RESPONSE TO ANONYMOUS REFEREE #1

REVIEW OF DAVRINCHE ET AL., 2023 – UNDERSTANDING THE DRIVERS OF NEAR-SURFACE WINDS IN ADÉLIE LAND, EAST ANTARCTICA

We thank the reviewer for their valuable and helpful comments on the manuscript. We propose to implement the following changes in a revised version.

Black = reviewer comment / Blue = author's response / *Italic* = revised text.

1. Major comments

The method of determining the minimum height above which the potential temperature profile is assumed linear is not explained clearly but seems inaccurate. This is a fundamental aspect of the work presented and will impact all additional results so it is critical that this method is clearly described, accurate and justified. As described starting on line 129 an initial linear vertical potential temperature gradient is estimated between 500 and 350 hPa. I assume that this is done at each 3 h time step anblackd for each model horizontal grid point but this should be stated explicitly in the text.

The interpolation is indeed performed at each 3-hourly time step and for each model horizontal grid point. We will state that explicitly in the text.

The minimum height to be used for the assumed linear potential temperature profile is then based on the height at which the vertical potential temperature gradient exceeds the average gradient between 500 and 350 hPa by a factor of 5. The logic in this seems flawed since what is desired is separating the portion of the profile, near the surface, where the gradient varies with height from further aloft where the gradient is nearly constant with height. Using a constant factor to compare the gradients only determines the height at which the gradient is larger than that in the 500 to 350 hPa layer, which is not the metric that is relevant. In this case what is relevant is assessing how the gradient changes with height - a 2^{nd} derivative of potential temperature with height. When this 2^{nd} derivative becomes small enough the profile can be assumed to be linear. The authors should consider using this more direct way of assessing the height at which the the potential temperature profile switches from being curved to being linear.

Many thanks for this comment. The method we have used is not straightforward and needs to be better justified. We will develop this point in the revised paper. We considered using a criterium based on the 2^{nd} derivative instead of a 1^{st} derivative and found that:

- Both of these methods require to chose a threshold and the choice of this threshold is not obvious
- These methods are equivalent and do not introduce significant changes in the final value of θ_0 for all stations (see Fig. RR2)

Mathematically, the 1^{st} derivative of a linear curve is a constant (the slope), while its 2^{nd} derivative is zero. Thus, in the linear part of the vertical profile of potential temperature, the 1^{st} derivative is a constant (γ) and the 2^{nd} derivative is zero. We chose to define the deviation from the linear part as the height ($H_{5*\gamma_{350-500hPa}}$)at which the 1^{st} vertical derivative becomes greater than a certain threshold ($Thresh_{350-500hPa}$=$5 * \gamma_{350-500hPa}$). Had we chosen to define the deviation from the linear part as the height ($H_{\frac{\partial^2\theta}{\partial z^2}}$) at which the 2^{nd} vertical derivative is no longer equal to zero, we would have had to define a thresshold as well ($Thresh_{\frac{\partial^2\theta}{\partial z^2}}$).

If we use a criterium on the 2^{nd} derivative, we have to define a value $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$ for which, as soon as $\frac{\partial^2\theta}{\partial z^2} > Thresh_{\frac{\partial^2\theta}{\partial z^2}}$, we are no longer in the linear part of the potential temperature profile of the atmosphere. The choice of a value $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$ is not obvious.

- The vertical discretization is different close to the ground than higher up in the atmosphere, meaning that there can be some artificial discontinuities in the 2^{nd} derivative
- The 2nd derivative in the "linear part" is not exactly zero, because the profile is not perfectly linear. Therefore, one must be carefull to define $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$ big enough, so that it does not result in an artificially high value of $H_{\frac{\partial^2\theta}{\partial z^2}}$.
- $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$ cannot be too big, because otherwise, we might miss the deviation and interpolate too low.
- $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$ must be valid for all 3-hourly time step and grid point

Using a threshold on the 2^{nd} derivative requires to find a compromise for the value of $H_{\frac{\partial^2\theta}{\partial z^2}}$ Therefore, a fixed threshold for the 2^{nd} derivative did not appear as an easier method than a threshold for the 1^{st} derivative.

