**Review of the GMD / egusphere-2022-1235 manuscript:**
A Model Instability ssue in the NCEP Global Forecast System Version 16 and Potential Solutions

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**Summary:**
The manuscript describes that a numerical detail in the nonhydrostatic cubed-sphere finite-volume dynamical core FV3 triggered numerical instabilities in the NOAA/NCEP operational weather forecast model GFS version 16. These instabilities occurred for rather extreme atmospheric conditions, such as landfalling tropical cyclones that approach mountain ranges. The authors describe the source of the problem which is connected to FV3's floating Lagrangian vertical coordinate. The numerical approach utilizes pressure and height thicknesses between the surrounding interface levels in each model layer which get periodically remapped to a reference coordinate. However, when instabilities occurred, it was diagnosed that the thicknesses had turned negative before the remap step. Two solutions are proposed. One solution is to remap more frequently by enforcing a stricter lower bound for the height thickness. The second approach is a modification of the boundary condition for an implicit tridiagonal solver. The latter is the more general solution and will be included in the forthcoming version GFS version 17. The manuscript is easy to read and informative, especially for the dynamical core research community. The research topic is narrow but should trigger enough interest to warrant a publication. The research is of high quality. However, the write-up in the manuscript contains many sloppy inaccuracies and inconsistencies (e.g., switching sign conventions, missing or confusing axis labels, legends, symbols, or physical units) that need to be addressed before a publication can be recommended. More rigorous proof-reading could have avoided most mistakes. In addition, the quality of some (fuzzy) figures is poor. These issues are detailed below.

**Details:**

1) Lines 62-64: avoid 1-sentence paragraphs
2) Line 97: quoted number of grid points is incorrect, should read 768 x 768
3) Lines 116-125: This section contains a discussion about sponge-layer diffusion mechanisms near the model top and triggers the impression that the crashes are caused by processes near the model top. However, later only the three lowest model levels are analyzed. Clarify where is crashes are triggered in this section. In addition, clarify why the discussion of the sponge-layer diffusion settings are relevant for the analysis in this paper. If they have no relevance, this needs to be clearly stated.
4) Line 124: this point is related to the previous point. What does 'not completely' mean here? It reads as if there were examples when the modification of the sponge-layer mechanisms were able to prevent the crash, but this was not a reliable modification. Clarify this.
5) Line 140 and many others: A sentence cannot start with the abbreviation 'Fig. XX …', use the word 'Figure' at the beginning of sentences.
6) Fig. 2, Fig. 3, Fig. 7, and definitions in the text (line 286): clarify whether level 1 is at the surface or the model top, e.g. are the vertical levels counted downwards or upwards? Figures 2 and 3 suggest that level 1 is at the surface, but Fig. 7 reverses the order of the levels and shows level 127 at the bottom (surface?). This also has implications for the computation of

the layer thickness δz, as e.g. defined in line 289. If layer 1 is the lowest as indicated by Figs. 2 and 3, then the definition of the height thickness becomes negative as also shown in Fig.4e. However, throughout the manuscript the impression is triggered that the height thickness is a positive quantity since its thresholds dz_min (line 253, 255) are always quoted as positive thresholds. This is contradicted by the positions of the lowest and second/third lowest model levels in (also in Figs. 5 and 6) that show that the geopotential height of the lowest level is indeed near the surface (providing negative thicknesses according to line 299). There is general confusion about the sign of δz which needs to be remedied. The line-up of the vertical levels needs to be unified in Figs. 2/3 and 7.

