We would like to thank both referees for their constructive comments and corrections that helped to improve our manuscript. Point-by-point comments/answers on the referees' comments are presented below. Comments are shown in bold letters, and answers in blue letters.

Anonymous Referee #1

P3L67: I recommend adding one more sentence that goes into the changes in ozone production under these scenarios. As it is written now the reader has to assume they know what happens to the changes in production.

The sentence now reads: *"… However, the relative contribution of the STE to the tropospheric O_3 levels is similar for both emission scenarios due to the resulting changes in O_3 production, which was estimated to increase by 15% in the RCP8.5 scenario, and decrease by 17% in the RCP6.0 (Kawase et al., 2011)."*

P3L95: I recommend checking the ACP style and also confirming the consistency of capitalization of "west" "east" Pacific, "southern" Africa etc.

Following the recommendation, the manuscript was checked for consistency of the capitalization of the terms "west", "east", "southern" etc. We have now used in capitals for West Pacific etc and non-capital letters for western Pacific etc

Also, I would recommend the authors consider showing on a map what exactly in east, west and central pacific. As the linkages seem to be very different, this is an important distinction. However, I do wonder if readers who don't work in this area will know where the exact line between these areas is. This could even just be in Figure 1 to colour code sites by these locations. This is just a suggestion.

Figure 1 is now changed so that stations are color-coded depending on if they are in the West (red) or East (blue) Pacific Ocean.

P4L101-105: There is a lot of important information in these two sentences. It may help to split them up a bit more. For example, the sentences above are describing in detail how ENSO is important and how it impacts on ozone. Then this statement starts that MJO is actually more important (on different timescale). I think the flow is ok, but this is an important point and as a reader I would find it helpful to have this described in a little more detail. This paragraph in general is very interesting, but there is a lot of information on how different aspects impact on ozone on different timescales, and I think this poin ton MJO might get a bit lost if not expanded a little. Also, the last half of the last sentence, why is it not anymore? Is it known?

Considering the length of our paper, we kept this paragraph short and provided references. But we concur with the reviewer in the sense that it would be much interesting to delve into the short-term variability due to MJO. In fact, it is our appreciation of the scientific literature that less attention has been paid to the impacts of MJO than those of ENSO on tropospheric ozone. This is partly explained by the fact that ozone and other tracer records, particularly ozonesondes do not always provide sufficient temporal resolution to address shorter-term variability (A problem we also face in this paper). We have extended the paragraph by a couple of lines, and added a couple of references, to draw the attention of the reader towards this interesting issue. '*The text now reads: 'However, the interannual change in tropospheric O3 due to ENSO is smaller than the combined impact of intra-seasonal MJO and shorter timescale variability that drives the non-ENSO variability (Ziemke et al., 2015). Satellite data, sometimes in combination with model simulations, have provided a fruitful* *ground to study this intraseasonal variability that follows from the eastward propagation of the MJO –i.e., anomalous tropical deep convection, winds, and surface pressure from the equatorial Indian Ocean to the Pacific Ocean – (Tweedy et al., 2020; Ziemke et al., 2015). Also, data collected in near surface platforms highlight the pervasive impact of MJO in air composition of the tropics and subtropics (Barrett et al., 2012; Barrett and Raga, 2016; Langley DeWitt et al., 2013).'*

Regarding the reasons explaining changes in ENSO, and the ability of ENSO indexes to predict changes in meteorological variables around the world, it certainly goes beyond the scope of our paper. Although, such changes are documented and discussed in the realm of atmospheric dynamics (e.g., Capotondi et al, 2014; Garreaud et al, 2020; Hu et al, 2020). We make a short reference to such changes in section 2.2.3. But we omit this discussion in this part of the text to avoid confusion.

P4L108: This citation still has authors first initials. I think this is not aligned with ACP style. There are a few places and references this has happened.

All references are now corrected to follow the ACP style

P4L116: I recommend noting these are surface as previous sentence was high altitude.

The sentence now reads: *"At the South Pole, the decreasing surface O_3 trend over the first part of the 1991-2010 period has been reversed in the last years, and O_3 levels have recovered."*

P6L168: Is there interannual variability of all emissions within these emission inventories? For biomass burning in particular, is it a monthly climatology or is there interannual variability?

There is indeed interannual variability for all emissions in the given inventories and biomass burning emissions vary also monthly. The sentence now reads: *"The model accounts for year-specific emissions of gases and aerosols from anthropogenic and biomass burning sources from the Atmospheric Chemistry and Climate Intercomparison Project (ACCMIP) emission database (Lamarque et al., 2013) up to the year 2000, and from the Representative Concentrations Pathways RCP6.0 (Fujino et al., 2006; van Vuuren et al., 2011) from 2001 onwards. In particular, biomass burning emissions vary monthly, and their injection height follows the Aerosol Comparison between Observations and Models (AEROCOM) recommendations (Dentener et al., 2006)."*

P6L183: Is this actually the formal name? It is not very precise. I would recommend a more exact name and not a nickname.