For the choice of $H_{5*\gamma_{350-500hPa}}$, the initial idea was the following:
Most of the profiles at a 3-hourly time step appeared to be approximately linear in the range of Z_{350hPa} and Z_{350hPa}. Therefore, $\gamma_{350-500hPa}$ is a first guess, all over Antarctica of the constant value of $\frac{\partial\theta}{\partial z}$ in the linear part of the vertical profile of θ. It corresponds to the reference value of $\frac{\partial\theta}{\partial z}$ from which $\frac{\partial\theta}{\partial z}$ will deviate under H_{min}. To determine H_{min}, we need to identify a threshold for the 1^{st} derivative.
First option would be to use a fixed threshold (i.e. $Thresh_{\frac{\partial\theta}{\partial z}} = \gamma_{350-500hPa} + \alpha$) with α a constant to be determined. However we realized that for vertical profiles displaying a high $\gamma_{350-500hPa}$, the threshold needed to be higher than for smaller

$\gamma_{350-500hPa}$. Therefore we decided to chose a threshold proportional to $\gamma_{350-500hPa}$:

$Thresh_{\frac{\partial\theta}{\partial z}} = \gamma_{350-500hPa} + N * \gamma_{350-500hPa}$ with N=4

A sensitivity study of the coefficient N is provided in the supplement.

As a conclusion, both of these methods require to define a threshold, and its definition is not obvious. This will be further discussed and explained in the next version of the manuscript. For the moment, the authors would like to share a first comparison of these methods in Fig. RR1 and Fig. RR2

If the original method will be retained the authors need to better justify this approach by showing a comparison with the more direct method described here. And, the text needs to more explicitly describe the process used to determine this minimum height above which the potential temperature profile is assumed to be linear.

While we decided to stick with the initial method, we understand that there is a concern regarding the robustness of this method. Therefore, we will add in the supplement a comparison of the results of our method with the ones from a method using the 2^{nd} derivative. We will also add in the manuscript a more comprehensive description of this approach.

Another concern comes from using 350 hPa as the upper height for the linear approximation of the potential temperature profile. How often is this height above the tropopause. It seems like it would be better to calculate the linear profile over a fixed depth above Hmin - maybe just 100 or 200 hPa - to minimize the possibility of estimating a linear gradient over different layers of the free atmosphere with possibly different air masses and potential temperature gradients.

The authors thank the referee for this comment. We will investigate the sensitivity of our method to the upper height for the linear approximation of the potential temperature. The range of pressure $[350-500hPa]$ was initially chosen because it corresponds to a height at which we were confident that we had approximately a linear response of the potential temperature profile on the Antarctic Plateau (at Concordia station for instance, where the surface pressure is around 650 hPa). However, this might be less accurate in coastal areas. First, we will assess the height of the tropopause and provide a comparison with $H_{max} = Z_{350hPa}$. We will make sure that $H_{max} \leq H_{tropopause}$. However, we want to have H_{max} as high as possible in order to be representative of the whole free troposphere. Furthermore, we want to avoid potential discontinuities related to the vertical discretization that might arise from a low H_{max}.

The use of the term thermal wind in your decomposition is confusing. The thermal wind, as defined in atmospheric dynamics text books (e.g. Holton and Hakim) refers to a change in geostrophic wind over some depth of the atmosphere. This is not what this term represents in your decomposition? Parish and Cassano (2003) have the same term in their decomposition and refer to it as the integrated deficit term while Cassano and Parish (2000) referred to this as an adverse pressure gradient force term since it often opposes the downslope flow due to a deepening of the boundary layer with downslope distance and thus a

