

Response to reviewer 1 (<https://doi.org/10.5194/egusphere-2025-5643-RC1>).

First, we greatly appreciate the detailed and very constructive review provided. Please find below a response (normal text in black) to each comment (text in cursive and blue).

This manuscript investigates the ozone trends and drivers at a Southern Hemisphere background site in Chile. It is well structured and written and illustrates original and interesting results. I suggest accepting the manuscript for publication after taking into consideration the following comments.

Thanks for your overall statement.

Comments

Page 2, lines 18-19: The sentence needs elaboration. The changes of the net stratospheric influx in STE are linked to changes of the stratospheric Brewer–Dobson Circulation and the amount of ozone in the lowermost stratosphere, which are strongly influenced in a changing climate by the emissions of ODSs and GHGs (Butchart 2014, Banerjee et al 2016, Morgenstern et al 2018, Meul et al 2018, Akritidis et al 2019).

This is certainly correct! The text has been expanded considering your suggestion.

Page 6, line 16: Please clarify within the text the meteorological reasons for this decision.

As stated in line 14 on page 6, we did not use the observed data because there were significant data gaps, particularly regarding humidity. Thus, to have consistent time series we used bias corrected reanalysis data.

Page 7, line 29: Please describe at first place what were the meteorological fields to drive the simulations of TM4-ECPL.

The CTM was driven by year-specific assimilated meteorology (ECMWF ERA-Interim for the years 1980–2014). This has been added to the text.

Page 8, lines 16-18: The criterion for the identification of SI is arbitrary and I suggest to comment the limitations. It could be possible that subsidence/transport of upper tropospheric air could possible have a similar result and can be accounted as SI. Another important issue for the identification is the lifetime of the filamentary structures of stratospheric origin in the troposphere which can persist for several days in the troposphere before cascading down to smaller scales and getting mixed with the surrounding air. Due to this dissipation process of stretched filamentary structures and the irreversible mixing associated with deep stratospheric intrusions, the characteristic signs of stratospheric air with high O₃ content, high values of potential vorticity (PV) and low humidity dilute and disappear over the period of a few days. This complicates the observation of stratospheric intrusions in the lower troposphere and especially within the atmospheric boundary layer and near surface unless in cases of direct and

intense deep intrusion events that can result to distinct spikes in measured stratospheric tracer concentrations (e.g. see Stohl et al., 2003).

We do agree that it is difficult to identify ozone of stratospheric origin, and that any threshold in ozone or humidity (or potential vorticity) is somewhat arbitrary due to the complexity of the processes involved. We would need measurements of ^7Be or ^{10}Be to better ascertain the occurrence of STE, and/or a detailed modeling of such episodes, including high-resolution back trajectories. But now we have neither (We only provide a few examples of back trajectories, crossing the PV=-2 units). Instead, we decided to identify the events of high ozone and low humidity of possible stratospheric and upper tropospheric origin occurring upwind or over Tololo. Furthermore, we put more strict thresholds on ozone and humidity aiming at more intense events and carried out a clustering that identifies several synoptic configurations, many of which are consistent with the possibility of stratospheric ozone intrusions. The text has been changed accordingly.

Page 9, lines 24-25: Maybe you also mention here that the explanatory variables used are indicated in Table 2. This will help the reader while reading this paragraph.

We added a few examples. The information is clearly stated later when applying the method.

Page 14, lines 20-23: Since TM4-ECPL has a dedicated stratospheric ozone tracer, you may look also O3s at Tololo for El Nino and La Nina years.

As stated in the text, while the seasonality in stratospheric ozone intrusions is adequate, we deem it to be overestimated due to the way the ozone upper boundary condition is treated. This was the reason not to analyze its changes between El Niño and La Niña years, nevertheless we have added a supplementary figure in the Supplementary Material. The differences in the seasonality in the model outputs using the stratospheric tracer for El Niño and La Niña years very is consistent with the one derived from the observations.

Page 15, lines 10-11: I am rather confused with the plots of anomalies (Figures 6 and 7) with respect to the 12-day period mean. I think that if you want to clearly illustrate more thoroughly the passage of the trough, I would rather suggest plotting the actual fields of geopotential height and vertical velocity at 500 hPa.

What we show in figures 6 and 7 corresponds in essence to averages of many synoptic conditions which are similar but not identical, and particularly not always occurring in the same spot. Therefore, we opted to show the anomalies, an approach that is rather conventional. Showing the actual composites (averages) and not the anomalies result in a blurry picture. In any case, as we have added a clustering of these events, the corresponding synoptic situations become more defined. All in all, we prefer to keep the anomalies.

Page 18, lines 1-4: There is a misunderstanding here. Stratospheric air penetrating into the troposphere is characterized by high PV- values (not negative as discussed here). I would rather

suggest plotting the actual PV values rather than the anomalies to see the evolution of the filament. Potential vorticity generally provides a good indication of air of recent stratospheric origin. Threshold values for dynamical tropopause reported in the literature range from 1.0 or 1.6 pvu (Stohl et al. 2000) to 3.5 pvu (Hoerling et al. 1991) with a value of 2 pvu used most often (Hoskins and Berrisford 1988; Stohl et al. 2003; Akritidis et al. 2019). Partly, this value depends on the vertical resolution of the meteorological data, and partly it depends on the synoptic situation and the geographical location (Hoinka 1997).

Indeed, there is confusion because we are discussing potential vorticity (PV) over the subtropics of the Southern Hemisphere, where the Coriolis parameter (f) is negative, and we are typically under very stable conditions ($g \frac{\partial \theta}{\partial p} < 0$). Thus, over an isentropic surface and considering cyclonic circulation ($\zeta < 0$), the following expression is strictly negative.

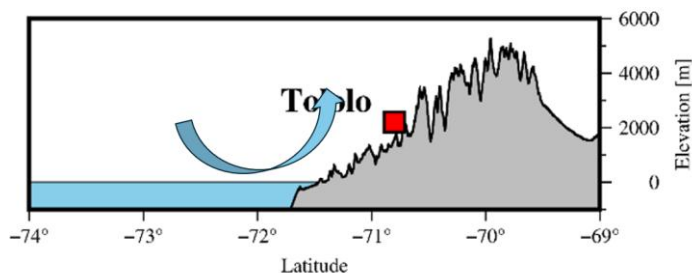
$$PV = -g(f + \zeta) \frac{\partial \theta}{\partial p}$$

Furthermore, in the Southern Hemisphere air, PV is characterized by large and negative values.

Again, we chose to show anomalies and not composite potential vorticity because the location of intrusions varies in space, which when averaged over many cases (places) is diluted. The reviewer can see analysis for individual intrusions showing the absolute fields in (Rondanelli, 2025; Rondanelli et al., 2002).

Page 22, line 25: The way is written here sounds more like a chemical effect (e.g. ozone chemical loss because of more water vapour in unpolluted environment). But later on you attribute it to dynamical effect from more efficient mixing upwards of boundary layer air rich in water vapour and low in ozone. Please clarify.

The first part refers to the role of water vapor as a chemical sink of ozone. The second part refers to the source of water vapor, which in the case of Tololo is the marine boundary layer (See illustration).



Water vapor entrainment to Tololo in connection with the vertical development of the marine boundary layer in summer, or when deep troughs and sometimes cutoff lows reach the subtropics vigorously mixing up wet air.