Authors' response to minor comments on "Stratospheric gravity waves over the mountainous island of South Georgia: testing a high-resolution dynamical model with 3-D satellite observations and radiosondes"

N. P. Hindley et al.

Once again we would like to thank the reviewers for their detailed and useful comments in their first and subsequent reviews. It is due to their care and attention that the manuscript is significantly improved, so we would like to extend our gratitude to them for taking the time to prepare their reviews.

Response to Reviewer #1

This is an improved version of the manuscript, where several aspects have been modified or corrected. Among them, some merely speculative arguments without substantiation. I have only one final request which may not have been clear enough in my first review. The redundancy issue regarding the 3DST method (former lines 449-453) should be clarified. The calculated GWs amplitudes cannot be considered reliable if a non-orthogonal basis is used. See e.g. Stockwell (2007) https://doi.org/10.1016/j.dsp.2006.04.006 as discussed for the Stockwell transform. So please explain this issue or cite an earlier discussion of this topic for the 3DST method.

The reviewer raises an important point. We apologise for not addressing this in our previous reply. We can provide a discussion of this issue below, and we have included this information in the revised paper.

The basis functions for the S-transform are normalised Gaussian-windowed sinusoidal functions. These basis functions are specified by frequency α in the spectral domain (which are at the discrete Fourier frequencies, if the discrete Fourier transform is used), and translation τ in the spatial domain. The width of the Gaussian window is scaled as $1/\alpha$, so all basis functions in the S-transform can be fully described by the two values α and τ.

But for a discrete-sampled timeseries, we do not need to consider all values of α at all translations τ to compute an accurate S-transform spectrum that can be readily inverted to recover the original signal with no loss of information.

Indeed, Stockwell (2007) showed that for carefully chosen sets of orthogonal basis functions for the S-transform , the S-transform could be successfully computed and inverted to recover the original signal for only a fraction of the calculations. This approach is highly efficient and can result in a very significant reduction in computation time by removing this redundancy. This forms the basis for the Discrete Orthonormal S-transform (DOST), which can be found widely online.

Our application of the S-transform however is different. Here we use the S-transform to exhaustively "probe" the internal structure of the measured wave field in the maximum possible detail. We are not interested in inverting the S-transform to recover the original signal, we are trying to "break down" and localise the spectral components of the wave field as much as possible at all locations.

To do this, our method considers all S-transform basis functions for all values of α at all translations τ. Of course, this approach is highly redundant, and much slower to compute, but it is actually advantageous for our purposes because it allows us to retain the maximum precision in α and τ at all locations. By inspecting the resulting S-transform $S(\alpha,\tau)$ spectrum, we are able to detect changes in the dominant (largest spectral amplitude) up to the scale of the data sampling. The scaling parameter for the Gaussian window, as discussed in Hindley et al. (2016, 2019), helps to adjust this capability, but the precision remains as fine as the spatial sampling and Fourier frequency sampling. Note that this is not the same as oversampling, since we do not sample between sample points or between Fourier frequencies, as we might do with a continuous wavelet transform.

Regarding the accuracy of GW amplitudes, the reviewer is correct that if we used sets of orthogonal basis functions (which would be much faster), the sets of coefficients must be considered together to recover accurate GW amplitudes. But because we consider all possible basis functions singly and one at a time, we are able to extract amplitudes from the S-transform coefficients directly. Of course, not all basis functions will provide a good estimate of the amplitude of a specific GW, so at each translation τ we select the coefficient of $S(\alpha,\tau)$ with the largest spectral amplitude and take the GW amplitude and frequency from that.

Naturally, there are disadvantages to extracting GW amplitudes directly from the S-transform spectrum, such as the issue of measuring GWs at "in-between" frequencies in the Fourier sampling, or the effect of spectral leakage for GW packets (see appendix of Hindley et al. (2019)). The maximum spectral amplitude method therefore is nominally an underestimate of GW amplitudes, depending on frequency sampling, but can work well in most cases, as demonstrated in Sect 3.5 of Hindley et al. (2019).

The measured GW amplitudes from our S-transform method are thoroughly tested for real and synthetic waves in Hindley et al. (2016, 2019) and Wright et al. (2017), and in preparatory work for the present study. This is very interesting topic however and we welcome the reviewer to contact us directly if there is a way we could improve GW amplitude measurement or reduce computation time.

Response to Reviewer #2

In the extensively revised version of the manuscript, the authors have greatly improved the model data processing that now results in a persuasive comparison between AIRS and the local-area simulations. My previous comments were adequately addressed and the structure of the manuscript improved. I only have a few technical corrections for this revised version of the manuscript.

We once again thank the reviewer for their careful and useful review of the revised submission.

