Author Response - Overview

We would like to thank the two anonymous referees, together with the handling editor, for taking time to review our work and in turn providing constructive feedback in helping to improve the manuscript. Please note that referee comments in the two author response files are indicated in black and author responses in red.

In accordance with our own re-assessment of the manuscript, we provide below details of a few minor additional revisions we have implemented in the track-change version of the revised manuscript that are not directly associated with the referee comments. All line (L) numbers refer to those indicated in the revised manuscript file:

- We consider the finding for an enhancement in near-surface ozone of stratospheric origin over the Arctic to be better quoted as a range as opposed to a central lag date. See L19-20:

  "An enhancement in STE leads to a significant 5–10% increase in near-surface ozone of stratospheric origin over the Arctic, with a typical time-lag between 20 and 80 days."

- We additionally cite the Hong and Reichler (2021) study mentioned by referee #2 on L59.

- We now abbreviate 'stratosphere-to-troposphere' (STT) at the first mention in the manuscript on L73:

  "…, due to enhanced downward stratosphere-to-troposphere (STT) transport".

- L94: The CAMS reanalysis is continuously being made available up until the present-day. Thus, '(2003-2018)' → '(2003-2022)' at the time of writing.

- The typical temporal frequency of ozonesonde profiles was previously understated as approximately weekly. It is rather *"…typically a sub-weekly to weekly basis,…"* (L130).

- In Sect. 2.7, we first add the chemical formula of ozone and water vapour in parentheses in the first instance. Subsequently, we now only refer to each species using the chemical formula (L181-196).

- L213: We composited ozone profiles over a seven as opposed to a 'six' month period as initially stated. However, since we reduce our period of focus from November-May to December-April, it is now a 'five' month period.

- L241: ‘Of particular note…’ → ‘Of note…’

- L309: Overbar missing from w* term which is now added.

- L321: ‘during’ → ‘following’

- L346: Paragraph break added here in Sect. 4.2.

- L356: Addition at end of sentence: ‘…, but primarily the PJO-type events’.

- L430: ‘Stratosphere-to-troposphere (STT)’ previously defined already so now referred only as ‘STT’.

- L476: Word ‘transport’ missing after use of STT acronym which is now added.

- L493: ‘conventional’ → ‘NWP’.

- L496: Additional citation added ‘Shepherd et al., 2018’. Reference below:

  Shepherd, T. G., Polichtchouk, I., Hogan, R. J., and Simmons, A. J.: Report on stratosphere task force. European Centre for Medium-Range Weather Forecasts. ECMWF Technical Memorandum, 824, https://doi.org/10.21957/0vkp0t1xx, 2018.

- L625: ‘transport’ added after mention of STT.

- L649: ‘Shepherd et al., 2018’ reference cited again.

Anonymous Referee #1 – Author Responses

general comments

This study investigates the impacts of SSWs on air quality at the surface and radiative effects in the UTLS. Regarding air quality at the surface, the focus is placed on the field of near-surface ozone. As to radiative effects in the UTLS, both ozone and water vapor are considered. In particular, they found that SSWs lead to changes in ozone and water vapor in the UTLS, which in turn drive changes in stratospheric temperature. The results presented in this study reveal a 5-10% increase in the stratospheric-origin ozone near the surface and a roughly 2 degrees stratospheric temperature anomaly driven by the radiative effects of ozone and water vapor. These results are new, clear, and robust.

My major concern is that these changes do not seem, or have not proven, to be of critical importance. (a) For the air quality impact part, the full ozone field at the surface does not seem to respond to SSWs, indicating that the SSW-induced changes in the stratospheric-origin ozone are not sufficient to drive changes in the full ozone field. As shown in Fig.1d, even for the most intense and prolonged event on record, the SSW profile is not well separated from the climatology profile below 500 hPa. The signal may be even weaker from a composite-based perspective, which includes multiple less intense and less prolonged events. (b) For the radiative impact part, the radiatively-driven stratospheric temperature change is roughly 2 degrees, the magnitude of which is quite small in comparison to the tens of degrees change associated with SSWs.

