Reply to the comments RC1 and RC3 of Referee #1 on the manuscript acp-2021-1066

Thanks again to Referee #1 for critical comments. In addition to the first and second reply in the open discussion, please find here the final reply to all comments including the specified changes in the revised manuscript (for orientation, the comments of Referee #1 are included in *Italic*).

General reply (summary)

As general statement to the major concerns: the paper does not want to claim that observed GWPED profiles *can be solely explained* by ozone-gravity wave interaction, or to *attribute the observed GWPED only to a specific gravity wave with defined properties.* This was, of course, not the intension of the paper. The aim of the paper is to demonstrate that ozone-gravity wave coupling can principally lead to significant amplitude amplifications and daylight-nighttime differences within an important range of mesoscale GWs, but not to provide a complete explanation of published GWPED profiles derived from measurements.

Accordingly, the motivation in the abstract and introduction is revised to avoid such a misunderstanding. Possible effects of ozone-gravity wave coupling on daylight-nighttime differences and polar day-polar night differences are now discussed in Section 3, including additional comments on other relevant processes (critical layer filtering by the zonal wind, atmospheric tides, secondary gravity waves). Additionally, section 2 (subsection 2.2.4) includes now information on the sensitivity of the effect of ozone-gravity wave coupling to the modulations of the background by the diurnal cycle of ozone or atmospheric tides.

General Comment (RC1)*:*

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.

As mentioned in the first reply, there might be a misunderstanding: HAMMONIA data are only used as prescribed constant background; the analytic solutions describing ozone-gravity wave coupling were not implemented in the HAMMONIA model, and related model runs were not carried out (previous preprint: ll. 96-99; revised manuscript: ll. 92-95).

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.

Specific comments (RC1)*:*

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 again. As mentioned in the first reply, this was a mistake of our administration. The citation was removed after I received the reviewer's comment.

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.

This part of the introduction, which summarizes the motivation of the paper, is revised; the notes on the seasonal cycle are skipped; instead, it is only highlighted that the published GWPED measurements of Baumgarten et al. (2017) might be uncertain but interesting enough to stimulate the examination of the present paper (page 3, ll. 51-62).

The relevance of ozone-gravity wave coupling in relation to the GWPED values derived by Baumgarten et al. (2017, 2018) and Kaifler et al. (2015) is now discussed in Section 3, including a statement that the latter did not separate daylight-nighttime differences explicitly (Section 3, ll. 489-530, particularly ll. 512-514).

As a conclusion, the potential relevance of ozone-gravity wave coupling is highlighted; however, it might be evident that the paper does not want to attribute the seasonal differences in the GWPED solely to this process (ll. 39-40, ll. 500-503, ll. 526-530).

Also, to avoid such a misunderstanding, two short conclusions on the quantitative agreement between the effect of ozone-gravity wave coupling and the observed relative increase in the GWPED between stratosphere and mesosphere are deleted; instead, the results of the theoretical approach are just summarized before discussing the relevance in section 3 (previous preprint: ll. 373-375, ll. 428-429; revised manuscript: ll. 391-394, ll. 493-494 and ll. 504-505).

A note on the seasonal differences in the stratospheric GW sources is included, and the possible contribution to the mesospheric GWPED is discussed (ll. 510-511, ll. 520-526).

Of course, critical level filtering by the zonal wind plays an important role in the seasonal cycle of the GWPED; it is now highlighted in the discussion (ll. 515-516).

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 general uncertainties of the cited GWPED measurements and its daylight-nighttime differences are now explicitly mentioned in the introduction, including more explicitly the conclusion of Baumgarten et al. (2017) that the daylight-nighttime differences might be of true geophysical origin (ll. 51-62).

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.

As stated in the first reply, I do not really understand all these critical points; obviously there was a misunderstanding concerning the constant background which does not include tidal variations (see above); however, I try to give a reply as far as I understand.

In the paper, nearly instantaneous temperature-dependent photo-chemical equilibrium is considered as an essential preliminary of the examination; accordingly, 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. The theoretical approach calculates this effect of ozone-gravity wave interaction straight forward assuming a constant background and does not need any convolution of the time scales (photo-chemical equilibrium is introduced in the introduction at ll. 63-64, and in subsection 2.1.2, ll. 145-165).

