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Interactive comment on "GRACILE: A comprehensive climatology of atmospheric gravity wave parameters based on satellite limb soundings" *by* Manfred Ern et al.

Manfred Ern et al.

m.ern@fz-juelich.de

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Dear Referee # 2,

Thank you very much for carefully reading our paper! Based on your comments the manuscript could be improved by, for example, a better description of several aspects of the data processing. Further, it was clarified that vertical wavelength biases as discussed by de la Torre et al. (2018) do not play an important role for the data presented.

Here first we will give short responses to the your concerns. A more detailed response will be given in the next stage of the review in the detailed point-by-point reply and the revised manuscript.

In the following, reviewer comments are given in black, our responses are given in blue.

Again, thank you very much for your careful review!

Main Concerns

(1) One major concern is about how to grid the data. Gridding highly intermittent features such as the atmospheric gravity wave requires the denominator to be the number of pass-by observations during a month. For example, monthly mean GWMF in the gridbox [5W,5S, 5E,5N] equals total of all retrieved GWMF values within this gridbox divided by all pass-by samples fall into this gridbox during the month. So when you use paired obs. for calculation and if they fall into two grid boxes, which one do you assign it? I'm not saying you are wrong, it's just not very clear to me in the description of gridding methodology.

Indeed, some care has to be taken when calculating averages. For clarification, we have added the following text in Sect. 4.2 where the gridding is described.

"A monthly mean value assigned to a gridbox equals the total of all values within this gridbox divided by the number of all data points within the gridbox. Each "paired observation" is treated as a new data point, and the center coordinates between the two single observations that contribute to this paired observation are taken as the new coordinates for the pair, i.e., we assign new coordinates in latitude, longitude and time to the pair. In this way, ambiguities are avoided at the cost of creating a new set of coordinates."

(2) Another concern is that, from Fig. 15 (c) I can see two stripes of enhancement of momentum flux samples around 50S and 50N. I suspect that's partly caused by the fact that GWs there are strong during wintertime, so you have greater chance to find pairs to complete your GWMF calculation. But I don't see such hints in HIRDLS map in Fig. 15 (f). Why that's the case? If my interpretation was wrong, then what causes the enhancements at 50S and 50N in Fig. 15 (c)?

The enhancements of the number of points available are just an effect of the satellite sampling geometry, and not an effect of the pair-selection for determining gravity wave momentum fluxes. This is evident already from the left and middle columns in Fig. 15 that show an enhanced number of samples at the same latitudes as in the panels of the right column. The left and middle columns are for single altitude profiles, i.e., selection of pairs was not applied. Further, as has been shown in Fig. 8, the rate at which pairs are selected is mostly between 55 and 65% and does not show much latitudinal variation.

The basic reason for enhancements in the measurement density is that during one orbit a satellite in low Earth orbit spends more time at the turning points (high latitudes) of the orbit than at low latitudes.

A more detailed discussion is given in the detailed point-by-point reply. Further, we have added some explanation in the revised manuscript in the middle of former page 20.

Minor Comments

(1) Page 3, Line 13: please considering adding one more recent reference that has validated the theoretical value proposed by van Zandt, 1985 from an observation or multiple different observations.

We have added the reference Placke et al. (2013) for the mesosphere (a combination of radar and lidar observations), and the reference Hertzog et al. (2002) for the lower stratosphere (superpressure balloon observations), as well as Tsuda et al. (2000) and Nastrom et al. (2000) using a combination of radar and GPS-RO.

Hertzog, A., Vial, F., Mechoso, C. R., Basdevant, C., and Coquerez, Ph.: Quasi-Lagrangian measurements in the lower stratosphere reveal an energy peak associated with near-inertial waves, Geophys. Res. Lett., 29, 1229, doi:10.1029/2001GL014083, 2002.

Nastrom, G. D., Hansen, A. R., Tsuda, T., Nishida, M., and Ware, R. H.: A comparison of gravity wave energy observed by VHF radar and GPS/MET over central North America, J. Geophys. Res., 105, 4685–4687, 2000.

Placke, M., Hoffmann, P., Gerding, M., Becker, E., and Rapp, M.: Testing linear gravity wave theory with simultaneous wind and temperature data from the mesosphere, J. Atmos. Solar-Terr. Phys., 93, 57–69, doi:10.1016/j.jastp.2012.11.012, 2013.

Tsuda, T., Nishida, M., Rocken, C., and Ware, R. H.: A global morphology of gravity wave activity in the stratosphere revealed by the GPS occultation data (GPS/MET), J. Geophys. Res., 105, 7257–7273, 2000.

(2) Page 5, Line 34: how do you read the new “observational filter” in de le Torre et al. (2018)? In their paper, they sort of suggests that SABER’s observational window is very narrow.

de la Torre, A., Alexander, P., Schmidt, T., Llamedo, P., and Hierro, R.: On the distortions in calculated GW parameters during slanted atmospheric soundings, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2017-192, in review, 2017.

De la Torre et al. (2018) address the fact that observed altitude profiles usually are not perfectly vertical and will therefore partly sample the horizontal structure of an observed gravity wave while performing an altitude scan. This can lead to biases in the observed vertical wavelength for gravity waves of short horizontal wavelengths.

There are several reasons why this effect is very likely not important for our results:

(1) Trinh et al. (2015) included this effect in their simulation of the overall observational filter of limb sounders, and the effect was found to be small for SABER.

(2) HIRDLS and SABER momentum fluxes agree well with CRISTA momentum fluxes. CRISTA momentum fluxes, however, are unaffected by this effect because CRISTA altitude profiles were measured almost vertically (cf. Riese et al., 1999).

