Planned set-up for the Brac-HR workshop (17-20/5/2010, Brač [near Split], Croatia)

The organising committee
7/4/2010

Pre-event steps:

All participants are urged to consider what they believe to be the major scientific issues to be faced for the forecast model in the coming years, in preparation for the workshop.
Each participant is invited to submit a two-pages-maximum position paper before May the 3rd. A similar possibility shall be offered to his/her colleagues, involved in the relevant topics but not attending the workshop. The practical way to organise the call for such additional contributions will be precised soon. A list of anticipated topics of discussion, added below, can be used as guideline for this exercise, but people may well go beyond it. Any scientist submitting such a paper gives thereby a personal opinion, or a critical evaluation, but the short notes shall be interpreted neither as official position papers nor as commitments to future workplans or collaborative actions.
Participants who are willing to deliver one of the keynote talks (see below) are requested to indicate this to the workshop organizers as soon as possible, ideally before April the 20th.

Five keynote speakers will be selected for day 1 of the workshop. The roadmap is that these talks should be based on: facts, the consolidated extracts from the preparatory papers and own opinion (but marked as such). There should also (like for the whole of the workshop) be a balance between lessons of the past, currently explored tracks and anticipations.

A draft will be prepared of the thematic parallel sessions of days 2 and 3 of the workshop.

Monday 17/5: two parts, both in plenary

a) Keynote speeches [each presentation (~30min) followed by discussion (~45min)]

Five keynote subjects:

- dynamical core developments – challenges in going to the (sub-)km scale, generic equations and possible discretisation approaches, horizontal/vertical vs. 3D, interplay between physics and dynamics
- physics parameterisations at the (sub-)km scale; cloud- and moisture related processes, turbulence and radiation; include aspects of limited predictability
- idem for surface modelling aspects
- Assimilation, initialization and nesting aspects - needs and possibilities for assimilation to constrain relevant processes, needs of DA for more advanced physics, possible use of DA to introduce uncertainty, initialization at high resolution (what is signal, what is noise?)
- present known weaknesses and ways to tackle them in the future; requirements for validation and verification tools at the km scale

b) In accordance with the outcome of part ‘a’ of the day, the draft plan for thematic parallel sessions will be updated and optimized. It should be noted that, as a rule, the split of sessions should not reflect the same partition as the keynote subjects, in order to facilitate the discussion of transversal or cross-cutting issues.

Tuesday 18/5 (full day) & Wednesday 19/5 (morning only): discussions in groups.

The subjects will be set up in function of the preparatory position papers, of the basic list of questions below and in order to allow both:

- division in sub-topics when needed;
- temporary merging of topical (or sub-topical) groups for treating specific common issues.

Let us recall that the prepared schedule will be amended as much as needed in function of the outcome of Day-1's discussions.

Wednesday 19/5 (afternoon):

- Excursion also meant as an occasion for spontaneous and free-grouping-type further discussions!

Thursday 20/5 (morning only):

- Plenary discussion on proposals from each topic. Draw up list of possible research topics, and issues to be considered in greater depth.

Draft list of questions to be addressed, as much as feasible, during the workshop

* Is there a specificity of LAM vs. global models in the march towards higher resolutions?
* What to do about the very tough issue of lateral coupling in operational NWP?
* More generally, is there a chance to see a consensus emerge on the best downscaling strategy? Or will empiricism continue to dominate here?
* Since there is a good chance that every topical solution for resolution-related problems will now be associated with a new problem, should increase in horizontal resolution continue to be presented as the main way out of 'parameterisation problems'?
* What are the links between the march towards higher resolution and data assimilation at scales where the slow manifold ceases to be constrained by geostrophism/non-hydrostatism? Is a strong increase (in relative terms) of the quantity and quality of the observations the only way to avoid being de facto restricted to longer effective time-space scales for the model's initial state?
* What about 'initialisation' at high resolution, where the distinction between signal and noise starts to be blurred (this also has to do with the previous issue and with the one five bullets down)?
* What will be the relative importance of EPS versus high resolution deterministic forecasts as resolution further increases?
* What is the NWP cost-effectiveness of enhancing the description of more and more physical processes, especially if their interplay with the core characteristics of the dynamical host models is more and more difficult to grasp and to control at the level of discretised equations?
* Concerning the stochastic aspects, what are going to be the relative weights of their internal (Self Organised Criticality) and external (Ensemble Prediction Systems) importance for the R&D concerning the modelling part of high-resolution NWP?

