Dear referees, dear editor

Below you'll find our final response to the reviewer comments. We are most grateful for the positive evaluation of our work by both referees. We are especially thankful for the euphoric assessment by Jeff Severinghaus. This means a lot to us, as Jeff is the pioneer of noble gas thermometry on ice cores.

We outline how we plan to meet the reviewer comments and what textual changes we plan in the revised version of our manuscript. We will not go through any of the grammar or typo corrections at this point but will carefully revise our manuscript also in this respect based on the minor comments by the referees. Here we concentrate on textual or argumentation changes. Our answer is split according to referee #1 and #2 where the review comments are given in red and our reply in black.

Referee #1

The reconstruction of mean ocean temperature from past gas composition from ice cores is very complicated and tricky. Numerous corrections need to be applied and the authors go through great length to explain what they do and why. I understand that they want to be maximum transparent on the method they use. However, the manuscript is very long and requires endurance to read. It would profit from being split into a main text and an appendix section with all the technical details.

We are well aware of the level of detail we provide in this manuscript and that for those readers only interested in the final results it may not so easy to digest. However, we regard this paper as a reference document also for future studies on MOT using noble gases in ice cores on the EDC and other ice cores, as a similar reference document does not exist yet in a peer-review publication, which provides all the detail that is needed to replicate the results. Accordingly, we would like to keep the discussion of noble gas corrections and gas loss issues in the manuscript. As we do not have a purely mathematical derivation in the manuscript either, it appears also difficult to us to transfer some of the material into an appendix. However, to make navigating this manuscript easier for all readers (whether they are interested in methodological aspects, air hydrates or just in the MOT numbers), we will add a paragraph at the end of the Introduction that explains the structure of the paper and refers to the sections that are of interest for a specific reader.

A sketch in isotope space showing the various corrections and their magnitude along with the respective effect on MOT would be useful.

We will add some information on the effect of the various corrections on MOT in Fig. 2

The temperature gradient in the firn layer is
an important correction. The authors favor a model based approach for that correction that fits the long term average of the individual reconstructions based on the data. This I find troublesome. From the denser measurements up to 40 kyr BP it looks like the signal is not random.

Fig. 4 displays the firn temperature gradient that we derive either using the model (model-based approach) or using the isotopic values only (data-based approach). In the model case (red squares) we see a small change in the firn temperature as expected, as the surface temperature at EDC was 8-10 °C colder in the LGM than in the Holocene while the temperature at the bedrock remained at the pressure melting point. Accordingly, the overall temperature difference between surface and bedrock increases in the glacial and hence also the temperature difference between surface and close-off depth. **Note that in the model-based approach we include this systematic variation in ΔT in our correction,** so referee #1 does not have to worry for this model-based approach.

In the data-based approach one may see also some systematic variation in ΔT (black squares in Fig. 4), however (i) the firn temperature difference and its variations are unphysically large nor can (ii) positive temperature differences physically occur at Dome C. The variation seen in the data points over the last 40 kyr is of the same size as the analytical error thus should not be interpreted. In the manuscript we only used the mean of the data to get a representative mean kinetic fractionation using our data-based approach to check the consistency of the results of the two approaches. Using this mean ΔT leads to a mean kinetic correction that is in line with the model-based approach. However, even using the mean ΔT our data-based MOT reconstruction is subject to too much analytical error to allow meaningful conclusions in terms of MOT changes. Hence, in the end we discarded the data-based approach for MOT reconstruction and the use of a mean ΔT in the data-based approach, criticized by the referee, is not included in any of the final results or the conclusions of the paper. We will revise the text accordingly, to stress these points.

Specific comments: Page 4 line 17. : How is Kr affected by drill fluid when all other components have been gettered away?

When we first saw our results, we shared the astonishment of referee #1 that any contamination may survive the gettering process, however, the data clearly shows that samples showing anomalies in d15N in the ungettered aliquot show also anomalies in 82Kr in the gettered aliquot. We have no conclusive evidence yet what is causing this interference, however, we are currently working on lab experiments to get more insight on this. Either the zoo of higher organic compounds in the drill fluids allows for some component to be not completely gettered if the drill fluid contamination is too large or the H2 released by the gettering of such organic compounds (and which may not be quantitatively trapped by getter material if its abundance is too high) leads to chemical effects in the source that cause the mass 82 interference. We will elaborate a little bit more on this in the revised version but cannot provide an ultimate answer at this point.