Therefore, in my opinion, instead of arguing that SSWs exhibit an impact on surface ozone (which the results do not support), the authors may focus mainly on the signal in the stratospheric-origin ozone and perhaps add some discussion about why surface ozone, or the full ozone field, does not show a clear and robust response to SSWs. Maybe because the stratospheric-origin ozone only accounts for a very small fraction of the full ozone field at the surface, and a 5-10% increase in the former is not able to drive any notable changes in the latter? Regarding the radiative impact part, it would be helpful to show that a 2 degrees change in stratospheric temperature can have a substantial impact on the numerical weather forecasts on sub-seasonal to seasonal timescales. This could be done by adding some relevant references.

Thank you for the overview and sharing your thoughts on the manuscript. We do not claim that our results are of “critical importance”, which seems to be a rather high bar to set for any paper; nevertheless, the effects we demonstrate are a systematic impact, and we did not seek to overclaim their significance (see, for example, the final sentence of our abstract) both in terms of the air quality and radiative impact aspects. Nevertheless, the impact of enhanced stratosphere-to-troposphere transport on near-surface ozone may be locally significant, particularly in mountainous regions which are more impacted by the free troposphere, but this cannot be discerned here using such coarse-resolution datasets. This is because tropopause folding, which serves as a primary conduit for influx of ozone-rich air into the troposphere, typically occurs on spatial scales of a few tens of km's or less. We rather view our study as a useful starting point for further research connecting downward-propagating (PJO-type) SSWs with near-surface ozone levels and radiative feedbacks on tropospheric circulation anomalies that may ensue up to ~2-3 months following an event onset.

While we agree with the reviewer that the full ozone fields do indeed not show a change during SSWs at the surface in a statistical way (Fig. S3), we would like to argue, that this finding should not be used as an argument that our results are meaningless (absence of evidence is not evidence of absence!). Instead, we would like to point the reviewer to our Fig. 7, which is an attempt to explain that the absent signal is likely due to confounding factors. The potentially most significant confounder is surface ozone trends, which are substantial over the considered time period, especially over the Asian region. By the construction of pseudo-climatologies that keep changes in tropospheric ozone over the considered time period constant, we provide evidence that O_3S (all else being equal) indeed would have a statistically significant impact on air pollution risks. While this is evidence for our hypothesis, we would like to stress that further investigations are needed (e.g., using no-emissions-change scenario simulations, which would be a cleaner way to disentangle the influence). Another confounding factor may be changes in tropospheric transport due to SSWs. Such changes, as a result of PJO-type SSWs, may counteract the enhancement of stratospheric-originating ozone near the surface in the full ozone field over the three mid-latitude continental regions examined (e.g., a -NAO tendency can lead to transport of ozone-rich air away from continental Europe). We now enhance the discussion on these confounders.

Concerning the radiative impacts aspect, we add some new references and expand the discussion to highlight the potential significance of Arctic UTLS temperature changes on the order of ~2 K for NWP forecasts on subseasonal to seasonal timescales. Though this value is small in relation to the several tens of kelvin temperature change associated with these events, we note that such deviations in temperature relax back towards radiative equilibrium with time (up to ~2-3 months in the lower stratosphere). But more importantly, as NWP models already account for adiabatic warming driven by enhanced descent, this radiative impact is additional and not accounted for so certainly these results and their implications are not trivial. As shortwave radiative heating becomes increasingly important as winter transitions into spring, the importance of this finding is likely to increase with time (the persistence/slower decay of UTLS composition anomalies acts to maintain a radiatively perturbed vertical profile).

Our responses are provided in order of each comment. Please note that line (L) numbers referred to in our responses correspond to the revised submission files.

specific comments

Line 19: "Resultant enhanced STE"

“Resultant” means there is a reason, which is not clear here.

We agree that such reason may be ambiguous. A reader may not follow that enriched ozone just above the tropopause will naturally lead to enhanced STE which is our reasoning for beginning the sentence with “Resultant” here.

We replace “Resultant enhanced” with “An enhancement in” on L19.

Lines 19-20: "a significant 5–10% increase in ozone of stratospheric origin over the Arctic, with a typical time-lag of 50 days."

it is not clear in which layer this 5–10% increase in ozone occurs, within UTLS or at the surface.

We apologise for this oversight – we mean the latter so now explicitly refer to this enhancement leading to an increase in “…near-surface ozone of stratospheric origin over the Arctic…” (L19-20).

Lines 25-26: "Our results imply that SSW-related transport of ozone needs to be accounted for when studying the drivers of surface air quality."

