Reply to the comments RC2 of Referee #1 on the manuscript acp-2021-1066

Thanks again to Referee #1 for the comments to the first reply. Please find a second reply below (again, for orientation, the comments of Referee #1 are included in *Italic*).

General reply:

Obviously, the way how I introduce the motivation (which mirrors the way how it arose) leads to some serious misinterpretations. I follow the suggestion of Referee #1 that "*skipping the observations to a large extent and just presenting the results as an idealized theoretical approach that requires observational justification"* might be the most meaningful way to improve this point in a revised manuscript.

Statements in the text that could lead to such misinterpretations will be skipped or revised. The rough estimations based on the measured GWPED cited in the preprint and used for introducing the motivation will be skipped in both the abstract (l. 3) and the introduction (ll. 51-66). However, some introducing remarks might be allowed, e.g., the citation that Baumgarten et al (2017) "assumed an unexplained process of true geophysical origin responsible for the daylight-nighttime differences in the GWPED during summer months", including a hint on the uncertainty of such measurements.

In Section 2, the short statement that the relative increase in the GWPED of specific GWs calculated by the theoretical approach is "quantitatively in agreement with the observations" (ll. 428-429) will be deleted because it seems to be misleading. Instead, the relevance of the suggested process will be shortly discussed in Section 3 (Summary and Conclusions), in the context of other processes.

A justification of the relevance is given because – for specific GWs with large horizontal and small vertical wavelengths – the change in the relative increase between stratospheric and mesospheric GW amplitudes calculated by the theoretical approach is in the order of observed relative increases (this was thought to be the original message of ll. 428-429). If this change calculated by the theoretical approach would be much smaller, I would not have submitted the preprint because then the process might be not significant. However, as I tried to explain in the first reply, the preprint is a theoretical approach and does not want to *attribute the observed GWPED only to a specific gravity wave with defined properties.* From my point of view, it is evident that one single GW cannot explain an observed GWPED at a specific location. Moreover, at the current stage of research, it is generally not possible to attribute the observed GWPED only to one selected specific process like primary or secondary GWs or tides. However, based on the findings of the preprint it is allowed to conclude that ozone-gravity wave interaction might play a significant role in the middle atmosphere to stimulate further research works.

Comment on public reply:

General Comment:

The reviewer appreciates the quick response to the raised concerns. However, the replies also caused further concerns on the manuscript and require clarification. The reviewer takes the freedom to rephrase the comments a bit to reduce the ambiguity.

HAMMONIA (minor comment):

In the acknowledgments, there is a statement about computational resources. If there were no computational resources used why acknowledge.

Some few resources have been used for handling the data. However, perhaps this statement should be deleted to avoid misunderstandings.

Day and night differences (major concern/ very critical):

The submitted paper points at Lidar observations conducted at the Antarctic and mid-latitudes. These measurements are essential to motivate the main narrative of the paper, but also to justify the results to be relevant. Thus, the paper should present a careful discussion of the observations in the context of this work. The shown GWPED in both publications includes all types of waves, viz. tides, planetary waves, and gravity waves. The different filtering approaches underline this aspect (Erhard et al., 2015, Baumgarten et al., 2017). There is a concern to generalize and attribute the observed GWPED only to a specific gravity wave with defined properties. The observational uncertainties are supposed to be mentioned and discussed here as well.

Yes, I agree again: a variety of dynamic processes contribute to the observed GWPED at a specific location, and not anyone of them can solely explain these observations. In the revised abstract and introduction, the quantifying interpretations of observations will be skipped, and the motivation will be focused on those statements that can be find in cited publications. Some statements in the text will be deleted or revised to avoid the impression that the preprint wants to *attribute the observed GWPED only to a specific gravity wave with defined properties*. In the revised manuscript, the relevance of the suggested process will be discussed in Section 3 in the context of observations and other processes.

There is another major concern when generalizing the polar day-night differences, which are, in fact, summer-winter seasonal differences and cannot be linked to the mid-latitude day-night difference. These are entirely different physical aspects due to critical level filtering, source variability, and gravity wave propagations conditions.

Looking at Figures 2 and 3 in Baumgarten et al., 2017 does not indicate any local time dependence of the gravity wave activity. Only monthly averaged GWPED results show a day-night difference. Baumgarten et al., 2017 even discussed the day-night differences as part of the analysis bias concerning tides. This was later confirmed by Baumgarten et al., 2019 when the day-to-day variability was analyzed combining spatial and temporal filters into one multi-dimensional retrieval. This is also an aspect for planetary waves and lidar observation as demonstrated by Eixmann et al., 2020 (AG), which is relevant for summer–winter comparison at the Arctic/Antarctic.

