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Geoscientific Model Development Discussions

Interactive comment on "Generalization and application of the flux-conservative thermodynamic equations in the AROME model of the ALADIN system" *by* D. Degrauwe et al.

Anonymous Referee #1

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This is an interesting and useful study on interactions of the physical parametrizations and and model dynamics in the mesoscale NWP model ALADIN-AROME. The manuscript reports about generalisation of the formulations suggested by Catry et al., 2007 and testing them in AROME framework. The time evolution of the specific mass of hydrometeor species and heat content (c_p T) due to the physical parametrizations is expressed in a universal compact form as the vertical divergence of the sum of terms related to the fluxes of precipitation, phase conversions of water (pseudofluxes), diffusion and radiation (Eqs. 12 and 13). It is shown that in terms of the standard validation the results show a neutral impact, but may be significant in specific (convection) cases. In any case, the suggested formulation allows consistent and systematic building of

the interfaces between model physics and dynamics, by default conserving energy and mass during the model integration.

A general comment is that the tone of the manuscript is somewhat apologising, defendig. The authors feel that the influence of the suggested formulations on weather forecast is small and try to convince the reader that in spite of this it should still be applied in the operational NWP model. They do not discuss systematically possible problems related to the introduction of the new formalism, but a feeling remains that such may exist as they try kind of convince invisible opponents? Based on the information given in the manuscript it is evident that the system of equations if physically logical and well based, allows using less simplifying assumptions and a clear definition of the remaining ones, conserves energy and mass. If, in addition, the application of the new approach does not lead to worse weather forecast (neutral impact), computational cost does not increase significantly, the model code does not grow extremely complicated etc, then it seems natural that it should be applied. In this context, it might be good to mention the limitations of the standard station verification applied to validate research results against surface pressure, screen-level temperature and relative humidity and anemometer-level wind when the verification is

In this sense, the key component seems to be formulation of the pseudofluxes, related to the phase changes between the chosen prognostic hydrometeor species, water vapour (and dry air). It would be good to discuss concepts related to these fluxes, perhaps using an example of some of the existing and/or not yet introduced R_j terms (conversion between cloud ice crystals and precipitating snow, cloud liquid droplets and cloud ice crystals, for example). There is most probably a whole system of microphysics parametrizations behind any of these terms. Not all details should be discussed but the principles discussed and references given, to allow understanding the concepts not only at the level of general formalism.

Another issue are the limitations of the approach. Three limitations are mentioned in the text but not discussed systematically: 1) interactions between microphysics related to the radiation transfer, on one hand, and that related to the evolution of clouds and precipitation, on the other hand, 2) interactions related to the surface, e.g. between moisture in air and in the soil, 3) the presented formulations being based on the hydrostatic assumption. In this case, the equations were evidently applied in the non-hydrostatic AROME. However, no details are given, but mentioned that it is safe to apply the hydrostatic equations also in the nonhydrostatic model (p.7,l.19). In the conclusions, the possibility to approach in the future these now missing interactions related to radiation and surface could be outlined.

The manuscript is generally well structured and written. It makes understandable features, possibly remaining unclear by reading the original Catry et al, 2007 paper. However, a lot of details should be presented more carefully as now concepts and variables are discussed before they are defined and some essential information seems to be missing. The English language is understandable to me but I would leave the Editor to judge if there is room for improvement.

My specific comments are given as comments in the manuscript pdf (made with Acrobat Reader). Hopefully, their contents are visible by using the same reader.

The manuscript could be approved after taking into account these general and specific comments. In my opinion, this requires at least a medium-size revision.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2015-279/gmd-2015-279-RC1-supplement.pdf

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