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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 # 1,

We greatly appreciate your detailed and constructive comments. Based on these comments the manuscript was significantly improved. In particular, we were made aware that in some places more background information was needed.

Here first we will give short responses to the main 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 detailed review!

Minor Comments

(1) lines 5-6 on page 6.
Here, the authors [...] don't caution that every observation method has its own coverage in gravity wave wavenumber and frequency space, and that needs to be taken into account.

We have added some text stating that for a meaningful comparison of different instruments their respective observational filter has to be taken into account.

(2) [page 9] on lines 19-22, they say that discussion of the vertical filter used for these data is "beyond the scope of the current study." I think a short paragraph summarizing those effects (with references) would be welcomed by the reader.

The statement "beyond the scope of the current study." did not refer to the vertical resolution of the instruments, but to the possibility of horizontal wave structures being undersampled by the horizontal sampling step of the instrument (aliasing effect). An empirical correction for those aliasing effects was suggested by Ern et al. (2004).

This correction, however, was based on assumptions on the shape of the wave spectrum in a given region — an assumption that we wanted to avoid, and our statement "Accounting for such effects is beyond the scope of our current study." referred to the non-application of this correction in our current work. We also did not correct for effects of the sensitivity function.

Of course, the reviewer is correct that some more background information is needed here. Therefore we added a detailed discussion in the revised manuscript, and the end of Sect. 3.1 was partly rewritten.

(3) line 6 of page 11. In Geller et al. (2013), on page 6387, there is a discussion on how data retention affects derived gravity wave momentum fluxes. That discussion contrasts the two methods used for deriving momentum fluxes from HRDLS used in that paper. The authors should point out how their data selection relates to the discussion in Geller et al. (2013). I believe that the statement on lines 12-13 on page 12 also relates strongly to this discussion in Geller et al. (2013).

(3a) About page 11, line 6:
In this line we state that in an altitude profile, at each given altitude, we consider only the gravity wave with the strongest amplitude. This assumption is common to both methods of deriving gravity wave momentum flux (“HIRDLS1” and “HIRDLS2”) that are presented in Geller et al. (2013). (For the “HIRDLS1” method see Alexander et al. (2008), the text related to their Eq. (5).) As has been discussed in more detail later in our Sect. 3.3.1, this assumption is justified because smaller-amplitude waves do not contribute much to the overall temperature variance. This can be seen by comparing rows 1 and rows 2 in Fig. 4 (for SABER) or rows 1 and rows 2 in Fig. 5 (for HIRDLS). In both these rows all altitude profiles are considered (i.e., no “data retention”). We find that squared temperature amplitudes are only somewhat lower than two times the temperature variance, which means that higher-order waves do not contribute much.

In the revised manuscript we now state that this assumption has been made also by other authors who derive gravity wave momentum fluxes using another approach. Further, we state that higher-order waves do not contribute much and refer to the later Sect. 3.3.1.

(3b) About page 12, lines 12–13:

Indeed, the case of non-matching gravity waves in a pair of considered altitude profiles is an issue that is treated differently in the two methods of deriving gravity wave momentum fluxes from satellite data that are presented in Geller et al. (2013).

The “HIRDLS1” method attributes small amplitudes and little momentum fluxes to non-matching pairs, even in cases when the amplitudes in both profiles of a pair are large, while in our method (“HIRDLS2”) non-matching pairs are not considered for the momentum flux calculation.

Regarding average momentum fluxes calculated in a certain region, the first method will result in much lower average values than the second method. The second method inherently assumes that the matching pairs are representative for the average momentum flux in this region. Figs. 4 and 5 provide evidence supporting this assumption: the whole number of single profiles and the reduced number of matching pairs of altitude profiles, and thus also the not-selected pairs, have almost the same distribution and magnitude of gravity wave squared amplitudes.

A detailed discussion was added after former l.7 on p.12.

(4) The discussion on lines 10-15, on page 13, leaves the reader wondering why this was done. Please explain the reasoning for this.