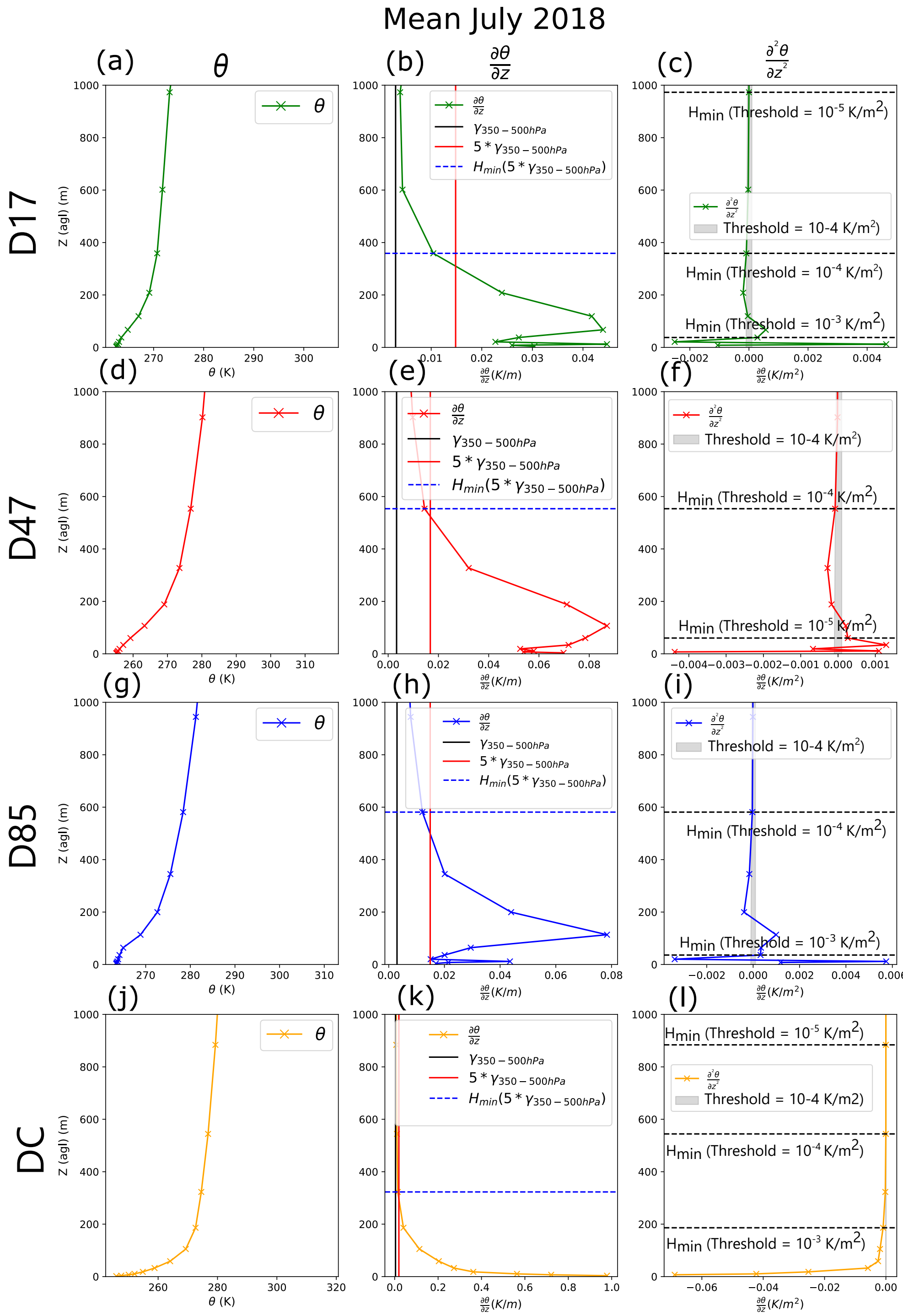

FIGURE R1. Vertical mean July 2018 profiles of (a, d, g, j) θ, (b, e, h, k) $\frac{\partial\theta}{\partial z}$ and (c, f, i, l) $\frac{\partial^2\theta}{\partial z^2}$ at D17, D47, 85 and DC (from top to bottom). The blue dotted lines in the middle pannels indicate the minimum height for interpolation of θ_0 computed using the 1^{st} order vertical derivative method described in the manuscript. The black dashed lines in the right pannels indicate the minimum height for interpolation of θ_0 computed using a 2^{nd} order derivative method for three different values of $Thresh_{\frac{\partial^2\theta}{\partial z^2}}$

