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Interactive comment on "OZO v.1.0: Software for solving a generalized omega equation and the Zwack-Okossi height tendency equation using WRF model output" *by* Mika Rantanen et al.

Anonymous Referee #1

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GENERAL COMMENTS:

Although the software described here does not provide new or recently developed diagnostic methods to potential end-users, it proposed a complete set of diagnostic equations that could be very useful to the WRF community.

My only concern is that, given the information provided in the paper, the software seems adapted only to numerical weather prediction at low resolution (∼10ˆ2km), which is likely to lower considerably the interest in a modelling community focusing nowadays on kilometric-scale (∼10ˆ0km) and sub-kilometric-scale (∼10ˆ-1km) applications. I do not consider this to be a showstopper for the publication of this paper but the authors needs to explain more clearly the application limits of their software and

why, given these limitations, it is still a relevant tool.

Otherwise, this is a well-written and well-organized paper.

MAJOR COMMENTS:

1. There are two main reasons why this software does not seems suitable for today's state-of-the-art limited-area applications: the hydrostatic assumption and the choice of the computational grids. The hydrostatic assumption, made in the derivation of the diagnostic equations, is probably not as dramatic given that non-hydrostatic phenomena are, in many cases, a secondary factor in the atmospheric flow. However, adopting horizontal and vertical computational grids different than WRF's computational grids is likely to introduce a significant amount of errors and noises, particularly in the smallest scales. It is the responsibility of the authors to state the limits of application of their software or to provide evidence that the above potential problems can actually be overcome by their software.

2. Even in the idealized simulation presented in the paper using a 100km horizontal grid configuration, numerical errors and noises should be generated by the software. The figures presented in this paper show fields that are generally smooth, but all the chosen fields are resulting from the inversion of a Laplacian operator, which project mostly on the largest scales, thus hiding the small-scale numerical error and noises. Do the authors use a filtering strategy in the software to alleviate this issue? It would be worthwhile to show some forcing fields (like vorticity and temperature advection). In order to study the dynamics of the atmosphere, looking at the forcing themselves is as important as looking at their contribution to different variables.

MINOR COMMENTS:

1. P.3 L.8-9: It is important to stress the role of the Laplacian operator in (4). Warm (cold) advections do not directly imply rising (sinking) motion; it is the horizontal distribution of the advection that matters. Therefore, I suggests the following modification to

the text: '. . . with height and a maximum of warm (cold) advection should induce rising (sinking) motion'

2. P.8, Section 3.3: Have you considered taking into the account the orographic forcing? Otherwise, this is likely to reduce considerably the performances of the software over complex terrain. . .

3. P. 12. L. 4-5: It is unlikely that non-hydrostatic effects where important in this simulation given the low horizontal resolution employed (at this resolution the deep convection scheme should be active well before a column of the simulated atmosphere becomes truly unstable). Given the relative small-scale of the differences, numerical errors are likely to be the main source of discrepancy.

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