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Interactive comment on "On the intermittency of orographic gravity wave hotspots and its importance for middle atmosphere dynamics" *by* Ales Kuchar et al.

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The authors use output from a specified-dynamics run of the Canadian Middle Atmosphere Model (CMAM) to investigate how its orographic gravity wave parameterisation captures the variability at short time and space scales ('intermittency') of gravity wave forcing. They focus on three northern-hemisphere wave 'hotspots', namely the Himalayas, Rocky Mountains and the generalised high topography of north-east Asia. They compare their results to intermittency in observational data from limb-sounding satellites and to previous studies. They conclude that (i) the assumption of vertically-propagating waves in parameterisations is realistic in the lower stratosphere; (ii) that

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peak events can have opposing effects on the upper stratosphere and above depending on the specific region chosen, and (iii) that in the zonal mean positive OGWD anomalies contribute to mesospheric cooling.

The paper is clearly laid-out and well-written, and I have no major criticisms of the content or the presentation. In particular, the work is well-contextualised within the literature, the figures well-designed (with good use of colour to avoid significant issues for red/green colourblind readers), and the implications of their results are made clear. There are a few minor spelling and grammar errors, but these should be easily resolvable during the copy-editing stage. I support publication with at most minor revisions.

I have only one significant comment, which should not impede publication. This is that in Section 3.3 (Composite Analysis) they justify their choice to analyse the data at the regional rather than local scale with the statement that "Figs. 6 and 7 showed that spatial averaging inside the hotspots maintains the intermittent feature of the OGWD and only the information of the long tails of the distribution with extreme (and apparently very localized) drag values of -20m/s/day is lost". I disagree with this statement, especially as relates to Figure 6 - to my eye, the form of the regional-scale distributon (blue bars) is quite distinct to that of the grid-scale distribution (coloured bars). I agree that the appropriate step to take in Section 3.3 is to analyse at the regional scale, and in particular that this is necessary to make the results useful for model developments - I just disagree with this justification, and think it would be better to admit that the distributions are a bit different and explain why their results are still meaningful despite this.

In addition to this I have a series of minor specific comments - most of these are requests for increased clarification in the text rather than errors.

L019: the paper comes in at quite a high level without much preamble, and in particular the mention of the BDC without any contextualisation might be hard for someone entering the field to follow. Would suggesting adding a brief description of what this to soften it for e.g. a starting research student.

L031: you refer to the GWs as being spatially asymmetric, but don't say in what direction. From later in the paper I assume this is E/W rather than (e.g.) N/S, but you should mention this here.

L051: This sentence expands out to "The study is based on a simulation performed with the [Canadian Middle Atmosphere Model] simulation". Suggest rephrasing to be a bit less repetitious.

L053: how variable a vertical resolution? Is it high enough for your purposes in the regions you study? Suggest briefly clarifying this.

L055: you refer to "spatial scales of <T21". I assume by this you mean scales which are physically larger than T21, but this is not what the sentence says - it instead refers to (relatively) small-physical-scale features. Suggest rephrasing for clarity.

L064: you say the parameterisation launches "two vertically propagating zero-phase-speed waves" but not how often. Per timestep? Per day? Please clarify.

L071: you say the parameters are tuned to exert a "reasonable" drag. In what sense?

L083: you also need to clarify: (i) whether HIRDLS was assimilated in MERRA [I assume not] and (ii) whether HIRDLS or SABER were assimilated by JRA55.

L088: missing close-bracket.

L100: "GWD constitutes about half of the net forcing in the LS" - at the global, zonally-local, or regional scale?

Figure 2: I would strongly suggest extending this figure further south - the boxes you highlight are so close to the "horizon" that it is very difficult visually to work out the areas they cover. This is particularly the case for the Himalayas box - I have real trouble working out which parts of the mountain range it covers.

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L115: You say that the meridional accelerations were not analysed at all. Are you certain that they have cannot make a significant contribution? For example, over the edge of Antarctica (a long way from your regions!), mountain-generated GWMFs often have significant meridional components up to 50% of the zonal component - is that definitely untrue for these Northern hemisphere regions? In particular, since the main ridge of the Himalayas is aligned at a slight angle to the E-W direction, I would perhaps expect some degree of meridional forcing from there, even if not the Rockies since they're so cleanly N-S.

L145: the large difference between HIRDLS/SABER and the model is in fact a lot less than it looks from the raw numbers, and definitely less in reality than the factor of two or more inferred here. This is due to two problems affecting limb-sounder GW measurements. Firstly, and most importantly, these satellites are making a 2d cut at a high angle through the 3d wave field, travelling primarily meridionally. As a result, if you are trying to measure zonally-oriented waves (which is implicit in your comparison), then these instruments will massively underestimate wave momentum fluxes, since they measure the PROJECTION of the horizontal wavelength in the ALONG-TRACK direction. This is discussed well by e.g. Ern et al (JGR, 2004) and discussed in slightly more detail in Wright et al (ACP, 2015), and this effect will massively low-bias the measured momentum fluxes. Secondly, and less importantly, spectral fitting methods such as those used to measure GWs in these data will inherently underestimate peak amplitudes, again skewing the data low. Indeed, I would expect the values in GRACILE to be closer to a lower bound than a true estimate of the actual MF. I'm not sure how to integrate this into your text, but a brief mention that these limb-sounder estimates are known to be significantly low-biased at a systematic level would be helpful.

L173: word choice is a little sticky here: a "negative [gravity-wave drag]" is, literally, a gravity-wave acceleration. Would suggest a slight rephrase to make absolutely clear what you mean.

Figure 4: it's quite tricky to follow the seasonal cycle in the GRACILE data due to the

scale. Maybe try a log-scale ordinate?

Figure 5: same comment for the NOWD line.

Figure 6 caption and Line 194: I think you have the colours of EA and WA described the wrong way around.

L207: "Surprisingly, the second most frequent OGWD value is a small positive drag for all hotspots". Is this that surprising? For example, +1 is a lot closer to -1 than -50 is, so it seems intuitive to me that a small positive value would occur more frequently than a large negative value given the form of the distribution. Would suggest removing the line.

Table 1: Why are the months numbered rather than named, given you discuss them by name? And why are SON and MAM included here but not JJA, when they haven't been in the rest of the paper.

Code/data availability statement: excellent, but the first two sentences are ungrammatical.

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