I don't think the current results provide strong support for this statement. A 5-10% increase in the ozone of stratospheric origin does not necessarily correspond to a substantial or noticeable increase in the full ozone field at the surface, which might be dominated by the ozone of tropospheric origin.

Please refer again to our answer to your major concern under general comments. We think the issue is that yes at face value, such statement is not sufficiently supported by our evaluations. However, it could be a valid assertion on a more localised basis if much higher resolution tools and datasets would confirm that enhanced STE significantly affects surface-level ozone. We think this potential significance is worth highlighting but nonetheless tone down this statement as it is too strong in the context our findings.

Please see the new revised sentence below (L27-29):

“Our results highlight that whilst any background increase in near-surface ozone due to SSW-related transport is likely to be small, this could be of greater importance locally (e.g., mountainous regions more susceptible to elevated ozone levels).”

Lines 61-63: "The sensitivity of tropospheric ozone to variations in the Arctic and North Atlantic Oscillations has been widely discussed (Li et al., 2002; Creilson et al., 2003; Duncan et al., 2004), but these variations were explained by purely tropospheric mechanisms."

This seems to imply that SSWs might be able to impact tropospheric ozone by driving variations in the AO/NAO. If this is true, then the ozone anomalies in certain regions (e.g., North America, Europe, Asia) following SSWs shown in this study may not arise from changes in the ozone of stratospheric origin, but from the re-distribution of ozone of tropospheric origin.

While we agree that redistribution of ozone of tropospheric origin, in response to induced tropospheric circulation anomalies, is an important aspect that will affect our results, we do not agree with the reviewer that this could be the cause of our findings. The advantage of our approach using a tagged stratospheric ozone tracer is that we can clearly show that ozone of stratospheric origin is enhanced even at the surface after SSWs. The question is perhaps whether this enhancement can affect the full ozone field in a significant way. Here (see also earlier responses to the reviewer’s comments above), we argue that if it wasn’t for confounding factors obscuring the statistics, an impact would be discernible as evidenced in Figure 7.

Lines 124-125: "For the three selected Arctic stations,..."

Please explain what your criteria were for selecting the three stations.

They were the only stations with somewhat routine observations (typically sub-weekly to weekly) spanning 15 years or more during the 1980-2013 period, poleward of 60°N, which we refer to as the Arctic in this study.

We have made this more explicit in that we exclude stations which do not provide good temporal coverage of observations during the period (L124-125):

“Vertical ozone profile data for the three long-running Arctic stations (Alert, Eureka and Ny-Ålesund) located poleward of 60°N, with measurements spanning at least 15 years of the 1980–2013 period (see Table 1 for details)...”.

Line 274: “The 50 hPa level appears throughout much of the time period to be close to an inflexion point”

This is hard to tell because 50 hPa is not marked in the y-axis of Fig.3.

We appreciate that the height dimension of the plot is too compressed, so it's not easy to read across individual pressure levels. We now enlarge this and also display a dashed line at 50 hPa. Also see L284:

"The 50 hPa level (indicated by the upper horizontal dashed line)…"

Line 277: "This is even more true for the 250 hPa level,"

Similar to the above, 250 hPa is also not marked in the y-axis of Fig.3.

In accordance with the above, we also mark on the 200 hPa (rather than 250 hPa which was an oversight) level (dashed line) to further help the reader. This is corrected in text (e.g., L287-288):

"This is even more true for the 200 hPa level (lower horizontal dashed line)…"

Lines 279-280: "Following the major PJO-type SSWs over this period, which includes January 2006, January 2009 and January 2013,"

In total, four PJO-type SSWs are shown in Fig. 3. I wonder why only three of them are called "major" events.

Indeed, February 2010 is also a major PJO-type event (as denoted by a solid black line). However, we did not highlight explicitly it as the water vapour anomalies following the event are not as strong and as well structured as the other three events.

For balance, we now include a short sentence to explain that February 2010 is not characterised by as coherent signature in such UTLS water vapour anomalies (L292-293):

"It should be noted that this signal is not so pronounced following the other major PJO-type event (February 2010) during this period according to either EMAC or the CAMS reanalysis".

Lines 280-281: "an anomalously dry region is found for heights above 250 hPa, which overrides an anomalously moist region immediately below this level."

