

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

### **Anonymous Referee #1**

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This paper gives a very comprehensive presentation on the background, data sources, methodology, and error estimates for GRACILE (GRAVity wave climatology based on Infrared Limb Emissions observed by satellite) climatology data set on gravity waves in the stratosphere and mesosphere. It gives monthly values for gravity wave temperature radiances and squared amplitudes and the gravity wave potential energies, vertical wavelengths, horizontal wavenumbers, and absolute momentum fluxes derived from the limb scanning measurements from the HRDLS and SABER satellite instruments. All of these GRACILE monthly averages are available through the open access world data center PANGEA.

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The background discussion in this paper is quite complete except for a few items. One is on lines 5-6 on page 6. Here, the authors point out the usefulness of this data set for comparison with other measurements, but they 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. It is also somewhat odd that 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. Another point that could benefit from further discussion is that on 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). The discussion on lines 10-15, on page 13, leaves the reader wondering why this was done. Please explain the reasoning for this.

The paper then goes on to show latitude-altitude cross-sections of the various parameters in GRACILE. There are a few things about those figures, and the accompanying discussion, where I think further comment is needed. 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. 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 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

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are consistent, or inconsistent, with works using different techniques and thus sensitive to different portions of the gravity wave spectrum. 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. 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. 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. The HIRDLS/SABER differences in figure 10 are quite large. The authors indicate the results are unreliable in some regions. Is this their explanation? The statement on lines 30-33 on page 21 is rather unsatisfactory. Why do the authors think the offsets are “minor?” 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.

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.

In general, this paper is very well written, but nonetheless, I do have a few detailed comments which follow.

1. Page 4, line 18: . . . , in the stratosphere, . . .
2. Page 12, line 23: “As expected” is an understatement. “As must be the case” is more appropriate.
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.
4. Page 22, line 14: What is GLIGOSS?
5. The statement

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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.

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