We thank very much the editor and the two reviewers for their very detailed and helpful comments. We have addressed all comments below and are willing to submit a manuscript taking into account all comments as explained in the answers to comment below.

Many thanks again for your help.

Review 2

This manuscripts presents a unique time series of stable water isotopes in water vapour in the under-sampled area of the Southern Indian Ocean. The calibration of the water vapour isotope data follows established standards in the community. While the dataset presented here is of unique coverage, the relevance of the analysis and the conclusions is not convincing. Furthermore, the figures presenting the analysis are often difficult to read and many important information is in the supplement figures. In its current state, the manuscript lacks a motivation for the presented analysis and how it improves the understanding of the atmospheric water cycle. It might be worth considering a submission to Earth System Science Data (ESSD) instead of ACP.

>> Many thanks for this review and detailed comments. The reason why we chose to submit to ACP is because we aim at combining for the first time water isotopes (data + 2 different models) and atmospheric species, here gaseous elemental mercury, over a long time series. The observation of concomitant water isotopes and Hg excursions suggests subsidence of air from high altitude.
We believe that water isotopes and elemental mercury records present an added value to the understanding of the dynamic of the atmospheric water cycle since they provide a surface diagnostic of processes happening higher in the atmosphere. They are very powerful for identification of change in vertical velocity which is a parameter which, otherwise, can only be reconstructed from the models. We agree with the reviewer that this objective was not clear enough in the manuscript, nor the added values of combining water isotopes, Hg and models and this will be corrected in the next version. Also, we will work on improving the readibility (and colorscale) of the figures and move some figures (backtrajectories for example) from the supplement to the main text or in appendix (see also suggestions from the editor).

Major comments:

• Model-measurement comparison

The comparison of ECHAM6-wiso and LMDZ-iso with the measurements at Amsterdam Island leads to the main conclusion that "the isotopic composition of water vapor is a powerful tool to identify aspects to be improved in the general circulation models, such as the horizontal resolution which may influence the representativity of the vertical dynamics." Why are *isotope-enabled* models needed to illustrate that horizontal resolution influences vertical dynamics in GCMs? This is, for example, already evident when comparing the vertical wind fields in Fig.7. Which "aspects to be improved", other than the horizontal resolution, were identified in this study?

Our study aims at exploring the mechanism driving the variations of water vapor $\delta^{18}O_v$ and especially the 11 events identified in the water vapor $\delta^{18}O_v$. With the aim to understand the associated processes, the comparison of the data series with outputs of atmospheric components of Earth System models equipped with water isotopes is a very useful tool. In addition to propose a mechanism for the water vapor $d18O_v$ excursions through our model – data comparison, we could also propose some ideas to explain why some models could not reproduce the water isotopic excursions at the surface. Isotope implementation is quite similar in the 2 models and is probably not the reason for the differences, a result that we should highlight in the next version of the manuscript. Nudging is slightly different between the two models (vorticity nudging in ECHAM6-wiso, shorter relaxation time in LMDZ-iso) but as the reviewer rightly points out, the conclusion is that if isotopes are controlled during these events by vertical dynamics, then it is probably necessary to have high-resolution models to unravel the mechanisms. We will adda couple of sentences saying that our model-data comparison validates the implemented physics of water isotopes and introduce in the conclusion the importance to have high-resolution models (mesoscale models) equipped with isotopes if one wants to study such events.

Finally, vertical wind fields from figure 7 are from reanalyses, themselves based on model. They were not measured. So we believe that our study, unique in this region, still has an added values by comparing a new measured data series (the records of water vapor $\delta^{18}O_v$ at the surface) with model outputs.

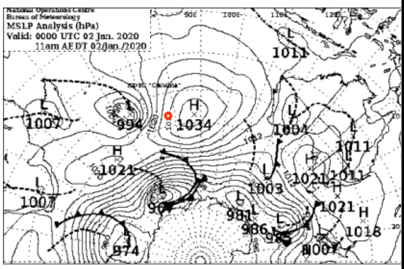
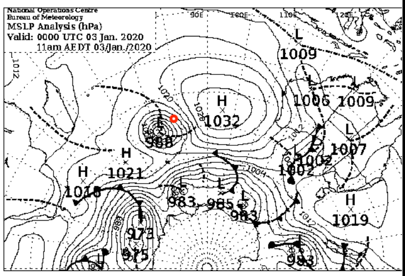
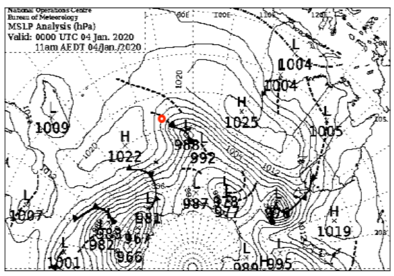
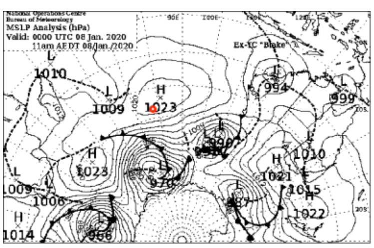
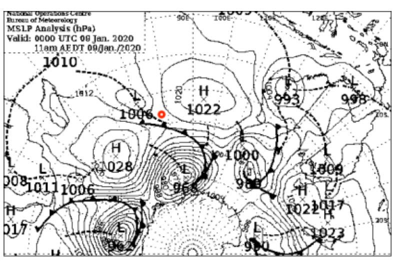
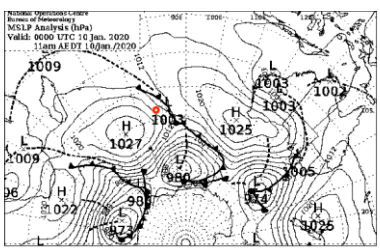
• **Cold front analysis**

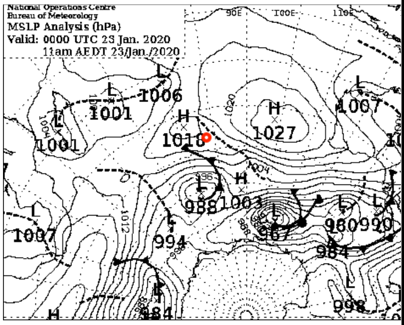
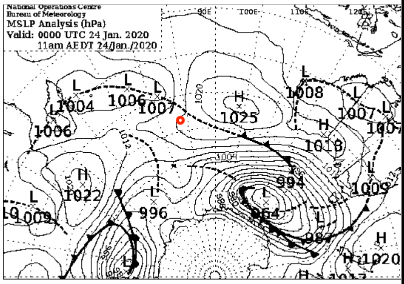
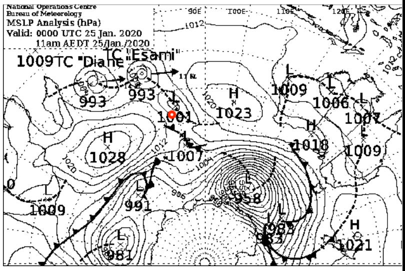
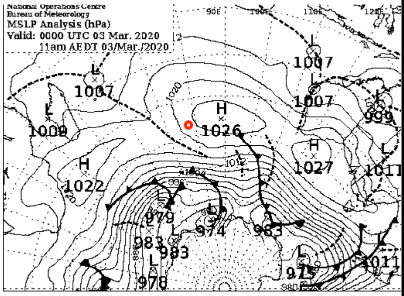
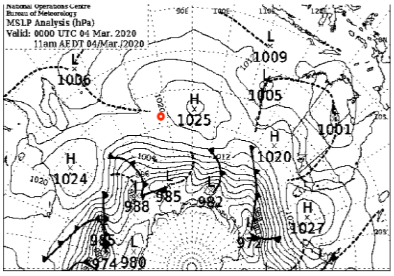
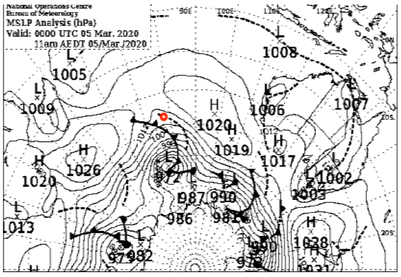
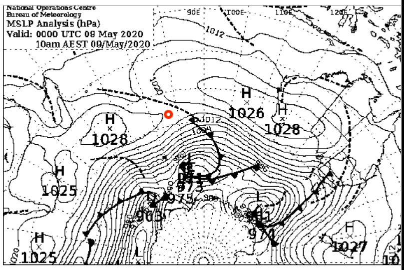
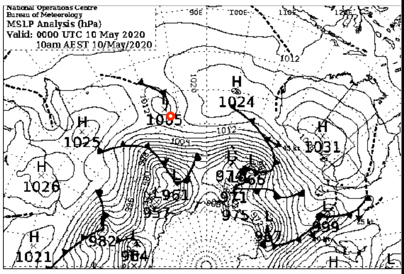
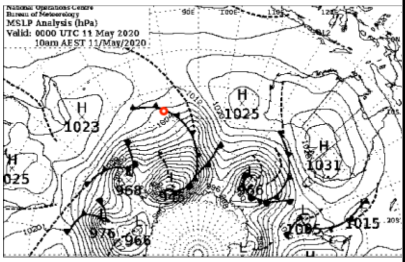
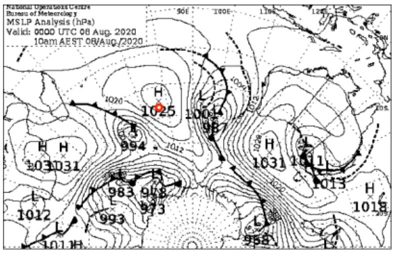
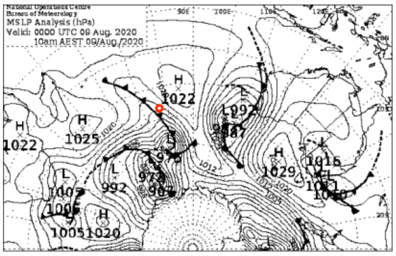
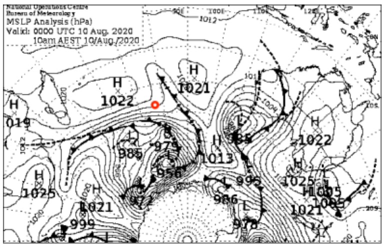
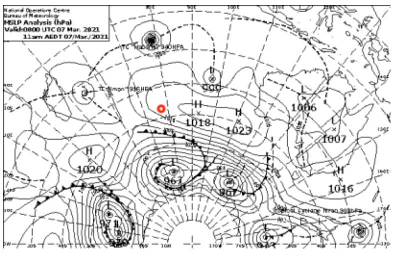
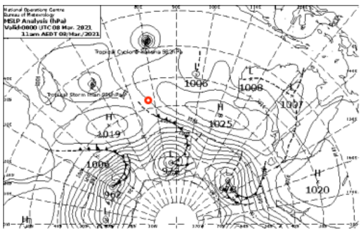
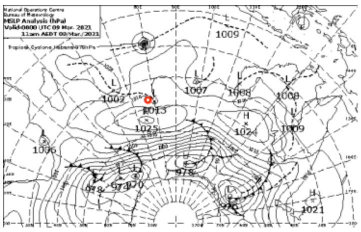
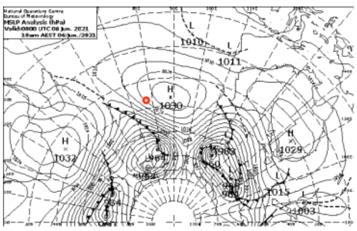
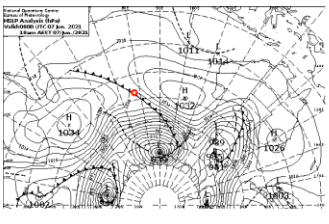
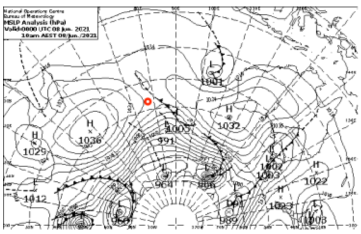
The analysis is based on d18Ov excursions which are described as cold front events. The selection and analysis of these excursion with respect to cold frontal dynamics on a synoptic scale is missing. It is therefore difficult to interpret the described events with respect to the large-scale dynamics. In detail:

◦ The analysis focuses on 11 events of cold fronts. These fronts and their spatial structure is not described in the manuscript and there are no synoptic figures describing a typical situation during a cold front passage, except for a few weather analysis charts in the supplementary information. How are the cold fronts identified? What are the properties of the cold fronts? How much of the annual precipitation is represented by cold frontal precipitation?

>> This comment is very sound. Our aim was actually not to focus on cold fronts but on the isotopic record and especially on periods when the isotopic record is providing an added value to understand features associated with the atmospheric water cycle, that is to say a different signal than the meteorological signals (humidity, temperature). This is why we concentrate on the abrupt negative water vapor $\delta^{18}O_v$ excursions which are not seen in the humidity signal. We then simply observed that the periods during which we observed water vapor $\delta^{18}O_v$ negative excursions were also associated with a cold front but there are certainly many cold fronts that are not exhibiting any water vapor $\delta^{18}O_v$ excursions. We definitively need to make it more clear in the manuscript and we will also remove the term “cold fronts” from the title and in other parts of the text since it was misleading.

In the previous manuscript, we had identified the cold fronts from the synoptic figures. All weather charts corresponding to the 11 water vapor $\delta^{18}O_v$ negative excursions are shown below:

	Day-1 of event at 00:00 UTC	Day of event at 00:00 UTC	Day+1 of event at 00:00 UTC
03/01/2020			
09/01/2020			

24/01/2020			
05/03/2020			
10/05/2020			
09/08/2020			
08/03/2021			
07/06/2021			

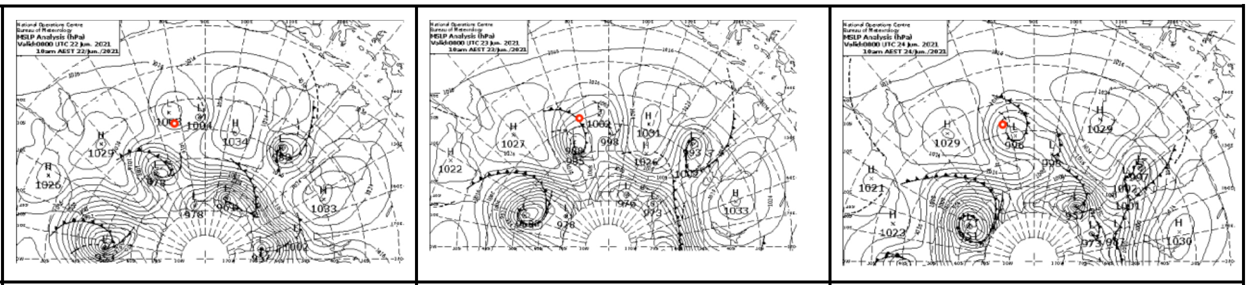
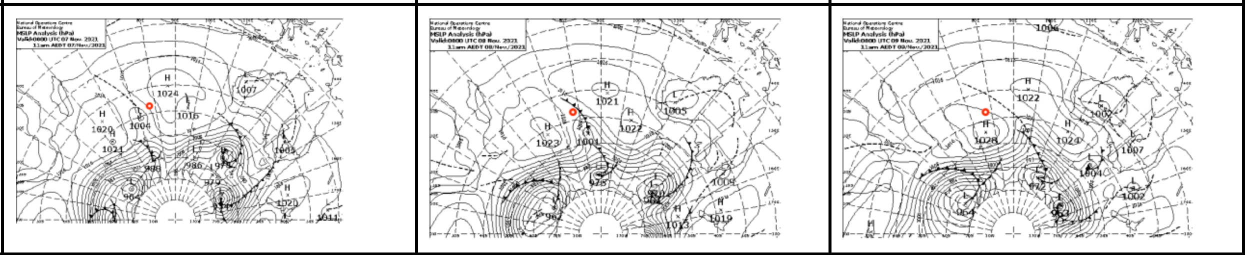
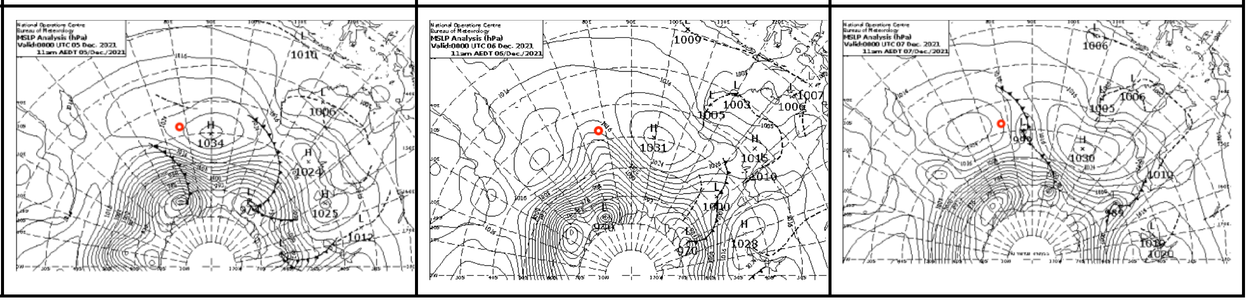
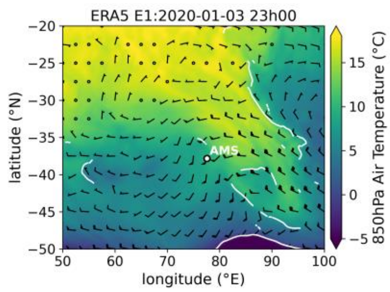
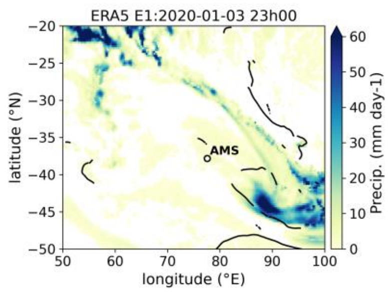
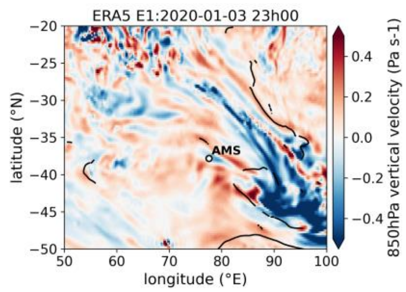
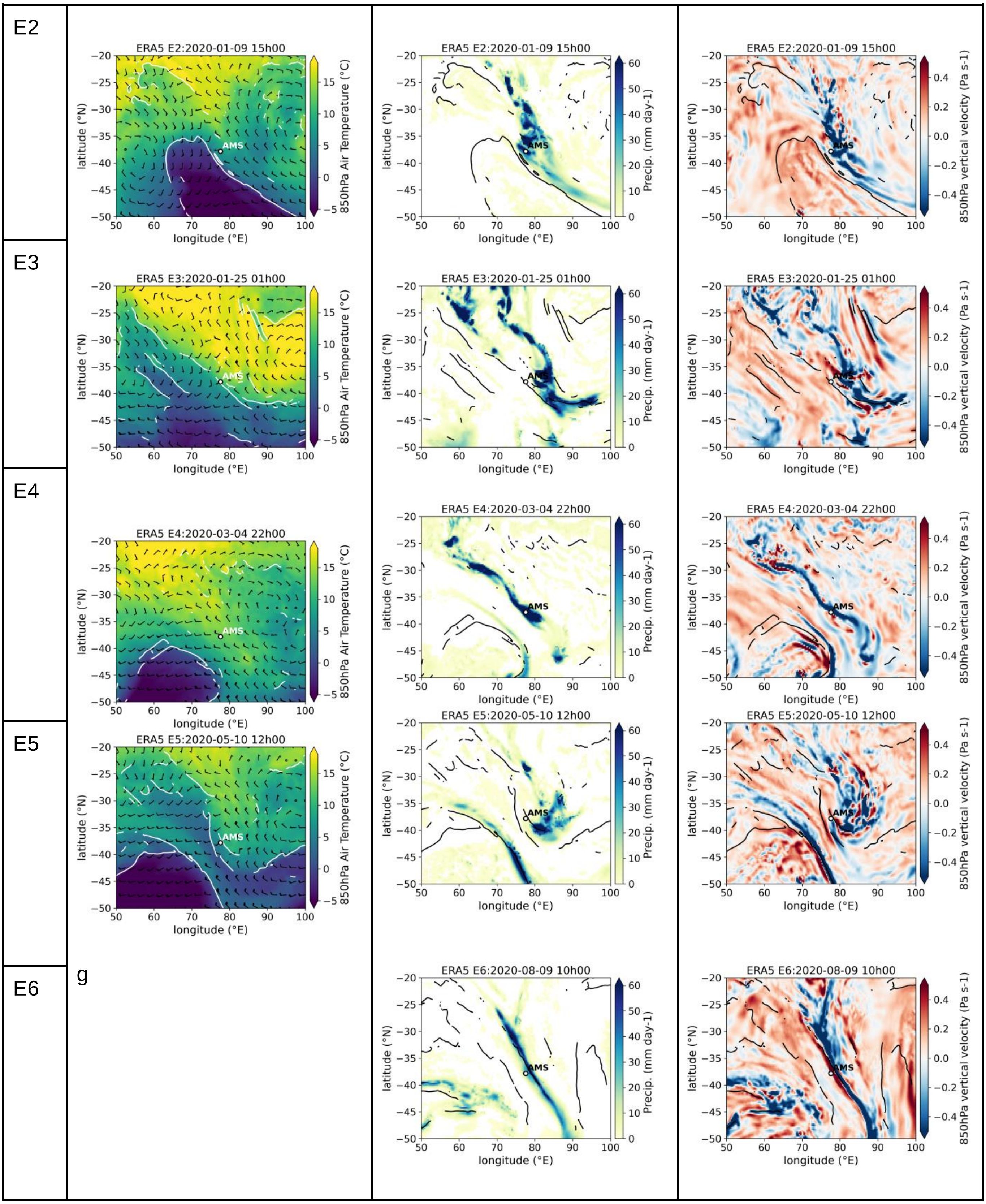
23/06/2021	
08/11/2021	
06/12/2021	