Yes, also here I agree again, all details of the observed *summer-winter seasonal differences cannot be linked to the mid-latitude day-night difference*. The processes responsible for the daylight-nighttime differences could play a role for polar-day-polar night differences, but much more research is needed to attribute the observed GWPED unequivocally to the different processes operating in the real atmosphere. The abovementioned aspects of Referee #1 can be included in the discussion in Section 3.

Critical level filtering:

The reviewer strongly disagrees with the statement in the replies that "Critical level filtering occurs during strong westerlies between April and October (see Figure 7 of Kaifler et al., 2015)". Critical level filtering is present at all times and during all seasons, however, depending on the sign of the stratospheric winds different gravity waves encounter the critical level depending on their propagation direction and phase speed. This is directly related to the source questions and multi-step-vertical coupling processes.

I do not really understand the strong disagreement. The first reply was clearly related to the change from westerlies to easterlies, which is undoubtedly the most important factor in the seasonal cycle of critical level filtering. Of course, critical level filtering is present during summer months; however, other processes influencing upward propagating GWs could be much more effective during summer because of the seasonal change in the zonal winds. Indeed, this change might be one of the most important factors leading to somewhat stronger mesospheric GWPED during summer than winter although the tropospheric GW sources are generally weaker during summer than winter.

However, in the revised manuscript, a short discussion of the potential relevance of ozone-gravity wave interaction will be given in Section 3, in the context of other relevant processes like critical level filtering.

Tidal amplitudes and gravity wave amplitudes:

Tidal amplitudes (semidiurnal or diurnal) can reach up to 8-15K (stratosphere/lower mesosphere) and occasionally 20 K (mesosphere) at the middle atmosphere between 30-80 km (e.g., from MERRA2). However, the amplitudes of tides are altitude-dependent and undergo the same exponential growth as gravity waves. The reviewer does not agree and has not seen observational evidence for an order of magnitude difference between tidal and gravity wave amplitudes (in a statistical sense) at the stratosphere and mesosphere. None of the lidar observations that are presented in the motivation are even close to the Rocky Mountains.

In the first response to the preprint, the reviewer #1 gave a general statement that tides *have almost similar or larger amplitudes compared to gravity waves at the stratosphere and mesosphere*, together with the hint on Baumgarten and Stober (2019) who found amplitudes of tides in the upper stratosphere/lower mesosphere in the order of 1 K to 2K, which is not almost larger than GW amplitudes. The example of the Rocky Mountains was only used as one counterexample to the general statement.

However, as above, the relevance of the suggested process of ozone-gravity wave interaction will be shortly discussed in Section 3, in the context of other processes like tides.

Sinusoidal approximation of gravity wave:

The reply draws an analogy between a gravity wave and a pendulum. This approximation seems to be by far too idealized as it skips key properties of a wave for real atmosphere application as they are found in observations. Gravity waves have a 3-dimensional wave vector and an intrinsic period and often occur not as an isolated plane wave but in wave packages. These packages have an envelope function, which is often assumed/approximated to be Gaussian. Depending on the background flow and the properties of the wave trains in the package cancelation effects are likely as updraft and downdraft phases can mix for a fixed observer on the ground in the Eulerian frame of reference. A pure vertical 1 D approximation is fine as a theoretical approach, but hard to be generalized in a real environment.

Of course, a gravity wave is usually described by an amplitude, a 3-dimensional vector, and an intrinsic period, as also used in the preprint (l. 132, l. 212) to derive the dispersion relations. The simple image of a pendulum was only used in the first reply to clarify that the discussed effect of ozone-temperature coupling amplifies both the maximum and minimum of the oscillating wave pattern, but it does not play any role in the preprint. The preprint uses a variety of single waves to highlight the process of ozone-temperature coupling as clear as possible. In summary, yes, the preprint is based on an idealized approach; however, the results might stimulate further research works based on observations or model simulations.

In summary:

The reviewer values the theoretical approach presented in the manuscript but has serious concerns about the motivation and justification of its importance. A revision of this manuscript either requires dealing with all the observations in more detail, including atmospheric tides and other dynamical effects as well as their biases, or skipping the observations to a large extent and just presenting the results as an idealized theoretical approach that requires observational justification. The way how the amplitude growth is well-founded between theory and observations is not appropriate. However, a justification could be also achieved by performing ICON model runs with high resolution to investigate the presented approach with resolved gravity waves in more detail. In principle, this is also possible with HIAMCM. Gravity wave resolving models permit a less ambiguous wave characterization. Such model runs will certainly strengthen the presented conclusions if confirmed. However, the reviewer understands that the model runs are a lot of work and might be postponed to future work.

Yes, I agree, abstract, introduction and discussion in Section 3 will be revised as outlined in the general reply above.

Indeed, currently I am going to perform UA-ICON model simulations with high resolution and interactively coupled ozone chemistry (with the help of some collaborators) for justifying the proposed process. This will need some time and is beyond the scope of the present preprint. However, it would be great if the results of the preprint would stimulate also other modelling groups to examine this process.