

***Parameterization and Evaluation of Nonhydrostatic Effect in the Orographic Gravity Wave Drag in China Meteorological Administration Global Forecast System (CMA-GFS) v4.0 Model***

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**General Comments**

This work presents an updated version of the orographic gravity wave drag parameterization for the CMA-GFS model. The revised scheme accounts for the impact of non-hydrostatic mountain wave dynamics on surface wave momentum flux and the resulting wave forcing. This development is increasingly relevant as model horizontal resolution continues to improve, making purely hydrostatic assumptions less adequate.

The motivation for this work is well founded. The reported improvements are generally positive, although at times relatively modest; nonetheless, they contribute to alleviating persistent model biases. Interestingly, the largest improvements are found in the Southern Hemisphere summer, where however wave drag is weaker due to seasonal conditions, and the authors do not focus much on it.

One other general consideration I have is about the evaluation of the updated parameterization against ERA5. While ERA5 provides a high-quality reanalysis dataset, its horizontal resolution ( $0.25^\circ \times 0.25^\circ$ ) is coarser than that of the CMA-GFS simulations used here ( $0.125^\circ \times 0.125^\circ$ ). It would therefore be useful for the authors to clarify why ERA5 is considered a more reliable reference for representing non-hydrostatic effects despite its lower resolution. Presumably, this is due to the assimilative nature of reanalysis products, but this justification should be explicitly stated in the manuscript.

I provide several comments below, along with a number of technical corrections highlighting unclear or awkward phrasing.

**Specific Comments**

Lines 82-86: is this not simply the inverse of  $\text{Na}/U$ ? - which is a well known concept (see e.g., Zangl et al, 2003, Guarino et al., 2017, etc.).

Line 103: "along with many other improvements in the model dynamics and physics." Here a reference, if available, would be needed.

Lines 114-116: "partitioning the momentum stress with the Scorer parameter *when model grid mainly locates at the downstream of the subgrid orography.*" This is not clear at all, I have read it multiple times and I still don't understand what this means.

Lines 144, 146: are (1) and (2) from KA95?

Line 147: how is “low level” defined? Its definition is important here.

Line 155: it is not clear what these two constants are, as they have not been introduced before.

Line 169: define explicitly tau tilde

Line 171: derivation for equation (5) needs to be expanded; some discussion is provided below but I suggest restructuring this presentation for clarity.

Line 210-211: do you mean that you are always selecting the 10th day of each forecast for all 31 simulations? For example:

Run 1 starting on 1 Dec gives output of 10 Dec;

Run 2 starting on 2 Dec -> 11 Dec

and so forth...

this should be better explained.

Lines 216-217: “These two jets are separated around 70 hPa and 50°N, with the stratospheric jet being stronger than its tropospheric counterpart.”

We can't really appreciate which one is stronger, given that the vertical axis stops at 1 hPa, above which the jet intensifies - in fact, in the figure as currently presented the tropospheric subtropical jet appears stronger than the stratospheric one. They are also two independent features, so I would remove this sentence entirely.

A general comment about lines 212 to 223 is that this section can be significantly shortened or removed, it simply describes well-known atmospheric circulation patterns. Better to go directly into comparison with model outputs (Fig.1b,c,d)

Line 238: “Antarctic region” comprises 90-60S. I would say over the polar cap.

Line 245: “the easterly biases in the mid-upper stratosphere” specify you refer here to NH.

Figure 1b,d and its description:

What about the Northern Hemisphere troposphere? At high latitudes (50–90°N), the dipole of anomalies appears to be a mirror image of the stratospheric dipole above. In my view, this strongly suggests that during Northern Hemisphere winter, north of ~60°N, gravity waves are breaking more frequently within the mid–upper troposphere (roughly between 500 and 50 hPa). This would locally decelerate the background flow, thereby reducing wave breaking in the overlying stratosphere, where the flow remains stronger than in the control simulation. Conversely, the opposite mechanism would apply to the positive anomalies south of 60°N. That said, the origin of the longitudinal (east–west) dipole in the anomalies remains unclear and deserves further discussion.

To fully explain this behavior, it would be important to examine the lower troposphere. One would expect that non-hydrostatic effects—by increasing wave dispersion and reducing wave amplitude—limit wave breaking at lower levels, thereby shifting the altitude of wave breaking upward. In that case, one could expect to see a vertical structure characterized by alternating positive–negative–positive anomalies from the surface to the stratosphere.

If this is not the case (as I am speculating), then the anomalies shown in Figure 2d require a more thorough explanation.

Line 247: 42% is a remarkable improvement. Similarly to the Northern Hemisphere, it would be interesting to see what happens at lower levels.

Whole section b and line 424 specifically “leading to greater upward motion poleward of”:

I am somewhat confused by this section.

The downward control states that the residual mean meridional circulation at a given level is determined by the integrated wave drag above that level. This circulation is characterized by upwelling at the tropics and downwelling at the poles.

A weakening OGWD in NHE, at the poles, should thus weaken this circulation and decrease downwelling at those latitudes, not increase upwelling as stated here. Perhaps results should be framed so that they are presented as: weaker polar downwelling (so less adiabatic warming) -not greater upward motion- which thus makes NHE colder than CTRL?

Line 428: Are these temperature changes an improvement compared to ERA5?

### **Technical Corrections**

Line 47: “when breaking transferring”

Lines 70-73: I suggests removing sentence between brackets.

Line 113: “descripted” -> described

Line 233: “As in the NH troposphere,” I am not sure the use of “as” grammarly makes sense here.

Line 389: “by the way”, too colloquial

Line 501: “in total 31 forecasts of 10-day forecasts”