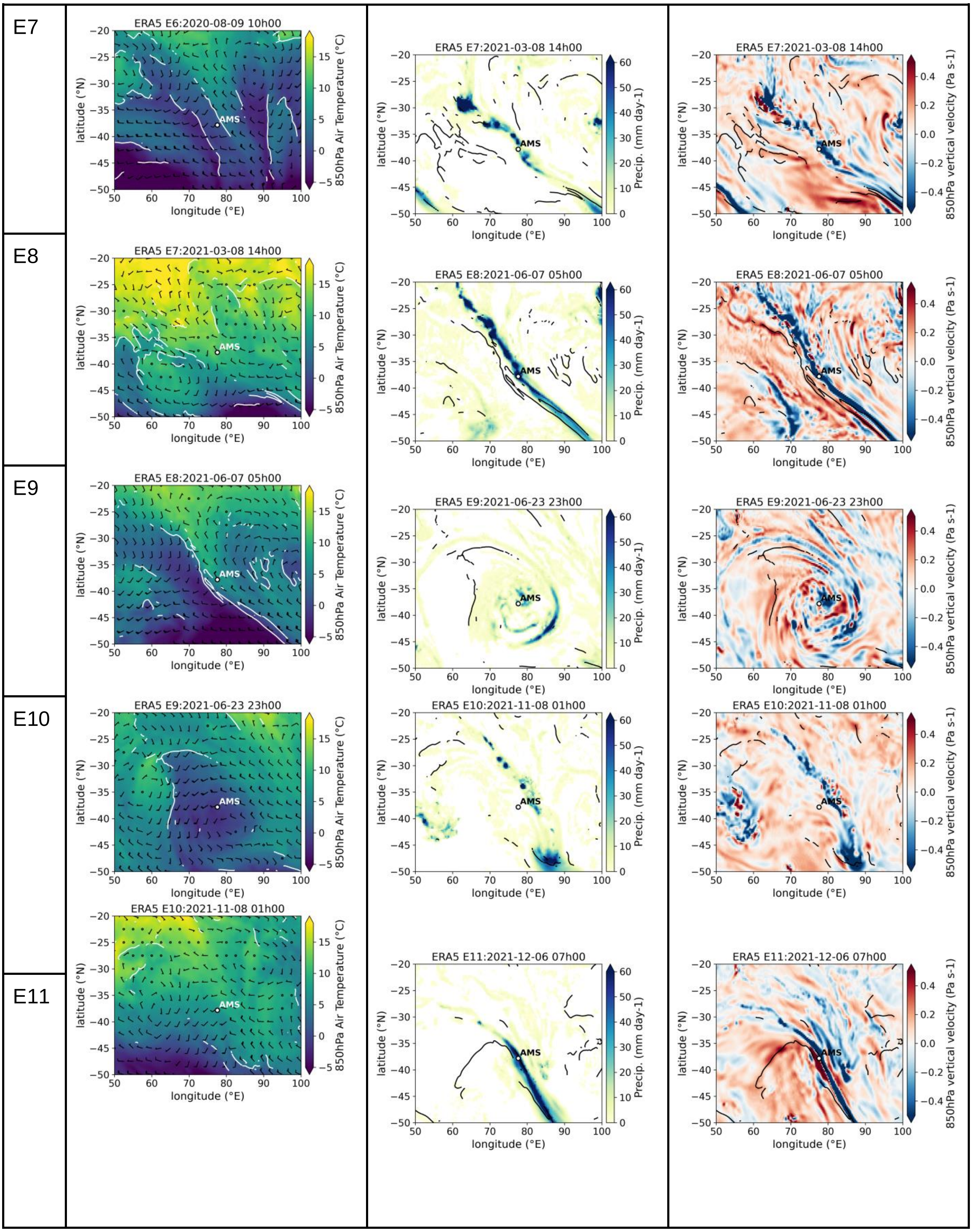
Figure R3 : Weather analysis charts provided once a day at 00:00 UTC by the Analysis Chart Archive service of the Australian Government Bureau of Meteorology http://www.bom.gov.au/australia/charts/archive/index.shtml. Red dot on weather charts displays Amsterdam Island location.

The idea was to check for the presence of a cold front in a distance of 100 km around Amsterdam Island in a 48h period covering the time of the event. We indeed see that we systematically have cold fronts in the vicinity of the Amsterdam Island at the time of the water vapor $\delta^{18}O_v$ excursions. Still, in some cases, such as the 06/12/2021, there are cold fronts identified on the weather charts in the vicinity of the Amsterdam Island 1 day before and 1 day after the excursions but no clear cold front over Amsterdam Island at the exact time of the water vapor $\delta^{18}O_v$ excursion.

To address the occurrence of cold fronts in a more detailed manner, we propose here a second synoptic analysis with the frontal passage, computed as the maximum gradient of 850 hPa potential temperature, when this gradient is greater than 2 K/100 km, following Schemm et al. (2015). These results are displayed in the figure below:

	(a) Air temperature at 850 hPa	(b) Precipitation	(c) Vertical velocity at 850 hPa
E1			


E2
ERA5 E2:2020-01-09 15h00
E3
ERA5 E3:2020-01-25 01h00
E4
ERA5 E4:2020-03-04 22h00
E5
ERA5 E5:2020-05-10 12h00
E6
g
ERA5 E6:2020-08-09 10h00
AMS
latitude (°N)
longitude (°E)
850hPa Air Temperature (°C)
Precip. (mm day-1)
850hPa vertical velocity (Pa s-1)


E7
E8
E9
E10
E11
ERA5 E6:2020-08-09 10h00
ERA5 E7:2021-03-08 14h00
ERA5 E7:2021-03-08 14h00
ERA5 E7:2021-03-08 14h00
ERA5 E8:2021-06-07 05h00
ERA5 E8:2021-06-07 05h00
ERA5 E8:2021-06-07 05h00
ERA5 E9:2021-06-23 23h00
ERA5 E9:2021-06-23 23h00
ERA5 E9:2021-06-23 23h00
ERA5 E10:2021-11-08 01h00
ERA5 E10:2021-11-08 01h00
ERA5 E10:2021-11-08 01h00
ERA5 E11:2021-12-06 07h00
ERA5 E11:2021-12-06 07h00
AMS
latitude (°N)
longitude (°E)
850hPa Air Temperature (°C)
Precip. (mm day-1)
850hPa vertical velocity (Pa s-1)

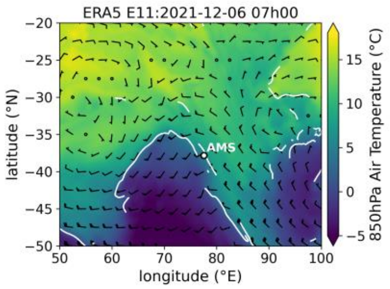


Figure R4: synoptic analysis using hourly ERA5 fields at the time of observed minimum $\delta^{18}O_v$ correspondint to the 11 events identified in the man,uscript (numbered E1 to E11): (a) air temperature at 850 hPa, (b) precipitation, and (c) vertical velocity at 850 hPa. White and black lines represent frontal passage, located at the maximum gradient of 850 hPa potential temperature. Front is computed as the zero-line of the gradient of the magnitude of the gradient of 850hPa air temperature, when the gradient of 850hPa air temperature is greater than 2 K/100 km, following Schemm et al. (2015).