Even though "The Stans" is a colloquial name for the countries in central Asia (Uzbekistan, Turkmenistan, Kazakhstan, Kyrgyzstan and Tajikistan), we changed it to central Asia.

P6L194: Does this study use the exact same model set-up? I think so, but it is not clear to me. I recommend being explicit. If it does, then as it won't be discussed, I would recommend adding a sentence on its performance in this other paper. If it is a different set-up, then I recommend stating that.

The two studies use the same model setup. Following the reviewer's comment, to summarize the performance for O_3 and CO, the sentence now reads: *"Since the model version used in the present study has been validated against the surface O_3 (R=0.48, NMB=17% with a sample of 2417 measurements globally) and CO (R=0.41, NMB=-1% with a sample of 229 measurements globally) observations around the globe (Daskalakis et al., 2016), the focus here is on the South Pacific regions."*

P7L201: Where these the same time of day across all sites? Did all sites stay at the same time across all years?

Not necessarily. The sampling was done on a per-sonde basis. The sentence now reads*: "For optimal comparison of the modeled data to ozonesonde measurements between 1994 and 2014, hourly model values were sampled for a 3-hour period covering each ozonesonde flight, while the measurements were averaged over height to fit the model's vertical resolution for the specific time and date of each ozonesonde flight."*

P7L207: Why only focus on these two sites?

In our study, we focus on two sites with long time-series for both O_3 and CO. These two sites are representative of the different regions of the Pacific Ocean; the East southern Pacific (Samoa) and West southern Pacific (Rapa Nui). This has now been added in the manuscript.

P7L211: Is this the same time as the sondes?

Not necessarily. In some cases, the sondes overlap with the satellites overpasses. However, for the majority of cases, the model simulations are compared to satellite data, without concurrent sonde observations.

P7L214: I recommend adding in the overpass time of Terra. Also, as I noted with Aura above, are any of these aligned with the sonde times? Are the satellite overpass times within the 3-hour sampling time of the model? I assume yes, but in order for it to be explicitly, I recommend adding that here.

Please note that the overpass times are noted later in the same sentence (10:30 and 22:30). As for overlapping with the sondes please see the comment above.

P8L228: With this dataset would you be able to test if the relationship to ENSO changes over time? If the metric is changing over time, then if the relationship between O3 or CO and ENSO does as well, that could be interesting. That is outside the scope of this study, but for future work, could these datasets be used to test this?

Thank you for bringing our attention to this! This data collection, and its updated version (more observations and longer simulations), is indeed promising to explore ENSO changes over time. We will explore this in subsequent studies.

P9L268: As these are point measurements compared to a coarse grid, the assumption in the comparison would be that within the grid cell ozone would be rather homogeneous. At these sites is that assumption robust? Is it robust across the different altitudes studied? I do see this mentioned below, but I do think it is an important consideration that should be discussed a little more to put these biases in perspective of the tools used and their limitations.

These are indeed point measurements. Please note that we follow the widely used method for model/measurements comparison, knowing that the large model grid can introduce uncertainties. That said, the atmosphere above remote oceans is relatively well mixed (due to the lack of anthropogenic activity or any natural source presenting large variability). Thus we consider the comparison to be robust. We added the following sentence in the manuscript for carification: *"Note that the atmosphere above remote oceans is relatively well mixed (due to the lack of anthropogenic* *activity or any natural source presenting large variability), which justifies the comparison of point measurements with the results in the corase model grid."*

P9L268: The biases are really hard to see on the log scale. I recommend the authors consider adding this information to the figure. Perhaps with another y-axis on this figure? This is just a thought of how to do.

The biases noted here are the result of statistical analysis on the results. We omitted the full analysis for simplicity purpose. Adding this information to the figure would make it even more difficult to interpret. Note however that statistics are provided in Table 2 for the two stations of focus in this study.

P9L284: I asked about this in methods and I see it is here! I would recommend this is moved to methods as the sampling time for the model versus sondes is stated in methods.

This information is present also in the methods: "*...(with a typical spatial resolution of 22 km x 22 km at 10:30 and 22:30 local time)...*"

P9L286: Were both times used? In above sentence only 22:30 is noted.

Please note that both times are noted in the sentence above (*"...at 10:30 and 22:30 local time..."*), and both times were used.

