Review of the GMDD Manuscript:
FVM 1.0: A nonhydrostatic finite-volume dynamical core formulation for IFS

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General comments:
The manuscript describes the design of the new nonhydrostatic dynamical core of ECMWF's Integrated Forecasting System (IFS) and compares it against IFS's traditional primitive-equation-based semi-Lagrangian spectral transform (IFS-ST) dynamical core. The new Finite-Volume Module of IFS (IFS-FVM) can be configured as both a shallow-atmosphere and deep-atmosphere model. The paper provides an in-depth description of the temporal and spatial discretizations, the computational grid, computational efficiency, and the physics-dynamics coupling. Both IFS-FVM and IFS-ST are compared via a baroclinic instability test case that was also used for the Dynamical Core Model Intercomparison Project (DCMIP) in 2016 (with slight variations). The numerical tests are conducted in a dry and idealized-moist configuration. The latter utilizes IFS's microphysics parameterization that triggers rainfall patterns along the developing frontal zones.
The manuscript is very well written. It includes most aspects of the model design (with the exception of the generalized vertical coordinate and the inclusion of topography, and some details about the deep atmosphere configuration) and will be a highly valuable resource in the literature. It is a comprehensive and very dense overview of the new dynamical core. Are there plans to also write an extensive scientific documentation, like an ECMWF Technical Memorandum, that could provide further details about the algorithms? It is interesting to see that IFS-FVM does not utilize any explicitly-added diffusion mechanisms. The diffusive properties are controlled by the implicit diffusion that also suppresses the computational artifacts of the co-located grid staggering (at least for this test case). The IFS-FVM and IFS-ST numerical results highly mimic each other. It demonstrates that the new dynamical core is accurate and ready for further more complex evaluations. However, the authors should take the following minor suggestions and clarifications into account before the final publication can be recommended.

Detailed comments:

1) Page 5, Table 1: Isn't the surface pressure forecast variable in IFS-ST $\ln(p_s)$ instead of p_s?

2) Page 6, after Eq. (1): Point to the Tables D1 and D2 right away for the explanations of the variables and physical constants. The first reference to the tables is currently on page 7 line 28 which is too late.

3) Page 6, Eq. (1):. Have you also experimented with moist adjustments of the physical constants like c_p?

4) Page 6, line 25: spell out 'rhs' here (first appearance of the acronym)

5) Page 7, Eq. (4): Please provide additional explanations how the ambient state is determined. Is the balance presented here computed numerically? This paper uses a stationary atmosphere in thermal wind balance as an example, but keeps it open whether this is always the best choice or whether the ambient state can (or must) also be time-dependent for more complex flows.

6) Page 7, line 24: Refer to Appendix C for further explanations of the Coriolis term (not just for the metric term). Also point out that Appendix C discusses the differences between the shallow- and deep-atmosphere equation sets for **f** and **M**.

7) Page 8, Eq. (7): Point to the Kühnlein and Smolarkiewicz (2017) paper right away to explain the meaning of the operator A_i.

8) Page 11, bottom: From the description of the semi-implicit time integration it is not clear whether this algorithms avoids any global communication. Is the iterative preconditioned GCR solution technique

entirely local without global communication? Please clarify.

9) Page 13, Fig. 1: the grid lines are too faint when printed, e.g. the open circles are barely visible. Reprint the figure with thicker contours.

10) Page 13, line 5: When switching to the deep-atmosphere equation set the surface area and volume of an element are no longer independent of height. Please clarify whether this statement only refers to the shallow-atmosphere configuration.

11) Page 14, section 2.1: The biggest omission in section 2.1 is the description of the generalized vertical coordinate and an explanation how topography is included. Please add this information for completeness, despite the fact that the presented test case does not have topography. Where is the lowest model level located in FVM: directly at the ground with z=0 or at about 12 m? Do you use equidistant grid spacing in the vertical direction in FVM? What is the accuracy of the finite-difference technique in the vertical, when non-equidistant grid spacing is used, e.g. does it reduce to a first-order numerical scheme?

12) Page 16/17, section 2.3: Describe how FVM with the height-based vertical coordinate deals with the fact that IFS's physical parameterizations assume a constant-pressure framework. The physics are not allowed to change the pressure. How is the pressure adjustment (after the variations of moisture) handled? Do you couple the IFS physics to FVM in an 'anelastic' way as analyzed by Malardel (ECMWF Workshop on Non-hydrostatic Modelling, 8-10 November 2010)?

13) Page 17, line 20: Please clarify whether the octahedral grid always adds about 20 points to the first latitude close to the poles regardless of the anticipated grid spacing.

Page 18, Fig. 2: I recommend re-plotting the right (color) figure and picking a more adequate color range for the grid spacings. E.g. the range between 4-7 km is not present and the colors are not used. Just display the range 7-9.5 km with a finer spacing like 0.25 km to enhance the near the details near the poles.

Page 19, line 29: the '30' seems to be spurious. Explain in the text (not just in the caption) which variable is shown in Figs. 3 and 4.

Page 20: Fig. 3, also other figures: Explain how this comparison was conducted. Since FVM uses a height coordinate and ST a pressure-based vertical coordinate, a comparison at the lowest full model level does not display the same cross sections. Do you use vertical interpolation/extrapolations to bring the results to the same vertical positions? Or do you assume that the variations of the vertical positions are so small (how small, please quantify) that you ignore them here. Please clarify.

Page 22, line 12: remove one of the double 'to to'

Page 27: Please comment briefly on the domain decompositions for FVM and ST. Are ghost cell added to the parallel domains? If yes, how wide are they?

Page 30, Appendix C: The appendix should be renamed to also reflect the fact that it contains the discussion of the shallow- versus deep-atmosphere configuration. There seems to be an inaccuracy how the deep atmosphere equations are formulated. In line 25, the deep atmosphere should use 'r' instead of the fixed Earth's radius 'a' in the formulation of geospherical coordinates in the horizontal direction. This also requires changes of related equations on page 31 that display the radius 'a' instead of 'r'. For example, the metric term in Eq. C3 is formulated with 'a' instead of 'r' for the deep atmosphere. Please clarify this issue and correct. How is this implemented in the model FVM?

Page 32, Table D1: add the symbol 'r' to the Table. Note that 'r' is also used in a different context as a residual in Appendix B which should be changed (e.g. with a subscript).

Page 33, Table D2: add the values of the Earth's radius 'a' and the Earth's angular velocity |Omega| to the table of physical constants.