Review of "Multi-instrumental analysis of ozone vertical profile and total column in South America: comparison between subtropical and equatorial latitudes"
Gabriela Dornelles Bittencourt et al.

Summary and General Comments:
This paper examines long time series of vertical profile and total column data to describe the differences in behavior of ozone between a subtropical station (Santa Maria) and tropical station (Natal) in Brazil. The authors leverage the SABER instrument (vertical profile; both stations) and TOMS and OMI (total column; both stations), Ozonesondes profiles (Natal), Dobson (Natal), and Brewer (Santa Maria) data. The authors validate the SABER profile data with Natal to justify its use to demonstrate the vertical profile differences between the two stations, compare TOMS and OMI to the ground-based total ozone instruments, and perform a wavelet analysis to show the effect of climate and solar oscillations on the total column and lower stratospheric ozone data.
The analyses and conclusions drawn from them are generally sound. However, the text needs a major reworking. The authors should carefully go through the paper and clean up the writing for better clarity. There are also several sections, particularly in the Introduction, that are highly repetitive. There are many times where very similar explanations of the Brewer-Dobson Circulation are invoked, for example.
A note on data availability: It is not made clear if all the data are publicly available and where the data sets are located. I am particularly interested in locating the Santa Maria ground-based total column ozone data. Please indicate in a Data Availability section where all the data used in this paper are located, and if they are not currently publicly available, they should be made so.

Recommendation:
While I do not find many problems with the analysis and technical details of the manuscript, the text requires substantial edits. A second version of this paper may become acceptable for publication.
The manuscript has been substantially edited and revised as suggested by the reviewer to improve English and clarity. Major edits are highlighted in red in the revised manuscript.

Specific and Line-by-Line Comments:
Please add the SHADOZ v6 Ozonesondes data Doi where appropriate (e.g., Line 137) and in the reference list: https://doi.org/10.57721/SHADOZ-V06
The SHADOZ data DOI was added as per the request of the reviewer.

Line 71: Natal has Ozonesondes data dating back to 1979 on WOUDC (before SHADOZ).
Yes, Natal has Ozonesondes data dating back to 1979. However, this ozone time series is not continuous and has significant periods of gaps. The corresponding paragraph was reworded as follows in the revised manuscript: Due to the limited number of radiosondes carried out at SM, a comparison between radiosondes and SABER was only made for the equatorial site Natal. Natal is one of the oldest ozone stations in South America with a record of ozone profiles dating back to 1979, although there are large gaps in the data. The most continuous ozone profile experiment in Natal is a weekly-based one, which started in 1998 in the SHADOZ framework.

Line 85: You are referring specifically to the total column ozone measurements here, correct? Please clarify.
It is correct. The sentence is changed as follows: These two locations are the most important stations in Brazil with ground-based **total ozone** measurements which began in 1992 for SM and in 1994.

Lines 121-125: These two sentences are not necessary and are another example of the repetitive nature of the text.
Removed as per the request of the reviewer.

Line 139: Based on Figure 2, it looks like data only up through 2018, not 2020, have been used.
We thank the reviewer for this relevant observation. The analysis period covers 1998-2020. There was a clerical error in creating Figure 2, which stopped in 2018. The figure has been redrawn in the revised version to show ozone mixing ratio vertical distributions over the whole period, from 1998 to 2000.

Line 139: What do you mean by "604 vertical levels?" Were the Ozonesondes profiles averaged into altitude bins? If so, why 604?
For our study, ozone and temperature profiles obtained from balloon-sonde were vertically interpolated from the surface up to ~30km altitude, with a resolution of 50m, which gives 604 altitude bins. This was clarified in the revised manuscript.

Lines 142-143: "Atmosphere Survey using Broadband Emission Radiometry" for the SABER acronym is incorrect, but it was correctly defined earlier in the paper, so this can be deleted.
Deleted as pointed out by the reviewer.

Line 163: Please use commas for "6,715" and "3,681" instead of "6.715" and "3.681"
Amended.

Line 183: The Aura satellite, not ERS-2.
Amended in the revised manuscript.

Section 2.2: I think this section can be removed. No need to spend time explaining these widely used statistical measures. I recommend keeping Section 2.2.1, however. If the decision is made to keep this Section, the Equations 1, 2, and 3 should have "SATELLITE" not "SATELITE"
Amended in the revised manuscript.

Figure 2: Similar to how Figures 3 and 4 were constructed, it would be very illuminating to see a third panel on Figure 2 showing the full time series of differences in ozone mixing ratio for coincident measurements from the two instruments (Ozonesondes and SABER).
Following the reviewer's recommendation, figure 2 has been modified.

Lines 216 and 256: Better to use "coincident" than "concomitant." As previously stated, the paper would benefit greatly from a thorough editing of the text.
Amended in the revised manuscript.

Line 248: Data gaps should not really affect the comparisons of coincident satellite and Ozonesondes data.
The sentence is amended accordingly.

Figures 3, 4, 5, 7, and 9: What do the error bars represent? One standard deviation?
Yes, error bars represent ±1s (standard deviation). This is added in the figure captions of Figures 3, 4, 5, 7, and 9.

Line 319: 45N and 45-60S are mid-latitudes, not polar latitudes.
Amended in the revised manuscript.

Figure 8: There are some typos in the titles of these figure panels.
Amended in the revised manuscript.