P9L287: "...and then both are averaged" Is that correct? If so, I recommend that "both" is inserted. If the specifics in the method are moved to the methods section as I recommended, then I think this is a very important sentence to keep here to remind readers of this point.

The sentence states that the model is sampled for the exact time and place where the satellite would have passed. "Both", therefore, is not correct here. We don't describe the method used to compare model and satellite in section 2.2.2, since section 2.2 in general is the section describing the observations and not the methods. For clarity, and since the manuscript is rather long, we have chosen to describe the methods alongside the results so that the reader does not get confused or forget what is done.

P10L294: Why is the performance shown for SH and NH and not focused more on the area of interest? As it is a global run, I do think it is important to understand the overall performance of the model. But I would have also expected to see a little more discussion on the performance in the study domain. It does seem from Fig 3 that there is average underestimate of the model. I would recommend that this is added to this section.

This was done in an attempt a) to show the overall performance of the model and b) to showcase the differences between the northern and southern hemispheres. We added an extra sentence at the end of that paragraph, noting the model's performance within the area of interest. The sentence reads: *"Focusing on the area of interest (5^{o}N-40^{o}S and 165^{o}E-85^{o}W), the model seems to perform even better when compared to OMI/MLS, with a Pearson correlation of 0.94, a slope of 1.7 and a normalized mean bias of -3.9%, showing an underestimation by the model over this region."*

P10L304: This should be Figure 5

Thank you for spotting the error. This has now been corrected.

P10L309-311: What comparison led to these correlations and slopes? The whole globe? Similar to the ozone discussion above, I would recommend that the performance over the study domain is explained a bit more here.

The comparison shown here is the global comparison between MOPITT and TM4-ECPL. We added an extra sentence for the comparison of the area of interest. The sentence reads: *"Focusing on the area of interest (5°N-40°S and 165°E-85°W), the model performs well with a Pearson correlation of 0.91 and a slope of 1.0"*

P11L333: But interannual varaibilty can be seen from a timeseries, so I am not certain why it is stated that it is not shown. with the measurements in dots, it is difficult in the timeseries to see the interannual variability - perhaps that is what is meant? I don't think the figure needs to be changed, just rather a comment on "not shown".

We have removed the "not shown"

P11L337: Was the statistical significance of the trends considered? When using this word, it may imply to readers that it is statistically insignificant. I recommend this is considered.

Statistical insignificance was implied. The sentence now reads: *"…or a negligible or statistically insignificant trend when considering only months with concurrent observations."*

P11L343: Interestingly, the obs and model* at 13.1 km are quite similar. I would recommend this is noted in the text as the change in the performance of the model as a function of altitude is interesting.

We added the sentence suggested by the reviewer: *"Interestingly, the model results concurrent to observations at 13.1 km are quite similar, although an improvement in the performance of the model is found towards lower altitudes (see Tables 2 and 3)."*

P11L346: But it does well at 1.3km.

The comment has been added. *"TM4-ECPL reproduces the sign of the trend in the mid-troposphere but overestimates the magnitude when considering all data points as well as for the data points with concurrent observations (see Table 3). It perfoms well at 1.3 km when compared with concurrent observations (see Tables 2 and 3)".*

P11L352: But this wouldn't be true for the model* results that are even more different than the model results to the obs.

To clarify this sentence we specify that the reason refers to the difference between the two model datasets. The world 'model' has been added. The already existing next sentence in the manuscript explains what could be the reason for the difference between the model results and the observations.

P12L353-354: But interestingly the model does perform well at 13.1 km at Rapa Nui, which has even fewer points.

We want to point out that the longer the dataseries is, more robust is the derived trend. In addition we claim that improvement in the stratospheric chemistry representation in the model will increase the model performance.

P12L355: These results don't seem to be in a table. I think it is ok if they are just in the text, but then all the information should be included. Is the significance always tested at 90%? I recommend this is clarified. I assume "significant correlation (-0.30)" mean statistically significant? Does "weak (0.22)" mean statistically insignificant? 0.30 is also weak, so the difference in the terms used to describe these is confusing to me. I recommend the word choice in this section is carefully reviewed. The last sentence line 368 doesn't show the negative correlation.