I do not see a dry-moist dipole very clearly. Highlighting the layer of 250 hPa would be helpful.

As mentioned, we have added a dashed line to highlight the 200 hPa level. As the height dimension has been enlarged, it is now clear that this level is typically between the two anomalous regions.

Line 324: tropospheric fraction of ozone of stratospheric origin (O_3F) using this tracer ($O_3F = O_3S/O_3 \times 100$).

Since O_3F is defined, adding a figure to show the climatology of O_3F would be helpful. For example, if stratospheric-origin ozone accounts for a very small fraction of the full ozone, then one may not expect its response to SSWs to drive a notable change in full ozone at the surface, or surface air quality.

Thank you for your suggestion. We agree that this is useful knowledge to make available to the reader in assessing the importance of the results.

We update our existing Fig. 5 by additionally overlaying contours of the climatological O_3F according to EMAC (panel b). It is now evident that the O_3F enhancement (>5 % anomaly) following a PJO-type event typically occurs when >50 % of tropospheric ozone originates from the stratosphere, at least initially (for a mean onset date of 17th January in our case). The much smaller ~1-2 % enhancement following an nPJO-event is even less significant than the value suggests since O_3F seasonally declines from winter to spring (the mean onset date for this event type is almost a month later with a mean date of 11th February).

Additionally, we make several revisions and additions to Sect. 4.2. In particular, we include the following in L350-354:

"For this subset of events, the mean SSW onset date (17th January) is sufficiently early that this enhancement typically occurs when >50 % of tropospheric ozone is sourced from the stratosphere, at least initially. In contrast, a minimal enhancement of O_3F (~1–2 %) for a much shorter duration is shown to follow nPJO events, which is even less significant as the fraction of ozone originating from the stratosphere is climatologically smaller (~30-50 %) just a few weeks later (mean nPJO event onset date: 11th February) as winter transitions to spring."

Following an earlier study which investigated regional and seasonal aspects of the climatology in tropospheric ozone and stratospheric influence, we add the following sentence for additional context which is helpful for interpreting Fig. 5:

"As shown in Williams et al. (2019), the seasonal peak in lower tropospheric O_3F over the Arctic occurs in winter, despite a maximum in tropospheric ozone during spring (>50 ppbv), since the partition of ozone from the stratosphere (O_3S) is similar in both seasons (~20-30 ppbv) meaning O_3F is typically ~10 % less in spring (~40-50 %)".

Reference: Williams, R. S., Hegglin, M. I., Kerridge, B. J., Jöckel, P., Latter, B. G., and Plummer, D. A.: Characterising the seasonal and geographical variability in tropospheric ozone, stratospheric influence and recent changes, Atmos. Chem. Phys., 19, 3589–3620, https://doi.org/10.5194/acp-19-3589-2019, 2019.

Lines 329-330: "with indication of an enhancement in ozone throughout the troposphere and elevated near-surface ozone which may impact air quality (see Sect. 5)."

This is not true. An enhancement in "stratospheric-origin" ozone is not equal to an enhancement in "full" ozone. The authors may want to add "stratospheric-origin" in front of "ozone".

We refer specifically to ozone of stratospheric-origin in this section, so we certainly are not suggesting this translates to an equal enhancement in "full" ozone (merely we are highlighting that air quality could be impacted in some cases).

We replace mention of 'ozone' in this sentence with 'O_3S' (L347).

Lines 482-486 and Fig. 8a,b

The radiative effect of O_3 seems to depend on the layer. While O_3 increases throughout the vertical layers, it is associated with warming in some layers but cooling in other layers. The authors may provide some explanation for this.

The fact that the (radiatively-determined) temperature response at a given altitude is not solely dependent on the ozone perturbation at that altitude has long been recognised. The important point is that both long-wave as well as short-wave heating rates are affected by the ozone perturbation (especially for wintertime high latitudes), and the low to mid-stratospheric radiation budget is impacted by absorption of upwelling infrared from the troposphere by ozone. Thus, it is not surprising that the peak (in percentage terms) ozone increase in the lower stratosphere (Fig. 8a) is accompanied by a local warming but a cooling above (Fig. 8b), as less upwelling long-wave radiation is now available to penetrate to these levels.

