

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-190

Anonymous Referee #2

Referee comment on "The semiannual oscillation (SAO) in the tropical middle atmosphere and its gravity wave driving in reanalyses and satellite observations" by Manfred Ern et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-190-RC2>, 2021

General comments:

This paper examines gravity wave (GW) contributions to the driving of semi-annual oscillation (SAO) in the upper stratosphere and mesosphere using four kinds of reanalyses and satellite datasets. Although the GWs have been believed to be one of primary sources for the SAO driving, it was difficult to estimate it quantitatively. This paper succeeded it based on vigorous work analyzing a plenty of data. I'd like to express my respect to this great work. On the other hand, it seems to me that the paper includes a lack of explanation on their method and some unnecessary parts. Thus I think the paper needs to address these issues before its publication. Detailed comments are given below.

Major comments:

- L. 270-272

The zonal wavenumber of more than 21 is defined as the resolved GW, but to what extent is it correct? If the zonal propagation of GWs is dominant in the tropics, the zonal wavenumber filter looks OK. However, in the previous studies using a gravity wave resolving model (Watanabe et al., 2008; Becker et al., 2018), GWs are extracted by a total wavenumber filter in spherical harmonics. In addition, showing the contribution of resolved GWs quantitatively will help readers think of the validity of this method.

On the other hand, this method may contradict the flux estimate of SABER, in which the along-track wavelength is regarded as the horizontal wavelength. If zonal propagation of GWs is dominant in the tropics, an error of flux estimate could be large. Please mention the error of flux estimate in more detail.

- Figures

Figures for respective years are shown throughout this paper. However, there is little discussion of interannual variation. It is better to reduce unnecessary figures and make them larger and easier to see.

- Above 80km

It is mentioned that the effect of tide is large for satellite data above 80 km and contaminate the GW contribution to the SAO driving throughout this paper. Although I understand that it is the signature of the interaction between the tide and GWs and important, it looks far from the primary purpose of this paper. I recommend moving it to another paper.

Minor comments:

- SPARC climatology

The SPARC climatology is regarded “true” and the difference from it is expressed as “bias” throughout this paper. However, it may be better to change the expression because it could be caused by the difference in time resolution and interannual variation as shown in L. 225-228. At least, it is better to compare monthly averages between SPARC climatology

and reanalysis/satellite data, which clarify the effect of time resolution.

- Chapter 8

It seems unnecessary to discuss the correlation in QBO and sponge layers in this paper.