We agree with the reviewer, the text is not clear and it has been changed. We meant statistically significant tested at 90% confidence level. The paragraph now reads: "*With respect to the ENSO driven variability as expressed by the correlation with MEIv2 index, observations of mid-atmospheric ozone over Rapa Nui show a positive correlation during La Niña years (+0.36); however, the correlation is not statistically significant at a 90% confidence level. This positive correlation is nevertheless consistent with a strengthened South Pacific high and increased subsidence. There are very few data observations at 6 km during El Niño years, that which hampers our analysisobscure a clear behavior at 6 km. The model, on the other hand, shows a negative but significant correlation (-0.30) during La Niña and a positive and significant but weak correlation during El Niño (0.22) (at a 90% confidence level). The latter is expected as the South Pacific high, and thus subsidence weakens in El Niño years. The former may be due to too strong influence of photochemistry compared with that of dynamics in the model despite the model's positive bias in the UTLS.*
Over Samoa, where O_3 levels are dominated by photochemistry, the observations show a positive correlation (0.33) during El Niño years linked to increased precursor emissions from biomass burning originating mostly from Southeast Asia, and a small negative correlation (-0.13) during La Niña years in connection with lesser emissions of ozone precursors in the Southern Hemisphere. However, this is not statistically significant (at a 90% confidence level), stressing the need to consider long time series to assess ENSO variability robustly. The model evidences a weaker but still positive and significant correlation during El Niño (0.23) and a clear negative correlation during La Niña."

P12L357: This relates to an earlier comment where i asked about statistical significance. I see it was done, but that this is the first time it is noted. I recommend noting this earlier up in the discussion of the findings.

Please see the answer to previous comment.

P12L358: In order to highlight this, would it be possible to add when there are El Nino and when La Nina and when neutral to the timeseries? It may make the figure too busy, so this is just a suggestion. But as the data are broken up this way, it may be helpful to show when these occurred. I just recommend that the authors consider it.

We thank the reviewer for this suggestion. We also though about it but the figure is already very busy and will become unreadable isf we also add the El Niño and La Niña periods.

P12L364: from where? Is this stating that all biomass burning areas have higher emissions in El Nino?

As shown in Figure 11, Samoa is mostly impacted by biomass burning emissions from S. E. Asia. The region is impacted by ENSO, with dryer conditions during ENSO years, resulting in more fires, hence higher biomass burning emissions. For clarity the sentence now reads: *"…increased precursor emissions from biomass burning originating mostly from Southeast Asia…"*

P12L379: very large font here.

This has been addressed. Now the same fonts are used throughout the manuscript.

P12L382: Is this pointing to the correct section? I don't see the discussion on the finding of the inaccuracies of biomass burning in this section.

It is indeed pointing to the correct section described in the discussion of the comparison between model and MOPITT.

P12L384: I believe this is Table 5

We thank the reviewer for spotting this. This now points to the correct table.

P13L390: I am not following this statement. By using Model* results, which still show differences between R for obs and simulated, the number of points for obs and model are the same. So how can the number of points have an impact on the differences between obs and model when model* is used? I am also confused why those sites are the exceptions? Also, as this has been noted a few times, is the problem that when you have data there aren't many occurrences of El Nino? If so, I think from my comment the authors can see that I did not automatically interpret it that way, If that is the case, then I recommend rewording to just ensure that is clear to all readers as it is a key point that I think I am missing.

The reviewer is absolutely correct. The paragraph is not clear enough, nor the explanation of the tables. We have reworded the text; it now reads: "*On the other hand, the simulated CO shows higher ENSO modulation than the observations as revealed by the Pearson correlations in Table 5. The Pearson correlation between observed CO and MEIV.2 is generally weaker than that found between simulated CO and the ENSO index. We hypothesized that this might be due to under/over representing the number of ENSO occurrences in the observations. However, the simulated CO still shows a stronger Pearson correlation than the observations when considering the same months when there are observations. Except in the mid-Pacific stations – 15^o N, 145^o W; 10^o N, 149^o W; 5^o N, 151^o W; 0N/S, 155^o W– where considering fewer data points brings relatively large changes in Pearson correlation. Thus the relatively weaker Pearson correlation calculated over the observations than over simulated CO indicates other reasons at play. Since we are using reanalysis meteorology, we attribute this mismatch to an ENSO driven variability in emissions that is not fully captured by the model or to shortcoming in the transport of those emissions.*"

P13L395: Statistically significant?

This has been corrected.

P13L401: These are very important findings, but I can't see where they are presented. I recommend that this is included in a figure or table (unless am I missing them somewhere?). There isn't a link to a table or figure, so I assume they aren't shown.

We have moved this discussion to the next section where it is documented with figure 11 and further discussed.

P13L414: In which run? With or without biomass burning or both? I assume with biomass burning, but then there are two different techniques used to estimate the percentages that are compared. As they are relative to each other and different runs were used to calculate their relative

importance, I recommend a sentence to describe this to indicate that it didn't introduce any error. Was the total amount from the stratosphere similar across both runs?