An interesting point could be indeed that atmospheric tides or the diurnal cycle of ozone are planetary-scale variations which can change the background conditions for the local propagation of the mesoscale GW perturbations. Based on the diurnal cycle of stratospheric ozone presented by the recommended paper of Schranz et al. (ACP, 2018), the revised manuscript includes an estimation of this effect in subsection 2.2.4 and a related note in the discussion (ll. 423-429, ll. 551-554).

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.

The general problem of tides in estimating reliable GWPED values based on local time series is now discussed in the introduction, together with a note in section 3 (ll. 53-59, ll. 517-520).

However, again, there might be a misunderstanding. The paper uses monthly means of HAMMONIA as constant background, and any tides simulated by HAMMONIA are not considered; in the theoretical approach, atmospheric tides are excluded as a first guess, like other processes, as now explicitly mentioned (ll. 286-287).

In this context, there is also not any potential cancelation of the updraft and downdraft phases; the basic idea and the theoretical approach describes the amplification of both the ascent (wave crest) and descent (wave trough) of a sinusoidal GW pattern during its propagation through the USLM; for clarification, the text is improved from the beginning (abstract and introduction: ll., 32-34, ll. 73-83; section 2.1.2: ll. 142-146, ll. 163-165, ll. 185-187; section 2.1.4, ll. 253-254, ll. 262-264; section 3, ll. 462-464).

In addition, like for the diurnal cycle of ozone, the revised manuscript now includes an estimation and discussion of the potential effect of tides on the cumulative amplification of the GW amplitudes by modulating the background temperature, based on the tidal amplitudes between 30 km and 70 km altitude shown in the recommended paper of Baumgarten and Stober (2019), but also assuming much larger modulations; overall, the related effects are smaller than the first-order effect of ozone-gravity wave coupling by approximately one order (ll. 430-438, ll. 551-554).

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.

Secondary gravity waves might be an interesting phenomenon and a note is included in the discussion; however, in the theoretical approach, they are excluded as a first guess like other processes, as now explicitly mentioned (ll. 286-287; ll. 518-520).

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.

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 in the general reply above, the preprint does not want to claim that observed GWPED profiles *can be solely explained* by ozone-gravity wave interaction. The abstract, introduction and discussion of the manuscript are improved to avoid such a misunderstanding, including additional sensitivity calculations and discussion in relation to the other processes mentioned by the reviewer (abstract: ll. 28-30 and ll. 39-40, introduction: ll. 51-62, subsection 2.2.4: ll.423-438, section 3: ll. 493-530 and ll. 551-554).

Comment on public reply:

General Comment (RC3)*:*

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.

As mentioned in the first reply, some few resources have been used for handling the data. However, this statement is deleted to avoid misunderstandings (l. 570).

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.*

As described above, the related motivation in the introduction is improved, including statements on the uncertainties in relation to the filtering methods; the discussion in section 3 is extended in relation to other important processes contributing to local GWPED profiles derived from measurements (ll. 51-62, ll. 493-530).

As mentioned above, the paper does not want to *attribute the observed GWPED only to a specific gravity wave with defined properties.* The results show a significant effect of ozone-gravity wave coupling within a wide range of mesoscale GWs, therefore it might be relevant for the middle atmospheric circulation. For clarification, the text of the manuscript is improved (ll. 39-40, ll. 493-504, ll. 526-530).

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.

As mentioned above, the considerations on polar day - polar night differences are skipped in the revised introduction, whereas comments on the uncertainties in the GWPED due to the effect of tides are included; again, the GWPED measurements of Baumgarten et al. (2017) might be uncertain but interesting enough to motivate the present paper; the potential relevance of ozone-gravity wave coupling on daylight-nighttime differences and polar day – polar night differences, and other aspects due to critical level filtering, source variability, and varying conditions are now discussed in section 3 (introduction: ll. 51-62, section 2.2.4: ll. 423-438, section 3: ll. 493-530 and ll. 551-554).

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.

As mentioned in the second reply, 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, this does not change the essential results of the paper which examines the principal effect of ozone-gravity wave coupling. A comment on critical level filtering is included in the discussion (ll. 515-516).

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.

Note again that, in the first comment to the preprint, 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 this general statement.