* What are the real ‘boundaries’ of the various ‘grey zones’ that we shall be confronted with? Is the question anyhow pertinent or should the search for multi-scale solutions become a standard priority?

* How are 3D aspects of the physical forcing going to influence the evolution of models? Shall they stay purely phenomenology-driven or is their algorithmic aspect going to be the dominating aspect as we start resolving turbulent motions in a 2D/3D relatively arbitrary mix?

* More generally, shall not we soon be forced to stop using the ‘absolute’ separation between ‘dynamics’ and ‘physics’ and consider as core issue the interplay between all forcings and the truly reversible part of the equations?

* How much additional prognostic character should we aim at for solving with the most possible consistency the problem touched in the four preceding bullets?

* How to evaluate and better control the interplay between micro-physical and macro-physical atmospheric representations?

* At the code level, which level of modularity should preferentially be sought (whole packages, integrated algorithms or individual processes)?

* Which medium-term strategy concerning the surface issues (oceanic, classical, urban, etc.)? Is not here the main problem the one of ‘software integration vs. generality of options’ than the one of ‘modularisation with respect to upper air computations’?

* Which of the above-mentioned issues might be beneficial in reducing the spread/uncertainties in climate projections?

* How does all the above interacts with the evolution of High Performance Computing?

* What are the necessary evolutions of the validation, assessment and verification tools needed to progress towards higher resolution?

* How to convert known present weaknesses in results into plans for as transversal as possible progress in the modelling part of NWP?

* What about more specific issues:

    - Characteristics of the generic equations (thin vs. thick atmosphere; conservation laws; compressibility and influence of the heat sources/sinks; …)? Should the latter aspect interact (or better not) with parameterisation considerations?
    - What about parameterisation terms in the budget of the vertical momentum component?
    - Choices for the time discretisation scheme?
    - Choices for the vertical functional representation and discretisation algorithms?
    - Choices for the horizontal functional representation and discretisation algorithms?
    - Problems of intermittency, cost-effectiveness and cloud scene representation in radiative computations?
    - How to choose the level of complexity of cloud-precipitation micro-physical schemes to avoid a diminishing return syndrome?
    - For sub-grid aspects, where to set the ‘model’ gap between organised and unorganised motions? How to ensure consistency of the related choices with the above-mentioned radiative and micro-physical considerations?
    - Choices for the various aspects of the closure assumptions for convection and for (moist) turbulence equations?
    - Which links between moist physics and momentum transport by all phenomena (resolved, vertically parameterised, 3D minus 1D)?
    - Choices for the representation of orography (resolved vs. subgrid; control of generated waves; …)?
    - Choices for the huge problem of the surface-atmosphere interplay (model and [specific] DA issues)?

Input to the workshop in BRAC 17 – 20 May 2010

By Bent H. Sass, Danish Meteorological Institute , Copenhagen, DK, - 26/4/2010

The present position paper will briefly address issues related to limitations of current approaches used to compute 'model physics' as the horizontal resolution increases. Additional remarks are included to mention that practical issues related to coding environment and the structure of collaboration may be very important in order to obtain a sufficient rate of modeling progress over the next 5 – 10 years

3D-effects in physics

When numerical solutions to problems in mathematics or physics are considered it is often a key issue to guarantee that the numerical approximation will converge to a true description as the grid size goes towards zero. Considering the current practice in operational meteorological models to 'compute physics in vertical columns' there seems to be a fundamental problem that the description due to this approximation becomes increasingly incorrect as the grid size is reduced.

After the introduction of a prognostic treatment of hydrometeors including precipitation species the deficiencies related to 'column physics' are linked mainly to radiation and turbulence where 3D-effects need to be taken into account as the grid size is reduced towards kilometer scale or sub-kilometer scale. For cloud/radiation parameterization at decreasing grid mesh Sass (in HIRLAM newsletter no. 55, part B, 2010, pp 23 – 27) highlights some key issues: It is concluded that the computed local surface energy balance can become seriously in error if the presence of cloud cover up to a considerable distance (~50 km) are not accounted for in solar radiation computations. The problems are most severe when deep convective clouds are involved.

