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

Thanks also to Referee #2 for critical comments. Please find a point-by-point response below (for orientation, the main comments of Referee #2 are numbered and included in *Italic*).

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.

1) *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.*

Yes, I agree, perhaps some more separation will be helpful for the reader. The revised manuscript will include some more subsections to separate the steps of the theoretical approach and the results. Section 2.1 will be separated into subsections including (a) the basic equations (ll. 107-143), (b) the derivation of the coupling parameters (ll. 144-193), (c) the formulation of the local amplification (ll. 194-235), and (d) the results illustrating the local amplification (ll. 236-277); Section 2.2 might be separated into subsections more related to the methodology (ll. 277-361) and presenting results (ll. 362-399).

2) *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".*

Of course, a sinusoidal gravity wave perturbation includes both a positive ($w'>0$) and a negative ($w'<0$) component. The feedback of ozone-temperature coupling to the initial perturbation is described by an initial positive component of the updraft as an example (increase of w' if $w'>0$), but it is – of course – also valid for the accompanying negative component (decrease of w' if $w'<0$). Accordingly, as described and illustrated in Section 2.1, the amplitude (or the difference between the maximum and minimum of the oscillating wave pattern) is increasing while the frequency is decreasing (i.e., the time between the maximum and minimum is increasing) when propagating through the upper stratosphere/lower mesosphere. From my point of view this is easy to understand; however, the text of the manuscript will be revised to make this point clearer from the beginning, perhaps with an additional figure illustrating the changes in the oscillating structure of a gravity wave.

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.

I hope that I understand these comments correctly, but I am not sure. Generally, an average over the horizontal wavelength or one period of a sinusoidal wave perturbation, either in case with or without ozone-temperature coupling, does not make sense, it must be zero because it is a sinusoidal wave perturbation. The theoretical approach describes an upward propagating gravity wave in a constant background atmosphere; therefore, in this specific standard approach, the gravity wave cannot change anything in the background.

The approach of Section 2 suggests that the GW amplitude becomes larger while the frequency is decreasing during the vertical propagation if ozone-temperature coupling is considered; however, the process only leads to a *net effect* on the time-mean temperature and circulation if the stronger increase in amplitude with height leads to a change in gravity wave breaking processes, which is discussed in Section 3 (ll. 440-459). In current GCMs, this process is described by the gravity wave drag (GWD) parameterization which describes the transformation of the potential gravity wave energy of upward propagating GWs into those gravity wave flux terms that deposit heat and drive the circulation in case of gravity wave breaking conditions. Including ozone-temperature coupling might lead to an improvement of the GWDs; however, a more detailed investigation needs extensive model calculations which are beyond the scope of the preprint. Some revisions in the text and some more comments will be included to make this point clearer.

3) *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 height-dependence of the density scale height H can have an effect on the amplitude growth especially in the summer mesosphere. The preprint focusses on the proposed effect of ozone-gravity wave coupling in the upper stratosphere/lower mesosphere, therefore the standard approach H=const might be suitable. A height-dependent H might particularly affect the approach of upward propagating GWs described in Section 2.2 (at ll. 286-312) but it might not be stronger than the feedback of the changing vertical group velocity to the amplitude growth described at ll. 382-390, because H does not change in the vertical by more than 10% to 20% between 30 km and 70 km. However, it might be indeed worthwhile to include an additional comment, or – perhaps – an additional figure, in the revised manuscript illustrating this effect.

4) *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.*

The paper of Reichert et al. (2021) is indeed interesting, and the addressed points are worthwhile for the discussion of the revised manuscript; however, in Figure 6 of this paper, I see a pronounced seasonal cycle in the layer 65 to 80 km, with around 31.4 Jkg^{-1} during summer and 82.4 Jkg^{-1} during winter, although it is evidently less pronounced than in the layers below. The suggested explanation (“*the strong summer increase can probably also be explained by the reduced local excitation conditions”*) could indeed explain a fraction of the different relative relation between mesospheric and stratospheric sources; however, this is not less speculative than any other thesis, even because seasonal changes in this relative relation are obviously much stronger in the full-day measurements at the somewhat more southward located Davis (69°S) including polar day and polar night conditions (Kaifler et al., 2015, Figure 6).

However, please note here that the preprint does not want to explain all the details of measured stratospheric and mesospheric GW amplitudes or GWPED by the proposed effects of ozone-gravity wave interaction. The GWPED measurements are cited in the introduction as a motivation specifying the open questions and to motivate the purpose of the preprint. From my point of view, it is evident that an unexplained process responsible for daylight-nighttime differences in the GWPED, as found by Baumgarten et al. (2017, Figures 6 and 9), must have an influence on polar day-polar night differences, and therefore on the seasonal cycle, although – of course – it cannot explain all the details of the measured GWPED variability. For clarification, the introduction and the discussion of the revised manuscript will be somewhat rearranged, i.e., interpretations of the measured seasonal cycle will be strongly reduced in the abstract and in the introduction, whereas a discussion of the possible effect of ozone-gravity wave interaction on the seasonal cycle will be included in Section 4 (see also the reply to Referee #1).

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.

As outlined above, the specific points of Referee #2 will be included in the revised manuscript. A revised manuscript with tracked changes and an additional point-by-point reply to the comments of Referee #2 will be uploaded following the regulations of ACP.

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?

Yes, here “over-exponential” means that the exponential growth rate increases with height in difference to the usually assumed constant exponential growth rate. I think it is a usual expression; however, it is not really necessary and will be no longer used in the revised 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 components of the wave (increase of w' if w'>0, decrease of w' if w'<0) suggesting an increase in the amplitude (or in the difference between the maximum and minimum of w') of the oscillating wave pattern. See also the reply to the main point 2) above.

line 114: introduce minus sign in density equation

Yes. Thank you.

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

Yes, it is missing. This will be improved.

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, which becomes evident when discussing the dependence of this effect on the horizontal and vertical wavelengths (ll. 258-270). Indeed, a hint on possible sources (cyclones, Andes) is not necessary in this subsection and will be deleted; the relation to measured vertical wavelengths and possible sources are discussed in detail in Section 4 (ll. 430-439).

Perhaps one additional comment to the mentioned findings of Reichert et al. (2021). Based on idealized approaches, orographically forced GWs might have the same spatial scales as the smoothed mountain ridge (e.g., as a function of the half width and the maximum height if it is approximated by a Gaussian-type function); therefore, I would expect horizontal wavelengths of a few hundred km and vertical wavelengths of around 3 km to 5 km for GWs forced by the Andes. The measurement site (Rio Grande) of Reichert et al. (2021) is located at the southern end of South America and not really downwind of the Andes; therefore, the identified GWs at Rio Grande might be forced by cyclones or convective patterns travelling over the South Pacific, but not by the Andes, where the very large vertical wavelengths > 10 km might be more related to convection over the ocean (this is, of course, speculation and not issue of the preprint). Other papers suggest most pronounced vertical wavelengths between about 5 km and 9 km in the upper stratosphere/lower mesosphere region (e.g., Baumgarten et al., 2018).

However, the findings of Reichert et al. (2021) are interesting and will be included in the discussion of the vertical wavelengths (ll. 430-439).

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

Yes, thank you, “and” is right here.

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