Author's response to Referee #4

1 General comments

Referee's comment: *The authors claim that, thanks to the equations used in this interface "the conservation of mass and energy is a built-in feature of the system". I don't agree with this affirmation. I think the problem is much more complicated, and the best solution for the coupling depends a lot of the method which has been chosen inside the physics driver and inside the parametrisations themselves. I don't think this interface replaces a global solver which would be the only clean way to solve consistently together all the processes represented by the system of equations (2)-(8).*

Writing equations in a flux-based formulation is in fact a fairly common way to enforce conservation laws. Several references are given in the manuscript to show this. Could the referee please explain why this technique would not lead to a conserving system when applied in the field of physics-dynamics coupling?

Referee's comment: *I think that what is really missing in this paper is a proper discussion about the discretization of the complex system given by equations (2)-(8). The only example which is given in page 8, line 12 is actually not valid for any physics package. In section 3 below, I illustrate with a simple example extracted from the paper why I don't think that the conservation of mass and energy is necessarily a built-in feature of the system of equations (2)-(8).*

The referee is entirely correct to point out that the resulting equation of the example given in page 8 is not valid for a time-split (sequential) coupling case. The fact that this example concerns the case of a process-split (parallel) coupling should definitely be stated explicitly in the manuscript. In the case of a time-split coupling, the equations (2)-(8) [or (12)-(13)] are still valid, but the discretization looks somewhat different.

Let's resume the same example of two parameterizations (denoted (a) and (b)), but this time with a time-split coupling. Assuming that each process is energy-conserving in itself means that the change in enthalpy due the first process is given by

$$\Delta h^a = (c_p^t + \Delta c_p^a)(T^t + \Delta T^a) - c_p^t T^t \tag{1}$$

To determine the enthalpy change due the second process, we should account for the fact that in a time-split coupling, it doesn't start from c_p^t and T^t, but from $\tilde{c_p} = c_p^t + \Delta c_p^a$ and $\tilde{T} = T^t + \Delta T^a$. Therefore, the change in enthalpy due to the second process is given by

$$\Delta h^b = (\tilde{c_p} + \Delta c_p^b)(\tilde{T} + \Delta T^b) - \tilde{c_p}\tilde{T} \tag{2}$$

The combined effect of both parameterizations is then written as

$$\Delta h = \Delta h^a + \Delta h^b = c_p^{t+\Delta t} T^{t+\Delta t} - c_p^t T^t \tag{3}$$

from which the temperature at the next timestep is solved as

$$T^{t+\Delta t} = \frac{c_p^t T^t + \Delta h}{c_p^{t+\Delta t}} = T^t + \Delta T^a + \Delta T^b \tag{4}$$

i.e. the same result as given by the referee in the Annexe. This shows that, when applied correctly, our set of equations is also valid for time-split coupling.

We agree with the referee that some explanation about the application of the presented equations in the case of a time-split coupling is missing in the manuscript, and a section has been added on

this issue. However, we do not agree on the point that the conserving property of the equations does not hold in the case of a time-split coupling.

The remark of the referee about the atmospheric state not being adjusted after a process-split coupling is correct. However, this is a problem inherent to process-split coupling, not to the presented set of equations.

Referee's comment: *I don't think either that for local processes such as autoconversion or condensation, pseudofluxes are necessary to ensure conservation. The pseudofluxes are not necessary to express the system in a barycentric form. If a parametrisation has not been written in term of pseudofluxes but the parametrisation gives tendencies for the qx, how should the pseudofluxes be computed ? Should for example the method take into account the fact that the latent heat used in a parametrisation is $L(T)$ instead of $L(T_0)$?*

The concept of pseudofluxes is indeed insufficiently explained in the manuscript. The referee is correct when saying that it is not necessary to ensure conservation: if a physics parameterization is constructed properly, it should conserve mass and energy in itself. However, the pseudofluxes are a way to enforce conservation at a higher level, for example to make sure that energy is also conserved when several parameterizations are put together (see previous comment). Writing the combined effect of all parameterizations in a single flux-conservative equation is only possible through the concept of pseudofluxes.

