RC1

Thanks for your time and suggestions, we are going to apply the changes an upload in a new draft once we have all the comments of the discussion process.

To address some queries in advance:

(235) We have opted not to employ the standard deviation owing to the skewed distribution of data (Figure 5) and the presence of extreme outliers. Instead, we used the interquartile range (IQR) to measure the spread of values given its robustness in handling outliers present in this kind of data (Mardones & Garreaud, 2020). Consequently, these values do not reflect the average ± standard deviation; rather, they indicate the maximum (minimum) value of the result between (H0 + (-) IQR) of a latitudinal or longitudinal profile, respectively.

These data can be seen in Table S1 of the accompanying supplementary material (attached). Besides, these findings are closely related to those presented in Figure 7. For instance, in the meridional profile of autumn, the maximum altitude reaches 2838 m a.s.l in the eastern zone, with a minimum of around 1066 m a.s.l at approximately 73.5°W (referred as 2848-1066 m a.s.l in line 236). Similar logic applies to the zonal profile of autumn (maximum: 2858 m a.s.l; minimum: 841 m a.s.l, line 237).

This methodology is consistently applied across all seasons and to the trends depicted in Table S5.

(430) Confirmed, it represents the annual average and median (New Manuscript L 422).

(435) Rectified values in new manuscript (L 427).

Reference:

- Mardones, P., & Garreaud, R. D. (2020). Future changes in the free tropospheric freezing level and rain–snow limit: The case of central Chile. Atmosphere, 11(11), 1259.

RC2

First of all, we want to thank you for your time and suggestions. We have implemented changes both in the document and in the supplementary material. Below, we will address all the comments made in order, starting with the major comments point by point and then moving on to the minor comments.

Major comments

1. Several passages of the text are hard to understand and/or could be written using more precise terms. I provide further explanation/examples in the minor comments.
    - R. The suggested change has been applied. More details are included in minor comments section.
2. One of the most relevant results, which readers likely are most expectant, is about how fast is warming Patagonia, or is decreasing the freezing level and so glaciers; however, this result is weakly presented, mainly in the abstract and conclusion, after other results, may be of 2nd It should be presented firstly.
    - R. Added to the abstract (L 21-23), results and conclusion. About conclusions, we abord the issues by order following the paper structure.
3. Related with previous comment, the freezing level trends obtained with pure observations, i.e. from radiosonde, are not shown. The authors have presented the trends derived only from reanalysis data which have some degree of errors or uncertainties, as they have even shown. The few radiosonde observations in Patagonia should be used as much as possible, not only to validate reanalysis. For instance, the seasonality (section 3.3) and Trend of H0 (section 3.6) sections could start showing first the results derived from the radiosonde, the closest to the ground truth, and then with those from reanalysis.
    - R. We decided not to include this suggestion in the manuscript because its development and results lead us to revisit certain information that was already addressed in the validation section (L 167-168). Additionally, it also diverts us into a broader discussion about issues that deviate from the main topic (for example, the estimated non-significant trend values of the observed 0°C isotherm). However, we have decided to add a section in the supplementary material (Table S1, Table S2) where interested parties can find results obtained from the observed data regarding trends and seasonality.
4. In order to give stronger emphasis to the positive trends or warming, the section of Trend of H0 (3.6) should be presented before the large-scale control (3.5), which even help to explain some possible causation of the warming.

Additionally, it should be included as the first result in the trend section, the annual total trends and then, may be only the seasons with the largest and lowest values, to reduce the text.

- R. We change the order of section as the referee suggest (Trends chapter, L 261; and Large-scale, L 292). Also, we decide added the maximum and minimum annual trend values here (following the logic of Point 7-Major Comment) and give it more relevance in the manuscript to aboard the point 2 of major comments. We will consider reduce the text of this section.

