Responses for reviews, "Energy conserving physics for nonhydrostatic dynamics in mass coordinate models", gmd-2023-184

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1 Response

Thank you for all reviewers for their very useful and insightful comments. We responded to each comment below. Reviewers' text below is in the normal font, our responses are in bold. We also attach a modified manuscript.

2 Review #1

General comments

This paper looks at different thermodynamics assumptions that can be used when coupling physical parametrizations and a dynamical core. Under the different assumptions, the updates to the dynamical prognostic variables change, thus changing the model's evolution. The authors then test the different approaches through a moist rising bubble simulation, in which the physics parametrization is the change of phase of moisture species, and the related latent heating effects.

This is an important and interesting question in the development of atmospheric models and has historically been neglected. The authors' work is a good contribution to investigating this question, and they have some nice maths to explain and derive the different approaches. The derivation of the constant-pressure update that conserves energy is particularly satisfying, and the results demonstrating the lack of energy closure for their CP-AL-HY and CP-CL-HY cases (without variable heat capacities) are convincing.

Besides some very minor points (listed below), I have a reservation about whether the authors' conclusions about the CV-VL-NH case (the constant-volume update) are fully supported by their numerical results. Otherwise, the paper is well written and communicated, so if this reservation can be addressed, I would be very happy to recommend this work for publication.

Specific comments

1. My main concern is whether the authors' conclusions are fully supported by their experiments? The authors argue that the constant pressure update "is more accurate at large timesteps despite being less consistent with the underlying equations", and that it therefore "may be more computationally affordable".

This argument is based on the errors in the final solution of the test case, when compared with a reference state (calculated with very small dt). The errors recorded are larger for the constant-volume update than the constant pressure update, at the larger values of dt that were investigated.

My worry is that rising bubble test cases, like the one used here, can be inherently unstable (even in the absence of moist physics). These typically look like a mushroom or pimple at the top of the main bubble (let's call this a "positive" instability), but can also be the opposite situation with the bubble's top having collapsed (a "negative" instability). I have previously seen that small changes in the model configuration can trigger these instabilities in bubble test cases or dramatically alter their appearance – for instance just changing the number of horizontal cells by 1 can flip whether there is a positivity or negative instability.

Figure 4(e) shows the final state of the rising bubble that corresponds to the larger errors that underpin the authors' conclusion, but to me this seems like a situation where this "negative" instability has occurred, and I believe that this is the dominant contribution to the error measurement. Because I think that the error is dominated by the effect of the instability, and because I suspect that the formation of such instabilities in this test case may be very sensitive to modeling choices, I am not (yet!) convinced that we can conclude that the constant-volume update may be less accurate at large time steps. This is particularly true because an instability in a small-scale rising bubble may not be representative of the much larger-scale motions in a climate model (which is the authors' primary motivation).

I'd like to propose several different actions that the authors could take to address this concern:

(a) In my opinion, the work would still be worthy of publication if the conclusions relating to the constant-volume update were softened, and if more explanation is given as to why the constant pressure update might appear to be more accurate. I note that the authors give some explanation in lines 366-368, but I think this could be expanded.

(b) However if the authors want to further support their conclusions, I think it would be helpful to know how robust their numerical results are. For instance, would they find the same result (that for large timesteps the constant pressure update gives a lower error) for the same bubble test but at different spatial resolutions? Or do they find the same result for similar bubble tests with slightly different initial conditions? Or do they find the same result for different numerical schemes used in the dynamical core?

(c) I recognise how challenging it is to choose a test case to investigate this question – I don't know of any idealised moist test cases with analytic solutions that aren't steady-state problems! However there are also other moist tests that may further support the authors' conclusions (for instance moist versions of the Skamarock-Klemp gravity wave in a vertical slice, moist versions of baroclinic wave tests or moist Held-Suarez configurations). I do realise that this would be much more work, and do not think that this is a requirement for this work to be published.

Yes, we agree that our conclusions about constant pressure and constant volume updates at large timesteps were overstated, especially in light of the instability pointed out by the reviewer. The reviewer outlined a very nice set of numerical experiments that could address this question, but would require substantial new effort. As such, we have instead reworded this claim so that it is in the form of an observation instead of a conclusion on accuracy. We reworded/removed corresponding text in Introduction, Section 2.5, Section 5.3, and Conclusion.