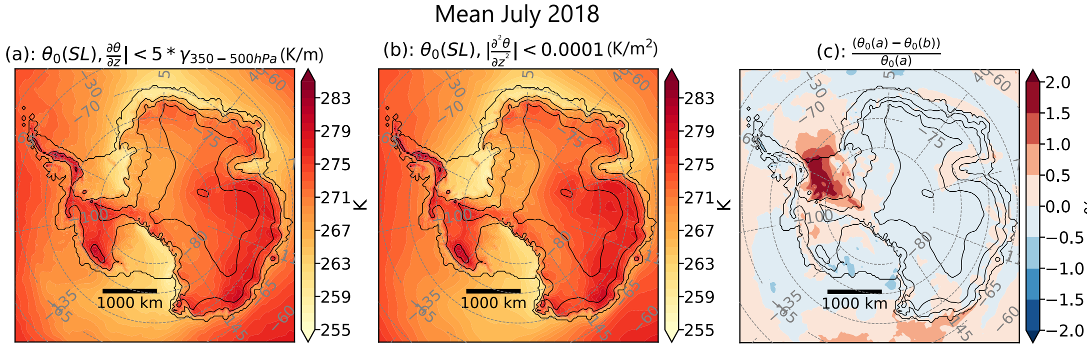

FIGURE R2. θ_0 at surface level computed computed (a) using the method (described in the manuscript) based on the 1^{st} order vertical derivative (b) using a method based on the 2^{nd} order vertical derivative, with a threshhold $\frac{\partial^2\theta}{\partial z^2} = 0.0001K/m^2$ (c) difference between θ_0 computed using method (a) and (b)

larger integrated potential temperature deficit. This term needs to be renamed to more accurately describe what it represents physically.

Here, we define the thermal wind as in Mahrt [1982], van den Broeke and van Lipzig [2003] and. Vihma et al. [2011]. As stated by van den Broeke and van Lipzig [2003]: "It represents the pressure gradient force due to horizontal changes in $\hat{\theta}$". From the geostrophic and hydrostatic equations, the Vallis [2017] textbook defines thermal wind as following:

$$\frac{\partial u_{THWD}}{\partial p} = \frac{R}{fP}\nabla_p T$$

Which transforms to: $u_{THWD} = \frac{g}{f\theta_0}\nabla_p \int_z^{h_s+h} \theta(z')dz'$

From the textbook indeed, thermal wind represents the pressure gradient force due to horizontal changes in the vertically integrated potential temperature and not the vertically integrated potential temperature deficit. It is true that it might be confusing for the reader to call it "Thermal wind". We propose to rename it $THWD_{TD}$ where TD stands for "temperature deficit". We will mention in the manuscript the different names found in the literature for this term and state more clearly that it does not correspond to a proper thermal wind.

Figure 4: There are obvious discontinuities in the pressure gradient force components (e.g. KAT and THWD between D17 and D85) seen in this figure. The source of these clearly non-physical results need to be discussed. Do these artifacts reflect a shortcoming in the decomposition that makes the results less trustworthy?

While it is true that there exist sharp gradients between D17 and D85 in the katabatic term, the authors would like to underline that these sharp gradients do not originate from the decomposition itself, but rather from the multiplication by the sinus of the slope. This is why we have plotted the vertical profile of the potential temperature deficit Δ next to the vertical profile of the katabatic acceleration. We do not have any sharp discontinuity in the profile of Δ. Regarding the thermal wind, we have plotted $\hat{\theta}$ next to the THWD acceleration. There is no discontinuity in the vertical profile of $\hat{\theta}$, but there is a strong minimum at the foot of the slope (next to D17), where turbulence and advection create a strong mixing of the boundary layer which reduces $\hat{\theta}$. Therefore, this zone corresponds to the sharpest horizontal gradient of $\hat{\theta}$.

Figure 7: I found that showing the direction of the momentum budget terms as the equivalent geostrophic wind to be confusing. It would be clearer to simply show vectors in the direction of each momentum budget term scaled by their magnitude. In this way it will be clear in which direction each force is acting rather than the reader needing to rotate the vectors mentally by 90 deg. If the authors wish to keep the vectors scaled relative to a geostrophic wind speed the magnitude of each term can simply be divided by the Coriolis parameter, which will retain the same magnitude as currently shown in Figure 7 but without the direction being rotated 90 deg from the true direction each force is acting.