The microphysical parameterizations of AROME are internally formulated in terms of specific humidity tendencies. The transformation of these tendencies to pseudofluxes is simply done by taking a vertical integral:

$$R_j = \int_0^p \frac{1}{g} \frac{\partial q_k}{\partial t} dp \tag{5}$$

This equation and method are added to the manuscript.

The transformation of tendencies into fluxes does not depend on whether $L(T)$ or $L(T_0)$ is used. It is important to keep in mind that the pseudofluxes only describe a mass exchange between different species. The parameterizations should not determine the thermodynamic effect of phase changes themselves (well, they can do it for internal purposes, but they should not pass this information to the dynamical core). Instead, they only determine the effect of the phase changes on the specific humidities (expressed through pseudofluxes), and equation (13) of the manuscript determines its thermodynamic effect.

Referee's comment: *The test case of section 4 shows that the main impact of the new interface has been to include a term which was missing (had been neglected ?) in the parametrisation of the precipitation sedimentation. From the text, it is not clear if adding this term in the parametrisation, but still using the old interface, would produce the same effect. Could the author try to separate more clearly the impact of the new design of the interface from the impact of the missing term ?*

It has been tested carefully what the origin is of the differences between results with the temperature-based interface and with the flux-based interface. This has led to the identification of the list of approximations that is given in section 3. The referee is correct when saying that the effect of heat transportation by precipitation could also be included in the temperature-based interface.

However, this is not the point we wish to make with this test case. The purpose of this case is merely to show that small terms in the energy budget, which can safely be neglected on a large scale, still can have a significant impact under specific circumstances. The use of the eqs. (12)-(13) avoids such approximations. This is stated clearly in the last paragraph of section 4.

Referee's comment: *I also think that the title of the paper is very misleading. The formulation of the Aladin dynamical core is not based on conservative flux-form equations. The new interface will surely not improve the non-conservative aspects of the Aladin semi-Lagrangian advection scheme for example. In the paper, conservative flux-form equations are used only to compute the tendencies from information provided by the physics parametrisations. This should be made clearer in the title and in the abstract.*

The first sentence of the abstract already defines the scope of this work as the "thermodynamic impact of physical parameterizations". However, we agree that it is quite important that the reader keeps this in mind. Therefore, the fact that only the effect physical parameterizations is considered, is added explicitly at several places in the manuscript: (i) in the abstract, (ii) in the introduction section, (iii) at the point where the equations are introduced, and (iv) in the conclusions.

Referee's comment: *I don't think there is a lot new information in the current state of the manuscript about physics/dynamics interface compared to Catry et al (2007) (the generalization to more water species and processes is quite straightforward). A more careful and general analysis of the physics/dynamics interfacing problem should be added to the manuscript to make it useful to the community. The results concerning the impact of the missing term in the sedimentation of precipitation are interesting but only more systematic sensitivity tests and comparison to observation would prove the importance of this effect versus many other sources of model errors for the simulation of organized convection in convection permitting NWP.*

We agree that the generalization is straightforward (from a mathematical point of view). However, this generalization is essential for the application in the AROME model, which is used for operations and/or research in 26 European and North-African countries. The application in AROME has led to the identification of some approximations that are present in the existing temperature-based interface of AROME. We believe that this is very valuable information for the users of this model. Moreover, our work provides a solution to get rid of these approximations, which is also of direct interest to these people.

A more complete investigation of the phenomenon discussed in the case study indeed would be interesting. This remark has been added to the manuscript. But as indicated in the manuscript and in reply to another comment, the phenomenon of heat transportation by precipitation is not the topic of the presented manuscript, but serves as an example of a case where the aforementioned approximations are not harmless.