2. A key part of the authors' argument is that using the time-split physics equations (12)-(17), it is not possible to simultaneously satisfy both δp and $\delta\phi = 0$ (for instance this is discussed in lines 128-129 of page 5). Sorry if I am missing something obvious here, but this actually wasn't clear to me just from looking at equations (12)-(17), so I think this could benefit from further explanation. I can see that if $\delta\phi = 0$ and $\delta u = 0$, then to satisfy $\delta e = 0$, we require a constant volume update. Is there anything more than that?

Correct, looking at eqns (12)-(17), $\delta\phi = 0$ comes out of of them naturally. If one decides to adapt constant volume update and impose $\delta p = 0$, the system would then be overdetermined. To clarify, we added the following sentence to Section 2.2: "Note that modifying the constant volume update to obey $\delta p = 0$ as well is impossible, because it leads to an overdetermined system which in general will not have energy conserving solutions."

3. Are the latent heats Ll and Lv dependent on temperature or constant? The notation used in Section 4 includes "VL"" to describe variable latent heats, but it is not clear to me if the analysis takes this into account. I think it would be helpful to clarify this in the text. If Ll and Lv are in fact constants, how would the analysis change if they were temperature dependent? Would the derivation of (24) and (28) still follow?

In our setup, the coefficients L_l and L_v that appears in the thermodynamic potentials are constants that are independent of temperature (in fact, they are just the latent heats at a specific reference temperature and pressure). Note that latent heats of phase changes are defined as differences of enthalpies of the two phases (Emanuel1994, eqn 4.4.1, etc.). For example, latent heat of vaporization equals to $c_p^v T - c_l T$ and it varies wrt temperature as long as $c_p^v \neq c_l$ (though theoretically these two heat capacities differ, the assumption that $c_p^v = c_l$ can be imposed in the numerical model). Unfortunately, there is very confusing terminology used here, where both the L_l & L_v coefficients and the difference in specific component enthalpies

are referred to as "latent heats". It is only the latter is actually the thermodynamic definition of latent heat, and the former is a reference latent heat at a specific (reference) temperature and pressure. However, in the dry heat capacities case (where we assume all the heat capacities are the same) then latent heat = L_l (or L_v), which is where a lot of confusion comes from.

Therefore, by using heat capacities of water forms close to theoretical (but still constants independent of temperature), we model *variable latent heats*. This is a consistent thermodynamic setup, as discussed in Eldred2022, and a huge improvement to the current design of EAM/CAM/E3SM/CESM, where the heat capacity of the dry air or zeros are used for water forms. In addition to already present text in Section 4.3 right after eqn 31, "... In other words, VL in the name of the update indicates that we use the full unapproximated thermodynamics, including the use of c_p^*, not c_p^{d} (specific heat capacity of the dry air), in (??)..." we added another paragraph (second in Section 4.3) with these details.

4. Similarly, the configurations are named either by variable latent heats or constant latent heats, but are these actually referring to the heat capacities rather than the Ll and Lv terms? Or do variable heat capacities and variable latent heats come hand-in-hand?

We hope this is addressed in the response above.

5. Page 6, lines 151-152. The authors discuss that traditionally the equation for ϕ does not include physics terms. Is this a simplification, and so something that could be reconsidered in order to derive a constant pressure approach that is still consistent with the underlying equations?

Good point. In the constant pressure approach, our formulation for both hydrostatic and nonhydrostatic does implicitly change ϕ. We have formulated it as a change in the thermodynamic variable which induces a change in ϕ from the equation of state, but this could instead be formulated as tendency terms on both the thermodynamic variable and ϕ. We have added a line mentioning this fact in Section 2.5.

Technical comments

1. Page 3, line 69, first sentence. Is the tense correct in this sentence? Maybe "is using" should be "uses"? **Thanks, fixed.**

2. Page 3, equation 4. The authors use D to represent the dynamical terms in the momentum equation, which is also used for the material derivative. I think choosing a different letter here might be help avoid confusion. **Thanks, yes, we replaced D with H for the "dynamical terms".**

3. Page 6, line 153. Should $\delta\phi = 0$ actually be $\delta\phi \neq 0$? Or are the authors making the point that $\delta\phi = 0$ will hold irrespective of the choice of physics-dynamics coupling? **There, $\delta\phi = 0$ is not a typo. What we meant is that from time-splitting approach, the only form of ϕ equation in nonhyrostaic case is $\delta\phi = 0$, because all terms in ϕ equation are solved in dynamics.**

4. Page 6, line 160. It might be worth reminding the readers here why $p = \pi$ (since π is the hydrostatic pressure). **Added a clarification.**

5. Page 8, line 199-200. I didn't quite follow the point that the authors are trying to make here: "instead, we reinterpret the first law as the general statement that the energy of the system must be conserved up to fluxes." Could there be more explanation here? Or could this point be reworded?