5. The discussion section, as it is currently written, seems to not deserve its inclusion. The three first paragraphs can be moved and shorten to the methodology section, while the rest of the discussion is somewhat vague, with weak physical interpretation and/or discussion of advantage or limitation of data or analyses, as well as somewhat repetitive of the results already introduced. For instance, in lines 380-387, the west-east difference in H0 is attributed to the topography but it is not explained their influence on contrasting climates at both sides of the Andes, basically, big difference in continentality, cloudiness, precipitation, among others. In lines 388-395, the localized highest warming in the NW of Patagonia is attributed to SAM, but why not affect in the southern Patagonia since the indices use atmospheric variables in R1 domain, south of the whole Patagonia region. What could be the connection between the SAM, global warming, and the localized warming in the NW? What about other large-scale phenomena? Could some of them have potential impacts? It would be a great opportunity here to link with works that quantified the ice losses? How much ice mass have the glaciers lost? It is coherent with the increasing levels of H0?

    - R. The referee aboard several points in this item:

        1. We rewrote this entire section (L 332), nevertheless, we keep the reanalysis limitation issue in the discussion (not in methodology, as we indicate in L 82), subchapter 1.

        2. Respect “lines 380-387” comment: We add a short discussion about the influence of H0 west-east differences using the reference Viale et al., 2019 (Discussion, The 0°C isotherm in Patagonia). https://doi.org/10.3389/fenvs.2019.00069

        3. Respect “lines 388-395” comment: We agree. We include this suggestion in order to consider the importance of R1-T850 in the variability of H0 in southern Patagonia (Discussion, Large-scale drivers).

4. RC2: “What could be the connection between the SAM, global warming, and the localized warming in the NW?” R. We discuss this issue in the new Discussion section (Large-scale drivers).
5. RC2: “What about other large-scale phenomena? “R. About the other large-scale indices as ENSO or PDO, we made the analysis but we don’t find strong and significant correlations with H0 (not shown in results or material supplementary). On the other hand, due the SAM relation with anthropogenic greenhouse gases (Morgenstern, 2021*), the potential link between H0 and SAM allow us to say that there is an anthropogenic influence in the H0 distribution and variations in Patagonia. So, in a scenario with an increment of this gases emission probably it will become in a strong response of the Patagonia troposphere warming and then an increment of the H0, bringing less solid precipitation in the region. This issue was included in discussion section.
6. RC2: “What could be the connection between the SAM, global warming, and the localized warming in the NW?” R. We discuss this issue in the new Discussion section (Large-scale drivers).
7. RC2: “Could some of them have potential impacts? It would be a great opportunity here to link with works that quantified the ice losses? How much ice mass have the glaciers lost? It is coherent with the increasing levels of H0? " R. Check the new section of Discussion (Impact on Patagonian Glaciers). We aboard these issues considered the investigation of Caro et al., 2021 and Bravo et al., 2019 (https://doi.org/10.3389/feart.2021.713011, doi: 10.3389/fenvs.2019.00030).

6. In the discussion, it could be also mentioned that the lowering of the freezing level over the windward slopes of the Andes typically observed during midlatitude frontal precipitation is not appreciable in cross-barrier climatological profiles. This is a mesoscale atmospheric phenomenon that should not be capture by the ERA5 coarse resolution. See Minder’s works: https://doi.org/10.1175/JAS-D-10-05006.1; https://doi.org/10.1175/JAS-D-12-0194.1
    - R. Check new Discussion (Isotherm 0°C and ERA5 reanalysis data).
    - From the validation process, we would like to address the following points:
        1. The validation process indicated a high similarity between the 0°C isotherm values obtained from radiosondes and ERA5 reanalysis. The greatest uncertainty occurred in Río Gallegos; however, the validation parameters (correlation, bias, standard

deviation, and RMSE) were acceptable. This is especially noteworthy considering that the data quantity in Río Gallegos was considerably lower (n=2194, 1967-1977) compared to other stations with observations, representing approximately 10% of the data when compared to the station with the highest records, Puerto Montt (n=21251, 1959-2021).