(3) For limb sounders the waves that pass the sensitivity function (cf. our Fig. 3) without being attenuated too much should have an aspect ratio λ_z/λ_h of smaller than about 0.1, resulting in a bias of the vertical wavelength of less than $\sim$20% for SABER (cf. de la Torre et al., 2018, Fig. 7).

This information has been included in the revised manuscript on former page 10 at the end of Sect. 3.1. A more detailed discussion is given in the detailed point-by-point reply.

References:

de la Torre, A., Alexander, P., Schmidt, T., Llamedo, P., and Hierro, R.: On the distortions in calculated GW parameters during slanted atmospheric soundings, Atmos. Meas. Tech., 11, 1363–1375, https://doi.org/10.5194/amt-11-1363-2018, 2018.

Riese, M., Spang, R., Preusse, P., Ern, M., Jarisch, M., Offermann, D., and Grossmann, K. U.: Cryogenic Infrared Spectrometers and Telescopes for the Atmosphere (CRISTA) data processing and atmospheric temperature and trace gas retrieval, J. Geophys. Res.-Atmos., 104, 16349–16367, https://doi.org/10.1029/1998JD100057, 1999.

(3) Page 6, Line 4: please consider also including Gong et al., 2015 in the reference Gong, J., J. Yue, and D. L. Wu (2015), Global survey of concentric gravity waves in AIRS images and ECMWF analysis. J. Geophys. Res. Atmos., 120, 2210–2228. doi:10.1002/2014JD022527.

Reference has been included, as recommended.

(4) Page 13, Line 21: I didn't read Gill et al., 2011, so can't comment further, but arbitrarily divide every number by two seems dangerous to me. Can you provide a rough picture like how many observed samples are 1/2 of theoretical value and what is the standard deviation?

The main problem is that, practically, it is difficult to determine "observed samples": observed altitude profiles will almost always be "contaminated" by gravity waves. For this reason, Gille et al. (2011) determined values of "measured precision" as the standard deviation calculated from of a number of consecutive altitude profiles in regions where little atmospheric variability is expected. Over a large altitude range those values are roughly a factor of two better than the theoretical ones. This indicates that the theoretical values may be high biased by this factor.

This finding is in good agreement with our Fig. 7, lower row, where we calculate the ratio:

$$R_{var} = (0.5 \cdot \text{theoretical HIRDLS precision})^2 / (\text{GW temperature variance})$$

Theoretically, the maximum value that R_{var} can attain is 1 (all data are noise). Values larger than 1 should not be possible. However, if we do not apply the factor of 0.5 for HIRDLS, R_{var} would be a factor of 4 higher than shown in Fig. 7, lower row, and would, for example, attain values of $\sim$4 in the summer lower stratosphere (which should not be possible). For all other latitudes and altitudes, R_{var} for HIRDLS would be about a factor of 4 higher than R_{var} for SABER. This is also unlikely, because there is a general agreement between HIRDLS and SABER temperature variances due to gravity waves — no offset due to noise can be identified in the HIRDLS gravity wave variances shown in Fig. 5 compared to those of SABER shown in Fig. 4.

Therefore, applying the factor of 0.5 for HIRDLS is very likely justified.

In the manuscript, the text about the HIRDLS precision on former page 13 has been revised to state this more clearly.

(5) Page 15, Line 29: you may want to add "except inside the jet streams".

This information has been added, as recommended.

(6) Page 20, paragraph 1: so GRACILE only starts from z=30 km, correct? If that's the case, I strongly suggest you change Fig. 15 (I'll mention my suggestion when comes to that point).

As described on page 20, global distributions are provided at 30km and higher, however, zonal averages for HIRDLS are given also for altitudes in the range 20–30km. Therefore Fig. 15 should be kept as is in order to give users information which range of altitudes and latitudes in the tropics should be considered less reliable due to sparser sampling.

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Discussion paper

(7) Page 20, Line 13: that's where my major concern #2 comes from. If you can draw a lat-lon map you can discover whether the enhancement of sample size is source-related. Also, it would be helpful to add a bit discussion here if that's the case. Since stronger source gives you larger chance to "see" them because it's easier to find pairs.

See our reply to Reviewer # 2, Main Concern (2):

The enhanced measurement density is an effect of the satellite orbit geometry, and not an effect of the pair selection process.

(8) Page 20, Line 31: do you provide a quality flag for each value in each grid box? So user can easily make their own plots according to their needs.

No, we did not provide a quality flag.

It would also be difficult to provide a simple quality flag. As already discussed in the paper, if the ratio of temperature precision squared and gravity wave temperature variance is relatively large the data may be affected by noise. Similarly, if in a region the data are dominated by noise, we would expect high values of zonal wavenumbers. However, if in a region zonal wavenumbers are high, this does NOT automatically mean that the data are noisy — these high zonal wavenumbers could indeed be the correct values for the observed waves.

(9) Page 22, Line 14: GLIGLOSS → GRACILE?

Thank you very much! Acronym has been corrected!

(10) Fig. 10: please consider use horizontal wavelength as the colorbar so the unit and physical meaning would be consistent with Fig. 9 (or the other way around).

For convenience, additional horizontal wavelength scales have been added in Figs. 10, 16, and 17.

(11) Fig. 15: Since the sample size is not height dependent, I strongly suggest you make only line plot so you only need two plots (one for SABER with three lines as a function of latitude, and one for HIRDLS) to explain the sample size matter. The only exception is at ∼20 km for HIRDLS tropics. But since you don't provide data below 30 km because of potential cloud contamination, no need to show and discuss about that anyway.

See our reply to Minor Comment (6). Zonal averages for HIRDLS are provided at altitudes below 30km. Therefore, Fig. 15 is kept as is.

(12) Fig. 18: Instead of using dashed lines to show the natural variability, I think you can use semi-transparent grey/color areas to enclose the natural variability.

Figure has been modified, as recommended.

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