Figure 8 c and d: The Natal TCO time series do not appear to be daily observations. Please check.
We thank the reviewer for this relevant observation. Figures 8a and b depict daily TCO values (in DU) from Brewer, TOMS and OMI observations, respectively, at SM, over the 1979-2020 period, while Figures 8c, and d show the monthly TCO averaged values from Dobson, TOMS and OMI at Natal. The caption of Figure 8 was amended accordingly.

Table 1: What are the units of the 0.34, -0.07, -0.41, and -0.66 values? It looks like percent, but please make that clear.
It is in per cent (%). For clarity, the table was changed.

Line 420: I'm not familiar with the "influence cone" for this wavelet analysis. Please provide a brief explanation as part of Section 2.2.1. I will say that Figures 10 and 11 are a nice demonstration of QBO and solar cycle oscillations on ozone amounts over the two stations.
The cone of influence is a numerical parameter used in wavelet analysis to define the region of the spectrum which should be considered in the analyses with confidence. It indicates areas where edge effects occur in the analyzed time series. This has been added to the revised manuscript.

The following is a reviewer's report for "Multi-instrumental analysis of ozone vertical profile and total column in South America: comparison between subtropical and equatorial latitudes"
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1. The introduction needs to be rewritten/restructured. The current form completely diluted the focus of this paper. The authors should rather consider a discussion of irregular distribution of global Ozonesondes stations (Tarasick et al., 2019), how the Santa Maria Ozonesondes can fill the observational gap in South America, and then emphasis that these data are an important addition to understand tropospheric ozone & air quality in a region where scientists are less studied before.

The co-authors would like to thank the reviewer for his comment. The introduction has been restructured and the main objectives reworded in line with his/her suggestions.

2. 163, does it mean that SM has a nearly doubled sampling frequency than NT? The SHADOZ network typically has once per week or fewer sampling frequency, if SM has a greater data coverage, the authors should explicitly state that SM dataset is expected to have a greater statistical power for analysis (e.g. Jaffe & Ray, 2007).

This concerns SABER profiles only, not SHADOZ profiles. Given the quasi-polar orbit of the TIMED satellite and the difference in latitude between SM and NT sites, one expects to obtain more coincidences for the SM site than for NT. This means that the OMR profiles obtained by SABER over SM are expected to have a greater statistical power for analysis than those for NT (Jaffe & Ray, 2007). This was stated in the revised manuscript.

3. Figure 3: it is not very informative for ozone variability, I would like to see all individual profiles at SM plotted in thin gray from the background at each corresponding month (e.g. Fig 2 of Jaffe et al., 2018). Same for Fig 4 & 5.

Figures 3 and 4 show the validation of satellite data through the SHADOZ network from the Natal station, equatorial region of South America. The objective in these analyzes is to show whether there is agreement between the two databases, for the same station. Furthermore, it is observed that satellite measurements below 20 km present many disagreements, with relative differences greater than 50% compared to SHADOZ data. After that, it was observed that, in the lower stratosphere, the two databases agree well with each other. Figure 5 then shows the comparison of Santa Maria and Natal with data from the SABER satellite, to show that this type of analysis, for mid-latitude regions, well identifies the vertical behavior of ozone.

4. Unlike tropospheric ozone, TCO and stratospheric ozone tend to be steadier and latitude-dependent, does this study suggest that the current SHADOZ network is sufficient to monitor tropical stratosphere, or additional stations, such as SM, are also desirable?

We agree with the reviewer that both TCO and stratospheric ozone levels tend to remain steadier and vary with latitude. This highlights the significance of establishing stratospheric ozone measurement stations in regions beyond equatorial and tropical latitudes, especially in the southern hemisphere. The SM site is an excellent location to participate in this mission of monitoring and observing stratospheric ozone since the site often experiences AOH events. Additionally, there are no long-term ozone profiling stations at this latitude or nearby in the SH. A short paragraph has been included in the conclusion to address the issue.

5. Data availability section is a requirement for AMT.

Data availability information has been added, as per the request of the reviewer.

6. I found it is unpleasant to read and grammar edits/checks are required throughout the paper. There are too many to identify in the minor comments below.

We thank the reviewer for his relevant comments, suggestions, and edits in improving our paper. The manuscript has been checked and revised to improve English and clarity. Major edits are highlighted in red in the revised manuscript.

l.205 “root”
Amended.

l.207 delete ‘also called residuals’, e.g. MAE is also a type of residuals
Amended.

l.221 ‘Variabilities the ozone’ is not English
Amended.

L.389 ‘interannual’
Amended.

Jaffe, D. A., Cooper, O. R., Fiore, A. M., Henderson, B. H., Tonnesen, G. S., Russell, A. G., ... & Moore, T. (2018). Scientific assessment of background ozone over the US: Implications for air quality management. Elem Sci Anth, 6, 56.
Jaffe, D., & Ray, J. (2007). Increase in surface ozone at rural sites in the western US. Atmospheric Environment, 41(26), 5452-5463.
Tarasick, D., Galbally, I. E., Cooper, O. R., Schultz, M. G., Ancellet, G., Leblanc, T., ... & Neu, J. L. (2019). Tropospheric Ozone Assessment Report: Tropospheric ozone from 1877 to 2016, observed levels, trends, and uncertainties. Elem Sci Anth, 7, 39.