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Comment on acp-2021-1066

Anonymous Referee #2

Referee comment on "Ozone–gravity wave interaction in the upper stratosphere/lower mesosphere" by Axel Gabriel, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1066-RC2, 2022

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.

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.

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.

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.

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.

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?

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.

line 114: introduce minus sign in density equation

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

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.

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

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