Geoscientific
Model Development
Discussions

Interactive comment on "The ALADIN System and its Canonical Model Configurations AROME CY41T1 and ALARO CY40T1" *by* Piet Termonia et al.

Anonymous Referee #1

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This paper aims to set a general terminology related to the limited area model (LAM) development, maintenance and scientific evaluation within the complex environment of the ALADIN international consortium. Two basic configurations are explained in detail serving as a reference canonical configuration. In addition they are also nicely illustrating the nearly infinite flexibility allowed by the common source code. This specific LAM version of the whole software is also influenced by the global model development at ECMWF and Meteo-France, being mostly based on the same code. This with the fact the ALADIN consortium is represented by 16 member states while mutually interfering with the other partners from HIRLAM consortium (and ECMWF) makes the work-flow and terminology around this software very challenging and bit cryptic for people not

directly involved.

The attempt to make this nomenclature more transparent plus offering quite detailed description of the two basic canonical configuration is naturally making this paper worth to be published. It will be certainly not just for a benefit to the ALADIN consortium members but can serve also as a very useful reference for the modellers from other communities. The usefulness of such summary paper could be also documented by the recent publication of Bengtsson et all documenting the HARMONIE system being a specific sub-group of the ALADIN software.

It is of no surprise that such an attempt is logically a result of several authors contribution. To serve however for the intended purpose the text needs some additional editorial work. Some parts are not very consistent. Some are duplicated and for some parts one just wonder why they are incorporated. The paper is relatively long. It would be thus of great benefit when the readability and its self consistence is improved. Apart from this the paper is certainly worth to be published. I have, however, a number of issues which I hope the authors can address.

General comments:

1/ The text on pages 20-21 (especially lines 544-574) is quite cryptic and inconsistent with the rest of the paper. Here rather than documenting the particular physics package it looks like some ideas or guidelines are presented. They are however hardly justified by some published evidence or experience from other NWP centers. The ACRANEB2 radiation is used without a word of introduction there. The explanation is jumping from microphysics to convection. The presented text is not self-explanatory: What is meant for example "modularity at the level of processes"? I suppose every microphysics scheme is in a way modular by describing separately process by process. Some extensive revision and shortening of this text resulting ideally in description on what has been actually implemented would be highly desirable. (Without too much arguing WHY: This should have been published already elsewhere.)

2/ The text has the ambition to serve as an reference (for the ALADIN and also external community). To help this purpose it would greatly improve the navigation when the schemes described in section 2.2 are highlighted (in bold or italic) near the area they are described. When they are mentioned later on in the text a general reader would easily search for them even working with printed text. (For example by highlighting the SSDFI at L276 you will improve the readability of the text at L777.) This could be seen as a poor man's glossary.

3/ Some part of the text looks like fillers. A reader may not see a clear reason for incorporating those into the paper. For example Figure 7 brings no specific extra information. It would make perhaps more sense if there is a comparison of the new and old Polish domains. But the same could be summarized easily without a figure. Especially when there is already Figure 6 illustrating roughly the same. Instead of complicated and sometimes very case specific results from newer model versions one would expect to find some highlight parameters of scores from global models (those used for boundary conditions), reference LAM version and new LAM version. This is clearly missing.

There are also plenty of references having no relevance to the paper. As the data assimilation is out of the scope of this paper, a reader may ask why there are so many papers referred to this subject? Some other references are definitely not the most appropriate to the presented subject. Please be honest and provide only relevant references to the presented text.

4/ On page 5-6 you describe a procedure of a new model version assembling. It is not clear however how the evolution of the global model code is interfering with this. How the decision about what is implemented at the level of the LAM code is taken? Or does it mean everything developed for global model automatically propagates to the LAM? What about some specific global model issues which are not relevant for the LAM community (like specific treatment of poles)? Do you have some general guidelines or those are solved on case to case basis? Is there some experience with the opposite

direction code propagation, like a code developed initially for the LAM model has been made available for the global model too? This perhaps rather particular question tries to reveal a bit more about this rather unique duality that the same code is used for LAM and global model communities.

Specific comments & Technical corrections:

1/ Purpose of the paper is given twice: p1/10 and once again p4/90-102. Could it be perhaps unified and reduced to just one list?

2/ Duplicated text describing the paper limitation: See p1/L15-6 and once again p4/L103-4. In this latter case the repetitive text brings just references to additional papers having no relevance to the described CMCs.

3/ p5/L132 text mentions a five-step process defining the scientific developments of the ALADIN System. Apparently there is no such description given in the paper (or it is well hidden). What this "five-step process" should be representing then? When it is given somewhere in the text please make it more explicit to be obvious without an extensive search.

