Dear editor,

Please find below the response to the comments from both you and the referees. We would like to thank you and both referees again for the comments on the manuscript which we believe, improved the clarity of the message.

On the behalf of all the co-authors,

Mathieu Casado

Editor's comments:

Dear Mathieu
I am pleased to inform you that your manuscript can be published with some minor revisions, as asked by the reviewer. I also suggest for clarity to add a list of author contributions.
I am looking forward to your revised version as soon as possible.
Best regards
Martin Schneebeli
Editor TC

We have included author contributions to the manuscript accordingly.

Referee 2 report:

General comments: The revised manuscript shows significant improvements in the structure and it is easier understandable for the reader. The crosslinking between the texts and the figures were significant improved and the individual contents are adequate to get the context. But I would still recommend some small corrections (see specific and detailed comments) before publications.

Specific comment 1: It would be interesting to see in the figures (e.g. figure 3, 5 and 7) the measured wind speed at the location (if available). In the experiments by Ebner et al. 2017, one can see a strong interaction between snow and airflow. Maybe there is also a link between temperature and wind speed on the isotopic composition of the snow.

Most of the wind speed data are available. We had previously tried to compare wind speed with the isotopic composition. Comparing the wind speed with the vapour isotopic composition shows interesting results, which were published in Casado et al, 2016 (ACP).

The comparison with the snow isotopic composition is not as straightforward, and we decided not to include them as the results were very difficult to interpret, especially at the seasonal scale (so which would correspond to figure 3). Indeed, the 2-m wind speed presents really strong high frequency variability, and thus, if the hourly data are included in figure 3, it is impossible to see anything. Then, we tried several post-treatment, including comparing the daily average wind speed and the strength of the wind gusts to the isotopic composition. The results were not convincing. For instance, you can see in Fig. R1, the comparison between d18O and wind speed in 2015. The correlation between the two is $r^2 = 0.03$ ($N = 103$). In this situation, it is difficult to interpret the results.

This is different for Figure 5 where we only study a 1 day period, and where the exchanges between vapour and snow are directly targeted. As indicated in the paper from 2016 in ACP, we believe that the impact of wind on boundary layer stratification may have a big impact. We have tried to include the wind speed in figure 5 (See Figure R2). The period we are studying is not particularly interesting in this regard as there is no particular signal in the wind speed. This is coherent with the suggestion of a close box as discussed in the manuscript. This has been added in the discussion, but we believe that the wind speed does not bring any additional information and we would rather not include it in figure 5. See Page 14 Line 23:

> "The wind speed and direction during this event remained constant, around 2.8 $m.s^{-1}$ and 165°."

