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

Thanks to Referee #1 for critical comments. First, I would like to give a general reply, and then a point-by-point response below (for orientation, the comments of Referee #1 are included in *Italic*).

General reply

Obviously, some points of the paper should be clearer from the beginning. The paper is indeed motivated by the conclusion of Baumgarten et al. (2017) that daytime-nighttime conditions have a significant but unexplained effect on the GWPED in the summer stratopause region; however, the paper does not intend to claim that all details of the observed GWPED profiles *can be solely explained* by ozone-gravity wave interaction as obviously concluded by Referee #1. The aim of the paper is to demonstrate that ozone-gravity wave interaction can principally lead to a significant amplification of gravity wave amplitudes, based on a theoretical approach excluding other perturbations like small-scale diffusion (l. 278), but also excluding other perturbations like atmospheric tides or so-called secondary gravity waves found in idealized model simulations, which are not explicitly discussed in the paper as required by Referee #1.

I agree that some revisions in the text and some more discussion could be helpful to make these points clearer. I also agree with Referee #1 that "*the theoretical model of dynamical coupling of the ozone heating rate with wave dynamics is certainly of interest*"; accordingly, the required points (critical level filtering due to the zonal wind, atmospheric tides, secondary gravity waves) will be included in the introduction and discussion of the revised manuscript.

General Comment:

Gravity waves (GW) are a major source of the internal variability of the middle atmosphere. Motivated by lidar observations there is a claim that the gravity wave potential energy density (GWPED) during daylight can be enhanced compared to nighttime measurements at the upper stratosphere and mesosphere. This study seeks to present a theoretical approach to explain this enhancement by gravity wave-ozone interaction, due to changed heating/cooling rates caused by the vertical transport of air parcels by GW assuming idealized inertia gravity waves and an upward level-to-level propagation. The derived theoretical model of GW-ozone interaction was implemented in the well-established HAMMONIA model and all results are based on such model runs.

I do not really understand the last sentence. Note here, for avoiding any misunderstandings, that the preprint uses HAMMONIA data only as a prescribed constant background for the analytic solutions derived in Section 2 (ll. 95-99); the analytic solutions describing ozone-gravity wave interaction (Sections 2.1 and 2.2) were not implemented in the HAMMONIA model, and related model runs were not carried out.

However, there are major (almost fatally flawed) concerns to some parts of the submitted paper, which certainly require a more controversial and critical scientific analysis to support the results.

As far as I understand, the *major (almost fatally flawed) concerns* of Referee #1 are related to the motivation of the paper by the full-day measurements of the GWPED published by Kaifler et al. (2015) and Baumgarten et al. (2017), and to a lack in discussing the seasonal cycle of the GWPED in relation to other relevant processes like critical layer filtering, atmospheric tides, and secondary gravity waves.

Perhaps it is meaningful to emphasize from the beginning that the preprint does not intend to analyze all the details of the observed GWPED profiles, or the reliability of these profiles which was already done in the cited papers. The preprint is motivated by the daytime-nighttime differences in the GWPED published by Baumgarten et al. (2017) revealing an unexplained stronger GW activity in the summer stratopause region during daytime than nighttime (compare Figure 6 and Figure 9 of Baumgarten et al., 2017). This must have an effect on the seasonal cycle at all latitudes, particularly on the change between polar night (or winter) and polar day (or summer), although – of course – not all features of the observed seasonal cycle can be explained by the daytime-nighttime differences. The aim of the preprint is to examine and to quantify the potential contribution of ozone-gravity wave interaction to this unexplained phenomenon of daytime-nighttime differences excluding any other processes contributing to the observations.

Related revisions in the introduction and discussion of the manuscript will be included following the replies to the specific comments of Referee #1 below.

Specific comments:

While reading the manuscript, the reviewer usually browses the web is to collect background information. During this search, I noticed that the Institute of the Author listed a similar paper with the same title as accepted publication in ACP. If the paper is already accepted this review might already be obsolete (see attached screenshot from 31.01.2022).

Thank you very much for this hint. This was a mistake of our administration managing the list of papers of the author's institute. I had ordered to put the citation of the ACPD preprint into the list as a possibility to stimulate the open discussion, but – of course – not an accepted ACP paper. I am very sorry that I didn't realize this mistake earlier. The citation is removed.

Lidar observations have become a standard technique to measure temperature fluctuations in the middle atmosphere. Already a few decades ago such observations were used to derive GWPED. This study was motivated by lidar observations conducted during a campaign at the Davis station (69°S) in Antarctica (Kaifler et al., 2015) and mid-latitude observations at Kühlungsborn (54°N) (Baumgarten et al., 2017,2018). The reviewer did look at all three publications and tried to understand what is mentioned on page 3 lines 53-62. The Antarctic observations (Kaifler et al., 2015) are seasonal summer and winter differences and do not allow to distinguish a day-night comparison and, thus, it is hard to attribute the seasonal GWPED difference between the stratosphere and mesosphere to be caused by GW-ozone interaction. The seasonal differences of the tropospheric GW sources and mean circulation at the middle atmosphere should be considered and are likely contributing a lot to these differences. Secondly, the wind profile is dramatically different between a polar summer and winter condition, which directly affects the critical level filtering due to the strong zonal wind reversal at the summer MLT.

