Author's response to Referee #1

Referee's comment: *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?*

The authors admit that they have been very careful in their formulations. This general comment aside, we agree that the virtues of the approach presented in this manuscript could be accentuated more. (see also next comment)

Referee's comment: *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.*

We obviously believe in the value of this work, and we are grateful that it is acknowledged by this referee. We share the referee's view that the advantages of the presented set of equations are sufficient to justify their application (given the neutral impact etc.). A sentence to emphasize this will be added to the conclusions of the manuscript.

Referee's comment: *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* (sentence truncated in Referee's document)

This is a good suggestion. Implicitly, the manuscript already points out the limitations of standard verification by the inclusion of a case-study, but we agree that it should be stated explicitly. A paragraph has been added to the manuscript to amend this.

Referee's comment: *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.*

The concept of pseudofluxes is indeed the key to the flux-conservative formulation. However, this does not mean that the physical parameterizations should use these pseudofluxes internally. In fact, the AROME microphysics are internally formulated in terms of local tendencies $\partial q_k/\partial t$. In order to use the flux-conservative equations, these tendencies are then converted to pseudofluxes with the formula

$$R_j = \int_0^p \frac{1}{g}\frac{\partial q_k}{\partial t} dp \qquad (1)$$

The referee is entirely right that the manuscript is lacking a more detailed explanation of why pseudofluxes are introduced, and how they relate to (the more conventional) local tendencies. A section will be added to the manuscript to remediate this.

As the referee indicates, the internal machinery of the (microphysical) parameterizations that provide the pseudofluxes is quite complex and a topic in itself. References to such parameterizations are already given in the manuscript, e.g. Lascaux et al. (2007). The authors therefore believe that the discussion of the internals of such schemes falls outside the scope of the presented manuscript.

Referee's comment: *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.*

1. Interactions between the microphysics parameterization and the radiation parameterization are not a limitation of the set of equations itself. The scope of the presented manuscript is the interaction between the parameterizations and the dynamical core, but *not* the interaction between one parameterization and another. To indicate that such interactions exist, the example is given of cloud microphysics influencing radiation. The scope limitations of the present work will be mentioned more explicitly in the introduction section.

2. The referee points out correctly that the interaction with the surface is somewhat underexposed in the manuscript. Regarding this topic, we would like to make the following remarks:

   - The presented set of equations describes the impact of physical parameterizations on atmospheric prognostic variables. In this sense, the evolution of the (sub)surface prognostic variables falls outside the scope of this document.
   - The presented flux-based equations do not pose any direct limitations regarding the interaction with the surface. Moreover, they are directly compatible with the work of Best et al. (2004), who present a generalized flux-based coupling between the surface scheme and the atmospheric model. The AROME model uses the interface from Best et al. (2004) to couple its externalized surface scheme SURFEX to the upper-air parameterizations.

   These points are added to the manuscript.

3. The issue of applying the presented set of equations in a non-hydrostatic model like AROME is discussed in section 2.4. The following 2 points are made there:

   - The extension of the flux-based equations to the non-hydrostatic case is pretty straightforward, as is shown in Catry et al. (2007). The main consequence of this extension is that heat appears as a source not only in the temperature equation, but also in the continuity equation.
   - Evidence is found in literature that the impact of the source term in the pressure equation is quite limited. This is not just 'mentioned' in the manuscript, but a reference is given.

   Given these points, we have chosen to use the 'hydrostatic formulation' in the nonhydrostatic AROME model. Maybe this was not sufficiently clear from the manuscript, so it has been rephrased somewhat in section 2.4, and it is repeated in section 4. A sentence is also added in the conclusions to mark an in-depth assessment of using the nonhydrostatic formulation of the flux-based thermodynamic equations as an interesting future research topic.

Referee's comment: *My specific comments are given as comments in the manuscript pdf*

We agree with most of these valuable suggestions. The manuscript will be adapted accordingly.