Page 8, lines 11-17: Instead of writing DT is negative write that the temperature is higher at depth due to geothermal heat flow (or do I misunderstand what is said here?)

will do

Figure 3: Please lower the top tags slightly so they do not interfere with the frame.

will do

Page 14, last paragraph: First, you argue that there may be a signal in the data then you invalidate that statement but do not say it.

as outlined above we will discuss this in more detail to justify our approach

Page 18, line 7,8: What is the argument to assume no change in the saturation state?

We refrain from including a change in the saturation state as no experimental evidence for the saturation during glacial times exist. In fact, Bereiter et al. (2018) argued for an increase in saturation due to reduced ocean overturn, however, the increased sea ice coverage especially in the Southern Ocean could also argue for a decrease in saturation. Thus, no robust assumption can be made on the change in saturation state. We will add a paragraph on this in the final manuscript.

Referee #2

This manuscript describes a heroic effort to use noble gases from the full 700-kyr EPICA Dome C ice core to infer past mean ocean temperature, based on the well-known temperature dependence of noble gas solubility in the ocean. The method takes advantage of the fortunate fact that the total amount of N2, Kr, and Xe in the combined ocean-atmosphere system is remarkably stable over million-year timescales, at a sufficiently high level that they can be assumed to be unchanging.
The difficulty that had to be overcome by the authors is substantial. Many unforeseen artifacts, such as gas loss and clathrate based issues, had to be wrestled with. This work truly pioneered the use of noble gases in very deep ice cores where geothermal heat made the ice core rather warm, and depressurization effects upon core recovery were extreme. Transport issues further vexed the effort, including failures of the cooling system that allowed the ice cores to get warm. Fractionation mechanisms are still incompletely understood in ice cores, leading to small disagreements between the three gas pairs used. Nonetheless, the authors persevered and the result is a spectacular advance in scientific understanding of the behavior of the planetary energy imbalance and ocean dynamics over the late Pleistocene ice ages. This is truly an excellent piece of science and a carefully and thoroughly executed and painstaking research tour de force.
It goes without saying, then, that this manuscript should be published with only very minor revisions.

we are very grateful for this positive evaluation of our work

I have attached a copy of the manuscript with my suggested edits in red. One area that needs a re-write is the paragraph on air clathrates, which seems to have been influenced by prior work done on Greenland ice. Antarctic ice has lower impurity content (and thus clathrate nucleation sites) than Greenland ice, and therefore has clathrates that are fewer in number than the number of bubbles, requiring air to permeate some distance through the ice lattice from the air bubble to the (relatively rare) growing clathrate. This nucleation limitation effect is not seen in Greenland ice to my knowledge.

We agree with the referee that mixing the observations on clathrate formation made in Greenland (e.g. Kipfstuhl) and in Antarctica (e.g. Uchida) was a bit confusing. Accordingly, we will rewrite and extend this discussion to base it entirely on the work of Uchida et al., 2011 (and references therein), who use samples from Dome Fuji (which are very similar in terms of climate boundary condition as those from Dome C). These results clearly show that in the BCTZ of Dome Fuji clathrate nucleation is slow and that early nucleating air hydrates grow by permeation of air from coexisting bubbles, while at the same time the number of hydrates increases due to successive nucleation of new hydrates. In the deep, fully clathrated ice, hydrates grow as well, but their number is declining. Here their total number is declining by an Ostwald ripening process, where air permeates from smaller hydrates to larger ones.

To the authors: well done! This is a beautiful piece of science and will no doubt have lasting value.
Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2020-127/cp-2020-127-RC2-supplement.pdf

Here we will not list grammar or typo corrections suggested by referee #2 but will correct them in the revised version. We will shortly respond to main points made in the annotated manuscript

equation (1): we will clarify the units

page 8: we will recalculate the values using the local gravitational acceleration

page 9 decrease/increase issue. We apologize that our wording is unclear. We agree with referee #2 but to avoid any confusion, we will delete this sentence

page 13: we will include the comment of the referee about a potential sampling artefact at the WAIS firn pumping as (Jeff Severinghaus, personal communication)

page 20: we will revise the discussion on clathrate formation and growth and the accompanying permeation processes as outlined above