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Interactive comment on "Verification of a non-hydrostatic dynamical core using horizontally spectral element vertically finite difference method: 2-D aspects" *by* S.-J. Choi et al.

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Many thanks for the replies to referee 2. There are some interesting points here which should be included in the revised manuscript:

Is auxiliary fig results from a resting atmosphere test case? This is not entirely clear from the text. I am keen for the new results in this reply to be included in the revised manuscript

You use diffusion along coordinate surfaces for the cold bubble test case which works

fine here, but won't work well for steep orography. Please mention in the revised manuscript that further work will be needed to implement diffusion on horizontal surfaces. At some point you will need anisotropic diffusion with horizontal diffusion bigger than vertical.

I also include here some additional comments that I have received by email from Referee 2:

By and large I'm happy with their reply to my review.

Although a comparison with WRF would have been nice to assess the accuracy vs. added cost trade-off of using high-order polynomials, I understand their aim in this paper is mostly to verify their results with a view to highlighting the scalability advantages in later papers.

The convergence test result is very good, assuming they did it at final time.

Also the generated velocities in the atmosphere at rest look alright.

The absolute value might be a bit high, but it bears good comparison with e.g. the graphs in Weller and Shahrokhi 2014 and others.

More importantly, the fact that their velocities appear not to grow is key.

I probably should have phrased a couple of comments differently, as they were misunderstood, for example, quoting their replies:

1) Page 3732, lines 9 and 12. What do you mean by "perfectly symmetric" and "concaving contours"?

=> "perfectly symmetric distribution" should be changed to "perfectly symmetrical distribution". "concaving contours" can be changed to "concave lines".

I was more concerned about the use of "perfectly" without providing any evidence but the "eye-norm" at figure 9 of their paper.

2) Page 3723, line 19: the text in the bracket is somehow confusing. Surely the basis functions cannot be constant?

=>The basis functions are constant. In terms of a continuous function, the basis functions oscillate between nodal points.

Their original sentence was:

"It is noteworthy that the ψk have the cardinal property, i.e., they can be represented as Kronecker delta functions where ψk are zero at all nodal points except xk (but are allowed to oscillate between nodal points)."

I still maintain that the text in the bracket is unnecessary and perhaps a bit confusing. Of course the basis functions are constant in time, but not in space.

Lastly, I think the following reply does not address the potential temperature vs. temperature confusion in the initial data of the density current:

3) Section 4.2. In the original study of Straka et al. (1993), the 15 K perturbation is actually on the temperature, not on the potential temperature, see also Müller et al. (2013) for corroboration. This results in an initial potential temperature perturbation of -16.63 K in the center of the cold bubble (see the caption of Figure 1 page 4 of Straka et al. (1993)).

=> Yes, you are right. In this study, however, 15 K potential temperature perturbation is adopted similar to Giraldo and Restelli (2008) and Li et al. (2013) in order to compare our model's solution to their solutions.

We will add the following sentence for clear description.

"Note that in this study, the potential temperature perturbation of θc = -15 K is adopted similar to Giraldo and Restelli (2008) and Li et al. (2013) for comparison, which corresponds to -16.63 K in the center of the cold bubble. Straka et al. (1993) originally use -15 K temperature perturbation."

I fail to understand what the -16.63 K refers to in their last sentence, because Giraldo and Restelli(2008) and Li et al. (2013) have -15 K in the potential temperature in the center. Theta_c = -16.63 K actually results from taking T_c=-15 K as in Straka et al.(1993). I find their last sentence confusing.

My concern is that they keep on using a version of the case with the "wrong" (w.r.t. Straka) initial data for theta, so that in the long run we run the risk of not having a single reference test anymore.

I don't think any of these three points require a further formal reply from my side. Also, probably the fully revised text will be clearer.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3717, 2014.