Reference :

Schemm, Sebastian, Irina Rudeva, et Ian Simmonds. « Extratropical Fronts in the Lower Troposphere–Global Perspectives Obtained from Two Automated Methods ». Quarterly Journal of the Royal Meteorological Society 141, no 690 (2015): 1686-98. https://doi.org/10.1002/qj.2471.

◦ The 11 events are chosen using the following criteria: " The green rectangles indicate the period with (1) correlation coefficient >-0.5 between d-excess and d18O of water vapor and (2) occurrence of a negative excursion in water vapor d18O." There are (from eye) other events that could fall into these criteria. For example, before the event in ~March 2021 (6th green rectangle in Fig. 3), there is an event agreeing with criteria (1) and showing a strong decrease in d18Ov. Why are other events not included? And how is a negative excursion in d18Ov defined?

>>> Many thanks for this comment and we agree that the definition of a negative excursion was not clear enough. The reason why the event in March 2021 has not been selected is that after the decrease of the $\delta^{18}O_v$, the $\delta^{18}O_v$ increases again but not to the level it had before the initial decrease. It stays several days on a low plateau.
We will thus precise in the revised manuscript that the $\delta^{18}O_v$ excursions are associated with $\delta^{18}O_v$ negative excursion larger than 2.5 permil (at 6h resolution) on a total length smaller than 24 h (definition of the length of the event is given in the caption of Table 1). We will also precise in the text that the average $\delta^{18}O_v$ 24h before and 24h after the event should not be larger than 1/4th of the amplitude of the $\delta^{18}O_v$ excursion. Note that some excursions were also discarded because of a too large interruption in the water isotopic record (21 March 2020).

◦ The analysis of the 11 events is mainly qualitative, and it is difficult to follow the description of the 4 events from 01-03/2020. All map plots and most of the vertical cross sections of these events are in the Supplementary Information, which makes it difficult to understand the synoptic situation during the events and the model performance. Further, it is not clear why these four events where chosen for a detailed description. Also, the description of the events is scattered in different paragraphs of Sections 3 and 4. The paragraphs should be better structure to lead the reader through the evolution of the cold front events.

The reason why we chose to show only the events from 01-03/2020 in the main text is simply to be able to read the figures easily and avoid too much repetitions. This period was favored because it encompasses several events which were then easily to see on a graph covering 3 months. As we wrote, « This period has been selected for display because it encompasses 4 out the 11 negative excursions of $\delta^{18}O_v$, but the extended comparison over the whole 2 years period is displayed on Figure S1. » and the same conclusions can be drawn if we consider the 11 events (all discussed in Table 1). To support this assumption, we provide online the figures corresponding to each event (such as current Figure 5 for the events of January 2020).

Following this comment, we will also reorganise the sections 3 and 4 to improve readibility and add more figures from the supplement in the main text. It is probably not possible to have the figures for all models in the main text but probably some figures from the ECHAMwiso model showing the best agreement with the data.

Further general comments:

• The section headings are not very specific. While Section 3 "Data description" has many short subsections, Section 4 "Discussion" has no subsection while introducding a lot of information and new analyses.

>>> This is a very valid comment and we will include subsection titles in the discussion so that it will be easier to follow the argumentation.

• Water isotope measurements:
Various information is missing in the description of the water isotope measurements:
◦ What was the material and length of the inlet line to the Picarro instrument? Was the inlet line heated ?

>>> The inlet line was indeed heated (40°C) and the 5 m inlet tube was of PFA. This will be explained in the new manuscript.

◦ Was the water vapour mixing ratio measured by Picarro calibrated? How does it compare to other humidity measurements on the Island?

>>> The calibration of water vapour mixing ratio was done in the laboratory before sending the instrument and this protocol is valid as this calibration depends on the laser cavity configurations. On the field, we found an excellent agreement between mixing ratio measured by the Picarro and by the weather station (the difference between the two records always stays below 2% and there is no systematic shift between the two records). This will be added in the revised version.

◦ When was the humidity-isotope dependency calibration done and what kind of calibration device was used?

>>> The humidity-isotope dependency calibration was checked every year and the calibration device is the standard delivery module by Picarro. These explanations will be given in the new manuscript.

Detailed comments:
Lines 68-69: What is (18O/16O) and (D/H)? Does this represent the isotopic ratio?

>> Indeed, these are isotopic ratios between heavy and light isotopes. This will be added.

Lines 90-93: “For this objective, several instruments have been installed either in observatory stations ... or on boat ...”.
Is there a reason that this summary omits aircraft measurements?

>> Many thanks for this comment, we will also mention aircraft measurements adding a reference to the following study as an example :

Henze, D., Noone, D., and Toohey, D.: Aircraft measurements of water vapor heavy isotope ratios in the marine boundary layer and lower troposphere during ORACLES, Earth Syst. Sci. Data, 14, 1811–1829, https://doi.org/10.5194/essd-14-1811-2022, 2022.

Lines 97-99: “Such data comparison enables one to test the performances of the models either in the simulation of the dynamic of the atmospheric water cycle or in the implementation of the water isotopes.”
I agree with this statement but I don’t see how this study adds any new knowledge on model performance or isotope parametrisations. Can you elaborate further?

>>> In the two models presented here, a very similar approach has been followed for water isotopes implementation. Because one model is able to reproduce well the observed water vapor $\delta^{18}O_v$ excursions, we can conclude that the isotopes parameterisation is appropriate (at least in this region). It means that the reason why the other model is not able to reproduce the excursion is not due to isotopic parameterisation but to the modeled atmospheric dynamic, most probably the horizontal resolution. As mentionned in the answer to a comment above, we will have a couple of sentences explaining that we can validate the implementation of water isotopes physics in the models by this model – data comparison.

Line 101-102: “This region is poorly documented with present-day observations despite its primary importance in governing CO2 sinks”
Do you have a reference for this statement?

>>> We propose to add the following reference.

Khatiwala, S., Primeau, F., & Hall, T. (2009). Reconstruction of the history of anthropogenic CO_2 concentrations in the ocean. *Nature*, 462(7271), 346–349. https://doi.org/10.1038/nature08526

Line 102-105: “Moreover, we lack precise descriptions of atmospheric processes associated with cloud microphysics and surface-atmosphere exchange in polar regions, and the evolution of westerly wind locations and strength (Fogt and Marshall, 2020).”
Why is this relevant for the presented study? The study site lies in the mid-latitudes.

>> We agree that this sentence is more general and not really adapted for this study in particular. Actually, it was more referring to the following paragraph where we explain that our initial aim to have an instrument at Amsterdam Island was to fill a gap between our observation in polar regions and in La Réunion. In the polar regions for example, we are concerned by the surface-atmosphere exchange. We will thus rewrite these paragraphs to better explain this general strategy.

Line 134: “Climate is temperate, generally mild with frequent presence of clouds.”
What do you mean with frequent presence of clouds?

>> We will precise this statement and better say that the average total sunshine hours is 1581 hours per year from the period 1981 - 2010 day statistics of MeteoFrance (https://donneespubliques.meteofrance.fr/FichesClim/FICHECLIM_98404002.pdf)

Lines 149-150: “CO_2, CO, CH_4 and Hg species have been continuously measured since 1980, 2014, and 2012 respectively.”There are four species but only three years are mentioned. It is not evident which species belongs to which year.

We propose to replace: "CO_2, CO, CH_4 and Hg species have been continuously measured since 1980, 2014, and 2012 respectively." by

« From 1980 to 2011, CO_2 and CO species have been monitored by non-dispersive infrared measurements (NDIR) systems. From 2012, dry air mole fractions of these two greenhouse species are currently measured by cavity ring down spectroscopy (CRDS) using commercial Picarro analyzers model G2401. Methane (CH_4) and nitrous oxide (N_2O) are also measured continuously using the same instrument model, but since 2014 and 2018 respectively, while Hg species are monitored since 2012 (see section 2.2.2.). »

Lines 160-167: The elevation of the two meteorological stations at Pointe Benedict observatory is given in meters agl, while the elevation of the station at Martin-de-Viviegs is given in meters asl. This makes it difficult to compare the elevations. Further, this paragraph gives a lot of detail on variables that are not shown later on.

The pointe Benedicte station is located 70 m above sea level. As a consequence, the meteorological stations referred here are 95-100 m above sea level or 25 m above ground level. We will try to simplify this paragraph.

Line 165: What is IGE?

>> IGE is the “Institut des Geosciences de l’Environnement”, it will be explained in the revised version.

Line 187: What is STP?

>> It means “Standard temperature and pressure” and will be explained in the new manuscript. Also there was a mistake in the first manuscript since the temperature should be 273.15 K.

Line 202: “high altitude air masses (lower/ upper troposphere, or even above)” This is very unspecific. What do you mean with lower/upper troposphere?