Considering the convergence issue in the atmosphere one may consider a resolved cloud on the grid scale with small vertical velocity. The atmosphere is assumed cloud free outside the vertical cloud covering a depth of , say 500m – 1000 m. For a typical case the column physics will compute an infrared cooling rate in the lower half of the cloud close to zero whereas a correct computation including 3D-effects will reveal that the net energy budget for small grid sizes of , say 100m -200m is dominated by significant cooling from radiation at the sides of the cloud. This extreme example is given just to illustrate the lack of convergence of column physics to a true description.

As a consequence of the considerations above it is suggested that a dedicated research group working with IFS-environment is established to

a) Investigate the deficiencies of the column physics approximation in various meteorological conditions by comparing present forecast approaches with more accurate reference computations including 3D-effects

b) Investigate economic computational approaches for approximate treatment of 3D-effects in operational meteorological models

c) Investigate technical issues related to exchange of information outside vertical columns in the physics computations. Which methods are most economical and promising ?

Other issues

Even though the main focus of the present workshop is on scientific issues it seems appropriate to pay attention to issues which might be critically important to improve the rate of progress in modeling over the coming 5 – 10 years. One question to ask is what is the possible role of code user friendliness ? especially to newcomers starting to work with the IFS environment ? This question is also related to the creation and maintenance of adequate code setup for the academic community. Normally there are fewer people working on technical issues (code architecture, and optimization) than people working on more traditional model development. It may be necessary to increase staff working with technical issues in the coming years. Finally it is important to consider the way to collaborate. How are the resources best spent on collaboration if we want maximum quality of the meteorological model systems ?

Aspects and links between upper-air data assimilation and the Brač discussions

C. Fischer
04/05/2010

This note mostly intends to list some links between the challenges in high resolution modelling (dynamics & physics for the atmospheric flow numerical simulation in the range [500m-3km], surface) and upper air data assimilation thematics. The note is NOT addressing the core issues for upper air assimilation like: which methods, workplans, etc. My note expresses a personal opinion, and should be interpreted neither as an official position paper nor as a commitment to future workplans or collaborative actions. There may actually even be some errors in it ... for which I apologize in advance !

- analysis methods (in the scope of an optimal control): the most prominent methods to control the grid representation of the atmospheric state are variational analysis (using adjoint tools and a minimization software), optimal interpolation (using some low rank simplification and direct inversion of a « gain » or a covariance-like matrix), sequential filters of various forms (using so-called square-root filters, ensemble forecasts, Transform matrices, ...). Despite the deep lying assumption of dealing with a probabilistic problem, it is obviously of importance that *the deterministic model is of the best possible quality especially with respect to those fields that have the most straightforward link with observables* (=the simplest observation operators): wind, temperature, total pressure, relative humidity at first place. Further interesting fields already require quite less trivial obs operators: total liquid water content, vertically integrated water vapour content, cloud cover. Another aspect here is that, *the smaller the model error biases, the more performing the assimilation algorithms*, since the latter are built on the assumption that errors are unbiased (=that error biases have been removed). In variational assimilation, the deterministic model basically provides the fields around which linear and adjoint operators are differentiated (the « trajectory »). Thus, *the quality of the gradient in principle increases when the direct model is improved*. In the specific case of 4D-VAR, the forecast model itself in principle should have its TL/AD counterparts. This includes both the dynamical kernel and the physics parametrizations. Two major matters of concern arise here: (1) *if tests and deeper changes in the dynamical kernel are expected in the mid-term (in IFS/AAAH)*, then there is little evidence to me that the TL/AD codes could be developed and maintained at the same speed; (2) for physics, non-linear processes of various nature would require specific attention. In the frame of a [5-10 year] prospective, and within the IFS/AAAH community, such ambitious goals are highly questionable to me in terms of manpower and cost of the system. This leaves basically only room for an « engineer's compromise » based on a hydrostatic, lower resolution TL/AD model[1]. Alternatives to 4D-VAR exist as well, in the form of ensemble-based assimilation, hybrid assimilation (3D-VAR+EnsKF, MLEF, ...).
- background information and model error information:
    - a background information is necessary in data assimilation, in order to ensure the existence of a solution to the inversion problem, considering that the total observational network cannot provide a complete and accurate representation of the atmospheric state in a 3D model grid. Ensembles of 6 to 100 members seem to provide nowadays the most promising approach to evaluate the statistical information for the B-matrix. I hypothetize that this approach will continue to be the mainstream for R&D and applications in the mid-term, possibly with a stronger link