It is worth noting also that the short-wave heating due to ozone is still small in late January (corresponding to the 5 to 15-day lag in Fig. 8a-b). Hence, the anomaly profile in ozone is much more closely synchronised with the ozone heating response for the +50-day lag calculation (Fig. 8c-d) corresponding to early-March. We note that the radiative heating effect due to the ozone anomaly in the LMS (between 200 and 300 hPa) is greater for the later date despite a reduction in the percentage anomaly from nearly 60 % to just under 40 %.

We include the additional/revised sentences below in this paragraph:

L515-520:

"This slight cooling, despite the increase in ozone at these levels, is mostly due to the change in upwelling longwave radiation due to the larger (in percentage terms) increase

in ozone in the lower stratosphere. This deprives the mid-stratosphere of upwelling infrared radiation which would otherwise warm this region; the reverse effect (where a lower stratospheric ozone depletion leads to cooling of the lower stratosphere but a warming of the mid-stratosphere) has been noted by e.g., Ramaswamy and Bowen (1994) and Shine (1996). Given this corresponds to late-January, changes in the longwave radiation budget will dominate over changes in the shortwave radiation budget".

Additional references:

Ramaswamy, V., and Bowen, M. M.: Effect of changes in radiatively active species upon the lower stratospheric temperatures, J. Geophys. Res., 99(D9), 18909–18921, https://doi.org/10.1029/94JD01310, 1994.

Shine, K.P.: On the modelled thermal response of the Antarctic stratosphere to a depletion of ozone. Geophys. Res. Lett., 13, 1331-1334, https://doi.org/10.1029/GL013i012p01331, 1996.

L524-527:

"The ozone anomaly profile more closely matches the heating profile due to ozone throughout the stratosphere as solar insolation is much greater by early-March. We note that the heating effect due to ozone between 200 and 300 hPa is slightly larger for this later lag despite a reduction in the percentage anomaly (from nearly 60 % to just under 40 %)".

Line 487: "highlighting that such radiative effects appear to be relatively long-lasting and potentially important for NWP."

This is not clear to me. Please specify what field in NWP may be sensitive to the radiative effects here. SSWs are associated with tens of degrees of changes in stratospheric temperature, whereas the radiative effects here are up to two degrees only.

We agree that such statement is ambiguous and have revised/expanded such sentence to make it clear that a systematic temperature bias in NWP models, with subsequent impacts on winds, would arise within the UTLS region especially. The revised/additional sentence is included below (L521-524):

"The additional set of calculations averaged 45–55 days after the central warming (onset) date (Fig. 8c-d) yields similar results, indicating that such radiative effects, if ignored, would lead to a systematic temperature bias in NWP models, with possible knock-on consequences for wind fields."

The point being NWP models will already include the adiabatic impact of SSWs and so the ~2K radiative perturbation due to ozone/water vapour is an additional and systematic effect that would largely not be accounted for in such models if at all. In terms of fields sensitive to this, of course temperature is impacted as well as all fields sensitive to the resultant geostrophic adjustment (i.e., winds).

Stratosphere-troposphere coupling should be simulated more accurately if NWP models were to include such effects, particularly on timescales of several weeks after a PJO-type SSW event when dynamical temperature effects have decayed. In particular, the anomalous enhancement of ozone in the LMS (which we show can persist for up to ~2-3 months) would be expected to have a renewed radiative impact. We even show that the ozone heating effect (Fig. 8) is larger for the 45-55-day calculation versus the 5-15-day calculation, despite a reduction in the magnitude of the anomaly, which is testament to the growing importance of solar heating from late-January to mid-March.

The importance of interactive ozone chemistry in representation of stratosphere-troposphere coupling in models is not only speculative on our part. We cite three additional studies in Sect. 7.2 which supports this claim (L650):

Domeisen, D. I., Butler, A. H., Charlton-Perez, A. J., Ayarzagüena, B., Baldwin, M. P., Dunn-Sigouin, E., and Taguchi, M.: The role of the stratosphere in subseasonal to seasonal prediction: 2. Predictability arising from stratosphere-troposphere coupling. *J. Geophys. Res-Atmos.*, *125*(2), e2019JD030923, https://doi.org/10.1029/2019JD030923, 2020.

Friedel, M., Chiodo, G., Stenke, A., Domeisen, D. I., Fueglistaler, S., Anet, J. G. and Peter, T.: Springtime arctic ozone depletion forces northern hemisphere climate anomalies. *Nat. Geosci.*, *15*(7), 541-547, https://doi.org/10.1038/s41561-022-00974-7, 2022.