Thank you for pointing out that this text is not clear. This subsection says that while it is common to solve $\delta h = f_T$ equation as the thermodynamic equation in hydrostatic models, and while the enthalpy eqn aligns with the 1st law of thermodynamics under constant pressure assumption, for the nonhydrostatic case, we have to re-derive the thermodynamic equation as $\delta h + (\pi/p - 1)\delta(R^*T) = f_T$ in order to conserve energy. We rewrote Section 2.7 to clarify it.

6. Page 9, section 4.2. What form does the initial perturbation take (e.g. a cosine bell or a Gaussian)? **Thanks, the perturbation is given by a cosine bell, added that to the text.**

7. Page 11, line 285. Is $\tilde{E}$ missing a subscript t? **No, the t subscript is used to denote a time derivative, and here $\tilde{E}$ is a component of total energy.**

8. Page 13, lines 321-322. The authors describe the plots as being "visually identical". The pedant inside of me doesn't agree! I think there are small differences can be spotted, so that they are almost visually identical. **Thanks, changed to 'visually very close'.**

9. Page 14, line 329. Should VP be VL here? **Thanks, fixed.**

10. Page 16, line 399, "properly modeling". Should this be "proper modeling"? **Thanks, fixed.**

3 Review #2

This is really impressive work. The authors have described an important (and surprisingly longstanding) problem and, more importantly, a constructive way of fixing it with minimal modifications to the model. I only have a few comments.

1. You have the line "A key difference between the two approaches is that with constant pressure, latent heat release results in only vertical transport (by changing the position of the layers, ϕ), while with constant volume, latent heat release increases the pressure leading to gradients that can result, through the dynamical terms, in both vertical and horizontal mass transport." I would recommend that this is more prominently highlighted, perhaps in the abstract. Physics inaccuracies can lead to a spurious pressure gradient force, which affects dynamics and transcends the splitting.

Thanks, we've expanded this sentence into a new short subsection, Sec. 2.6, and also added a sentence on this to the abstract.

2. I would like to see a mention in the conclusions of how widespread this problem is (or might be). I know this is a paper focusing on solutions for EAM, but are there other models that are affected? Or is this a unique EAM feature?

This is a great question for which we have limited information. We added a note on the usage of specific heat capacities in the introduction around line 30. Recently we participated in a survey, organized by WMO working group on numerical experimentation for modeling groups on exactly this topic. We hope this type of information will appear in a future WMO report.

4 Review #3

This manuscript derives and evaluates several different methods of updating physics tendencies in an atmospheric model. One method assumes that volume is constant, another that hydrostatic pressure is constant, and a third that non-hydrostatic pressure is constant. The manuscript is a useful contribution but parts of it could be clarified.

I have no major suggestions, but listed below are some minor ones.

Line 7: "variable latent heats". What quantity does latent heat vary with? Temperature?

Yes, the definition of the latent heat as the difference of enthalpies in the unapproximated thermodynamics we use leads to its temperature dependence. It is not the same as temperature dependence of L_v,... constants. Please, see the response to the Review #1 (comments 3 and 4 above the technical comments). We added a note on "unapproximated thermodynamics" around line 100: "We use the *unapproximated* thermodynamics (or *variable latent heats* as explained further in Section 4.3)..." and added a paragraph about it in Sec 4.3 (second paragraph in Section 4.3).

Line 70: Could you define phi and pi more clearly? What are their dimensions?

Thanks, we modified that sentence "... on arbitrary level positions given by geopotential ϕ and mass coordinate values π, based on hydrostatic pressure...". The units for geopotential are m^2/s^3 and pressure is in Pa.

Eqn. 10: Could you please indicate, with a subscript, e.g., which variables are being held constant in each of these partial derivatives?