---

[1] At the 2.5 km resolution, there seems to be evidence that many parts of the flow remain close in hydrostatic and NH simulations. Only where vertical accelerations are substantial do both solutions differ significantly. Such statement certainly would require more theoretical or experimental support if it should serve as basis for any type of « hybrid incremental » 4D-VAR.

  with EPS (works by Houtekamer and others in Canada; ECMWF; Raynaud, Desroziers and Berre at MF). This means that quality but also *numerical performance (eg. Scalability)* of the forecast model will be valuable for R&D and applications in DA.
  - Model error for the short term forecast error covariances can become a matter of interest, when seen from the perspective of numerical modelling. Indeed, besides indirect techniques (a posteriori evaluation, perturbing the physics empirically), some aspects of probabilistic measure or stochastic ingredients in the parametrizations may provide more realistic modelling of model uncertainty with a flow dependent flavor. For instance, setting on convection in some sense « randomly » in areas where CAPE>0, and sampling over several ensemble members, may provide a valuable estimate of location error. Some diagnostics and « perturbations » targetting known weaknesses in the model could be valuable as well (presently, multi-physics seem to be the most efficient way to take into account the weaknesses in individual formulations of parametrizations: can one do better, that is, more refined ?). *These considerations might even be extended to the whole forecast model, including its dynamics, if dynamics are found to impact as much the quality of a high resolution model as physics*.
- cycling, initialization:
  - If leaving aside questions about boundary conditions, my view so far is that the experimental evidence that a high resolution model with its assimilation system can be run in a cycled mode, with a beneficial impact in terms of meteorological performance, means that the forecast model is of a similar quality to the observations used in its assimilation and verification (this is particularly true in the range of cycling [1h-6h], but also beyond). Conversely, if cycling seems to be less beneficial than dynamical adaptation or discontinuous assimilation, then this *may* point to some more severe forecast model weakness.
  - High frequency cycling raises the issue of *spin up* in the model. A proper way of controlling spin up would in principle be to make sure that the analysis increment lies on the slow manifold of the model. The latter however is not clearly defined at high resolution, neither in terms of spatial nor temporal scales. There is evidence in litterature that strong convection is associated with gravity wave generation; the speed of displacement of individual cells can be rapid; convective systems can have multiple scales of organisation, with a mixture of developing/mature/decaying cells. *There may well be no tractable way to define a projection operator onto the attractor of the problem*. As surrogates, so far, one may envisage using digital filters with very high cut-off frequencies (equivalent to only a few model time steps, i.e. Not more than a few minutes) or concentrate the work for removing spurious spin up by backtracing its origin in the dynamics/physics/surface aspects of the model.
- Coupling: in the sense of nested systems, I do believe that one should in principle assess whether the very large scales, not completely resolved in the nested model, could be adjusted with information from a larger scale system. There certainly are several ways how to pose this problem (weak constraint, forcing scales, interpolating, ...). *Large scales may impact almost immediatly a short range forecast locally*, if the adjustment is to modify vertical stability or a large scale wind field for instance. On the opposite, my view is that the boundary conditions around the gridpoint lateral boundaries would have a smaller impact, because of the time it would take the associated error to propagate (at an inflow boundary). A natural reflex seems to be to push the boundaries acceptably far away from the inner domain of interest.
- observations: *model physics would become part of observation operators dealing with rain or cloud-affected data*, like some IR and microwave radiances, but also radar reflectivity. Being able to develop and to design a system where pieces of the software can be used both

in the forecast model and in the observation operator would then be important for a direct assimilation (one example is the all-sky radiance assimilation in the IFS). Yet, it is totally open whether direct assimilation should be seriously tackled for high resolution, for instance for reflectivity. Other possible valuable sources of information could come from the new humidity sounders onboard civil aviation aircraft, extended usage of GPS, denser collection of surface stations (road). One « fuzzy » topic: can windmills provide an information about the wind that steers them ?

Major Scientific Issues for km-scale NWP Models

D. Majewski, Deutscher Wetterdienst (DWD) for the COSMO Consortium

April 2010

1. Introduction

DWD operates the non-hydrostatic, convective-scale model COSMO-DE at a 2.8 km grid spacing since April 2007. The model provides 21-h forecasts eight times per day with a very short data cut-off of 30 minutes past 00, 03, …, 18 and 21 UTC. High-resolution radar data from 16 German plus a number of European sites are inserted into COSMO-DE via a latent heat nudging approach.

