Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-317-RC1, 2020

Geoscientific Model Development Discussions

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Interactive comment on "A process-based evaluation of the Intermediate Complexity Atmospheric Research Model (ICAR) 1.0.1" *by* Johannes Horak et al.

Anonymous Referee #1

Received and published: 23 November 2020

Note: comments are structured "[P]age #:[L]ine #:"

General comments:

In this paper, the authors present a detailed investigation of the upper boundary within the ICAR model, as well as a well-supported correction to the calculation of the Brunt-Väisälä frequency for linear theory calculations. They perform rigorous comparisons of their improved model (ICAR-N) with the old model (ICAR-O), the analytic solution to the underlying equations of ICAR, and to a more complex atmospheric model, WRF. The questions posed, methodologies employed to answer them, and final conclusions reached, are of value to the development community of ICAR. In the test case, these

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results support their conclusion that some model top height exists which reduces errors to the advected quantities and microphysics of the model which maximizing computational efficiency. However, after testing a number of boundary conditions (BC) for the upper boundary, they fail to provide a clear recommendation for which combination of upper BC is most favorable. Such a determination would surely strengthen their conclusions, and be of great value to the community using ICAR. The demonstrated lack of dependence between minimum model top height (Zmin) and combination of BCs seems to contradict the hypothesis that Zmin is chosen to avoid errors in the assumed downward fluxes. The authors should explain this discrepancy and, if possible, provide evidence in support of a combination of upper BCs to be used as default in ICAR going forward.

Specific comments:

P7:L10-11: Why use a constant dz spacing for ICAR, but not for WRF? ICAR v1 supports this.

P7:L23-24: How did you test Thompson MP code to see that ICAR and WRF produce the same results? This sounds like you have ICAR producing identical output to WRF. This would be exceptional, but unlikely.

Section 2.2.2 – Please also state explicitly that upward fluxes result in quantities being lost (no longer tracked) by ICAR. This would motivate the use of a downward flux BC that seeks to balance this, and may explain why the current downward flux BC in ICAR does not produce drastically unrealistic simulations on first pass.

Section 3.4 – I could use some discussion of the different BC's, what they try to represent, and why you chose them. Not too much, perhaps just a sentence or two for BC's 1-4. Especially prior to where you refer to them on P9:L26.

Section 4.1 – Do you think ICAR should be considering wave amplification as a result of decreased density with height? This seems to be the cause of a major difference between ICAR-N, the analytical solution, and WRF. EQ7 seems to suggest that such a correction is not too difficult to implement, but this may just appear deceptively simple.

P18: L6-7: “If ztop is set high enough these deviations therefore do not affect the cloud processes below” Would you expect these deviations to be advected down into the model where cloud processes occur? Please provide some discussion for why the errors in potential temperature remain in the upper most layers and are not advected elsewhere in the domain.

P19:L5-6: If all of the BC combinations are similar, why do you decide on BC code 111 in the end? Is this the most physical? The simplest computationally? Provide some support for this choice. Do you think that this should be the upper BC by default for ICAR?

Section 4.5 I would restructure/rewrite this section. If WRF and ICAR are using the same MP schemes, then all differences in the hydrometeor, water vapor, and precipitation fields should be due to the wind field and advection code. Indeed, this seems to be the main conclusions of sections 4.5.1 and 4.5.2. So, I would just approach this section by way of differences in the ICAR and WRF wind fields, and then use those differences to explain the observed differences in ICAR and WRF hydrometeor, water vapor, and precipitation fields. This way it is organized cause→effect instead of effect→cause.

P 28:L5: This point should be reflected in your conclusions.

P29:L14-15: I do not agree with this statement. Horak et al. 2019 did not test model top heights up to the Zmin of 15.2 km. It has not been shown that comparing simulation output with measurements leads to an incorrect result – you would have to show me the comparison with measurements for ICAR-O with a model top of 15.2 km for me to believe that statement. Your background information on the MSE of ICAR-O with the mentioned measurements given on P35:L21-23 clarifies your statement though – perhaps you could move some of this to the earlier reference.

P29:L15: This difference in model top heights between the two simulations (ICAR-N and ICAR-O) seems unfair. ICAR-N represents an altered model. However, the model top height is chosen to minimize errors relative to an ideal model setup. This choice of the model top height is a user parameter given to ICAR, not a feature of ICAR-N itself. I can see why you do this though, to show the "best-case" setup following your procedure. Still, I would like to see an "ICAR-O/N" simulation in this section with ICAR-O run with a model top of 15.2 km. If you also wanted to compare these simulations to the measurement dataset used in Horak et al. 2019, it would make this section much stronger.

P 31:L3: To support this statement of the upper BC in ICAR-O causing the excess hydrometeor concentration, I would like to see the vertical wind field for the model top of ICAR-O, or some evidence of strong downward fluxes.

P34:L3: You suggest that the model top height has an effect on the model mainly by controlling if the model top cuts through up and down drafts. To me, this suggests that the presence of negative fluxes, as you discuss in Section 2.2.2, and the elimination of these fluxes, should be dictating where the model top is. Following this logic, different upper boundary conditions should then also have an effect on the model top height. However, in figure 5, you demonstrate that this is only the case for potential temperature using a CG vs a ZG. Can you explain this inconsistency? Isn't it strange that there was 0 effect on Zmin by changing the upper BCs? Especially given that you conclude in section 4.6 that the ICAR-O simulations are affected by the upper BC used.

Technical comments:

P5:21 – sentence needs to be fixed for clarity

P6:L30-31: This description of ICAR-N, "calculates N from the perturbed state of the atmosphere predicted by the ICAR-O" should be somewhere in section 2.2.1. It makes clear the differences between ICAR-N and ICAR-O, I could have used it earlier.

P9: L22: “to as [a] set of boundary conditions”

P12:L29: “This section”, please give section number of case study results.

Figure 6, Figure 13 caption should read: “Reduction of error (RE)”

Figure 13: I feel that this should preceded figure 12. Figure 13 supports your model configuration as discussed in the third paragraph of section 4.6, while figure 12 provides results relevant to paragraph 4. I see that they are ordered this way since you refer to the South Island DEM first, but the order ends up illogical when the whole figures are taken into consideration.

P36:L1: should read “ possible model top elevation Zmin to produce”

P36:L8-9: This is not a finding or a recommendation. It should be removed from the list.

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