2. The widespread underestimation of the 0°C isotherm obtained from the ERA5 reanalysis has been documented in Schauwecker et al., 2022. They estimated an underestimation (overestimation) at low (high) elevation sites by the reanalysis. We are unaware of the reasons for this underestimation in areas near the surface, but this fact is consistent with all our comparisons, as all radiosonde points are located near sea level with elevations not exceeding 84 meters. It is worth noting that we conducted seasonal validation and compared the results (Table S2). During the summer months (DJF), the underestimation of the reanalysis was less compared to winter (JJA) in the stations on the west side (Puerto Montt and Punta Arenas). Conversely, in the stations on the east side (Comodoro Rivadavia and Río Gallegos), the underestimation decreased (increased) during the winter (summer) months. Because of this, we suspect that the underestimation of the reanalysis data, in our case, could be better linked to the meteorological condition characteristic of the radiosonde points, since in topographical terms the sites share similarities (low elevation, close to the sea, and at least more than 30 km from the Andes or high mountains). According to Schauwecker et al., 2022*, some of these variables could be humidity, wind speed, stratification, air temperature; however, further research and more data (stations or radiosondes) near the Andes are necessary to better understand both the underestimation of the reanalysis and the regional differences in Patagonia.

7. Presenting unique mean annual and seasonal values of the H0, in the abstract and conclusion as a main result, for the huge whole region (> 10° latitude), does not make sense. They should be introduced as a range of values, between the lowest (in the north) and highest values (in the south).
    - R. The suggested change has been applied in the whole manuscript.
8. It should be highlighted in sec. 3.1 the overall underestimation of H0 by the ERA5, if there is some documentation of that somewhere and possible causes, as well as what could be the implication for this study.

- R. We abord this issue in the response of Major Point 6 – Major Comment.

Minor Comments

- All of them Related to major comment 1)

1. The term "free tropospheric" freezing level would not be used properly here. The free troposphere refers to as the part of the atmosphere above the boundary layer, free of the friction with the earth surface, and so in this study the freezing level not necessarily is present above the boundary layer. It does not make sense in the context of this study, at least the H0 found within the boundary layer were excluded from the study. If not, I suggest to not use it, and the title could be replaced by “Spatial and temporal variability of the freezing level in the Patagonia’s atmosphere”.
    - R. The suggested change has been implemented throughout the entire manuscript.
2. In the results, discussion and conclusion, the authors refer as “modeled or simulated” data or results to those coming from the ERA5. Since the goal of the reanalysis is to use model to assimilate already observed data, and not to predict in the future, I suggest to refer to as directly reanalysis ERA5 data/results. The terms model or simulated data could be intrinsically associated with projections or predictions by the model, and so may create confusion.
    - R. The suggested change has been implemented throughout the entire manuscript.
3. In the abstract, it should be mentioned the data used in the study.
    - R. The suggested change has been applied (Lines 16-17).
4. L41: Viale et 2019 is more appropriated reference here. doi: 10.3389/fenvs.2019.00069
    - R. We change the redaction and reference, but it is not the same; rather, a more appropriate one should be used (L 41-43).
        1. Garreaud et al, 2013 (https://doi.org/10.1175/JCLI-D-12-00001.1)
        2. Lenaerts et al., 2014 (https://doi.org/10.1175/JCLI-D-13-00579.1)
5. L42: “…being the SAM the primary driver of extratropical climate variability in the Southern Hemisphere.”. This strong statement should be supported by references.
    - R. Requested reference has been added (L 44-46).