(*Reply to comment on seasonal summer and winter differences*) The Antarctic observations illustrated in Figure 6 of Kaifler et al. (2015) show monthly means, therefore it allows to distinguish polar day conditions (December-January) from other months. Following Baumgarten et al. (2017), the GWPED at mid-latitudes is significantly stronger during daylight than nighttime (as summarized at ll. 56-61). It is evident that the related process must contribute to observed polar day-polar night differences (the question is how large), and therefore to the observed seasonal cycle or latitudinal dependence of the GWPED. This might become somewhat clearer as follows.

Critical level filtering occurs during strong westerlies between April and October (see Figure 7 of Kaifler et al., 2015), and the relative increase in GWPED with height (see Figure 6 of Kaifler et al., 2015) is stronger during polar day (factor ~6 during December and factor ~13 during January) than during times of increasing or decreasing nighttime without strong westerlies (factor ~5.3 during February and factor ~4.5 during November). Roughly estimated, the relative increase is stronger during polar day (mean values of December and January) than during February or November by a factor of about ~2, suggesting that the responsible process enhancing the GWPED becomes much stronger when changing from summer mid-latitudes to polar day conditions. This supports – from my point of view – the preliminary thesis that the stronger amplitudes during polar day could be related to the same process responsible for the daylight-nighttime differences found by Baumgarten et al. (2017); however, it does not intend to give a final explanation of the whole seasonal cycle of the observed GWPED but only an illustration of the gap in understanding the observations.

(*Reply to comment on seasonal differences in tropospheric GW sources*) Of course, the mesospheric GWPED depends on the GW sources in the troposphere or stratosphere; the question is what happens in between. In summary, the preprint provides better understanding of the principal dependence of the mesospheric GWPED on the stratospheric GW sources if ozone-gravity wave interaction is considered. This might be more clearer as follows.

GW sources are specified by GW amplitudes and other GW characteristics (horizontal and vertical wavelength, or intrinsic frequency). The seasonal differences of the amplitudes of the stratospheric GWPED amplitudes and the resulting mesospheric GWPED are – for example – given in Figure 6 of Kaifler et al. (2015) and Figures 6 and 9 of Baumgarten et al. (2017). The focus of the preprint is the relative ratio between the mesospheric and stratospheric GW amplitudes (which shows the discussed strong daylight-nighttime or polar day-polar night differences) in case of a specific prescribed GW source in the middle stratosphere, assuming exponential growth of the GW amplitude with height as first guess. This approach allows simplified solutions describing the principal process of ozone-gravity wave coupling for any initial GW perturbation excluding any other processes.

The preprint needs middle stratospheric GW sources as initial conditions. For evidence, the preprint assumes a moderate GW amplitude of 1 K in the middle stratosphere as starting point (page 8, line 245), which can be set to other values; however, these other values would not change the relative increase between middle stratosphere and mesosphere, either in case with or without ozone-temperature coupling. The dependence of the process on the other GW characteristics (horizontal and vertical wavelength, or intrinsic frequency) is discussed throughout the whole paper. Associating roughly the seasonal cycle and the latitudinal distribution of the GWPED in terms of daylight-nighttime conditions, Figures 5 and 6 of the preprint illustrate this effect of ozone-gravity wave interaction on the relative ratio between the mesospheric and stratospheric GWPED for different GW sources.

(*Reply to comment on critical level filtering)* Of course, the seasonal cycle in critical level filtering due to the zonal wind contributes to the seasonal cycle of the observed GWPED, as mentioned in the introduction of the preprint (page 3, lines 53-66). However, as discussed above, this process is operating during Apr-Oct when the westerlies are strong, but not during Nov-Mar, and the question arises why the GWPED increases much stronger during polar day than during Feb or Nov. Obviously there is a gap in understanding the observed GWPED during polar day, and the preprint examines a specific process to fill this gap.

For clarification, some revisions will be included in the introduction and discussion according to the replies above.

At the mid-latitudes, Baumgarten et al., 2017 showed different climatologies of GWPED for different filtering methods. This points to another major concern when using the numbers. The GWPED seems to depend on the analysis method, which does not provide confidence that the ratios between the stratosphere and mesosphere can be derived reliable enough to support the hypothesis of the proposed GW-ozone effect. In particular, this is also mentioned in Kaifler et al., 2015 as well. Due to the decreased iron layer thickness during the summer at the MLT, the estimated GWPED values are more uncertain and sometimes not derivable applying the same filtering methodology. Erhard et al., 2015 also performed a detailed study to investigate the sensitivity of the different methods to estimate GWPED. These aspects deserve some more clarification in the introduction.