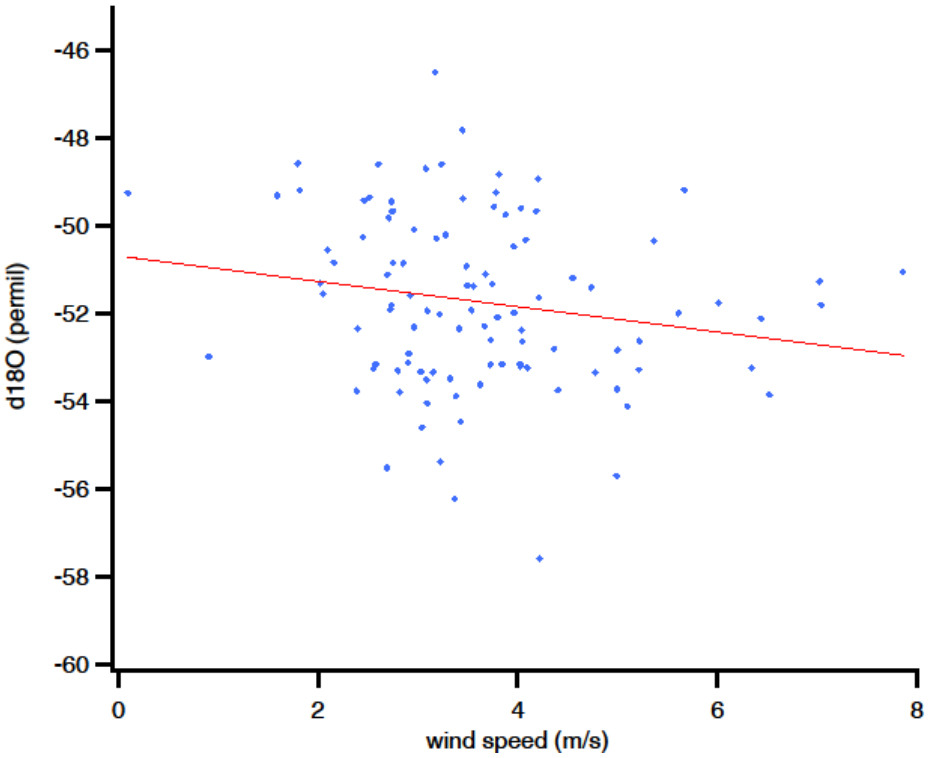

Figure R1: *d18O versus wind speed in 2015 at Dome C.*

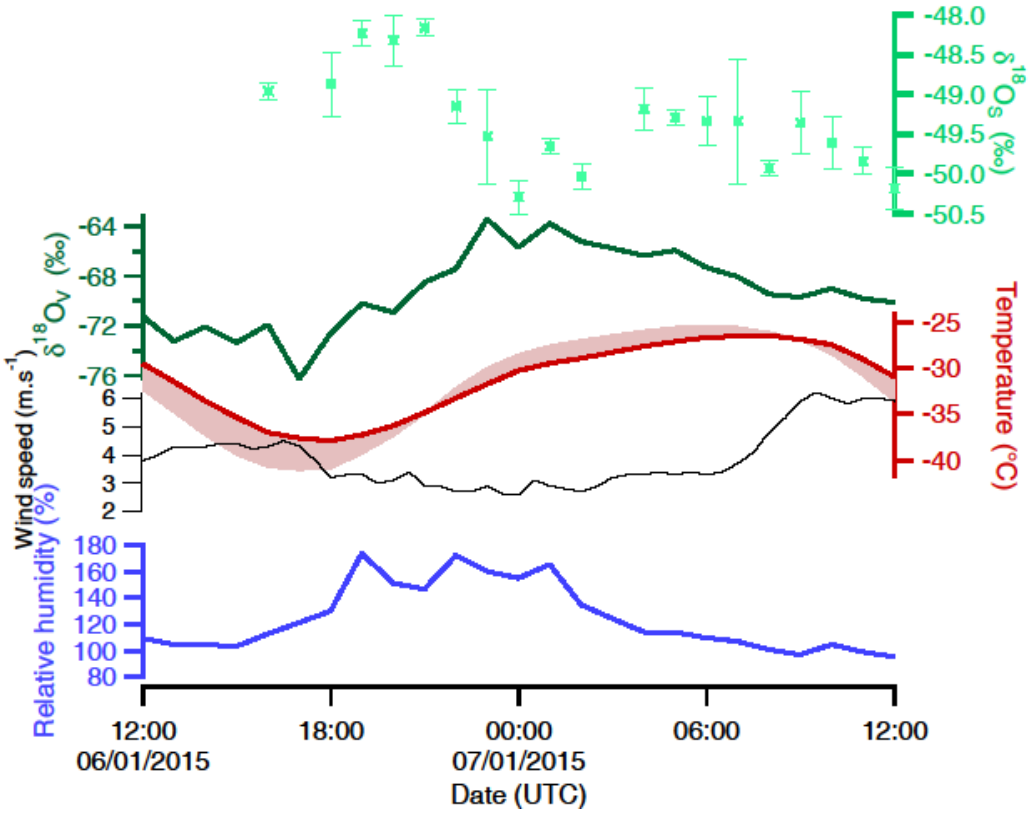

Figure R2: *Modification of the figure 5 with the wind speed during the study period. The period of interest (between 9pm on 6/01/2015 and 2am on 7/01/2015) does not*

exhibit any particular wind speed signal, the value remains at 2.8 m.s^{-1}, which is slightly below the average wind speed (3.3 m.s^{-1}).

We agree with the referee that the wind speed is most likely to be an important variable. It may be important to include it in future studies. Still, we did not manageto make the analogy between the air flow in Ebner et al, 2017 experiment and the surface wind speed in the field. Indeed, the air flow inside the firn is imposed by wind pumping, itself influenced by wind speed but also surface relief which is not well defined in our case. We thus leave this for future studies.

Detailed comment:

Page 17 Line 12: The font of nvDRv18 has to be adapted.

Corrected.

Page 21 Line 3: "… index, SSA decreases under the influence of metamorphism)" -> suggest to delete it -> It's the same statement already mention at the beginning of this sentence.

We were afraid that the opposite behaviour between grain index presented earlier and SSA presented here could confuse the reader, hence the repeated statement. Nevertheless, it has been deleted as suggested.

Page 21 Line 4: "On 19 December, it is likely that a precipitation event occurred" -> why "likely"? Based on the Figure 7, there was a precipitation event, also the days before but without a significant influence on the SSA.

It has been deleted.

Page 23 Line 8: "… to the accumulation rate (around 8 cm)." -> Where is this number from? Maybe I missed it in the manuscript.

We have included the suggested calculation which gives 8 cm in snow equivalent, see page 23, line 6:

> "(7.7 cm in snow equivalent, obtained using the values in Table 1 and an average snow density of 350 kg m^{-3}, see Table 4)"