We are not sure adding subscripts in eqn (10) would lead to more clarity.

Line 84: What is pi, the mass coordinate/hydrostatic pressure? How is pi related to p? Is there an equation that relates the two?

Yes, π is the hydrostatic pressure, as noted after eqn (10). We agree that some details on vertical coordinate systems in atmospheric models are omitted in the text. The choice of a vertical coordinate in atmospheric models is a rich subject extensively covered in the literature, and it is out of scope of this paper. We cite relevant papers on the subject, including Lin2004, Taylor2020, and others.

Line 85: What are the units of pi and s?

Please, see the above comment. s is a dimensionless Lagrangian parameter, explained in works of Starr1945 and Lin2004.

Eqn. (12): It might be worth mentioning that by assuming no change in wind, dissipative heating is neglected.

Around line 143 we state that $f_u = 0$, which includes all possible momentum tendencies, like heating, turbulence, etc.

Line 119: In this equation, what do the subscripts mean? Are they derivatives? Or do they indicate that a variable is held constant?

The subscript π there denotes differentiation wrt π.

Line 136: "we simplify the algebra by neglecting momentum tendencies by taking fu = 0" This omits frictional heating.

Yes, since our simplified physics removes rain instantaneously, the frictional heating of rain is not present in our setup.

Line 140: How can a physics update keep both pressure and volume (and hence density) constant? A parameterization will increase temperature, thus changing density by the ideal gas law.

Thank you for pointing out that the text is not clear. It is correct that both pressure and volume cannot be held constant. To clarify, we further modified Section 2.2: "Note that modifying the constant volume update to obey $\delta p = 0$ as well is impossible, because it leads to an overdetermined system which in general will not have energy conserving solutions."

Lines 150–153: "An update which holds pressure constant while allowing the volume to change is impossible to derive via time-splitting for the nonhydrostatic equations, since the prognostic equation for layer positions does not have any traditional physics tendency terms, and thus any dynamics/physics time-split approach will lead to $\delta\phi = 0$ for the update." Why can't an equation for dphi be derived based on, e.g., the expansion of the layer thickness due to heating at constant pressure? In this way, the physics parameterizations can indirectly affect phi even if they don't directly modify phi.

Good point. In the constant pressure approach, our formulation for both hydrostatic and nonhydrostatic does implicitly change ϕ. We have formulated it as a change in the thermodynamic variable which induces a change in ϕ from the equation of state, but this could instead be formulated as tendency terms on both the thermodynamic variable and ϕ. We have added a line mentioning this fact in Section 2.5.

Lines 153–154: "a diagnostic equation for ϕ": What is the diagnostic equation for phi? Could you write out an example of such an equation?

Thanks, we added to the text a reference to the hydrostatic integral that is used for ϕ in hydrostatic models.

Lines 170-171: "In the nonhydrostatic 170 case, one cannot derive a $\delta p = 0$ update consistent with the time-split equations since the combination $\delta p = 0$ and $\delta\phi = 0$ prohibits changes to any state variables." What does dphi=0 have to do with time splitting? Where is the inconsistency with time-split equations?

Equation $\delta\phi = 0$ comes out of time-splitting approach applied to equations (12)-(17). However, $\delta p = 0$ is a very common approach used in the models. There is no time-splitting that would lead to $\delta p = 0$, therefore we consider $\delta p = 0$ even though it is not consistent with time-splitting.

Lines 186–187: "As noted above, the nonhydrostatic constant pressure update is not consistent with our original time-split equations, since it induces a change in volume which in our original equations is only allowed through dynamical terms." Why can't a change in volume occur through physics terms?

Note that the fundamental thermodynamic equations (Equations 4-10), when timesplit, do not allow the physics to change volume, as shown in Equation 15. It's only when we depart from the fundamental equations for constant volume that the physics now must change volume, per your comment above.

Lines 190–192: "A key difference between the two approaches is that with constant pressure, latent heat release results in only vertical transport (by changing the position of the layers, ϕ), while with constant volume, latent heat release increases the pressure leading to gradients that can result, through the dynamical terms, in both vertical and horizontal mass transport." Please elaborate this sentence. It is important enough and subtle enough to deserve its own paragraph.

Thanks, we've expanded this sentence into a new short subsection, Sec. 2.6, and also added a sentence on this to the abstract.