The scientific issues for km-scale modelling outlined in this position paper are mainly based on this three-year operational experience with convective-scale modelling at DWD. A broader overview of scientific issues is given in the COSMO Science Plan which was distributed to key scientists of EUMETNET-SRNWP in April 2010 for a review.

2. Dynamics

In the field of dynamical cores of km-scale models we foresee three (four) main scientific issues, namely

a) Stable and accurate treatment of steep slopes,
b) Accurate and efficient transport schemes for tracers, e.g. humidity variables and aerosols,
c) Conservation of (at least) mass and tracers, and
d) Scalability on massively parallel computers with more than 100,000 cores and heterogeneous architectures, e.g. accelerators like GPUs.

a) The usual terrain-following approaches like Gal-Chen coordinates give rise to metric terms in the prognostic equations. If discretized explicitly these terms may lead to instabilities for steep terrain slopes. An implicit treatment which increases the stability of the scheme requires the costly solution of fully 3D elliptic equations. These solvers usually do not scale very well on massively parallel computers if they require global communication. Alternative approaches like shaved cells or unstructured grids as used in CFD models should be evaluated.

b) Transport schemes for tracers will dominate the total cost of dynamical cores in the future if the number of tracers will increase considerably, e.g. because of two-moment cloud microphysics, aerosols and chemistry modelling. At the convective-scale the local accuracy of the transport scheme for the moisture variables as well as conservation issues play a major role for the proper simulation of the life cycle of deep convection and realistic precipitation forecasts. There are two different transport schemes available in COSMO-DE, a fully 3D semi-Lagrangian scheme with a 2nd order accurate backward trajectory and tri-cubic spatial interpolation, and a 3D extension of the Bott (1989) scheme in a mass-consistent manner. Since the SL-scheme is hampered by spurious excessive precipitation maxima, the modified Bott scheme is used operationally.

c) Conservation of mass and tracers plays a crucial role in the future because convective-scale precipitation forecasts are very sensitive to fictitious sources and sinks of moisture. Moreover, conservation is an important issue for regional climate models which will be based on regional NWP models.

d) Future peta- and exascale computer systems will consist of 100,000 to a million cores which might include additional accelerators like GPUs, too. Thus scalability of km-scale models on such heterogeneous systems will be an important requirement in the near future.

2. Physics

In the field of physical parameterizations we foresee five (six) main issues for km-scale models, namely

a) Consistent treatment of turbulence (especially in the PBL) and shallow convection,

b) Detailed cloud microphysics, e.g. two-moment schemes and the incorporation of cloud-aerosol effects, and an improved cloud – radiation coupling,
c) Consideration of 3D aspects, especially in radiation and turbulence,
d) Proper treatment of surface/soil heterogeneity, and
e) Physics – dynamics coupling taking care of faster and slower adaptation time scales.
f) Stochastic physics, especially with a view to EPS applications.

a) Convective-scale NWP models have to predict the initiation of deep convection explicitly which requires a proper interplay between turbulence in the PBL and shallow convection. Moreover, the number of model layers in the PBL must be high enough for the detailed simulation of the diurnal cycle of temperature, moisture and wind profiles in the PBL. A parameterization like UTCS (Unified Turbulence and shallow Convection Scheme, A COSMO Priority Project) which combines turbulence and convection into a single scheme seems to be promising.

b) Cloud microphysics of convective storms is much more complicated than in stratiform clouds because e.g. the formation of cold pools which trigger new convective developments depend to a large extent on the evaporative cooling in the downdrafts of existing cells. In COSMO-DE, a two-moment scheme is available as alternative to the operational Lin-type one-moment scheme. The application of a two-moment scheme is mandatory for the future inclusion of aerosol effects in cloud microphysics.

c) The incorporation of 3D effects in physical parameterizations of convective-scale models is currently at an early stage. Turbulence and radiation are the two obvious first candidates for 3D approaches. Since the computational cost of fully 3D schemes might be prohibitive for operational applications, some simplified approaches taking only the leading 3D effects into account might be necessary.

d) The lower boundary condition which is provided by the soil model and its components like vegetation, snow or lakes is crucial for e.g. the initiation of deep convection or the detailed prediction of the diurnal cycle of temperature, humidity and wind. Proper modelling of the heat, moisture and momentum fluxes requires detailed, high resolution data sets of external parameters like soil type, albedo, vegetation parameters, too.

e) Convective-scale models are quite sensitive to the physics – dynamics coupling, especially if the dynamics time step becomes large with respect to intrinsic physical adaptation time scales. Our experience is that fast processes like cloud microphysics and condensation should be calculated at the end of a forecast step while slower processes (like radiation) might be calculated less frequently and kept constant during one integration step.

f) Convective-scale ensemble prediction systems (EPS) will be an important application in the future. To include the model uncertainty “stochastic physics” for convective-scale models should be explored.