However, as mentioned above, planetary-scale atmospheric tides can indeed modulate the background conditions for mesoscale GWs; in the revised manuscript, the related modulation of ozone-gravity wave coupling is considered, based on the tidal amplitudes shown in the recommended paper of Baumgarten and Stober (2019) but also assuming much larger modulations, together with a note in section 3 (ll. 430-438, ll. 551-554).

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.

As mentioned in the second reply, the simple image of a pendulum in the first reply was only used 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 paper. In the paper, the specified gravity waves are described as usual by an amplitude, a 3-dimensional vector, and an intrinsic period (now at l. 129, l. 214).

The paper uses not only one but a variety of single waves within the range of mesoscale GWs. This idealized approach is useful to achieve evidence whether the effect of ozone-gravity wave coupling is a relevant factor in the middle atmospheric circulation. For clarification, the text of the manuscript is improved (ll. 39-41, ll. 493-504, ll. 511-512, ll. 526-530).

Further investigations are needed to examine this effect in case of upward propagating gravity wave packages, particularly based on GW resolving numerical models with interactive ozone photochemistry, which is beyond on the scope of the paper; in the revised manuscript, this perspective is somewhat more highlighted (ll. 501-503, ll. 528-530).

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.

The first part of the introduction summarizing the motivation is revised, where the considerations on observations done in the preprint are skipped to a large extent; instead, the potential relevance of the analyzed effect is discussed in section 3, just presenting the results in the context of observations, and not without emphasizing that this process is one of others, and that more investigations are needed to fully understand its effects in the real atmosphere or in GW resolving numerical model simulations (ll. 51-62, ll. 493-530).

As mentioned in the second reply, I am going to perform UA-ICON model simulations with high resolution and interactively coupled ozone chemistry (with the help of some collaborators); however, this will need some time in terms of carrying out the simulations and analyzing the details of GW activity and GWPED, which might be not easier than analyzing observed temperature fluctuations; an additional note on this perspective is included in section 3 (ll. 501-503, ll. 528-530).

Reply to the comments RC2 of Referee #2 on the manuscript acp-2021-1066

Thanks again to Referee #2 for critical comments. In addition to the first reply in the open discussion, please find here the final reply including the specified changes in the revised manuscript (again, for orientation, the comments of Referee #2 are included in *Italic*).

General reply (summary)

The layout of the manuscript (section 2) is revised separating more clearly method and results.

The presentation of the concept is improved (both in the introduction and in section 2) – it might be now evident that both the ascent and descent of a sinusoidal gravity wave are amplified (i.e., the amplitude is increasing while the frequency is decreasing).

A sensitivity test on using a height-dependent or a constant scale height is included; this leads to a change in the effect of ozone-gravity wave coupling in the order of 10% to 20%.

A note on a possible relative increase of the GWPED between stratosphere and mesosphere in relation to the different seasonal cycle in stratospheric and mesospheric GW activity derived from local measurements is included in the discussion.

The paper presents a possible mechanism of amplitude amplification of gravity waves by the interaction between ozone and gravity waves in the upper stratosphere/lower mesosphere. The paper is divided into three parts: an introduction, a section on the interaction between ozone and gravity waves, and a section titled "Summary and Conclusions." There are 6 Figures that present the results. I had difficulty following the content of the paper for several reasons.

First, the paper is written very compactly. The derivation of the main equations proving the positive feedback of the ozone-gravity wave coupling uses components from different sources, and I would have liked a clearer separation to make it easier for the reader. Also a clear distinction between methodology and results would be most welcome! Therefore, I propose to revise the layout of the manuscript and make it clearer.

In the revised manuscript, sections 2.1 and 2.2 include now subsections (2.1.1 to 2.1.4, 2.2.1 to 2.2.4), separating somewhat clearer the different steps of the methodology and the results.

Further, a paragraph describing a part of the methodology (daylight-nighttime conditions for mid- and equatorial latitudes related to Figure 4d) is shifted somewhat backward to include in subsection 2.2.1 (previous preprint: ll. 344-361, revised manuscript: ll. 326-343).