In this figure, we wanted to show the direction of the resulting wind speed associated with each accelerations in the quasi geostrophic hypothesis. We have demonstrated that this hypothesis is valid in Section 4.1. With this hypothesis, the resulting wind speed associated for instance with the katabatic acceleration is rotated by 90 and the total wind-speed is the sum of the rotated wind components. We thought that the readers would rather like to see the wind-direction than the direction of the accelerations themselves. For example, they would expect the large-scale winds on the ocean to be westerlies. We also wanted to underpin the cross-slope direction of the katabatic winds, that are sometimes wrongly assumed to blow downslope. We would like to keep on showing the wind vectors, but we will make sure to add a better description for more clarity.

2. Minor comments

Line 14: remove latitudes after sub polar - it is redundant and not needed
This will be corrected in the next revision

Figure 1 caption: Last sentence of caption describing color of dots does not match what is shown in the figure.
This will be corrected in the next revision

Line 69: model should be model's
This will be corrected in the next revision

Line 73: What is meant by "the data are slightly better correlated to our model"? This sounds like you are selecting observational data that matches the model which is not appropriate - you cannot preferentially choose observations that match your model and ignore and de-emphasize those that don't.

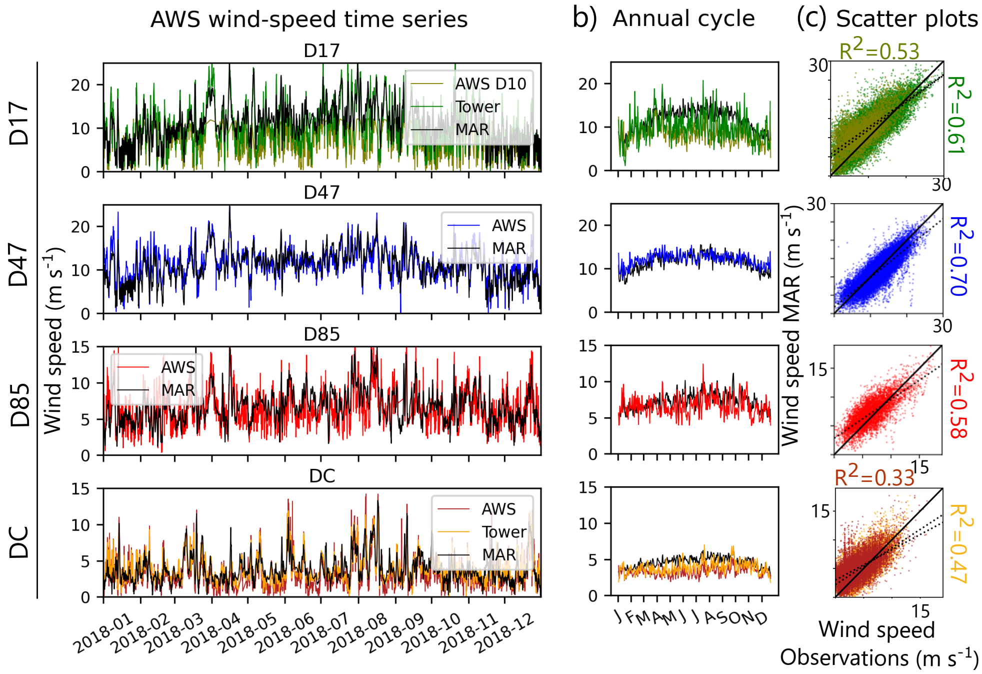

FIGURE R3. From top to bottom D17, D47, D85 and Dome C (a) Comparison of 3-Hourly MAR outputs (black lines) with meteorological tower measurements (when available, i.e. at DC and D17/D10) and AWS (coloured lines). (b) Seasonal cycle computed for the years available in each AWS (see Table ??), with MAR, AWS and the meteorological towers. (c) Scatter plots comparing observations and model outputs for each station. Black solid lines indicate the y=x line while the dotted ones are the linear fit associated with each evaluations. The determination coefficient R^2 is indicated next to each scatter plot.

This is a poor choice of word. MAR's grid cell includes both D10 and D17. The centre of the grid cell is 14 km from D10 and 13 km from D17. The model output is closer to D17, because this station is more continental. We know that MAR does not properly represent the oceanic conditions at the coast. Furthermore, we realized that we had made a mistake in the preprocessing of the D10 AWS. It will be corrected in the next revised version (Fig. RR3(c)). For Concordia, the reason is different: the wind sensors on the American tower and the AWS are different. Genthon et al. [2010]. showed that the AWS temperature was biased because the instruments were not ventilated. As a result, people tend to trust the American tower's measurements more, even though it has not been demonstrated that the tower had a better performance for wind's measurements. We acknowledge that we cannot favour one site over another, and will add the AWS data (at least for DC) to Fig. 7c of the manuscript in the next revision.

