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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric gravity waves at southern hemisphere orographic hotspots: 2003—2014 AIRS/Aqua observations" *by* L. Hoffmann et al.

Anonymous Referee #2

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Review comments for "Stratospheric gravity waves at southern hemisphere orographic hotspots: 2003-2014 AIRS/Aqua observations" by Hoffmann et al.

This manuscript develops a novel method ("two-box" method) to detect orographic gravity waves (OGWs) that are associated with Southern Ocean hotspots from AIRS images. The orographic sources are delineated from all other GW cases using this method, and the relationship between the occurrence frequency of OGWs and the background wind at the generation level and the observational level, and the terrain orientation and altitude are studied. A simple deterministic model is proposed to predict the OGW occurrence using zonal wind threshold at the generation and observational levels (750 and 3 hPa, respectively). This model can roughly capture the interannual

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variability of the observed GW time series.

This paper is in general well written, the logic flow is clear, natural and fluent. The two box method is a novel way to disentangle OGWs from other types of GWs. The deterministic model is a simple yet effective way to capture potential OGW events that would therefore facilitate future analysis with huge amount of satellite images. This paper worth a final publication as it has good quality of scientific outcomes.

Having said above positive points, I do have a few major issues related to the content/presentation. They are: (1) The "prediction" model is actually not a forecast model, at least on traditional sense. It is more or less a deterministic model that given the lower and upper level wind condition, you can tell how possible an OGW event to occur, but you cannot forecast when and where it occurs. Or to state it in another way, this model does not have a term of (t) and (t+dt) in it, where "t" is time. So please clarify this point clearly in the paper, and modify the wording accordingly.

(2) Another question I have with the "prediction" model is that why do you use this Gilbert skill score (GSS) to construct the 2-dimensional Probability Density Function, not the averaged variance (i.e., \sigmaˆ2_{oro}) within the designated boxes? Also, there is no introduction or reference to this GSS, which I have no idea how it is calculated or what physical quantity it can represent.

(3) For the two box method, the authors do play around with the threshold to check its sensitivity, and it turns out it is indeed sensitive to the threshold. Have you also checked the lower threshold (e.g., 0.05Kˆ2)? How do you trade-off the detection rate and false alarm rate? In other words, the standard to determine a good threshold is not stated clearly in this paper. I think this part (the last paragraph on Page 6) requires more details and more sensitivity study to determine the best threshold. Also, the box size is really largely dependent on topography size and nearby surroundings. Can you summarize a more generalized way if possible?

(4) Since the number of rays are very limited for the ray-tracing experiment, the critical

level in October is probably just case-by-case. Besides, this is a 2D ray-tracing that has many limitations. Other than illustrating the wind effect on shifting the vertical wavelength toward AIRS-favorable window, I don't see a particular reason of including the 1st paragraph on Page 9 and Fig. 5. It's rather distracted of the main topic of this paper. The linear wave theory presented on Page 8 is pretty straightforward. I suggest deleting the 1st paragraph on Page 9 and Fig. 5.

(5) Instead, since all the factors that play a role in the simple "prediction" model has been included in the GCM OGWD parameterization, I believe. What are the differences? Can your findings shed light on improving the model parameterization? Can your simple model be used to study inter-annual variability? Can you elaborate more on the value of your model?

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