The second aspect may be a misunderstanding on my part: I cannot accept the dynamical concept of assumed gravity wave-ozone coupling (heating rate). My understanding is that propagating internal gravity waves cause positive and negative vertical displacements of the background airflow. Therefore, air transported through a gravity wave experiences both adiabatic cooling and heating. It seems to me (I found no other reference in the text) that only positive vertical velocities (i.e., displacements) are considered here to establish the "successive" or "cumulative amplitude amplification". Averaged over a horizontal wavelength *or one period, the net effect of gravity wave-induced cooling and warming should be zero. In conclusion, I don't see any point in publishing the results as they have been written up now. A better presentation of the underlying concept is urgently needed. Again, I could be wrong: reading the text, I would assume that gravity wave-ozone coupling leads to an increase in background temperature when gravity waves are present and ozone photochemistry is working. Is this correct? I hope, I'm right in this aspect. If not, any clarification of the dynamical concept in the paper is highly appreciated.*

Yes, of course, a sinusoidal gravity wave perturbation includes both ascent ($w'>0$, adiabatic heating) and descent ($w'<0$, adiabatic warming), and both the ascent (wave crest) and the descent (wave trough) are amplified during its propagation through the USLM; accordingly, the amplitude is increasing while the frequency is decreasing when propagating through the upper stratosphere/lower mesosphere.

For clarification, the text of the manuscript is improved from the beginning (abstract and introduction: ll., 32-34, ll. 73-83; section 2.1.2: ll. 142-146, ll. 163-165, ll. 185-187; section 2.1.4, ll. 253-254, ll. 262-264; section 3, ll. 462-464).

As mentioned in the first reply, the theoretical approach describes upward propagating gravity waves in a constant background, i.e., they cannot change anything in the background. The identified process could only lead to a change in a varying background if the stronger increase in the GW amplitudes with height would lead to a change in gravity wave breaking processes driving the middle atmospheric circulation; this potential effect is discussed as an outlook of further examinations, which are beyond the scope of the paper because extensive model calculations are needed. In the revised manuscript, this is somewhat more highlighted (abstract: ll. 39-41, and section 3: ll. 497-504 and ll. 526-530, in addition to ll. 538-540).

There is a third point that should be considered in a new version of the manuscript. The whole gravity wave concept relies on linear wave theory. However, the authors use a density scale height H that is strictly only applicable for an isothermal atmosphere as it is constant with altitude. Already in the textbooks by Gill (1982, page 50 top) and by Dutton (1976, pages 67-68) altitude-dependent scale heights are mentioned or proposed. Recently, Reichert et al. (2021) used a height-dependent H for investigating conservative growth rates from ground-based lidar measurements. So, it would be worthwhile to estimate the amplitude growth in an atmosphere with temperature varying with altitude. Especially, in the summer mesosphere where the temperatures can drop drastically from the stratopause to the cold mesopause, this effect might account for some of the observed exponential increase.

Yes, the choice of the scale height H can affect the change in the amplitude growth due to ozone-gravity wave coupling especially in the summer mesosphere. For clarification, the revised manuscript includes a sensitivity test as summarized as follows.

In the present paper, the solutions of the amplitude growth are formulated on pressure levels, therefore the height-dependence of $H=H(T_0)$ is included in the cumulative level-by-level amplification; for clarification, this is explicitly mentioned in the revised manuscript, including a hint on the sensitivity test using a constant scale height H_0 (section 2.1.4: ll. 249-250, section 2.2.1: ll. 307-312, and ll. 319-321).

The sensitivity test is realized by setting H_0=7 km in the specification of the distance between the levels used in the calculation of the cumulative amplitude amplification. As expected, this leads to a significant change in the upper mesospheric GW amplitudes (up to 10%) and GWPED (up to 20%) particularly at summer polar latitudes, where $H=H(T_0)$ varies in the USLM region between 6.5 km and 7.5 km (i.e., less than 10%); however, this modulation is less than the first-order process of ozone-gravity wave coupling (details of the sensitivity test and a conclusive note are included in subsection 2.2.4, ll. 439-452, and section 3, ll. 555-559).