Stratospheric O_3 as a tagged species exists in all simulations. It is coded so that tagging it does not affect the total ozone concentrations. The total ozone concentrations were used to derive the differences in ozone between the simulations with and without biomass burning emissions. There are differences in the calculated stratospheric ozone between the simulations. These are considered to be negligible (maximum difference found in the stratosphere is about 9 ppb, with an average maximum of 6ppb over the full time period compared to O_3 concentrations in the stratosphere). Please note that the maximum difference is in the order of ~3ppb if we only consider the altitudes that are presented in this manuscript.

P14L426: I recommend using E and W as used in x-axis in figure that is being discussed.

The text was changed as proposed.

P14L427: The absolute amounts can't be seen in the figure. I recommend also stating the % here which can be related to the Figure 9.

Figure 9f shows indeed the percentage contribution of biomass burning to the O_3 concentration. However to keep the scale to 100% as in figures 9b,c,e makes this figure difficult to read. Therefore we re-plotted this panel using a scale spanning from 0 to 50%. This difference in scale is also noted in the Figure caption.

P14L427: The relative is larger in 9c though (i.e. the red peaks are higher). I recommend this is explained more. Is it that the absolute (i.e. 3 ppb (which is not shown)) is higher sub-tropics even though the percentage is higher in the tropics? The actual percentages at the sites are hard to see with the black line. Perhaps the line can be made dotted to help see. Also, the colours at the sites in 9c and 9f look very similar on this colour scale.

The red peaks are outside of the region of discussion here, which is between 150E and 80W. For clarity, we plotted a rectangle enclosing the discussion region and referred to it in the text.

P14L438: I recommend adding to the figure legend that this is for SON.

It was added as requested.

P14L438: these are the average over the full time period?

These are for the high burning period. Now added also in the Figure caption and in the text that now reads: *"Figure 11 depicts the simulated vertical profiles of CO at the Samoa and Rapa Nui islands from the surface to 15 km altitude for the high burning period (SON) over the 21 year period studied here."*

P14L329: in SH or global?

The figure shows both SH and global with different colors, as explained in the figure caption, which now reads: *"Vertical distribution of CO from different source regions (see legend) in Samoa (left) and Rapa Nui (right) for the high burning period (September-October-November). "Tot CO" represents the* *contribution of all sources of CO, whereas ''BB CO'' represents the sum of all biomass burning sources. The line in red (sum) indicates the sum of all individual BB contributions shown."*

P14L443: But in absolute terms this doesn't look very different for SAmerica it looks like 3 versus 5ppb? The difference between the red and black line looks bigger really. I believe if the black line is better defined it may also help to explain why the red and black line aren't the same. I think the black must include all global regions?

Please see comment above.

P14L446: I recommend using "altitude".

Changed as requested.

P14L447: I strongly recommend that these sources are shown on a map somewhere. I think it would help to understand which exact areas are being investigated here. I understand it is shown in other papers, but as it is important part of the analysis here, I recommend it is shown somewhere.

The biomass burning source regions are defined following the HTAP regions and a figure is added in the supplement.

P14L447: I recommend the figures are reordered to the order they are discussed here

Following this comment, the figures were reordered according to the discussion.

P15L457: Is this the correct figure? it seems CO is transported aloft in all figures.

Thank you for spotting this errot. The text now points to the correct figure

P15L466: I recommend noting where this smaller part is in the figure.

The authors believe that these figures are already extremely crowded and complex. Adding extra information could make them even more difficult to understand.

P15L469: Similar to above, I recommend that it is noted where in Figure 14 this larger part is seen.

Please see comment above

P15L478: This is a key finding that is noted in text and highlighted here. however, the absolute contribution of biomass burning to ozone is not shown. I recommend the authors consider showing this.

The ozone vertical profiles are now added as a figure in the supplementary material, showing the quantities at Samoa and Rapa Nui.

P16L486: The manuscript discusses in different places the limitations with the information and the tools used. I recommend highlighting the research needs here. I think that would be very helpful to the community to have these authors highlight those.

Following the reviewer's comment we have added the following sentence in the conclusion at the end of the first paragraph *"To increase the accuracy of ozone simulations requires an explicit represenation of statospheric chemistry as well as halogen chemistry and higher spatial resolution than used in the present study".*

Figure 1: What is the dotted vertical line?

The dotted vertical line denotes the chemical tropopause defined in Prather et al., 2011. This information is now given in the figure caption

Figure 7: I strongly recommend that the colours showing SH and NH are the same here as is figure 4. They are switched and it did confuse me.

Following the recommendation, the colours are now changed to be consistent between the figures.

Figure 8: What percentiles do the top and bottom of the boxes show?