The uncertainties of the cited Lidar measurements have not been discussed in the preprint because this is already explained and discussed in Baumgarten et al. (2017) and Kaifler et al. (2015), where both the stratospheric and mesospheric GWPED are shown as reliable results of these measurements. Baumgarten et al. (2017) explicitly discuss the daylight-nighttime differences in relation to the used filtering methods concluding that these differences might be of true geophysical origin (see Figures 6 and 9 of Baumgarten et al., 2017, and the related discussion), which is worthwhile enough for other researchers to examine a potential process leading to this phenomenon. However, for clarification, a related comment on the reliability of the measured GWPED will be included in the revised introduction.

Another crucial concern when dealing with lidar and model data to investigate day-and-night differences are atmospheric tides. The ozone volume mixing ratio shows a very fast response to the terminator (sunlight) (e.g., https://doi.org/10.5194/acp-18-4113-2018). This time scale is much shorter than the investigated intrinsic gravity wave periods. Thus, it appears to be unlikely that an air parcel that is in the updraft part of an inertia gravity wave could sustain the volume mixing ratio over hours without getting back to the chemical equilibrium to the ambient atmosphere. Radiative processes seem to happen on much shorter time scales. Thus, the theoretical description of the paper might be correct, but the total effect could be much smaller as one needs a convolution with the time scales.

Thanks a lot for the hint on the interesting paper of Schranz et al. (2018). However, I do not really understand the related critics on the present preprint.

In the upper stratosphere, ozone shows indeed a very fast response to changes in radiation because it is approximately in temperature-dependent photo-chemical equilibrium, which is an essential preliminary of the preprint (ll. 67-68; ll. 146-164). Therefore, local ozone and temperature perturbations due to an upward propagating mesoscale gravity wave must nearly instantaneously lead to local changes in the temperature-dependent photo-chemical equilibrium, including nearly instantaneously coupled perturbations in ozone and temperature over the time-scale of the gravity wave perturbation compared to the unperturbed environment.

In comparison to the mesoscale gravity waves, atmospheric tides or the diurnal cycle of ozone are planetary-scale variations; indeed, they can change the background conditions for the local propagation of the mesoscale GW perturbations. One main point of the diurnal cycle is included in the preprint: the addressed process of ozone-gravity wave interaction is operating during daylight only but not during nighttime (this change is particularly important for slowly propagating GWs and considered in Section 2).

Following Schranz et al. (2018), the relative amplitude of the diurnal cycle of upper stratospheric ozone is in the order of 5% (summer solstice) to 8% (May); a somewhat stronger planetary-scale ozone background during daylight could principally lead to a somewhat stronger effect of ozone-gravity wave interaction on the mesoscale GW perturbations, analogously to a somewhat stronger or weaker effect in case of a long-term change in stratospheric ozone (in Section 2.2, ll. 391-399, the sensitivity is given in case of a change in the ozone background of 10%).

For clarification, the revised manuscript will include some more comments and discussion on the diurnal cycle of ozone. However, the theoretical approach calculates the effect of ozone-gravity wave interaction straight forward assuming a constant background and does not need any convolution of the time scales.

Atmospheric tides are also important to estimate reliable GWPED. Baumgarten et al., 2019 (https://doi.org/10.5194/angeo-37-581-2019) demonstrated that there is also some interday tidal variability. Most of the above mention filtering techniques do not account for tides, which have almost similar or larger amplitudes compared to gravity waves at the stratosphere and mesosphere. Thus, the GWPED needs to be corrected for such tidal contaminations. This is also an issue for the HAMMONIA data, which is also affected by tidal modes. It remains unclear how day-night differences could be distinguished from the diurnal excitation due to the ozone absorption and associated heating rates. The advantage of tides is that the migrating tidal modes DW1, SW2, TW3 are sun-synchronous and fulfill the requirements assumed for the theoretical framework presented in the submitted manuscript. GW have random phases concerning their temporal behavior due to the various excitation mechanisms therefore it is unlikely that the updraft phase remains sun-synchronous, which is the key assumption in the manuscript. More likely is a random superposition of GW and a potential cancelation of the updraft and downdraft phases, which may result in a total zero effect.

Yes, considering atmospheric tides is certainly important to understand the details of the observed GWPED variability, and it is challenging to separate mesoscale GW perturbations and planetary-scale tidal variations from a time series of measured GWPED at a specific location. However, this is not the purpose of the preprint. The theoretical approach of the preprint intends to examine the effect of ozone-gravity wave interaction on the increase of mesoscale gravity wave amplitudes with height in the upper stratosphere/lower mesosphere region excluding – for a first guess – other perturbations or variability. However, an additional comment in the revised manuscript might be necessary that tidal variations can principally modulate the planetary-scale background for the propagation of mesoscale gravity waves, and that an amplification of GW amplitudes due to ozone-gravity wave interaction can principally lead to a stronger dissipation of the tidal signals in observations.

