Geoscientific

Model Development

Discussions

Open Access

[GMDD]

Interactive

comment

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

Piet Termonia et al.

piet.termonia@meteo.be

Received and published: 30 September 2017

Indeed, this paper documents a NWP system that is the result of more than 25 years of R&D activities, and it is quite a challenge to document this. Reviewer 1 is right that this paper became the result of many contributions from many authors. All of his/her comments are relevant and to the point. We took the opportunity to implement them in a revised manuscript and we believe it improved a lot by doing so and thank him/her for this. We also made an effort the remove some sections that are not relevant for the presentation.

Replies to the 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.)

Reply:

Indeed, this part is now rewritten, (see lines 575-629).

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.

Reply:

Good suggestion, in the revised manuscript we introduce subsubsections with titles that include the name of the schemes.

We have moved the paragraph that identifies the differentiation of the LAM specific features w.r.t. to the global model features, to improve the structure of the section (now to be found in lines 221-226).

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.

Reply:

Indeed, and section 4 is mostly problematic for this.

We have restructured the text of this section in two clearly distinct subsections: 4.1 Current status of the implementations and 4.2 Added value. By doing so we removed the repetitions in the text. Added value is addressed in 4.2 in terms increased realism and by few scores targeting extreme precipitation (comparing the LAM to the global models ARPEGE and ECMWF). It is impossible to give an overview of all of the verifications of the applications in all of the 16 ALADIN countries. We select here a only few cases: Météo-France, ZAMG, RMI and Croatia. We also removed the example of Poland, it is indeed a filler. The other examples are now functional in our opinion.

The ALADIN System is the result of code developments since 1990. So it is rather normal that there is a lot of literature. Also, it describes three physics packages of ALADIN, AROME and ALARO, which increases the cited literature. Nevertheless, we

have taken the opportunity to go through the citations and we removed the ones that we think are redundant, specifically the ones you question in your specific comments below.

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.

Reply:

First of all, this is not a new method, but this is the first time we describe the existing one in a publication. We have added a paragraph describing the general guidelines (called fundamental rules in the text), in lines 130-157.

Replies to the 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?

Reply:

Indeed it is given once in the abstract and once in the introduction. We have reworded the scope in the abstract (lines 11-14) and have removed the scope description from the introduction. It is a filler as you mention above.

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.

Reply:

Here also, the introduction elaborates what is announced in the abstract. We have moved the climate application of the CMC to section 4.3 (lines 848 - 850). Data assimilation is an important part of NWP so it should be mentioned at least. It comes now after the "This paper is organized as follows ..." part (lines 105-107). The references are removed removed from the introduction.

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.

Reply:

Indeed. In fact this paragraph is very general and we moved it to the consortium description in the introduction (lines 27 - 37). If you read it carefully it contains 5 steps. But this is irrelevant here, so we do not mentioned the five steps anymore.

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.

Reply:

The model has to be run on a large variety of computing platforms. This is now added in a footnote to the text on p. 7.

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

Reply:

The first one is not relevant. The second was meant as a review. We removed both of them.

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 âĹij100m 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.

Reply:

This is a fundamental discussion. It is not the goal of the paper to have that discussion here, but we describe the current practice with the consortium. We have reworded this, see lines 229 – 233.

We have also removed the reference to (1-5 km) in the introduction, it is not mentioned

in line 90 of the new manuscript.

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.

Reply:

Untch and Hortal 2004 are the implementations for the hydrostatic dynamical core. Vivoda and Smolikova developed a new VFE scheme for the NH dynamics. This is now reworded in lines 236 – 239.

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)?

Reply:

We actually have written that it is "usually" 4-th order.

This is indeed resolution dependent and may vary quite a lot among the various applications in table 5. Also some applications rely more on SLHD than on the Laplacian operator. A detailed description would lead us too far, so we remove this sentence about the 4th order from the text.

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?

Reply:

This is now explained (319 - 321). For the power p, ALADIN partners use the values from the standard namelists, i.e. the values mentioned in the paper.

10/ Several places like p12/L354 and p13/L363,364 are using term "dual paralleliza-tion". 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.

Reply:

Yes. We now use the wording "hybrid (MPI/OpenMPI) parallelization" to clarify that, see line 194 and line 379.

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.

Reply:

We drop the line.

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?

Reply:

Yes, indeed, this 15 minutes choice does not depend of the horizontal resolution nor time step of the model. In Table 4, 90 should refer to vertical levels, not time step. We change "full radiation computations are performed once every 15 min" by "in all AROME configurations (2.5 or 1.3 horizontal resolution) full radiation computations are performed once every 15 min. For intermediate time steps, only solar azimuth angle varies." (lines 515 -518).

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.

Reply:

Yes, indeed, at 1.3km scales, there is probably a not negligible contribution of horizontal mixing/turbulence, but due to diffusive processes (Semi-Lagrangian advection for instance), the 'effective' resolution of the model is far from being 1.3km (Ricard et al.,2013) and we can still use a 1-D turbulence scheme (Honnert et al., 2016).

Honnert, R.Âă: Representation of the grey zone of turbulence in the atmospheric boundary layer, Adv. Sci. Res., 13, 63-67, https://doi.org/10.5194/asr-13-63-2016, 2016. Ricard, D., Lac, C., Riette, S., Legrand, R. and Mary, A. (2013), Kinetic energy spectra characteristics of two convection-permitting limited-area models AROME and Meso-NH. Q.J.R. Meteorol. Soc., 139: 1327–1341. doi:10.1002/qj.2025

This discussion would lead us too far so we prefer to not change the text.

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.

Reply:

We agree. The separation issue has not been very well explained, thank you for pointing it out. We redrafted the relevant part of the text to make it clear we speak about diverging parameterization concepts. We also clarify there is a single (and not double) call to the microphysics, see lines 586 - 594.

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?

Reply:

We now provide an explanation: lines 669 – 681.

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?

Reply:

The paragraph is now rewritten: 690 – 694.

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

Reply:

This sentence is now rewritten in lines 723 – 726.

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.

Reply:

It is the Catry et al. 2008 GWD. This is now introduced in a bit more detail in the new version of the text (it is the same as the one for the ALADIN CMC), see lines 727 – 732.

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?

Reply:

This has been addressed in the new text that was provided to respond to your general comment 1, see line 626.

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

Reply:

Indeed. This is now corrected, line 755.

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

Reply:

Good point. A priori it is the date of submission, but it helps if it is added to the caption of the table. We also added it to the figure of the domains.

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.

Reply:

This part of the text was removed in reply to your above comment on fillers.

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?

Reply:

The red dots are now replaced by black dots.

The difference in scores between the 1.3 km and our current 4 km resolution operational version is minor. This figure illustrates the increase in realism when increasing the resolution. It would lead too far to provide a case study.

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.

Reply:

Indeed. This is now done.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-103/gmd-2017-103-AC1-
supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-103,
2017.