>> There is not a precise altitude above which the GEM profile shows a decrease and is replaced by Hg oxidized species. The observations show that when we are above the free troposphere (in general above 5-6 km) and in the low stratosphere and when there is no biomass burning transportation from Africa, the GEM concentrations decrease with height (and this is the inverse for oxidized species). We thus propose to replace the following text:

"In this study, atmospheric GEM is used as potential tracer of intrusion and/or subsidence of high altitude air masses (lower/ upper troposphere, or even above) that may possibly impact the atmospheric records in Pointe Benedicte Observatory which collects marine boundary layer most of the time" by:

"In this study, and even if long-range transport and a variable tropopause height may modulate, atmospheric GEM is used as potential tracer of stratosphere-to-troposphere intrusion and/or subsidence of upper troposphere (above 5-6 kms) that may possibly impact the atmospheric records in Pointe Benedicte Observatory which collects marine boundary layer most of the time"

Lines 204-205: " As mentioned above, mercury in the atmosphere is detected in three defined forms:"This has not been properly introduced earlier.

>>> We propose to replace the first sentence of section 2.2 by:

"Mercury (Hg) in the atmosphere consists of three forms: gaseous elemental Hg (GEM), gaseous oxidized Hg (GOM) and particulate-bound Hg (PBM) . Gaseous Elemental Mercury, dominant form (> 90%) of natural and anthropogenic Hg emissions transported globally through the atmosphere (Krabbenhof and Sunderland, 2013; Driscoll et al., 2013; Sprovieri et al., 2016), is the one the IPEV GMOStral-1028 observatory program is concentrating its analytical strengths, at the Pointe Benedicte atmospheric research facility. Data are freely...."

With the additional references:

Krabbenhof, D. P. & Sunderland, E. M. Global change and mercury. Science 341, 1457–1458 (2013).

Driscoll, C. T., Mason, R. P., Chan, H. M., Jacob, D. J. & Pirrone, N. Mercury as a global pollutant: sources, pathways, and efects. Environ. Sci. Technol. 47, 4967–4983 (2013).

Sprovieri, F. et al. Atmospheric mercury concentrations observed at ground-based monitoring sites globally distributed in the framework of the GMOS network. Atmos. Chem. Phys. 16, 11915–11935 (2016).

Line 233: "outside at ~ 6 m above ground level." What is this relative to m agl/asl, i.e. compared to the other measurements?

>> See previous answers to comment. It was indeed not very clear, 6 m above ground level (agl) is then 76 m above sea level. We will give this last number in the revised version.

Line 279: What do mean with "quadratic error"?

>> Because d-excess is defined as d-excess = $\delta D-8\times\delta^{18}O$ (to be added in the new version), the "quadratic error" on d-excess (σ^2) is calculated as the root square of ($\sigma_{\delta D}{}^2+64\times\sigma_{\delta 18O}{}^2$). The term "quadratic error" is confusing and will be replaced by "uncertainty" as for the $\delta^{18}O$ and δD uncertainties given above.

Line 290: Why are you starting the trajectories at 100m a.s.l.?

>> Because our instrument is at the surface and the Pointe Bénédicte observatory is 70 m above the ground, we chose the 100 m level. But you are right that we could also have taken a lower level. We have checked that we obtain exactly the same pattern when starting the back-trajectories at 100 m or 50 m high.

Line 324: “high spatial”: 0.9° horizontal resolution is high compared to the LMDZiso simulation of this study but low compared to convection permitting climate simulations. I would therefore skip “high”.

>> OK

Line 389: “d-excess of the precipitation”. I don’t see this in Fig. 3.

>> Indeed, we did not show this record because it does not add much to the study. This will be explained in the new version of the manuscript.

Line 395, 395, 399: What is R_2? The correlation coefficient R to the power of 2? What kind of correlation are you calculating? R_2 is used before R is introduced.

>> R2 is the coefficient of determination for a linear regression between the time series at hourly resolution. We will precise “with R2 being the coefficient of determination for a linear regression”.

Line 399-400. “...(R is calculated continuously from hourly records in 8 consecutive days)...”
Do you mean that you used an 8-day moving window?

>> Yes, this is correct and the wording will be changed

Line 402-403: “d-$excess_v$” has not been introduced.

>> It will be introduced as”d-$excess_v$ = δD_v - 8×$\delta^{18}O_v$”

Lines 434 – 436: “...the agreement with measured precipitation amount is better for ECHAM6-wiso (R2 = 0.45) than for LMDZ-iso (R2 = 0.08 – 0.13 for VLR – LR)...”
The correlation of LMDZ-iso with the measurements is close to zero, i.e. there seems to be nearly no agreement. The statement that the agreement with measured precipitation is better for ECHAM6-wiso than for LMDZ-iso seems weak in this context.

>>> We will change the wording and say that the correlation for LMDZiso is close to zero while there is a correlation for ECHAM6-wiso.

Lines 437-439: “...they are in general more strongly expressed in the data series than in the model series which is only partly due to the hourly resolution of the d18Ov record compared to the 3h and 6h resolution of the outputs of the LMDZ-iso...).” What is the basis of this conclusion?

>> Our idea is simply to see if part of the disagreement between data and model is linked to the fact that the models have only a 3h or 6h resolution while the data were displayed at 1h resolution. To test this, we have reinterpoled our data at 6h resolution in the figure 4 and calculated the amplitude of the water vapor $\delta^{18}O_v$ excursions with data resampled at 6h resolution. We will better explain that our aim was here to look if the difference resolution between the models and between models and data can explain the bad agreement between model outputs and data for the LMDZ-iso model.

Linbes 450-451: “ They always occurred during low pressure periods (atmospheric pressure below 1005 mbar).” What is the synoptic situation leading to this low pressure and cold fronts?

>> As mentioned above, we will remove this reference to cold fronts because it was confusing and we actually wanted to focus on the water vapor $\delta^{18}O_v$ excursions. So we do not think that we need to include detailed descriptions of the synoptic situation.

Lines 501-503: "However, for the 11 events highlighted above, the d18Ov vs qv evolution follows an evolution characteristic of remoistening processes, i.e. a curve standing below the curve of the d18Ov vs qv evolution observed for the rest of the series…"
The single events show a much steeper evolution in the d18Ov-qv diagram than the remoistening curve. Why is this?

>> You are correct. As mentioned as answer to the editor, this figure is a "first order" approach following previous study of Guilpart et al. (2017) but is not 100% appropriate since the simple modeling curves are idealized $\delta^{18}O_v$ vs q_v trajectories calculated for the free troposphere and we are looking at surface records which are strongly influenced also by vertical and horizontal atmosphere dynamic. We thus do not expect the curves to be aligned with our events and we simply use them at first approach to show that enhanced remoistening or water-rain interactions may be a good candidate to explain the $\delta^{18}O_v$ vs q_v relationship during the events.

We propose to better explain as:
" Even if the water vapor $\delta^{18}O_v$ vs q_v evolution is rather steep, there is some resemblance with the idealized theoretical curve for remoistening calculated for the free troposphere (Noone, 2012). Even if the analogy between our measurements at the surface and the free troposphere should be taken with cautious, the fact that the water vapor $\delta^{18}O_v$ vs q_v evolution lies below the idealized curve for condensation processes supports the depleting effect of vapor-rain interactions for our negative water vapor $\delta^{18}O_v$ excursions (Worden et al., 2007; Noone, 2012). "

Lines 504-506: "Since relative humidity is relatively high during these events (values given in Table 1 compared to a mean value of 77 %), it more likely reflects rain-vapor diffusive exchanges than rain evaporation." Are you referring to the relative humidity at the surface? How about relative humidity above that will also influence the interaction of the rain with its surroundings?

>> This is correct. We should refer here to local surface re-evaporation only. We fully agree that rain - vapor interaction (including rain evaporation) in the upper atmosphere can have an influence as shown in Figure 8 and in l. 585 - 588. This will be explained better in the new version.

Line 519ff.: As the trajectories are only shown in the supplement, it is difficult to follow this paragraph. The beginning of the paragraph leads to think that the trajectories indicate that subsidence is import but in the end the conclusion is that "back trajectories are however not supporting systematic subsidence for other cases".

>> Indeed, the backtrajectories do not evidence systematic air subsidence for the water vapor $\delta^{18}O_v$ excursions and this is the reason why the figures were initially put in the supplement but we agree that it is better to have them in the main text. Actually, when looking in details the atmospheric dynamic, it is clear that the $\delta^{18}O_v$ excursions occur at the transition between ascendence and subsidence and this is probably the reason why we could not easily detect it from 10 days back-trajectories. This will be better explained in the text with back-trajectories of 5 days only (cf next answer to comment and figure R5).

Lines 526-527: “... the maximum altitude of the envelope of the back trajectories increases from 5,000 to 8,000 m...”

What it the mean/standard deviation of the maximum trajectory height? How many days before arrival are the trajectories at their maximum height? How relevant is this for the isotopic composition upon arrival? E.g. if the trajectories descend over the ocean and take up moisture, their maximum height before the moisture take-up is less relevant for the isotopic composition at Amsterdam Island. If you are using the full 10-days backward trajectories to calculate the maximum altitude, I don’t think that the maximum altitude is a good measure of subsidence in front of the cold front.

>> Actually, what we wanted to test initially was if there is a change of the origin of the air mass during the excursion which may explain different isotopic signature of the water vapor. To answer the questions, we propose a new representation of the back-trajectories on only 5 days and with the indication of the location of the average (using humidity weighting) back-trajectory. We propose also to better explain that back-trajectories are used to mainly study the change of air origin.

