Author's response to Referee #3

Referee's comment: *The equation set (2-8) is only the equation set for the physics, not for the whole model. This should be made clear.*

The scope of the presented manuscript is indeed restricted to the equation set of the physics, something that was not sufficiently clear in the original manuscript.

The introduction is modified in order to set this scope more clearly. Also at the point where the equations are introduced, it is now stated explicitly that these equations only describe the physics part. The abstract and conclusions section also have been modified to emphasize this important restriction of the scope.

Referee's comment: *It is not clear why pseudo-fluxes are employed to describe source terms. I think that this makes the issue unecessarily unintuitive. Why the flux-conservative form is enforced here? Is there a coding style advantage?*

Writing equations in a flux-conservative formulation is a way to ensure that conservation laws are obeyed after spatial and temporal discretization. This technique is applied commonly in the dynamics (literature references are given in the introduction section), and in this manuscript it is applied in the physics.

In order to write the physics equations, and more specifically the contributions of phase changes, in a flux-conservative way, the concept of pseudofluxes is introduced in Catry *et al.*, (2007) . We agree that this concept deserves more explanation, and the manuscript is adopted in this sense. Also, it is noted that this does not mean that the internals of the physics parameterizations should be formulated in terms of pseudofluxes. Instead, it is quite easy to calculate pseudofluxes from local species tendencies by integrating from the top of the atmosphere:

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

(same notations as in the manuscript).

This equation is also added to the manuscript.

Referee's comment: *Regarding the sedimentation fluxes in equations (9) and (10), I cant see at a glance why the rain flux Pr shoud depend on both absolute Pr* and Ps*. Could you explain this?*

The relative precipitation flux P_r describes the movement of rain with respect to the mass center of the parcel, including *all* components. It may be easier to understand when writing the fluxes explicitly as a product of (vertical) velocity and specific humidity:

$$P_r^* = \rho_r w_r^* = \rho_{tot} q_r w_r^* \tag{2}$$

$$P_s^* = \rho_s w_s^* = \rho_{tot} q_s w_s^* \tag{3}$$

where ρ_r, ρ_s and ρ_{tot} are the rain density, snow density and total density, and w_r^* and w_s^* are the absolute velocities of rain and snow. If we now consider a parcel only consisting of dry air, rain and snow, the absolute velocity w^* of the center of mass of this parcel is equal to

$$w^* = w_r^* q_r + w_s^* q_s \tag{4}$$

The relative velocities of rain and snow then become

$$w_r = w_r^* - w^* \tag{5}$$

$$w_s = w_s^* - w^* \tag{6}$$

and the relative precipitation fluxes become

$$P_r = \rho_{tot} q_r w_r = \rho_{tot} q_r (w_r^* - w^*) = (1 - q_r) P_r^* - q_r P_s^* \tag{7}$$

$$P_s = \rho_{tot} q_s w_s = \rho_{tot} q_r (w_s^* - w^*) = (1 - q_s) P_s^* - q_s P_r^* \tag{8}$$

as given in the manuscript.

Some explanation is added to the manuscript to clarify the relationship between absolute and relative precipitation fluxes.

Referee's comment: *You also mention the relative flux of dry air to be defined as Pd=-sum(Pk) (Page 7 about lines 10). This is correct. What is about the flux of water vapour or other nonsedimenting species? It should have the same compensating velocity as dry air.*

The mentioned equation ($P_d = -\sum_k P_k$) only holds when the approximation is made that all mass transport by physical processes is compensated for by a fictitious flux of dry air. The fact that this is a rather artificial approximation is indeed insufficiently clear from the manuscript. The text has been modified to amend this.

Regarding water vapour and nonsedimenting species: under this approximation, they do not have the same vertical velocity as dry air; in fact, their vertical velocity is zero. In other words, the behavior of dry air is rather artificial (relatively large vertical velocity), but the description of the transport of the suspended water species is close to what happens in the atmosphere. It should be noted that this artificial behavior of dry air is restricted to the physics-dynamics interface: it does not affect the dynamical equations, nor does it affect the internals of the physics parameterizations.

Referee's comment: *In Section 4 it is mentioned that the surface boundary condition of AROME does not allow for mass exchange between soil and atmosphere. A consequence is then, that energy exchange associated with moisture and precip is also not possible? Do I understand this correctly? Then, with regard to the cold pool example you give later on in Figure 7, which consequences would this imply? Could you try to implement this boundary condition? An why should it not be possible to implement this boundary condition?*

This is a correct remark by the referee. The statement that mass exchange is not allowed is not entirely accurate. It should rather be that there is no *net* mass exchange. In other words, all mass exchange of water at the surface (evapotranspiration and precipitation) is compensated for by a fictitious exchange of dry air.

Therefore, water mass exchange is possible –and accounted for– in AROME, as are the thermodynamic effects of these processes.

The reason that it is quite difficult to implement this boundary condition in AROME is that the vertical coordinate of the AROME model is mass-based. Correctly accounting for a net mass exchange between atmosphere and surface would have far-reaching consequences, especially in the solution of the dynamical equations. We agree with the referee that this was not explained sufficiently in the manuscript, and some sentences are added to amend this. This issue is also mentioned in the conclusions section as an interesting future research topic.