6. L43: The SAM topic should start in a new paragraph. The introduction has mainly a unique paragraph mixing many main topics, making them harder to capture by the readers.
    - R. The requested change is adopted by subdividing paragraphs in Chapter 1.
7. L176-180: In the text, the authors refer to as “meridional and zonal profiles” and, in the Fig4, they were labeled as Zonal and Meridional mean, respectively. This is confusing. I suggest in the label or caption refer to as “Zonally (Meridionally) averaged Latitudinal (Longitudinal) Section”, and in the text to refer as “Latitudinal and Longitudinal variations”, respectively. Please also indicate in the text when you are talking about Fig 4a and 4b.
    - R. The suggested change has been implemented throughout the entire manuscript.
8. L182-186: These sentences are not clear. Do you refer to mountain or interquartile ranges? When it is said higher or bigger, finishing the comparison w.r.t what is higher or bigger, if not it is hard to understand. Indicate the Fig that show that. Can the AA be located on average around 72W? Please re-write these sentences.
    - R. Suggested changes applied (L 182-188). The ranges being referred to are specified more clearly, and comparisons are clarified. It is indicated which figure shows the results (Figure 4a, b, or c). The rationale for using 72°W to separate the west-east zones has been revised.
9. L197: Please indicate you are referring now to Fig. 5.
    - R. The suggested change has been applied.
10. L200: I see skewed to the left (low values).
    - We disagree, it's a Right-Skewed distribution, also known as Positive Skew (median < mean; check Table S3).
11. L200-202: It is not clear what line (of the 3 plotted) correspond to these stated values. Please indicate.
    - R. We add new supplementary material where these values are showed (Table S3 and Figure S1).
12. L291: Please indicate you are referring to Fig. 8b.
    - R. The suggested change has been applied (L 299-301).
13. L322: “with a marked and unprecedent trend in the period that began 2010”. Where was that result shown? Please provide evidences.
    - R. The suggested change has been applied (L 301-304). Also check Discussion (Large-scale drivers).

- In summary, we performed a trend calculation using a linear model with least squares yields trends. Trends are calculated for each decade of the period and compared. The results of this analysis indicate that the highest trends correspond to the periods 1970-1980 (0.149 m/decade) and 2010-2020 (0.144 m/decade). However, there are two noteworthy facts regarding these periods. The first is that during the 1970s, the anomalies of the 0°C isotherm were negative for much of the period, with few years showing positive anomalies (1975, 1978, and 1979). Besides, during this decade we report a negative phase of AAO. In contrast, during 2010-2020, negative anomalies were only present in 2010 (-0.28) and 2012 (-0.1), with predominantly positive anomalies in the remaining years. Additionally, in this decade a positive AAO phase is documented. Secondly, during the 2010-2020 period, specifically in the years 2017 and 2018, there was a decrease in anomalies (not reaching negative anomalies). However, this late-decade decrease does not capture the increase in anomalies of H0 persisting in the following years (2019-2021). We believe these two points are essential in highlighting a difference between the 1970s and the 2010s, as while the estimated trend for the 2010s is slightly lower than that of the 1970s, the consistent presence of positive AAO phase and positive anomalies along with the persistent increase since 2019-2021, indicate that the last decade has witnessed the greatest increase in 0°C isotherm anomalies since the 1960s.

14. L362: What model do you refer to? Please see comment 1.2.
    - R. Discussion chapter rewrote.
15. L363: It is not clear this sentence. Please rewrite.
    - R. Discussion chapter rewrote.
16. L364: What do you mean by “3 observed data points”? It is not clear, please explain.
    - R. Discussion chapter rewrote.
17. L365: What methodology? Please specify.
    - R. Discussion chapter rewrote.

Bibliography to add

- Morgenstern, O. The southern annular mode in 6th coupled model intercomparison project models. *Journal of Geophysical Research: Atmospheres*, *126*(5), e2020JD034161. https://doi.org/10.1029/2020JD034161, 2021.

- Schauwecker, S., Palma, G., MacDonell, S., Ayala, Á., & Viale, M. The snowline and 0 C isotherm altitudes during precipitation events in the dry subtropical Chilean Andes as seen by citizen science, surface stations, and ERA5 reanalysis data. Frontiers in Earth Science, 10, 875795. https://doi.org/10.3389/feart.2022.875795, 2022.