2 More detailed comments

Referee's comment: *1. p 2, l3 : It is not correct to say that the dynamical core equations are equations written for a perfect gas. The equations of the Aladin dynamical core are written for a "barycentric" multiphase system which may contain condensed water phases, even if the physics is switched off. The water vapor but also the condensed species are taken into account to compute the mass of air in a given volume (e.g. liquid and solid species are "loading" the air parcel) and are then changing the "inertia" of the air parcel in the momentum equation. The gas law also knows about the composition of the air parcel, and only the "gas" part of the total mass is used in the gas law. But the full weight is used for the hydrostatic equilibrium. The information about the composition of the air parcels is known thanks to the definition of the specific water contents which are defined as the ratio between the mass of a given species and the total mass (including condensates). The specific quantities are used to compute the virtual temperature and the moist c_p (thermal inertia) and the moist gas constant R which are also used inside the dynamics.*

The statement that the dynamical core only considers a perfect gas has been removed from the manuscript.

Referee's comment: *2. p 2, l1-2 : Why only mass and enthalpy budgets but no momentum budget in this interface ?*

Exactly the same flux-conservative interface indeed could be used for momentum, as well as for other prognostic variables like TKE, passive tracers, etc. A sentence has been added to the manuscript to point this out. An important difference with water species, is that the evolution of water species affects the evolution of temperature in a complicated way, e.g. through latent heat effects or through the specific heat capacity. Therefore, the application of the presented methodology is much more important and interesting for the case of water species.

Referee's comment: *3. p2 l25 : I don't really understand this sentence. Shouldn't it be "to be described" instead of "to described"?*

Indeed, this has been adapted.

Referee's comment: *4. in section 2.1, the authors list the main hypotheses made for the design of Catry et al (2007) interface. However, nothing is said about the "Eulerian" and "vertical column" hypotheses used to write the system of equation (2)-(8) and how such a system should be interfaced with the dynamics. For example, in the case of a semi-Lagrangian dynamics, should the tendencies be computed at the beginning/middle/end of the semi-Lagrangian trajectories or along the trajectories?*

The hypothesis framework is indeed restricted to the hypotheses that relate to the thermodynamics of the model. This indeed should be stated more clearly in the manuscript.

The other hypotheses mentioned by the referee are more related to the general design of an atmospheric model. The use of the presented equations is rather independent of these hypotheses. For instance, the Green-Ostrogradsky theorem that forms the basis of the choice for a flux-conservative formulation, is also valid in 3 dimensions, so our work is not only relevant for physics parameterizations that are organized in vertical columns.

Referee's comment: *5. p3 l7-10 : It is not clear if the precipitation are supposed to immediately get the temperature of the layers they are crossing during a time step, or only the temperature of the layer where they "seat" at the end of the time step (in other word, is there an exchange of energy with all layers which are crossed by the condensed phases or only inside the layer where they stop at the end of the time step).*

Precipitation also takes the temperature of the layers in-between, and the heat exchange between precipitation and these layers is accounted for. This can be seen from the relevant term in the equation (13) of the original manuscript:

$$\frac{\partial}{\partial t}(c_p T) = -g\frac{\partial}{\partial p}\left[\ldots + \sum_k (c_k - c_p) P_k T\right] \tag{6}$$

The fact that the product of the precipitation fluxes (P_k) with temperature (T) occurs here, means that there's an energy transfer, even when precipitation just 'falls through' a layer: in such case the divergence of the precipitation flux is zero ($\partial P_k/\partial p = 0$), but the divergence of the heat flux is not necessarily zero ($\partial(P_k T)/\partial p \neq 0$).

Referee's comment: *6. p3 l8 : Bott (2008) → (Bott, 2008)*

This has been adapted.

Referee's comment: *7. p3 l 21 : it should be said more clearly that the q_x are specific fractions (i.e. ratios with respect to total mass of the multiphase system).*

This has been adapted.

Referee's comment: *8. p4 l8 : I understand that in a barycentric system, the total mass should be conserved, i.e the sum of eq 2-7 gives 0 = 0. Does it mean that the surface scheme should produce diffusive fluxes of condensates to compensate the diffusive flux of water vapor?*

Barycentrism doesn't mean that the total mass should be conserved. It means that the motions are considered relative to the (moving) center of mass of the parcel. That the right-hand sides of eqs. (2)-(7) cancel out, is due to the definition of specific humidities: $q_k = \rho_k/\rho_{tot}$. From this, it follows that $\sum_k q_k = 1$, so $\sum_k \partial q_k/\partial t = 0$.