Last but not least, I see an essential difference in the gravity wave regimes of the upper stratosphere and lower mesosphere between summer and winter. This picture results from Figure 6 of Reichert et al (2021): it shows almost no seasonal variability of Ep in the layer 65 to 80 km altitude in contrast to the layers below. Thus, the mesosphere seems to be a region where gravity waves always exist almost independent from the local excitation at the place of the observations. Where these waves come from, if they are from primary or secondary or other sources, I don't know but they seem to be present all the time. In conclusion, the strong summer increase can probably also be explained by the reduced local excitation conditions, i.e. the strongly reduced Ep values at lower layers. Sure, this is for one location in the lee of the Andes but it is a convincing example. By the way, there is a further aspect not discussed in the paper: the superposition of gravity waves from different sources entering the observational volume horizontally and leading to enhanced Ep values as indicated by Reichert et al. (2021) as well.

Thanks again for the comment, and the hint on the publication of Reichert et al. (2021). A related comment on the stratospheric GW sources and the relative relation between mesospheric and stratospheric GWPED is included (section 3, ll. 510-511, ll. 520-526).

Please note again that the paper does not want to explain all the details of published GWPED profiles. The aim of the paper is to demonstrate that the process of ozone-gravity wave coupling might principally lead to significant GW amplitude amplifications and daylight-nighttime differences in the GWPED, as earlier suggested by Baumgarten et al. (2017). The results of the theoretical approach are in the order of observed relative relations between mesospheric and stratospheric GWPED values, therefore it is justified to conclude that this process is a relevant factor in the summer middle atmosphere, although the addressed process but also a lot of other processes play an important role for understanding the total GWPED at a specific location. The manuscript is revised from the beginning to make this point as clear as possible (abstract: ll. 28-31, ll. 39-40, introduction: ll. 51-62, section 3: ll. 493-530).

I would have liked to see the authors pay more attention to these possible dynamical aspects and their potential impact on growth rates. A discussion of both the dynamical and ozone temperature aspects would improve the paper and relate its new results to known published knowledge.

The addressed aspects are included in the revised manuscript. The discussion is extended including some additional references. Also, some sensitivity calculations are added to provide information on the involved dynamical and ozone-temperature coupling aspects (see particularly subsection 2.2.4: ll. 423-452, section 3: ll. 493-530 and ll. 551-559).

Minor Comments:

line 48: "over-exponential" is probably not well-selected as term: what does it mean? I guess, you refer to exponential growth with a enhanced rate, correct?

The terminus “over-exponential” is deleted in the whole manuscript.

line 79-80: here, the concept of w'>0 is introduced for the first time. I thought, well, why do the author not consider w'<0 as well as vertical displacements related to these vertical oscillations vary in time and space regularly in a gravity wave.

Of course, the description is valid for both w'>0 and w'<0. The manuscript is improved from the beginning to make this clearer (e.g., abstract and introduction: ll., 32-34, ll. 73-83; see also the reply to your second aspect above).

line 114: introduce minus sign in density equation

Done. Thank you.

line 115: why is v_0 d/dy missing in the total derivative?

Done. Thank you.

line 238: Figure 8 of Reichert et al. (2021) shows that the majority of vertical wavelengths is about and large than 15 km. So, the choice of the selected parameters (especially with reference to the Andes) is not clear to me.

The selected parameters are used as examples of GW characteristics where ozone-gravity wave interaction is particularly efficient (as shown in Figure 3). The reference to the Andes is deleted because it is not necessary in this subsection (the sentence is now at ll. 241).

Considering the orography of the Andes (horizontal extension and height), I would expect orographically forced GWs with horizontal wavelengths of several hundred km and vertical wavelengths of around 3 km to 5 km, travelling over the South Atlantic in case of the usual westerlies; therefore, it was especially mentioned. In the revised manuscript, the potential physical GW sources including the Andes are mentioned in the discussion (l. 469, l. 562).

line 266: Why do you use "but" not "and"?

Done. Thank you.

References:

Dutton, J. A., 1976: The Ceaseless Wind. 1st ed., McGraw-Hill, New York and London, 579 pp.

Gill, A. E., 1982: Atmosphere-Ocean Dynamics, Academic Press, 1st edn.,662 pp.

Reichert, R. et al. 2021: High-cadence lidar observations of middle atmospheric temperature and gravity waves at the Southern Andes hot spot. Journal of Geophysical Research: Atmospheres, 126, e2021JD034683. https://doi.org/10.1029/2021JD034683