such.

It seems that you want to discuss:

- the MOT at MIS 4 compared to MOT during MIS 5e and MIS 2 with implication on the CO2 atmospheric concentration
- The link between MOT and Antarctic dD at glacial – interglacial and millennial scale with a discussion on the associated mechanisms

The discussion on d18Obenthic is not very clear here – Is it a perspective of this study to compare with d18Obenthic or should these data be used to refine uncertainty in the MOT determination.

The discussion has been substantially reworked to reflect our main messages. The main sections now include 4.1.1: Evolving control of ocean temperature and ice sheet volume on benthic δ^{18}O, 4.1.2 Early role of ocean cooling in atmospheric CO_2 drawdown, and 4.1.3 Strong correlation between MOT and Antarctic climate on orbital and millennial timescales.

Conclusions:

- to be rewritten (the abstract is more explicit)
    - *Following the restructuring/revising of the discussion we have rewritten the conclusions to be more consistent with the main arguments of the manuscript.*

- the discussion on MIS 4-3 beginning on l. 266 was not present (or I missed it) in the sections of the discussion.
    - *This has been removed*
- The paragraph beginning l. 276 seems disconnected.
    - *The conclusions have been reorganized so that this paragraph immediately follows the discussion on the value of complete MOT records within MIS 5.*