As indicated in Catry *et al.* (2007), a change in total mass due to exchange with the surface, is

reflected in a change of the surface pressure.

Referee's comment: *9. p4 l20 : center of mass of what?*

The center of mass of the air parcel. This has been adapted in the manuscript, and a more elaborate explanation of the relation between relative and absolute precipitation fluxes has been added.

Referee's comment: *10. p7 l12-l15 : "all vertical transport is compensated by a flux of dry air" : I don't think it applies to "all vertical transport" (in particular, it does not applied to resolved vertical transport), but only to precipitation and subgrid mass transports (top of page 5 of Courtier et al, 1991).*

Indeed, this has been adapted.

Referee's comment: *11. p8 l7-12 : Formulae p8, l12 ensure conservation only if the parametrisations are called in parallel (process split in Williamson, 2002). It also supposes that the final specific ratios are given by process (a) and (b) (and not by a common resolution of both process (a) and (b)). If the parametrisations are called sequentially (time split), parametrisation (b) will already know about the evolution in time of both T and c_p after process (a). In this case, the conservation is ensure if $\Delta T = \Delta T^a + \Delta T^b$. See annexe for details.*

A correct remark. The fact that the mentioned formula only holds for process-split coupling has been added to the manuscript. The example also has been extended for the time-split case. (cfr. General comment)

Referee's comment: *12. p9, l31-32 and p10 l1-2 : The authors say that, in Arome, it is not possible to take into account the correct mass budget, therefore all vertical transport is compensated by a flux of dry air. Does it mean that equation 43 of Catry et al is used instead of equation (8) in the new interface ? Why the correct mass budget could not be taken into account in Arome ? Is the problem only at the surface or at every level ?*

In the system with 4 hydrometeors of Catry *et al.* (2007), it would indeed be their equation (43) that is used. In the generalized system presented in our manuscript, equation (13) is still valid if a modified relative mass flux of dry air $P_d = -\sum_{k=1}^{n} P_k$ is used.

The reason that this approximation is necessary is that the vertical coordinate of AROME is mass-based. Hence, correctly accounting for a net mass exchange between atmosphere and surface would have far-reaching consequences, especially for the surface boundary condition of the dynamical core. We agree with the referee that this was not explained sufficiently in the manuscript, and some sentences are added to amend this.

Referee's comment: *13. p10 l17 : I don't clearly understand why no significant improvement could be expected without a new tuning. Are the author thinking of model error compensation? If it is the case, it should be explained more clearly.*

Indeed, compensating errors are what we have in mind. This has been added to the manuscript.

Referee's comment: *14. section 4.2 : the discussion for this case study mainly consider the missing term (heat transport by the precipitation). If this term is the main problem in Arome, couldn't it be added to the old interface ? Would it give similar results ?*

It could be added to the old interface, and it has been verified that this gives similar results. But this is besides the point of the presented work. The strength of the presented framework lies exactly in the fact that one doesn't have to worry whether one term or another is accounted for in the physics-dynamics interface.

Referee's comment: *15. It would also be interesting to see the impact of the new interface independently from the addition of the missing term for the case in section 4.2 (i.e. also neglect the missing term in the new interface).*

As mentioned in response to the previous comment, the value of our work doesn't lie in discussing the impact of individual terms of the energy budget of the AROME model. This is interesting research in itself, but it is not the focus of this manuscript. The focus of this paper rather lies in the presentation of an overall framework that takes care of all terms in a consistent and conserving way.

In this sense, the case of the cold-pool formation under heavy precipitation is just an example where a clear impact of this framework can be noticed. A systematic study of the effect of heat transport of precipitation on the life-cycle of a cold-pool would be interesting in itself, but falls outside the scope of the present work. A sentence has been added to the conclusions to indicate this.

Referee's comment: *16. The author should be more modest in their conclusion. The simulation of such convective systems are very sensitive to many other source of model errors (time step, horizontal or vertical resolution, level of complexity in microphysics etc). A more systematic study would be necessary to really conclude about the importance of the heat transport by precipitation in Arome.*

We agree entirely that such a systematic study would be very interesting. But again, it is besides the point we try to make in this manuscript.