4/ p7/L182-5 Could you explain what is the driving force for you to insist on long time steps? Is it the computational efficiency? Or do you have some specific scientific reason for it? (The computational efficiency doesn't need to be necessarily always justified by long time steps.) This claim feels bit like a dogma. But it is not clear for a reader why this is so important here.

5/ p7/L195 Is the best reference for the SI scheme really the given papers from Caluwaerts et al?

6/ p8/L215 Could you bit develop on this claim relating the 3km threshold and "important" role for the non-hydrostatic dynamics? First, it is not clear how this threshold is defined in terms of model: Are you referring here the grid point distance of the computational mesh, the shortest wave represented by the model or even a size of the

smallest fully resolved feature of the model? Second, please specify the "important role". Could you perhaps give some reference to clarify this claim? To the reviewer's knowledge there is no clear agreement on it. One can perhaps find some effects not simulated with hydrostatic dynamics at those scales. But this still doesn't justify the necessity to use non-hydrostatic dynamics there. One can argue that the non-hydrostatic schemes are only essential when it comes to the simulation of the convective effects. Here we are however referring scales bellow ∼100m of grid resolution. Finally, the role of "details of the used numerical scheme" is also not very clear here. Do you mean the true resolution given by the particular numerical scheme? Or something else is meant? To conclude: this sentence sounds like referring some common truth. If there is such an evidence, please provide some reference. Alternatively please make this statement less controversial.

7/ p8/L220 For VFE there is more fundamental reference of Untch and Hortal to be used rather than the one given in the text.

8/ p9/L256 When you give the diffusion order, you should also specify the resolution (and/or) truncation. Does it mean all presented configurations from Table 4 are running with this 4th order horizontal diffusion (including 18 km Aladin-NORAF and 1.3 km Arome France)?

9/ p11/L291-3002 It is nowhere specified how wide the relaxation area is. From the text at p27 it is apparent the number of coupling zone points is varying. How the given values of parameters p (L301) are modified with respect to the changing z? It is quite evident the optimal value of p must be related to the number of points in the coupling zone and model resolution. Can it be precise here?

10/ Several places like p12/L354 and p13/L363,364 are using term "dual parallelization". This is not at all very common term. Presumably it is meant mixed or hybrid MPI/OpenMPI parallelization? If so please change it to hybrid parallelization which is more commonly used name.

11/ p16/L448 The sensitivity of the scheme to the time step length has been... changes. This sentence brings no information to a general reader. Please either provide some details or drop it.

12/ p17/L487 This 15 minutes intermittency is used at every Arome configuration? Your example is given with Arome 1.3 km and 50s time-step. But there are some 2.5 km and 90s Arome configurations in the Table 4. Does it mean the 15 minutes remains fixed regardless the actual time-step length?

13/ p14-p19 (Arome CMC) At scales of 1.3km is certainly not negligible a contribution of horizontal mixing/turbulence. Please give some details about your representation of those highly non-liner horizontal effects.

14/ p20/L546-8 Separation of scales is not unnatural. I believe it is meant rather arbitrary. The separation of processes to dry and moist is equally unnatural/arbitrary, by the way. The text is not correct. There can't be such clear separation. This just says the microphysics is called twice in this case.

15/ p22/23 Could you specify the closure used for the turbulence scheme? Is it closed by a mixing length? And if so, which one?

16/ p23/L622-626 Rather strange text with a link to turbulence but then mentioning microphysics. What is the relevance of it? Does the microphysics influences the turbulence?

17/ p24/L654-5 This is rather strong claim. Could you perhaps give some reference or bring some more evidence supporting it?

18/ p22/24 Can you give some description for the microphysics and gravity wave drag parameterization? A reader may wonder what makes those two schemes so unattractive that the only information about them can be found in the table 3.

19/ The paper of Lopez(2002) being referred as the microphysics description is introducing only three prognostic variables: water vapour, cloud condensates and falling

precipitation. Is this really the case for the presently used microphysics? If not could you explain the choice of prognostic variables related to the microphysics in ALARO CMC?

20/ p25/L671: Missing "with" or "to"?

21/ p26/Table 4. Please specify the date of validity. The actual state could be evolving.

22/ p28/Fig 8 Are the curves based on annual verification of the two models? If so it is truly impressive, but better to say it more explicitly. In the other case please specify the verification period. It would be also useful to add the zero horizontal line (especially to the upper panel) in order to help the results interpretation.

23/ p32/Fig 11 The red dot is nearly invisible (especially when printed). Please use some better way to highlight it. This figure demonstrates the superiority of the newer version of ALARO over the operational one. Could you then add the operational results to illustrate it graphically?

24/ p33/Fig 12 Could you please zoom the figure to its lower third? It is really difficult to follow the presented the multiple lines of HR-alaro-88 and HR-alaro-HRDA.