Monge-Sanz, B. M., Bozzo, A., Byrne, N., Chipperfield, M. P., Diamantakis, M., Flemming, J., and Weisheimer, A.: A stratospheric prognostic ozone for seamless Earth system models: performance, impacts and future. *Atmos. Chem Phys.*, *22*(7), 4277-4302, https://doi.org/10.5194/acp-22-4277-2022, 2022.

In particular, we wish to draw the reviewer's attention to Fig. 5a in Friedel et al. (2022). Their study demonstrates a similar temperature response of ~2 K during episodes of Arctic ozone depletion, which translates to a significant impact on stratosphere-troposphere coupling representation between interactive ozone chemistry versus no ozone chemistry model experiments.

Anonymous Referee #3 – Author Responses

Review of the manuscript “Air quality and radiative impacts of downward propagating sudden stratospheric warmings (SSWs)” (No. egusphere-2023-1175) by Williams et al.

General comments:

This paper investigated anomalous ozone and water vapor perturbations in both the stratosphere and the troposphere following stratospheric sudden warming events (SSWs), with specifically emphasis on the Polar-night Jet Oscillation (PJO) events and on the upper troposphere-lower stratosphere (UTLS) and surface regions. The capability of the EMAC chemistry-climate model in simulating historical ozone and water vapor anomalies was first evaluated compared to the CAMS atmospheric composition reanalysis dataset. A longer simulation from the EMAC model with a stratospheric origin ozone tracer (O3S) was later used to investigate the ozone and water vapor anomalies and their corresponding radiative impacts for the PJO and nonPJO SSWs. The main finding are that significantly prolonged ozone anomalies and vertical water vapor dipole can be addressed in the lower most stratosphere (LMS) following PJO-type SSWs. SSW composites of the O3S further indicate pronounced increases of ozone in the troposphere and a higher frequency for the exceedance of WMO air quality standard at the surface over specific extratropical continental areas. Overall, the paper is well organized and written, and I see the merit of this study in addressing the potential influence of stratospheric ozone on the troposphere and the surface air quality based on model simulations, albeit SSW-related ozone anomalies in the stratosphere have been widely investigated in previous studies. I would recommend publication of this paper after the following comments are fully addressed.

We appreciate your summary of our manuscript and are pleased to hear that you recognise the importance of the study findings. Note that whilst SSW-related ozone anomalies have been studied in detail in several previous studies, the lowermost stratosphere has not been a region of primary focus. This lack of attention is likely due to the small enhancement in absolute terms but which we show leads to non-trivial radiative impacts in the Arctic UTLS, with a statistically significant enhancement in STE of ozone both in the Arctic and affecting the three mid-latitude continental regions of the Northern Hemisphere.

We hope to have satisfactorily addressed your concerns in this revised submission. Our responses are provided in order of each comment. Please note that line (L) numbers referred to in our responses correspond to the revised submission files.

Specific comments:

1. Figure 1b: I’m concerned about the lags used to perform the ozone statistics associated with SSWs. According to previous studies (e.g., de la Cámara et al., 2018, Hong and Reichler, 2021), negative ozone anomalies were found before the onset of SSWs. Therefore, including ozone perturbation at negative lags (i.e., -20 to -5 day) may reduce the significance of SSW composite for ozone. I suggest to modified the analysis of Figure 1b by performing the SSW composite after lag 0. Reference de la Cámara, A., Abalos, M., Hitchcock, P., Calvo, N., and Garcia, R. R.: Response of Arctic ozone to sudden stratospheric warmings, Atmos. Chem. Phys., 18, 16499–16513, https://doi.org/10.5194/acp-18-16499-2018, 2018b. Hong, H.-J. and Reichler, T.: Local and remote response of ozone to Arctic stratospheric circulation extremes, Atmos. Chem. Phys., 21, 1159–1171, https://doi.org/10.5194/acp-21-1159- 2021, 2021.

Thanks for your comment. It is a valid concern that including the short period before the event onset could dilute the signal as any SSW-driven enhancement in STE of ozone would not be expected until the event occurs (or even a few days after). We were merely conscious that Arctic ozone will start to become perturbed in the days leading up to the onset, but the magnitude and extent of a developing positive anomaly is very much event-dependent (and as shown in the two referenced studies, ozone may instead be anomalously low prior to an event over the polar-cap).