As an example, we show below the new figure S2 that we propose (on the excursion of the 3-4 January 2020):

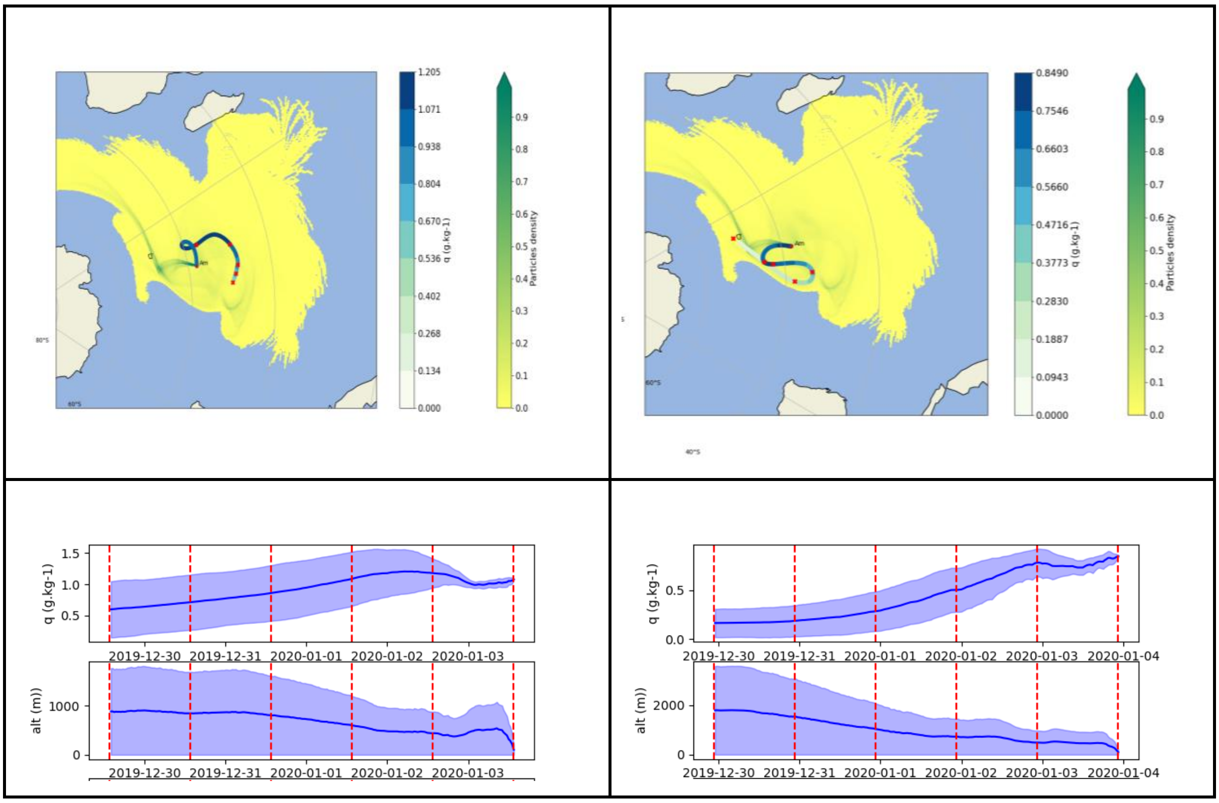


Figure R5 : FLEXPART footprints in 2D projections for the event of the 3rd-4th of January. The colors on each grid point of these projections represent the density of particles over the 5-day back trajectories (1000 particules per launch). A dark red color indicates a zone with a high concentration of particles, hence a region from which a large part of the air mass originates. a: latitude-longitude projection of the FLEXPART back trajectory footprint for the 3rd of January 2020 at 13h30. b: same as a for the 3rd of January 2020 at 22h30. c: left is the longitude-altitude projection of the FLEXPART back trajectory footprint for the 3rd of January 2020 at 13h30; right is the latitude-altitude projection of the FLEXPART back trajectory footprint for the 3rd of January 2020 at 13h30. d: same as (a) for the 3rd of January 2020 at 22h30.

Lines 543-555: " There is no evidence for changes in the horizontal advection of air over the 11 particular events from the observation of wind direction around these cold front events."

How is the cold front identified? Does it divide different air masses? A cold front normally implies a horizontal transport of air, why is this not the case for these cold fronts?

>> The cold front were initially identified with the synoptic weather charts as explained in answers to other comments. As mentioned elsewhere, the reference to these cold fronts will be muted in the new version.

Lines 556-558: "Such abrupt d18Ov events can hence be used as a test of the performances of general circulation models equipped with water isotopes."

What was learned about the performance of the GCMs involved in this study? Was it necessary to include d18O in such a performance test instead of just using traditional humidity variables (e.g. relative humidity, specific humidity, precipitation)?

>> You are right that many performances could already be tested using only meteorological data. This is the reason why we focused here only on the periods when water vapor $\delta^{18}O_v$ was showing a different signal than the one inferred from humidity to study what we can learn from this signal. Also, the combination with Hg measurements suggesting subsidence was useful to complement the traditional variable records over these excursions. Finally, because more and more models are equipped with water isotopes, we wanted to show an example of the peculiar signal seen in the water isotopes and not in traditional humidity variable to test the performances of these models, both on the implementation of water isotopes and on the dynamic of the atmospheric water cycle. This will be explained better in the revised version of the manuscript.

Line 562: What is "SOM"?

>> Supplementary Online Material, it will be explained in the revised version

Lines 559-584: As both isotope-enabled models were nudged to ERA5 dynamics, it is to be expected that the GCMs reproduce the ERA5 reanalysis wind fields rather well with some caveats due to the lower horizontal resolution. This paragraph (and Fig.7) is mostly describing the smoothing of ERA5 due to the coarser resolution of the isotope-enables GCMs. Why do we need isotope measurements to see the effect of a coarser horizontal resolution? What do we learn about the GCM performances by decreasing the horizontal resolution?

>> It is true that the GCMs are nudged to reanalyses. As mentioned in other answers to comments, the idea with water isotopes is to have a record at the surface of what is happening above in the atmosphere (without relying on reanalyses only). Also, we wanted to test, with different resolutions,

if the implementation of water isotopes was correct in the model or if the disagreement between models and data.

Lines 586-590: "A rain event is associated with a strong ascending column in which d18Ov is depleted by progressive precipitation during the ascent and by interaction between rain and water vapor. This ascending column is coupled to the subsidence of d18Ov depleted air at the rear of the event which is pushed toward Amsterdam Island through a south west advection of cold air."

How is the isotopic composition of the subsiding air behind the cold front connected to the progressive precipitation during the ascent? Can the ascending and descending column be differentiated in the d18O excursions?

>> This is a very valid question which is not so simple to answer with the model outputs since we do not have water tagging. Still, we did a simple analysis using the outputs of the ECHAM6-wiso model looking at the water vapor $\delta^{18}O_v$ vs q_v in front of the event (ascending column), during the event and at the rear of the event (subsiding column) (Figure R6).

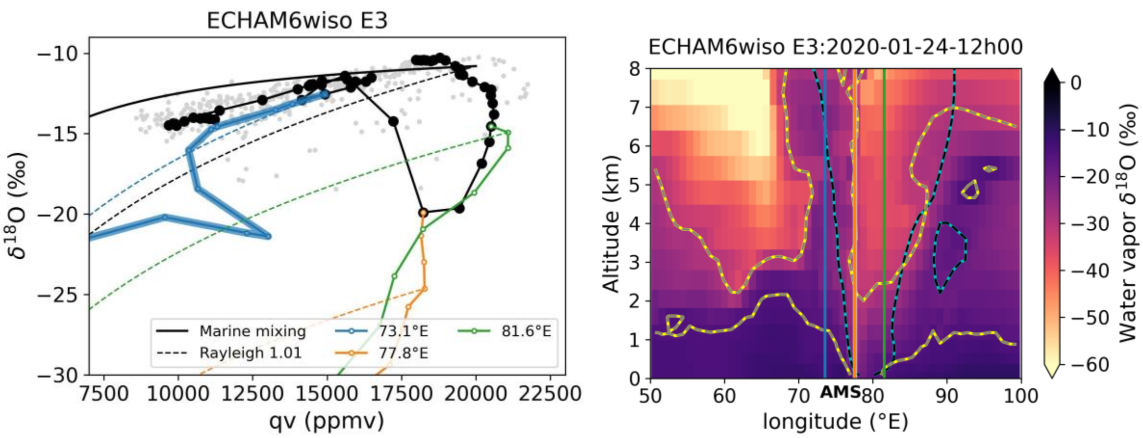


Figure R6: (left) Evolution of the water vapor $\delta^{18}O_v$ vs q_v in ECHAM6wiso, at the surface at the nearest grid cell to Amsterdam island for all time steps between 01/01/2020 and 31/03/2020 (grey dots), at the surface for event 3 (24th of January 2020) at the latitude of Amsterdam and for all longitudes between 50°E and 100°E (black dots), and for three atmospheric columns at the time of the event (plain colored lines). The three vertical atmospheric columns are taken as best representations of the situation before the water vapor $\delta^{18}O_v$ anomaly of the 24th of January 2020, as an example. Ascending column is represented by the vertical atmospheric column at 81.6°E (upstream of / before the anomalous $\delta^{18}O_v$ event, green line); the situation during the event can be visualized as the vertical atmospheric column at 77.8°E, orange line; the vertical atmospheric column after the event (downstream) can be visualized by the column at 73.1°E, blue line. Vertical velocity directed downward is represented by a thick line (only present for the blue line). The black line indicates the distribution $\delta^{18}O_v$ vs q_v for marine mixing and the dashed lines show Rayleigh distillation distributions. (right) as in Figure 7b of the article, but for event 3, and showing the location of the three atmospheric columns with same color lines as in the right.