We thank the reviewer for catching this mistake. We have now corrected the figure caption that reads: *"Timeseries (top row) of observed (red line) and simulated (blue line) CO volume mixing ratios for Samoa (left) and Rapa Nui (right). The seasonal variability is depicted in box plots as shown by observations (middle row) and the simulation (bottom row) for CO for the same stations. Boxes show the 25 to 75 percentiles of the data. Whiskers show the 0^{th} and 100^{th} percentile, excluding outliers. Circles show outliers, the green triangle is the average, and the blue line is the median of each monthly distribution."*

Anonymous Referee #2

Line 31: ppbv and other ppbv in the paper: v should not be subscripted. There are also some similar typewriting problems, e.g. different font size at line 379, not subscripted of 3 in O3. Please correct them all.

There are two ways of denoting volume mixing ratios (ppbv or ppb_v). For consistency reasons, we now followed the first one. Further, Tthe manuscript was carefully read and corrected

Line 34: Do you mean 15-23 ppb CO account for about 25% of the total CO in the troposphere of the tropical and subtropical South Pacific? How did calculated the number 25%? It is confusing. To account for the total troposphere, it is precise if you calculate the contribution based on tropospheric column CO.

The following lines clarify this point: As the sentence states, 25% of CO in the tropical and subtropical troposphere comes from biomass burning. The number is calculated by comparing the base and the no biomass burning simulations that were performed, and described in the manuscript. The contribution is representative for the troposphere because 1) it was calculated as such, 2) all of biomass burning takes place in the troposphere and 3) our experiments show that nearly all CO is in the troposphere in the region of interest. For clarity, we rephrased the sentence *"All biomass burning sources contribute about 15-23 ppbv of CO at RapaNui and Samoa and account for …about 25% of the total CO in the entire troposphere of the tropical and subtropical South Pacific"*.

Line 68: Rewrite this sentence. Convert it into several short sentences.
The projected increasing trend in STE O3 flux has been attributed to decreasing levels in O3 depleting substances (ODS), and to increasing greenhouse gas concentrations (Meul et al., 2018). Then describe them separately.

The sentence now reads: *"The projected increasing trend in STE O_3 flux has been attributed to two factors. First, the decreasing levels in O_3 depleting substances (ODS) that affect stratospheric O_3 levels. Second, the increasing greenhouse gas concentrations that intensify stratospheric circulation and affect chemistry through changes in temperature and in water vapor levels (Meul et al., 2018)."*

Line 84 & 88: Zeng & Pyle, 2005. I also notice some similar problems existing in other references. Please correct all of them.

All references were corrected to conform with ACP style

Line 90: driving instead of drives? which driving the Walker circulation weakens.

A comma has been added after circulation and the sentence was broken down to two – the sentence now reads: *"During the El Niño years, the large scale dipole area of low and high pressure over the tropical Pacific, which drives the Walker circulation, weakens. In addition, the low-pressure convective area over the maritime continent moves towards the central Pacific, resulting in large scale changes over the whole Pacific Ocean such as those intensifying wildfires over Indonesia."*

Line 104, not clear, what does ENSO-3.4 warming mean? Why is not a predictor anymore?

Thank you for pointing this out. ENSO-3.4 is jergon in this context, so we have omitted this in the revised text of this section (ENSO-3.4 is a region of the Pacific whose sea surface temperature anomalies are often used to infer ENSO indexes, https://www.ncdc.noaa.gov/teleconnections/enso/sst).

Now, regarding the reasons explaining changes in ENSO, and the ability of ENSO indexes to predict changes in meteorological variables around the world, it certainly goes beyond the scope of our paper. Although, such changes are documented and discussed in the realm of atmospheric dynamics (e.g., Capotondi et al, 2014; Garreaud et al, 2020; Hu et al, 2020). We make a short reference to such changes in section 2.3.3. But we omit this discussion in this part of the text to avoid confusion.

Line 108: Double check the year range of early part and most recent decade of Samoa record. Do you mean: The increase observed in the early part of the O3 record (1981-2000) has leveled off in the most recent decade (2000-2010)?

Thank you for this comment: The year range is correct, the sentence has been rephrased to avoid confusion. *"…has leveled off from 1991 to 2010…"*

Line 121: evaluate -> derive

It is now changed as suggested

Line 126: I don't think this sentence is correct. CO also has a relative long lifetime and is a good transport tracer. Also, this sentence is opposite of what you discussed later in this paragraph.

We thank the reviewer for bringing this to our attention. The sentence has bene rephrased as follows: *"While O_3 is of secondary origin and, thus, its variability is impacted by changes in precursors' emissions and dynamical effects (long-range transport and troposphere stratosphere exchanges), CO concentrations are strongly affected by its emissions (Daskalakis et al., 2015; Inness et al., 2015)".*

Line 134: has been based -> was based

Changed

Line 142: The authors should rewrite the objectives of this work. For example, objective b is overlap with objective e. The objective of this paper 1: understand tropospheric ozone variability over tropical south Pacific, 2: understand the drivers of O3 changes from dynamical part (STE and transport) and from source part (emissions).