We performed some sensitivity tests and find that confining the period from 0- to 70-day does indeed slightly enhance the signal. It should be noted that we still index out the full -20 to +70-day period before aggregating the climatological profiles for the above reason. We furthermore excluded all November and May profiles from our composite Clim profiles, since no SSWs occur in November throughout the selected periods (with only a couple of March SSW which might have a waning influence ozone in May). This further enhances the signal throughout the profile.

We have replaced Fig. 1d with the updated result and modified Table 1 to reflect the reduced number of profiles now included in calculating the SSW composite profile. The number of Clim profiles included has reduced in number too (since November and May are now omitted). However, the number has reduced more substantially as all profiles in the November-May period extracted were previously counted erroneously. So, we have now corrected for this (the actual value of profiles included is much less and closer in terms of number to the amount of SSW profiles included for each station). Note that the number of Clim profiles represents all soundings from December to April for the period extracted minus any that occur within -20 to +70 days of an SSW.

We have also updated Fig. S1 to show the difference only for the ~50 % of PJO-type SSWs, now that we concentrate on a shorter seasonal (December to April) period in the main manuscript. This decision is in part due to a stronger signal which also emerges when neglecting the nPJO (largely synonymous with non-downward propagating SSWs), particularly in the lower troposphere.

We have implemented minor amendments to the first paragraph in Sect. 3.1 and add a couple of sentences to highlight the much larger shift near the surface when only PJO-type events are used and a recommendation that this maybe examined more closely in the future (L217-221):

"We note that the shift between the two profiles overall increases slightly when only including the PJO-type SSWs during this period, with the greatest difference interestingly near the surface (Fig. S1). Whether this shift is physical (e.g., enhanced deep-STE events) or perhaps artificially inflated (e.g., due to temporal sampling bias) is an interesting question that merits further investigation on a case-study basis, although the small sample size of events inhibits such an assessment in a composite-based approach".

2. Ln 279-281: Why the PJO-type SSW in February 2010 was excluded?

We excluded mention of the February 2010 event as the anomaly evolution is much less pronounced following this event according to both CAMS and EMAC. Also, in response to Reviewer One, we add a new sentence noting this. See below (L292-293):

"It should be noted that this signal is not so pronounced following the other major PJO-type event (February 2010) during this period according to either EMAC or the CAMS reanalysis".

3. Ln 327-330: The persistent ozone anomalies in the lower stratosphere after the PJO-type SSWs can be attribute to the weakening planetary wave influence and the longer chemical relaxation time scale. Is there an explanation for the long-lasting O3S or O3F in the troposphere as the dynamical time scale becomes relatively short toward the lower troposphere?

Thank you for your comment. Indeed, the chemical lifetime of ozone in the troposphere is around 3 weeks (note that the amount of O_3S is subject to the same chemical sink reactions as the full ozone field). Further to this, net transport of ozone out of the polar-cap region (60-90°N) can be expected under typically -AO conditions after such events so such signal might be expected to be short-lived if STE were only temporarily enhanced.

Such a protracted signal can therefore likely be attributable to enhanced STE for a sustained period following such an event (the O_3S tracer allows us to state this with confidence). We add a sentence to provide this additional context which should help the reader to fully appreciate the reasoning for a protracted signal See below (L341-345):

"Given a photochemical lifetime of ozone in the free troposphere around 3 weeks (Lelieveld et al., 2009) and a statistically significant negative Arctic Oscillation pattern at least following PJO-type events (Hitchcock et al., 2013), favouring net equatorward transport in the troposphere, such a prolonged signal for elevated O_3S in the lower troposphere can only be attributable to enhanced STE for a sustained period (note that this tracer is subject to the same chemical sink reactions as the full ozone field)".

4. Ln 380-382: As indicated by Figure S3a and Table S5, significant increases in the frequency of grid point incidences can also be found over the LMS (100-300hPa) region from the full O3 tracer. I'm wondering whether this result can also be verified using the CAMS dataset. Could you repeat the analysis of Figure S3a but use the CAMS dataset instead?