Technical corrections:

- *L22: ... agreement with the model-as-AIRS.*

  Fixed, thanks.
- *L123: no need to introduce 2GW abbreviation because it is never used elsewhere in the paper*

  Fixed, thanks.
- *L267: (Fritts and Alexander, 2003; Ern et al, 2004).*

  Fixed, thanks.
- *L267: Eq1 is the general definition and not restricted to mid-frequency GWs. Applies for Eq. 2 later in the paper.*

  Fixed, thanks!
- *L269: extracted > separated*

  Fixed, thanks.
- *L277: during periods where surface zonal winds are low (Figs. 2a,b)...*

  Fixed, thanks.

- *L460: Subsection "6.3.1 3DST analysis" could be introduced to have not only one subsection in 6.3*

  Fixed, thanks. The new sections headings are "6.3.1 3DST measurements of GW amplitude and wavelength" and "6.3.2 Zonal and meridional momentum fluxes".

- *L781: could be "Summary and Conclusions"*

  Agreed, thanks.

- *L811: 30-40 km*

  Fixed, thanks.

- *Fig.7: You can add that the assumption of westward propagation is based on model results, e.g. In this example, westward propagation has been assumed based on sequential model results in order to...*

  Good suggestion. Added, thanks.

Response to Reviewer #3

The authors provided a thoughtful and extensive revision. This paper is now of very high quality, and it is packed with insight and useful information. My former comments on the submitted manuscript were accounted to a very high degree. I recommend publication of revised manuscript in its present form.

I wish to comment on the authors' answer to my previous critical remarks on vertical resolution and subgrid-scale processes. I still believe that the paper could have mentioned how dissipation processes are represented in the model in terms of either numerical filters (implicit methods) and/or physics-based parameterizations (explicit methods like, for example, the Smagorinsky scheme).

Our apologies for not including this information in the paper. The Met Office Unified Model (UM) uses a semi-Lagrangian dynamical core, so there is some implicit numerical diffusion as a result of the interpolation methods used to determine the departure points. In the local-area simulations used here, the "Smagorinsky-type" 3-D subgrid horizontal turbulence scheme is used, as the reviewer correctly suggests. Useful descriptions of the 3-D Smagorinsky scheme used in high-spatial resolution configurations of the UM, and some comparisons with other schemes, can be found in Pearson et al. (2014), Boutle et al. (2014) and citations therein. We have included this information in the revised paper.

Below are some miscellaneous remarks that the authors may wish to consider in their page proofs:

- *L172: exceeded*

  Changed to "travelled beyond", thanks.

- *L276-277: incomplete sentence*

  Fixed, thanks.

- *L283: What is "southward component of the characteristic GW pattern" ?*

  Apologies, this was unclear. For a typical mountain wave pattern from an isolated island source, a chevron-shaped "bow-wave" pattern is formed. This wave field has GWMF directed opposite to the prevailing wind, but it also has both northward and southward components of GW momentum to the north and south. The text has been revised to make this clearer.

- *L317,320,336: "time step" -> "snapshot" (your model time step is certainly much smaller than your snapshot interval of 1 hour)*

  Agreed, thanks, this is an important distinction. We have changed instances like this to "hourly model output".

- *L396: A GW with a horizontal wavelength of 50-150 km is usually not termed a "large-scale GW". Consider to use "small-to-medium-scale GW" instead.*

  Apologies, we simply meant "larger horizontal scales". The text has been fixed, thanks.

- *L583: define "NGWs" or just write "non-orographic GWs"; same in Sec. 9.2*

  Fixed, thanks.

- *L696: "upwind"?*

  Changed to "measured upwind of the island to the west".

- *L764-768: The simulation result should depend on the time step. If such a dependence is found, the numerical solution has not yet converged with respect to the time step.*

  The reviewer is correct, simulation results do certainly depend on model time step in the general case, but we did not find this to be an issue in the Unified Model for time step choices close to the value considered here (30 seconds).

  As discussed by Vosper (2015): "the results of the local area simulation were not sensitive to choices around this time step, which is expected because of the second-order accuracy of the time integration scheme in the ENDGame dynamical core (Wood et al., 2014). ... The improved stability of the dynamical core [over previous versions] allows the model to be run with reduced temporal off-centring in the semi-implicit method, such that less weight is placed on the future time step. As discussed by Shutts and Vosper (2011), off-centring can cause heavy damping in time, damping gravity-wave motion when long time steps are used. The reduced off-centring allows longer time steps to be used without causing significant damping of gravity waves that are spatially well-resolved."

  We apologise if we have misunderstood. We encourage the reviewer to contact us directly to clarify this or any other aspects of the Met Office model.

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