Following the reviewer comment we summarized the objectives as follows:
"Hence, the present study takes into consideration the entire south tropical and subtropical Pacific Ocean to a) understand the tropospheric O_3 variability in this region, b) understand the drivers of O_3 changes due to dynamics (STE and transport), and sources (emissions) and their relative importance, and, c) attribute and quantify the CO enhancement by biomass burning to specific source regions."

Line 150: add a reference of HTAP task force.

The reference is now added.

Line 154: What is the full name of TM4-ECPL, similar problem for HALOE, AEROCOM et al. Make sure you specify all the abbreviations at the first time they appear in the paper.

These are the actual names of the models/datasets used that were produced either in the respective labs (ECPL), or during the respective projects (AEROCOM/HALOE). TM4-ECPL stands for Tracer Model version 4 of the Environmental Chemical Processes Laboratory but is commonly used in the publications as TM4-ECPL. HALOE stands for NASA'S Halogen Occultation Experiment and AEROCOM stands for AEROsol COMparison between Observations and Models program. The explanations are now provided in the text.

Line 159: What do you mean of 'between 20 and 25"?

Since the model has hybrid pressure levels, the tropopause is not in one specific box vertically, rather depends on the surface pressure. The tropopause falls somewhere between levels 20 and 25, depending on location and time. For clarity we replaced *"depending on location"* by *"depending on surface pressure"*.

Line 160: Does the oversimplified chemical scheme in the stratosphere affect its simulation of stratospheric o3? If so, how trustable of model's O3 STE?

As described in the model description, the stratospheric O_3 concentrations are nudged towards observations, hence the model O_3 STE is trustable.

Line 198: Figure 1: need modify the figure to make stations clearer. 1: Guam, Christmas and Lauder are CO locations and need use + symbol. 2: What are the names for other CO stations? 3: for Samoa and Papa Nui where both O3 and CO are measured, the red dot plus + symbol would be proper.

The locations of CO data in Figure 1 marked with + correspond to measurements during oceanographic cruises. The Pacific Ocean Cruise (POC, between the US west coast and New Zealand or Australia) data were merged and grouped into 5 degree latitude bins. The data cover the period 1993-2015 on a monthly basis. The map in this figure has been redrawn and the figure caption has been modified for clarity.

Line 200: Table 1: is m.a.s.l meter above sea level? Please specify it.

It is now explained as a table footnote.

Line 216: MOPITT CO product is total column and profiles, but not tropospheric column. Although CO concentration is small in the stratosphere, it still generates significant difference between total and tropospheric column.

MOPITT also provides tropospheric columns, as described in the manuscript by Deeter et al. (2017) to which we refer in the manuscript.

Line 243: Please rewrite and be precise when describing the relationship between ITCZ and SPCZ and give references. SPCZ should be a portion of the ITCZ.

The purpose of this section is to provide an overall description of the circulation affecting the South Pacific, and not giving an in detail description of the dynamics of the region. That is why we didn't discuss the multiple connections between the ITZC and the SPCZ. Nevertheless, we have arranged the first paragraph of section 3.1.1 and added a reference describing the ITCZ in depth. The first paragraph now reads: "*The main feature of the tropical atmosphere is the Inter Tropical Convergence Zone (ITCZ),* *which for most of the South Pacific is climatologically located in the Northern Hemisphere (Wodzicki and Rapp, 2016). Therefore, near surface tropical circulation in the southeastern Pacific is characterized by cross-equatorial northeasterly trade winds. However, since ITCZ remains mostly over the Northern Hemisphere in the Pacific, the near-surface atmospheric circulation in the tropical and subtropical South Pacific is dominated by the South Pacific Convergence Zone (SPCZ), which consists of a mainly zonal tropical convergence zone and a diagonal subtropical branch that extends from Indonesia down to the mid-latitudes of the Ssoutheastern Pacific (Brown et al., 2020)."*

Line 255: "In addition, winds blow towards the SPCZ from the northeast in the east tropical Pacific and weakly from the west in the west tropical Pacific (Figure 2b)" Do you mean the easterly winds from tropical east Pacific is stronger than the westerly winds from the west tropical Pacific?

The easterly circulation is stronger than the near surface westerly circulation, as can be seen in our Figure 2b.

Line 290: Figure 3: the colorbar is confusing, which red stands lower values than green. Which two stations are the two black dots in the figure? Also it would be helpful if you can also label all the station in these figures.