Thank you for your suggestion. Although there would be merit in comparing the approach with the observationally constrained CAMS dataset, the limited 9-year period of common overlap hinders our ability to assess this in a composite-based approach. This comparison is more practical, however, if focussing on individual events which is worthy of mention as possible future work.

We add this suggestion to complement the discussion in later Sect. 7.1 (L613-615):

"However, it remains to be seen if such signal is matched in the CAMS reanalysis, in which such assessment is hindered following our composite-based approach over a longer historical period, although this could be looked at on an individual event basis."

5. Ln 483-485: Why an increase of ozone between 1-100hPa leads to cooling temperature anomalies (Figure 8a) while ozone mostly absorbs shortwave radiation on the layer?

This is a good question and one we should have addressed more clearly in the paper. The fact that the (radiatively-determined) temperature response at a given altitude is not solely dependent on the ozone perturbation at that altitude has long been recognised. The important point is that both long-wave as well as short-wave heating rates are affected by the ozone perturbation (especially for wintertime high latitudes), and the low to mid-stratospheric radiation budget is impacted by absorption of upwelling infrared from the troposphere by ozone. Thus, it is not surprising that the peak (in percentage terms) ozone increase in the lower stratosphere (Fig. 8a) is accompanied by a local warming but a cooling above (Fig. 8b), as less upwelling long-wave radiation is now available to penetrate to these levels.

It is worth noting also that the short-wave heating due to ozone is still small in late January (corresponding to the 5 to 15-day lag in Fig. 8a-b). Hence, the anomaly profile in ozone is much more closely synchronised with the ozone heating response for the +50-day lag calculation (Fig. 8c-d) corresponding to early-March. We note that the radiative heating effect due to the ozone anomaly in the LMS (between 200 and 300 hPa) is greater for the later date despite a reduction in the percentage anomaly from nearly 60 % to just under 40 %.

We include the additional/revised sentences below in this paragraph:

L515-520:

“This slight cooling, despite the increase in ozone at these levels, is mostly due to the change in upwelling longwave radiation due to the larger (in percentage terms) increase in ozone in the lower stratosphere. This deprives the mid-stratosphere of upwelling infrared radiation which would otherwise warm this region; the reverse effect (where a lower stratospheric ozone depletion leads to cooling of the lower stratosphere but a warming of the mid-stratosphere) has been noted by e.g., Ramaswamy and Bowen (1994) and Shine (1996). Given this corresponds to late-January, changes in the longwave radiation budget will dominate over changes in the shortwave radiation budget”.

Additional references:

Ramaswamy, V., and Bowen, M. M.: Effect of changes in radiatively active species upon the lower stratospheric temperatures, J. Geophys. Res., 99(D9), 18909–18921, https://doi.org/10.1029/94JD01310, 1994.

Shine, K.P.: On the modelled thermal response of the Antarctic stratosphere to a depletion of ozone. Geophys. Res. Lett., 13, 1331-1334, https://doi.org/10.1029/GL013i012p01331, 1996.

L524-527:

“The ozone anomaly profile more closely matches the heating profile due to ozone throughout the stratosphere as solar insolation is much greater by early-March. We note that the heating effect due to ozone between 200 and 300 hPa is slightly larger for this later lag despite a reduction in the percentage anomaly (from nearly 60 % to just under 40 %)”.

Technical corrections:

1. Ln 371: “… in all cases pertaining to the mean, 90th and 95th …” should be “median, 90th and 95th …” according to Table S1. The same correction should be applied to Ln 553, too.

Corrected.

2. Ln 432: “… when using the O3 tracer with the O3S amount for …” should be corrected as “… when using the O3 tracer with the O3 amount for …” if I understand the methodology for performing the ‘pseudo’ climatology correctly.

This suggestion is incorrect. We first subtract the O_3S partition from the full ozone field for the SSW composites (leaving only the residual of tropospheric-origin ozone). We then add the O_3S partition from the climatological composites in creating these ‘pseudo’ climatological distributions for the full ozone field.

Hopefully this explanation above is clearer, and we revise this sentence accordingly. See below (L459-L461):

“We explore this by first subtracting the O_3S partition from the full ozone amount for the SSW composites, leaving the residual amount of tropospheric-origin ozone, and then add the O_3S partition from the constructed climatological composites (non-SSW years), in creating a set of ‘pseudo’ climatological ozone distributions”.