I do not really agree with the statement that tides *have almost similar or larger amplitudes compared to gravity waves at the stratosphere and mesosphere*. Particularly in the stratosphere and lower mesosphere, gravity wave amplitudes can be much larger than tidal variations. For example, the GW amplitudes forced by the Rocky Mountains can be one order larger (5 K to 10 K) than the tidal variations between 30 km and 70 km altitude shown in the above recommended paper of Baumgarten and Stober (2019).

Note also again that the preprint does not analyze any variations of the HAMMONIA data. The HAMMONIA data are only used for prescribing a constant monthly mean background for the theoretical approach of upward propagating gravity waves. Any diurnal variations or tides produced by the HAMMONIA model are not considered.

In this context I do not really understand the comment of *a potential cancelation of the updraft and downdraft phases, which may result in a total zero effect.* The gravity wave is a sinusoidal perturbation oscillating between (positive) updraft and (negative) downdraft, analogously to the swing of a pendulum, and the result of the preprint is that the amplitude is increasing while the frequency is decreasing (i.e., the time between the maximum and minimum of the oscillation becomes larger) when propagating through the upper stratosphere/lower mesosphere. Following Section 2, this process can be expressed in terms of the static stability in the dispersion relation. This process depends on the planetary-scale background conditions, which is varying – for example – due to long-term changes in ozone or changes in the static stability due to tidal variations; however, I do not see any cancelation of the updraft and downdraft phases.

Some related comments on atmospheric tides will be included in the revised manuscript to clarify this point from the beginning.

The results indicate that the effect of gravity-wave-ozone coupling is most pronounced above the stratopause. Recently, a concept called multi-step vertical coupling (MSVC) was introduced Becker and Vadas, 2018 and later publications. Primary GW launched in the troposphere such as mountain waves, frontal waves, jet instabilities, etc. propagate vertically and dissipate generating a body force, which again causes secondary waves, which propagate further upward and so forth up to the thermosphere.

Yes, these secondary waves found in idealized model simulations could be a significant factor contributing to the observed GWPED. However, the preprint wants to highlight the effect of ozone-gravity wave interaction on the amplitudes of upward propagating gravity waves, therefore any other perturbations like secondary gravity waves are excluded as a first guess. If the multi-step process producing secondary gravity waves described by Becker and Vadas (2018) is operating in the real atmosphere, the process of ozone-gravity wave interaction might have a significant effect on the amplitudes of these waves as far as they are vertically propagating through the upper stratosphere/lower mesosphere; however, this is speculative at the moment and needs some more numerical model simulations with both resolved gravity waves and interactive ozone photochemistry, which is beyond the scope of the present preprint. A related comment will be included in the revised introduction and discussion.

Considering the above-mentioned physical processes it appears to be unlikely that the ratios between the stratospheric and mesospheric GWPED can be solely explained by the proposed GW-ozone interaction. MSVC, the horizontal propagation of GW, or atmospheric tides play also important roles and deserve a detailed and critical assessment in this regard to understand the vertical profile of GWPED.

As mentioned in the general reply above, the preprint does not want to claim that the observed GWPED profiles mentioned in the introduction *can be solely explained* by ozone-gravity wave interaction. The preprint also does not want to explain all details of the observed GWPED. The aim of the preprint is to demonstrate that ozone-gravity wave interaction can significantly contribute to the unexplained daylight-nighttime differences in the gravity wave amplitudes. Therefore, the theoretical approach excludes other perturbations or variability modulating the background, like secondary gravity waves or atmospheric tides, to highlight this process. Some more comments on the specific points of Referee #1 will be included in the revised introduction and discussion of the manuscript, as outlined above.

However, the theoretical model of dynamical coupling of the ozone heating rate with wave dynamics is certainly of interest but should be contextualized with atmospheric tides and tidal excitations. The claim in the abstract that "ozone-gravity wave interaction is largely responsible for this effect" is certainly not so straightforward justified given the other dynamical aspects and the idealized model simulations.

As mentioned above, I completely agree with Referee #1 that "*the theoretical model of dynamical coupling of the ozone heating rate with wave dynamics is certainly of interest*". In the abstract, a revised statement that "ozone-gravity wave interaction can significantly contribute to this phenomenon" (i.e., to the stronger increase in amplitudes with height during daylight than nighttime) might be better than "ozone-gravity wave interaction is largely responsible for this phenomenon".

A revised manuscript and an additional point-by-point reply to the comments of Referee #1 will be uploaded following the regulations of ACP.