3. Additional requirements

Here we list some additional requirements for convective-scale modelling which we like to mention for completeness.

a) Based on the convective-scale model, a regional-scale version for grid spacings between 5 and 30 km including a parameterization of deep convection must be available, too, to serve as an interface between global models and the convective-scale model.
b) Both model versions (regional-scale and convective-scale) should be available for regional climate studies, too, because regional adaptation to global climate change will receive a lot of attention and funding in the future. Moreover, NWP and climate applications will benefit from each other.
c) A model version which treats at least aerosols and reactive trace gases will be an advantage as it allows the simulation of e.g. volcanic ash, dust storms or pollen transport.

d) A proper balance must be sought between flexibility, e.g. different options in the dynamical core or physical packages of different complexity, code maintenance and efficiency.
e) Portability of the model to different types of computer platforms, e.g. vector and scalar systems, must be always guaranteed.
f) The model code must be freely available to universities and research institutes worldwide, easy to install and well documented.
g) Data sets, e.g. initial and lateral boundary fields, must be freely available to universities and research institutes.
h) It will be an advantage if the convective-scale model shares as many physical parameterizations as possible with the driving larger scale model as this reduces the width of the physical adaptation zone, allows the application of the model to any region worldwide and eases the testing of new components.
i) Diagnostics, verification and validation of convective-scale forecasts still need a lot of research and development.

SCIENTIFIC ISSUES for NWP models a (sub)kilometric scale

CHRISTOPH WITTMANN and YONG WANG
Central Institute for Meteorology and Geophysics, Vienna

- **Predictability of Convection:** High-Resolution models (kilometric scale) are already able to create realistic scenarios for convective activity over a given domain. But considering e.g. a topographically highly structured and complex terrain, the result of a deterministic model will stay just one single possible realization for initialization, development, secondary triggering, etc. of convective activity during e.g. a typical summer day. Considering all the limiting factors (orography, surface characteristics, atmospheric stability, representation of (micro)physical processes in the model, etc) for a perfect localization of connective activity over a given domain it is important to determine the limit of predictability for convection for the (sub)kilometric scale. To forecast the correct triggering time for convection should be the primary interest. It can be expected that the high resolution EPS will play a crucial role to give estimates for convective scenarios to be the most likely ones to happen (see next point).

- **Importance of EPS:** One may expect that probabilistic approaches will play a major role to guarantee useful additional information for users receiving high resolution forecasts. Considering the problem of computing resources, approaches addressing the uncertainty of deterministic forecasts (especially on (sub)kilometric scales) within a single model run ("probabilistic parameterizations") producing additional probabilistic output may be a topic for scientific planning (see also point "Further requirements"). The usage of "stochastic physics" within LAMEPS should be further explored.

- **Representation of (near) surface in connection with assimilation:** The representation of surface (and the related processes) is one major source for errors for near surface forecasts (snow cover, vegetation, soil types, cities, etc). E.g. the existence/missing of snow cover in an Alpine valley can create temperature errors with magnitudes up to 10-15 degrees. This behavior is not easily explained to users of the model forecasts.
  A better representation of surface characteristics and a better determination of the relevant surface fields (in assimilation) should play a major role in scientific planning.

- **Spectral vs. Gridpoint:** Considering the advantages/disadvantages of spectral/grid-grid point representation in a (LAM) model the future method should be clear for the subkilometric scale.

- **Assimilation (Time aspect):** Techniques for assimilation and for the introduction of new observation types are evolving and leading to more complex assimilation systems. The further developments related to assimilation should be also directed in a way to minimize the time loss (analysis time -> availability of forecasts) to maximize usability for high resolution models running with high frequency (runs per day).

- **Verification:** The development for verification methods to examine the quality of forecasts coming from high-resolution model should be addressed with similar effort as other scientific questions. Running (sub)kilometric scale models makes less sense when there is no adequate way to show the benefit to users. There are already some useful verification tools available or under development (e.g. precipitation). One should also address less “popular” model output fields with new verification methods (e.g. cloudiness).