Eqn. (24): Shouldn't the RHS have another term that equals f_T?
Thanks, yes, fixed.

Eqns. (31)-(35); These equations for the physics updates include various zero updates, e.g., $\delta p = 0$ in (35). Could you please explain how you derive which quantities have a zero update? **Section 2.2 explains updates behind notations $\delta p = 0$ and $\delta\phi = 0$. There, line 147 explains that we consider no mass fluxes in phase transitions, so $\delta\pi = 0$.**

Line 329: "two constant pressure VP updates" What is a "VP" update?
Thanks, it should be "VL", fixed.

I don't understand why, in Fig. 2, the solutions at different time steps differ from each other early in the simulation (200 to 450 s) and then magically collapse onto identical solutions before the peak precipitation is reached at 550 s, and remain identical thereafter. Why do diverging solutions come back into agreement with each other? Does something constrain the precipitation, or is there a bug in the simulations?

Thanks, it is a good observation. Figure 2 plots only precipitated water, which shows that precipitation amounts diverge and then converge. The corresponding solutions do not converge, as seen on Figure 4 (compare plots, say, in the first row). The explanation about precipitation convergence is that at the beginning of any simulation, the flow causes a lot of oversaturation, and excessive amounts of water are rained out. In contrast, at the end of simulations, there is not nearly the same amount of water left in the model.

Fig. 3: The line colors are not distinct enough. CV-VL-NH and CP-CL-HY both look green. CP-VL-HY and CP-AL-HY look similar too.

Agreed. However, per GMD directions, we attempted to comply with requirements on plots for people with color-blind vision. We used a color-blind scheme for our plots and ran them through special software that was recommended by GMD that displays plots as seen by people with various disorders. We ended up with colors that are still distinguishable for all options.

Fig. 3(b) shows several purple lines. Please describe their meaning in the legend or the caption.
Thanks, fixed. We added that the purple curves are F_P values for each simulation.

5 Review #4

General comments:

The paper written by Oksana Guba presented energy conserving physics in a nonhydrostatic dynamical core. The result is clearly presented and this development is also useful for other applications. I have some questions and several minor comments for improving the presentation quality.

Specific comments:

Line 95: It will be better to separately express these three eqs., or please add a comma for clear reading. **Thanks, fixed.**

Line 157: Eq. (25) seems to be the same as Eq. (17). Please confirm. **Yes, they are the same. Eqn 25 is repeated for readability.**

Line 159: It will be better to separately express eH in Eq. (26), or use a comma for clear reading. **Thanks, fixed.**

Line 168: Why no eq. nos. here? **Yes, it is an oversight. We did not add an equation number at this stage to not to mess equations numbers used in the reviews and the response. We will sort this out if the paper is accepted, at the final stage.**

Line 179: Again, Eq. (29) seems to be the same as Eq. (17). Please confirm. **Yes, they are the same for readability.**

Line 180: It will be better to separately express these three eqs., or please add a comma for clear reading. **Thanks, punctuation added.**

Line 271: Please move the caption at the top of this table. This caption seems to be long, so it could partly remain as a footnote for this table. **Agreed. However, if we move it now, the format of the final draft, if accepted, will be very different (GMD uses 2-column formats). Thus at this stage we prefer to defer this decision to the final GMD typesetting team.**

Line 315: Why fails to converge for CP-AL-HY and CP-CL-HY? These would be the best performance (minimized errors) at 10-1 timestep. **Since there is no analytic solution, the reference solution is given by a highly resolved simulation. After a certain threshold, the level of uncertainty in the reference solution becomes too high to achieve convergence.**

Line 327: Is the difference unitless? Please confirm. **Yes, it is dimensionless, since the definition in eqn 37 is.**

Technical comments:

Abstract: Need to define "E3SM" within the abstract. **Thanks, done.**

Line 196: The accessed date is shown in the reference list, so there maybe no need to show it in the main text. **Agreed, however we are following the bibliography template provided by GMD.**

6 Other minor changes

Slightly modified the 1st sentence in the abstract "Motivated by reducing errors in the energy budget related to enthalpy fluxes with E3SM..." to "Motivated by reducing errors in the energy budget related to enthalpy fluxes within E3SM..."

Changed signs in two last rows for $c^l - c_p^v$ terms in Table 1 to be consistent with eqn (38).