The authors believe that the color choice is suitable to represent the comparison results. As for the black dots, they represent the location of Samoa and Rapa Nui. This was added in the figure caption and as labels on the figure now.

Line 295: Need rewrite this part: It is hard to tell that model overestimates happen over most polluted regions from the figure. The overestimate over northern India is less than 10ppb, while we see much higher overestimate in the higher latitude in both hemispheres. Do you know what caused those high model bias? Are the overestimates in the model over polluted regions are caused by the possible excessive emissions in the model? What is the emission inventory used in the model for both anthropogenic and biomass burning?

Please see answers to reviewer #1

Line 304: This should be the total column CO, not the tropospheric CO. Please correct other places in the paper as well. MOPPT CO profile only have 10 level, which is impossible to get a correct estimate of tropospheric column.

Please see the previous comment about MOPITT.

Line 307: modelled -> simulated

It is now changed as suggested.

Line 331-332: It looks to me that model shows a large underestimate at Rapa Nui at 13.1km. Should the normalized mean bias be -67% instead of 67%? And comparing Figure 6 with table 2, the NMB seems has an opposite sign with the difference between model and observations (MD-OB). Could you please check whether you have the right sign? The Equation should be NMB=sum(model-obs)/sum(obs)*100%

Here, the reported sign is the correct one. The model overestimates the concentrations at 13.1 km.

Line 351: do you mean that the interannual variations induced by ENSO affects the calculated trends? Any way to separate them?

Since model simulations are continuous, any ENSO impacts on O_3 trends are included in the modelled results, whereas its not the case on observations.

Line 355: Need rewrite this paragraph. My understanding is ENSO years, the strength of the South Pacific high weaken, which weaken the subsidence and the stratosphere ozone input? On the other side, ENSO may lead to intense fires over Indonesia. These two effects counteract with each other.

Please see answer to reviewer #1 relevant comment.

Line379: different fonts

This has been corrected. Now the manuscript uses the same fonts everywhere.

Lin 414: it is not clear how the stratospheric ozone tagged tracer is defined. Please be more specific on this important variable of your analysis.

Tagging stratospheric ozone has been described in detail by Roelofs and Lelieveld, 1997. Stratospheric O_3 tracer is transported from the stratosphere to the troposphere following the air motion and within the troposphere it is not produced chemically but it is destroyed by the same photochemical reactions with tropospheric O_3 and is also subject to dry deposition. This is now explained in section 2.1.1. : '*In addition, tagging of stratospheric ozone is performed using a stratospheric O3 tracer that is transported from the stratosphere to the troposphere; within the troposphere the tracer is destroyed by the same photochemical reactions with tropospheric O3 and is also subject to dry deposition (Roelofs et al., 1997).*'

Lin425-430: Between 150E and 280E, I could not see that "can see that there is a higher contribution of biomass burning to O3 levels (about 3 ppbv more) in the subtropics (Figure 9f) than in the tropics (Figure 9c)". Which level do you refer to? In addition to the transport patterns discussed here, it would be clearer if the authors also discuss the relative location of ITCZ and emission source regions during this season.

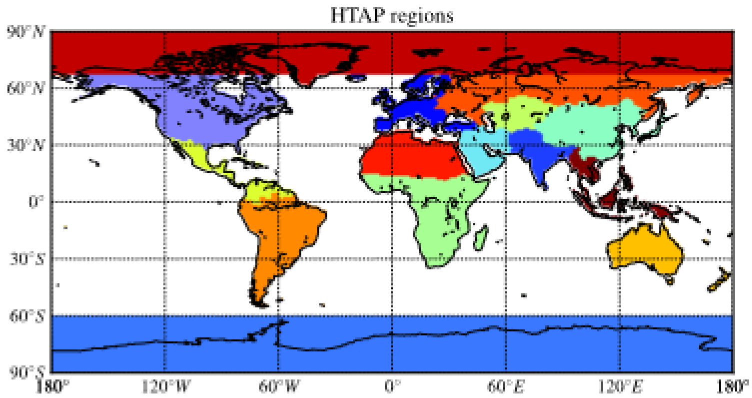
Please see answer to reviewer #1

Figure 9: are we supposed to see the sum of two contribution is 100%?

Please note that the stratospheric O_3 and biomass burning O_3 are not the only sources of O_3. Therefore, the sum of the two contributions is lower than 100%.

Line 438: Please specify the lon-lat ranges of individual biomass burning source regions.

Logistically, this is not doable since the source regions are not defined as squares. Please refer to the HTAP source regions for more information.



Taken from the supplementary material of Daskalakis et al., ACP, 2016