As stated in the manuscript, our purpose is to provide cross sections of temperature random error variances for all calendar months. SABER random error variances are however given only for the two cases of “normal” conditions and “cold summer mesopause” conditions. We therefore used “cold summer mesopause” estimates for those months and latitudes where the SPARC climatology indicates cold summer mesopause temperatures (usually poleward of 50deg in the summer hemisphere during these months) and “normal” estimates elsewhere. In order to avoid jumps, a smooth transition between 40deg and 50deg latitude was introduced, where applicable. This is now written more clearly in the revised manuscript.

(5) In figure 9, the HRDLS vertical wavelengths look longer. I find the discussion on lines 29 on page 15 to line 2 on page 16 to be confusing on this issue. Would the authors please work to make their points clearer on this issue.

This confusion is probably caused by mixing two different effects in the same paragraph. The paragraph has been split and the content moved to where it fits better. Partly, the text has been rewritten. For changes see the revised manuscript.

(6) I don't understand why results for vertical wavelengths are shown, but results for horizontal wavenumbers are shown. Unless there is a good reason for this, I urge their results be shown for wavelengths in both cases unless the authors have a good reason for showing wavelengths in one case and wavenumbers in the other. If there is such a good reason , the authors should give their explanation.

The reason for showing horizontal wavenumbers instead of horizontal wavelengths is that the range of vertical wavelengths is limited to below ~25km due to the used vertical analysis interval and sensitivity function. Horizontal wavelengths, however, are not limited and single values can attain very large values of a few thousand km. Showing average horizontal wavelengths would therefore overemphasize those values that do not contribute much to average momentum fluxes and that therefore are not representative for the average distribution of gravity wave momentum fluxes.

This is now stated after former line 3 on page 16. For convenience, additional horizontal wavelength scales have been added in Figs. 10, 16, and 17.

(7) reduced values of kh at low latitudes and at higher altitudes has been previously noted by Wang et al. (2005, J. Atmos. Sci.), albeit from radiosonde data in the troposphere and stratosphere. In general, it would be good if the authors noted where their results are consistent, or inconsistent, with works using different techniques and thus sensitive to different portions of the gravity wave spectrum.

As recommended, the reference Wang et al. (2005, J. Atmos. Sci.) has been added.

(8) The statement on lines 19-20 of page 17 is a good one, but it should be reinforced by saying that, for that reason, the vertical derivative of the gravity wave momentum fluxes from GRACILE are likely not indicative of quantitative mean flow accelerations due to this.

For two reasons, we keep the statement as is:

First, the reason for the discrepancy of momentum flux vertical gradients between models and observations as discussed in Geller et al. (2013) is still not clear and could be an effect of the measurements, the models, or of both.

Second, the discussion in Geller et al. (2013) addresses an overall "weak" vertical gradient in momentum fluxes based on globally averaged data. Accordingly, gravity wave drag resulting from those gradients would be also weak. This situation is much different from "localized" phenomena, such as the strong gradients at the top of strong wind jets. In such situations, vertical gradients in observed momentum fluxes will still provide valuable information on the forcing of the background flow, as has been shown, for example, for the summertime mesospheric jets, the QBO, the SAO, and the wintertime polar vortex (Ern et al., 2013, 2014, 2015, 2016). For these situations, observations mostly show good agreement with model results.

(9) In the discussion of figure 12, no mention is made of the HIRDLS/SABER differences. The HIRDLS momentum fluxes look larger to me than those from SABER where they overlap.

At the end of Sect. 3.4.3, we have added the information that HIRDLS values of gravity wave momentum flux are somewhat higher in the polar vortices. One possible reason is that in these regions average horizontal wavelengths are relatively short (cf. Fig. 10).

Accordingly, the better HIRDLS along-track sampling will lead to reduced aliasing effects compared to SABER and result in higher momentum fluxes.

(10) The short paragraph on lines 6-11 on page 18 might say more about the work of Trinh et al. (2016) who wrote a paper on this subject.