Table 1: List lon, lat and elevation for DC-tower. I assume that this is the

same as for the DC-AWS but this should be confirmed by listing these values in the table.
Yes, this will be added in the next revision

Line 86: What is meant by model bases? Please clarify.
The authors are referring to the equations of the atmospheric model, the lateral boundary conditions, the upper and lower boundary conditions and the main parametrizations

Lines 93-94: As written it seems like the 30 snow/ice layers are each 20 m thick. I think what you mean is that the total depth of snow/ice is 20 m and there are 30 layers distributed over this depth. Please rephrase.
Yes, this will be corrected in the next revision

Table 2: It would be more informative if the table listed the start and end distance from the coast for each section and gave the range of terrain slope in addition to the average slope.
Yes, this will be done in the next revision

Line 111: winds variability should be wind variability and near-surface should be capitalized since it is at the start of new sentence.
Yes, this will be corrected in the next revision

Line 132: larger that should larger than
Yes, this will be corrected in the next revision

Line 192: Delete boundary between surface and layer. I think you are referring simply to the surface layer here.
Yes, this will be corrected in the next revision

Table 3: It would be helpful if the relative magnitude of each term was given. For example, the terms could be normalized relative to the LSC term or total PGF to indicate how much larger or smaller each term is relative to the LSC or overall PGF forcing. This could be given as a percentage in parenthesis after the seasonal value is listed. The total PGF should also be listed in this table.
We will add the percentage of the PGF for each accelerations

Figure 7: It would be helpful to add a panel showing the total pressure gradient force, which can then be compared to the other terms in the momentum equation.
We will replace pannel 7.d by the total PGF

Figure 7: Similar to the comment regarding Table 3, showing figures of the ratio of KAT, THWD and TURB to LSC would be very helpful, especially if a color bar with different colors above (forcing greater than LSC) and below (forcing less than LSC) was used. This would clearly show where each forcing term exceeds

the LSC forcing.
We will add this figure in the supplement

Line 330: Replace outputs with output
Yes, this will be corrected in the next revision

References

L. Mahrt. Momentum balance of gravity flows. 1982.

M. R. van den Broeke and N. P. M. van Lipzig. Factors Controlling the Near-Surface Wind Field in Antarctica*. *Monthly Weather Review*, 131(4):733–743, April 2003. ISSN 0027-0644, 1520-0493. doi: 10.1175/1520-0493(2003)131⟨0733:FCTNSW⟩2.0.CO;2. URL `http://journals.ametsoc.org/doi/10.1175/1520-0493(2003)131<0733:FCTNSW>2.0.CO;2`.

Timo Vihma, Eveliina Tuovinen, and Hannu Savijärvi. Interaction of katabatic winds and near-surface temperatures in the Antarctic: KATABATIC WINDS IN THE ANTARCTIC. *Journal of Geophysical Research: Atmospheres*, 116 (D21), November 2011. ISSN 01480227. doi: 10.1029/2010JD014917. URL `http://doi.wiley.com/10.1029/2010JD014917`.

Geoffrey K. Vallis. *Atmospheric and Oceanic Fluid Dynamics: Fundamentals and Large-Scale Circulation*. Cambridge University Press, 2 edition, June 2017. ISBN 978-1-107-06550-5 978-1-107-58841-7. doi: 10.1017/9781107588417. URL `https://www.cambridge.org/core/product/identifier/9781107588417/type/book`.

Christophe Genthon, Michael S. Town, Delphine Six, Vincent Favier, Stefania Argentini, and Andrea Pellegrini. Meteorological atmospheric boundary layer measurements and ECMWF analyses during summer at Dome C, Antarctica. *Journal of Geophysical Research: Atmospheres*, 115(D5):2009JD012741, March 2010. ISSN 0148-0227. doi: 10.1029/2009JD012741. URL `https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2009JD012741`.