The period before the $\delta^{18}O_v$ anomaly corresponds in most cases to the end of the rain event. This period is associated with a strong lift of the moist air in which we see a water vapor $\delta^{18}O_v$ vs qv distribution (low $\delta^{18}O_v$ with high humidity, green curve on Figure R6) which looks like an extreme case of remoistening (super Rayleigh as described in the Figure 6 of the main manuscript). We can interpret this as ongoing rain-steam exchanges. The situation is different from what happens at the surface where water vapor $\delta^{18}O_v$ vs q_v is not showing any anomalous behavior yet.

During the event (orange curve on Figure R6), the water vapor $\delta^{18}O_v$ vs q_v evolution is completely vertical and really difficult to explain by only remoistening effect. We thus believe that a dynamic aspect (mixing) is also involved in bringing in the surface boundary layer low water vapor $\delta^{18}O_v$ with relatively high humidity.

After the event (blue curve on Figure R6), we are back to a classic Rayleigh situation, so the water vapor $\delta^{18}O_v$ returns to its initial value.

Line 621: "hours/days". Is there a cold front passage that has a duration of several days?

>> You are right, we do not have cold fronts lasting several days over the Amsterdam Island. As noted in previous answers to comments, we did put too much emphasis on cold fronts which was misleading. We will remove "associated with cold fronts" on l. 623.

Lines 635-640: "This study highlighted the added value of combining different data from an atmospheric observatory to understand the dynamic of the atmospheric circulation. The two-year records are also a good benchmark for model evaluation. We have especially shown that the isotopic composition of water vapor is a powerful tool to identify aspects to be improved in the general circulation models, such as the horizontal resolution which may influence the representativity of the vertical dynamics."

As also mention above, why are stable water isotopes needed to show that the horizontal resolution may influence the vertical dynamics? The vertical cross sections of vertical wind speed (Fig. 7) illustrates this already quite well.

>> This comment has been addressed in the list of major comments above.

on the Zenodo platform, and the dataset does not include the water vapour mixing ratio.

>>> We will complete the dataset with a version 2 after acceptation of the manuscript including water vapour mixing ratio.

Figures:

General: The figures are often difficult to read, especially the described phenomena are small (e.g. d18O excursions of a few hours in a 2-year or 3 month timeline). Additionally, the colors are not color-blinded friendly and the caption are not concise.

>> We agree that it is difficult to see the excursions on a 2-year timeline and this is the reason why it has been decomposed in several figures of 3-months. In addition, some excursions are detailed in the main text. We propose to provide in the supplementary material or in the appendix a focus on each excursion like Figure 5. The colors have been changed on Figure 4 as shown below.

Fig.1: Is this figure needed? Fig.1 is not mentioned in the text.

>> We will mention it in the text at first instance. If the editor wants to remove it, it is also fine of course.

Fig.3: x-axis too coarse, light green shading difficult to see.

We will change to the following figure:

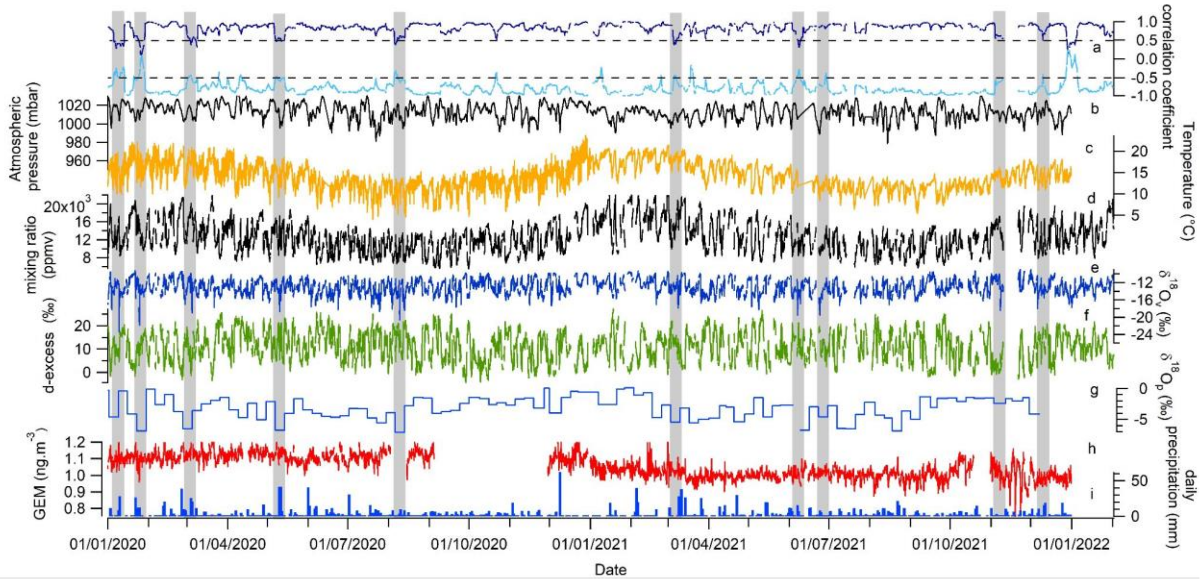
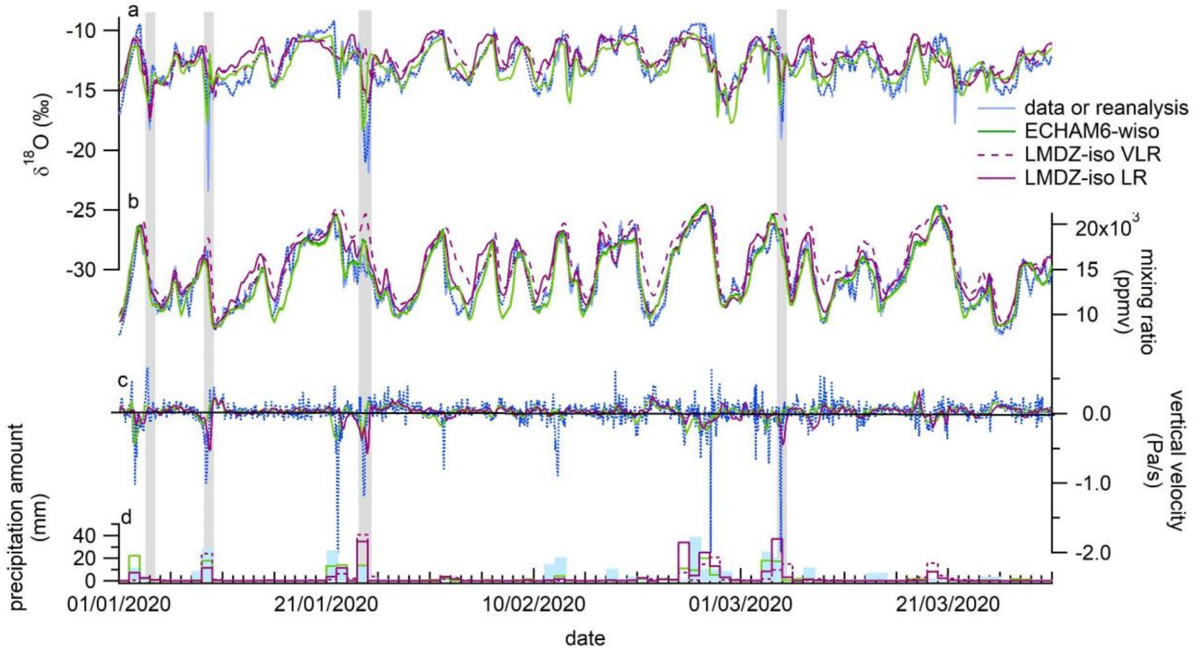


Fig. 4: Colors red/blue/green are not colorblind friendly. The caption text includes many repetition and should be improved. A legend in the figure could improve the readability.

>> We propose to have this modified figure (new color code) which will also enable to shorten the caption (since information is on the legend in the figure):



On this panel, we also removed the vertical velocity at 500 hPa from panel c to improve readability and because it did not bring much additional information (the information is given in Table 1).

Fig. 6: What is ϕ=0.025?

This value is indicated together with the "remoistening curve" which has been obtained following the expression in Noone (2012). Basically, the idea is to express remoistening through a modification of the equilibrium fractionation coefficient between water vapor and rain (α_e) so that the effective fractionation factor will be $\alpha=(1+\phi)\times\alpha_e$

This effective fractionation coefficient is then introduced in the Rayleigh distillation equation to deduce the link between $\delta^{18}O_v$ and mixing ratio:

$(\delta-\delta_0)=(\alpha-1)*\ln(q/q_0)$

We will provide this information in the revised version of the manuscript.

Figure 8: What is SBL? The ascent of air in front of the cold front rises nearly vertical at a constant longitudinal position. As a cold front is moving system (mostly associated with an extratropical cyclone), the ascent does not occur at a constant location (in latitude and longitude). Further, all precipitation seems to fall in front of the cold front, which is unlikely.

SBL is Surface Boundary Layer, it will be changed by Marine Boundary Layer.

We agree that we need to include a more detailed analysis of the synoptic situation during the events. In Figure R4 shown above, we show front location, precipitation and 850 hPa vertical velocities from ERA5 at the time of the events. Precipitation generally falls just ahead of the cold front.

Figure 8 scheme is based on the profiles modelled by ECHAM for event 2 (09/01/2020). We show these profiles below (Figure R7). We will make this clear in the new legend to Figure 8 and add these profiles to the Supplement. We will show the same profiles for all events in the revised Supplement, but you can see in the current Supplement that these profiles share similar patterns for all events.