As recommended, at the end of Sect. 3 we have now summarized the different effects of the observational filter that are mentioned in Trinh et al. (2015, 2016) and that have to be taken into account for measurement/model comparisons. For details see the revised manuscript.

(11) The HIRDLS/SABER differences in figure 10 are quite large. The authors indicate the results are unreliable in some regions. Is this their explanation?

Of course, there are some regions where horizontal wavenumbers are not very reliable. These regions are discussed in detail on former page 16, lines 19–32.

The general difference between HIRDLS and SABER, however, is another effect. SABER has a coarser sampling step and will therefore horizontally undersample a larger number of short horizontal wavelength waves than HIRDLS, resulting in lower horizontal wavenumbers on average.

This issue was addressed on former page 16, lines 12–14, however, it should indeed be pointed out more clearly that these lines address the main difference between HIRDLS and SABER. This paragraph has been reworded accordingly. Further, we refer to the discussion about aliasing effects that was newly introduced at the end of Sect. 3.1. See also our reply to Reviewer # 1, main comment (2).

(12) The statement on lines 30-33 on page 21 is rather unsatisfactory. Why do the authors think the offsets are "minor?"

Considering an overall error of momentum fluxes of a factor of two or more, as stated in Sect. 3.4.4, the differences between SABER and HIRDLS momentum fluxes can be considered small. The statement has been reworded accordingly.

(13) Again, on lines 20-21 on page 22, the offsets are relatively small, but they look systematic, not indicative of random error. In general, it is my impression that the authors tend to downplay HIRDLS/SABER differences too much. It would be better if they indicated what the readers should quantitatively trust and what should be more qualitatively trusted.

We are sorry, we did not want to downplay HIRDLS/SABER differences. Our statement on page 22, lines 20/21 was intended to remind the reader that the small differences between SABER and HIRDLS should not be taken as a measure of the overall uncertainty of the momentum flux values presented, which is still a factor of two or more, as indicated in lines lines 20/21. This is now stated more clearly in the revised manuscript.

(14) I find it odd that, while the paper by Meyer et al. (2017) is mentioned in line 4 on page 9, nowhere do I remember seeing a statement that the satellite limb scanning gravity waves will not be seeing waves that comprise much, if not most, of the gravity wave momentum fluxes in many regions. I think this needs to be said. This does not detract from the value of the GRACILE dataset, but this should be explicitly pointed out.

Yes, indeed, this is an important point that should be more emphasized. On former page 9 we have therefore added a few sentences to emphasize this point.

Technical Comments

(1) Page 4, line 18: ..., in the stratosphere, ...

We omitted "also" because it is not needed here, and started the new sentence with: "In the stratosphere,..."

(2) Page 12, line 23: "As expected" is an understatement. "As must be the case" is more appropriate.

Changed as requested.

(3) Page 16, line 9: It's not that the limitation is "more relaxed." Rather, the Coriolis parameter is smaller so there is more space between the two limitations of the Coriolis parameter and the Brunt Vaisala frequency.

As suggested, the sentence has been reworded as follows:

"This effect is caused by the fact that in the tropics the Coriolis parameter is smaller, i.e., there is more space between the two limitations of the Coriolis parameter and the buoyancy frequency and longer horizontal wavelength gravity waves can exist."

(4) Page 22, line 14: What is GLIGOSS?

Thank you very much! Acronym has been corrected!

(5) The statement of a likely solar cycle in gravity wave parameters in GRACILE is very weak, given the length of measurement. Perhaps, just point out what Li et al. (2016) have said.

Also Li et al. (2016) discussed a correlation of gravity wave activity with the solar cycle. Of course, similar as SABER, the radiosondes used by Li et al. (2016) cover only somewhat more than one 11-year solar cycle. Therefore our statement has been downtoned, and we mention that the data sets are relatively short:

"In addition, there is a weak quasi-decadal variation (see also Ern et al., 2011). Similar quasi-decadal variations are also found in gravity wave energy densities observed by radiosondes (Li et al., 2016). These variations might be correlated with the 11-year solar cycle, however, much longer data sets would be needed for an in-depth investigation of this effect."

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