7) The confusion about the level count for the vertical levels also has implications for the legends in Figs. 4, 5, and 6. If level 1 is the lowest level, it is very confusing to name the second lowest level km-1 in the Fig. 4 legend. It should read km+1 in this case. There is general confusion how the legends of these three figures refer to the levels. In Fig. 4, the labels 'km', 'km-1', 'km-2' are used for the lowest, second lowest and third lowest levels, respectively. Note that there is a typo in Fig. 4b, that lists 'km' twice and leaves out km-1. However, in Figs. 5 and 6 the labels change to 'km+1' for the lowest level (was km before), 'km' for the second lowest (was km-1) and 'km-1' for the third lowest (was km-2). If 'km' denotes the total number of levels like 127 in Fig. 4, then level 127 is located at the surface in contradiction to Figs. 2 and 3 and level numbers decrease upwards. This all needs to be made consistent with unified legends and a unique way how levels are counted.
8) Figures 2 and 3 contain confusing x-axis labels. What are the label J-grid and I-grid? Switch to some readable labels like 'latitudes' or 'longitudes'. Do these figures show cross sections along interpolated latitudes or longitudes, or data along cubed-sphere coordinates? The captions of Fig. 2 and 3 need to list the physical unit of w. clarify whether this is the vertical height velocity or vertical pressure velocity. I assume it is the height velocity, but die to the missing unit this is not clear.
9) Line 175-176: inaccurate definition of the symbols δz and p*. δz is defined as the layer height, but this is the symbol for the layer thickness. Explain how the layer thickness is computed and whether it is negative or positive. Line 175 needs to state the physical meaning of the symbol 'w' for clarity. In addition, the symbol p* is incorrectly defined as the hydrostatic layer thickness, but p* symbolizes the hydrostatic pressure (not the thickness). The symbol δp* needs to be used for the hydrostatic pressure thickness.
10) Line 178: all symbols in Eq. (1) need to be explained including $R_d$, m, z and γ. Is $R_d$ the moist or dry gas constant
11) Line 211 needs to explain the definition of the symbol δm. Again, is this considered a positive or negative quantity in connection with δz? In Fig. 4c, δm is positive, but δz is negative in Fig. 4e which renders a quantity like δm/δz negative. A negative quantity like this will not work for the computation of the pressure in Eq. (1), leading to imaginary parts. If the authors insist on these sign conventions, Eq. (1) seems to be wrong.
12) Line 218: should read '0-200'
13) Figs, 4, 5, 6 captions: specify which test case this is and refer to the location of the circle in Fig. 1 (which subfigure?)
14) Fig. 4b: it is unusual to plot pressure from lower pressure to higher pressure along the y-axis. The higher pressure is at the lower location, so the pressure axis needs to be reversed starting from the higher pressure decreasing along the y-axis.

15) Fig. 4c, the title of the plot ‘mass’ and the caption are incorrect. This is $\delta m$ and not m. Correct. The physical unit $kg/s^2$ does not make sense. Do you mean $kg/m^2$ ?
16) Fig. 4d: The values for the virtual potential temperature in the range 11-16.5 (which physical unit? units are omitted, add the units) do not make physical sense. Potential temperatures near the surface lie around 300 K depending on location. It is likely that there is another (sloppy) oversight when defining the symbol $\theta_v$. It is not the actual virtual potential temperature, but a scaled version of it. The definition needs to be shown when referring to it in line 184.
17) Fig 4: all legends are too small, enlarge the font size and line thickness
18) Figs. 5 and 6: specify the physical unit along the y-axis.
19) Lines 323: here, the layer numbering suggests that level 1 is at the top of the atmosphere since its boundary condition is described as the ‘upper’ boundary condition. As specified earlier, this all needs to be remedied.
20) Fig. 7: Does ‘K’ denote the level number? This is undefined. The ordering switches in comparsion to Figs. 2 and 3 with the level number 127 at the bottom (surface?). This needs to be cleaned up (see also the earlier comments).
21) Line 374: what is the modification? Specify it to make the test reproducible.
22) Fig. 8c: the color range looks inadequate, but maybe there are tiny (invisible) points that range over this scale. Clarify. The caption states that the x-axis has units of degrees, but the actual axis label states ‘km’. Correct this inconsistency. Add the physical units.
23) Line 386: does this implementation utilize a sponge layer near the model top? If yes, descrive it.
24) Line 400: An analysis is discussed, but no figure or scores are provided. Is this intentional or an oversight? If no figure was planned state ‘(not shown)’.
25) Line 409 typo ‘zerio’
26) Lines 507 and 509: The phrases ‘NOAA-EMC/global-workflow at gfs.v16.2.2 (github.com)’ and ‘Index of /data/nccf/com/gfs/prod (noaa.gov)’ look like links, but they are not functional. Correct this.
27) Line 541: Point to the newer version of the Harris et al FV3 model description (Technical document from June 2021). The provided link leads to an empty page. It is incorrect to also state that this was published in J. Adv. Model Earth Sys.,12 (10), 2020b (remove the journal name)
28) Lines 525 & 584: incomplete references, both needs the location and conference dates. Capitalize fv3gfs and fv3.
29) Figs. 4, 5, 6, 7 are rather fuzzy and of low quality. Improve the quality.