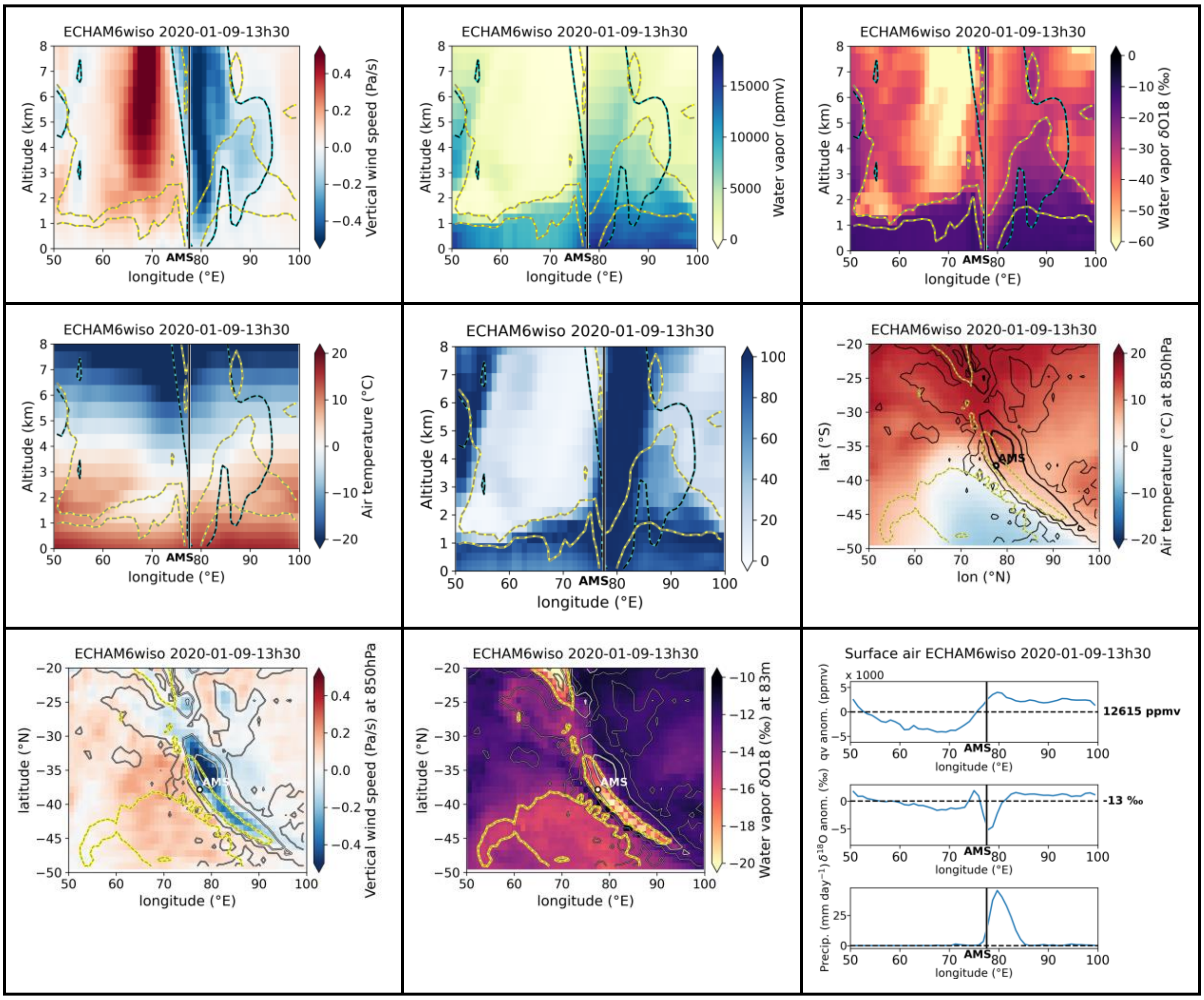


Figure R7: ECHAM6wiso profiles during Event 2, used to design Figure 8.

How is the subsidence at 100°E and ascent at 90°E related to the cold front?

The subsidence at 100°E seems to be linked to background conditions, while ascent at 90°E is caused by the cold front moving eastwards, causing precipitations just ahead of the front (Figures R4 and R7)

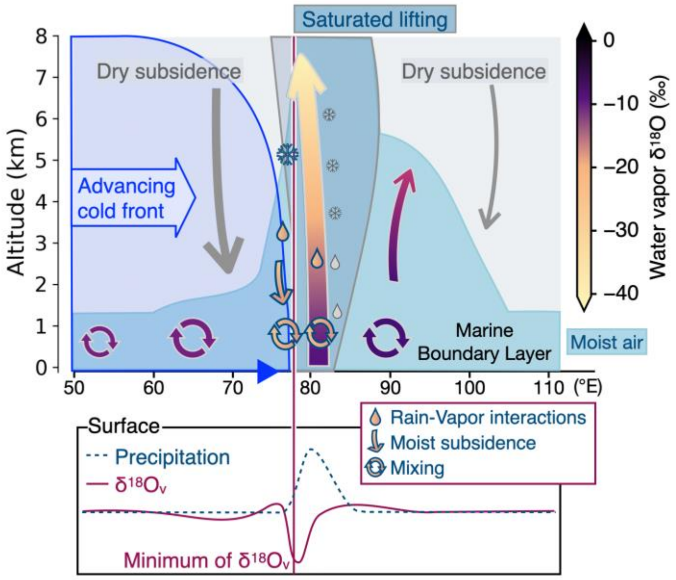
What does a composite of precipitation and d18Ov for all cold front events look like?

>> We already tried to make a composite before the first version of the manuscript but because the water $\delta^{18}O_v$ anomalies have different amplitude and durations (Table 1) and precipitation amount is very different from one event to the other (some event being associated with no rain, Table 1), a composite does not show any useful additional value.

Can it reproduce the schematic as shown in the "surface box"?

The surface box of the schematic follows the surface state modeled by ECHAM for event 2 (09/01/2020) (Figure R7, lower right plots).

We propose this new version for the Figure 8:



Technical comments

Generally: there are many abbreviations in the text that are only used a few times. Can you reduce the number of abbreviations?

>> This is true and will be modified in the next version.

Line 60-61 (and many others): The references are not in chronological order.

>> We will correct this.

Line 65-66: "We express the abundance of the heavy isotopes D and 18O with respect to the amount of light isotopes 16O and H in the water molecules…" should be "We express the abundance of the heavy isotopes D and 18O with respect to the amount of light isotopes **H and 16O, respectively,** in the water molecules…"

>> This will be modified

Line 68: Eq. 1 has strange symbols (squares).*

>> It is a problem from the word to pdf conversion, it should be possible to change it

Line 88-89: "water cycle processes such as water cycle processes such as "

>> This will be corrected

Lines 106-109: "Over the previous years, we have installed 3 water vapor analyzers on Reunion Island at the Maido observatory (21.079°S, 55.383°E, 2160m) (Guilpart et al., 2017) and in Antarctica (Dumont d'Urville and Concordia; (Leroy-Dos Santos et al., 2021; Breant et al., 2019; Casado et al., 2016). " Check usage of brackets.

>> This will be corrected following the editorial style

Line 133: "from the nearest lands, Madagascar**,** and"

>> It will be added

Lines 140-141: "...and were continuously monitored at the site **since** 1960..."

>> This will be corrected

Lines 145-150: The section is very difficult to read, the websites and datasets should better be included as references. Same for link to AERIS on line 178 and 200.

>> References will be given instead

Lines 180-181: "instrument models (Tekran Inc., Toronto, Canada) (Angot et al., 2014; Slemr et al., 2015, 2020; Sprovieri et al., 2016; Li et al., 2023). "

>>> This will be simplified in the new version.

Line 202: "may possibly" Doubling, omit either.

>> We will remove "possibly"

Lines 211-221: This sentence is too long. Can you divide into two sentences?

>>> Sure, we propose this new formulation:

"Chemical cycling and spatiotemporal distribution of mercury in the air is still poorly understood whatever atmospheric layer considered (surface, mixed or free troposphere, stratosphere), and complete GEM oxidation schemes remain still unclear (Shah et al., 2021 and associated references). Still, several studies provided evidence that vertical distribution of atmospheric mercury measurements from boundary layer to lower/upper troposphere and stratosphere shows a decreasing trend in GEM concentration with increasing altitude, in parallel with an increase in the concentration of divalent mercury (GOM + PBM) resulting from GEM oxidation mechanisms (Murphy et al., 2006 ; Swartzendruber et al., 2006, 2008 ; Talbot et al., 2007 ; Fain et al., 2009 ; Sheu et al., 2010 ; Lyman and Jaffe, 2012 ; Brooks et al., 2014 ; Fu et al., 2016 ; Koenig et al., 2023). "

Line 228: "The isotopic composition of near-surface water vapor (d18Ov and dDv in ‰ versus SMOW)"

>> It will be modified

Line 242: "The calibration of the data is performed in different**several** steps following previous studies"

>> It will be modified

Lines 301-303: "...identical **to** the atmospheric setup of IPSL-CM6A (Boucher et al., 2020) used for phase 6 of the Coupled Model Intercomparison Project (CMIP6, (Eyring et al., 2016)). "

>> It will be modified

Line 373: "… very close to the one observed in Angot et al**.** (2014)."

>> It will be modified

Line 374: "During the period (2020-2021) of water vapor isotope measurements in AMS…"

Do you mean: During the period 2020-2021 of water vapor isotope measurements in AMS.. ?

>> Yes, this will be changed

Lines 390-391: "The annual cycles are also not visible…" Do you mean: "An annual cycle is not visible…" ?

>> Yes, this will be changed