- **Further requirements:** The needs and requirements coming from customer sides (warnings, risk management, etc) show that there is high interest for high-resolution modeling. Ideas for further scientific planning should take into account aspects raised by “downstream” users (e.g. topics like hail potential, lightening hazards, pollen, hydrological aspects, etc)

Contribution of Filip Vana

For the moment I have only two short rather technical points which I suppose should be also considered for the forthcoming meeting.

Ascending compatibility

It is known fact that increased sophistication of a numerical scheme usually means also increased numerical cost due to more complicated scheme evaluation. It is less known that in order to benefit from a higher sophistication one has to usually provide it with sufficient (spatial or temporal) resolution. Any increase of sophistication thus generally means double increase of model cost: through the more complicated scheme and in the adequate increase of model resolution. The aim then should be to find a balance gaining the most benefits for a given available computing resources.

To me the important point for any future oriented visions is also to aim some compatibility with the existing scales already resolved by the current NWP systems. From this point of view any solution making some distinctive line between "scales of interest" to which a model is only guaranteed to deliver meaningful results and the "other scales" which should be rather avoided is hardly viable concept for NWP. This is even more supported by the fact that the "borders" of the "scales of interest" are usually not well defined and might be a situation dependent.

Code organization

The current initiative of the ECMWF is to redesign the code in the Object-Oriented (OO) manner. This concept is very welcomed for any future code development and maintenance. On the other hand it triggers some questions about the computing efficiency of the future system. It is naive to consider that there's a way to have fully OO system (i.e. allowing all the advantages of OO programming) which would posses by the same computing efficiency as the current model. It is quite evident that a compromise solution between the highest possible code efficiency and a full OO code has to be defined.

The first prototype of OOPS seems to be logically more in favor of the computational efficiency. However it is natural to imagine that with evolving time the OO features would penetrate deeper in the code allowing much easier model development while leading to compromises in terms of computing efficiency. Although I am very much in favor of this evolution I think there should be cleanly defined some policy for the maintenance of some minimal acceptable code efficiency. I feel an increased need to support the efficiency aspects to be equally considered with respect to the other (scientific, technical,...) attitudes. The same policy should be also applied to any external software coming from outside of the model community.

SCIENTIFIC ISSUES FOR KM-SCALE NWP MODELS

JAN BARKMEIJER - KNMI

(a) During the recent ET-EPS (SRNWP) meeting (March 2010) representatives of all NWP consortia active in Europe agreed unanimously that the current knowledge of predictability with respect to convection permitting models is fragmentary. This certainly applies to model components like microphysics, which appear to be very sensitive to modifications, but also to data-assimilation components (e.g., impact of model error on analysis increments). Close cooperation between the model and data-assimilation communities (both upper air and surface schemes) was regarded as mandatory. The same point of view emerged during the combined annual HIRLAM and ALADIN workshop (April 2010)

(b) In anticipation of the development of a convective-scale EPS (Perhaps the only viable approach in providing km-scale forecasts?), tools should be explored, which may help to access and describe analysis/model uncertainty. Examples of such tools are singular vectors (role of non-modal instability mechanisms), stochastic tendency perturbations (formerly referred to as 'stochastic physics'), stochastic model components (clouds, convection).

(c) A module, which handles the dynamics of aerosols/tracers would be welcome (Iceland, health risk warnings)

(d) Km-scale models should benefit as much as possible from advances in the use of GNU's and massively parallel mainframes. This may have consequences for the model architecture (Is there still a future for spectral models?).

(e) Making progress in the development of km-scale NWP models (Which model produces realistic precipitation forecasts?) requires specialised verification packages and extensive observation sets. Hopefully, in this way a workable balance between model flexibility and options in physical modules can be maintained.

(f) It is important that academia have free access to the model source code, tools to quickly browse through the code and detailed documentation. Otherwise, maintaining some level of cooperation with research groups on improving meteorological services' operational models will be hard to sustain. Also the model should be easily portable to various computer platforms.

An attempt at diagnosing current trends in the physical parameterisation trade for higher resolution modelling (Brac-NH workshop position paper)

J.-F. Geleyn (also on the basis of much work done in common with N. Pristov, J.-M. Piriou, D. Mironov, L. Gerard, J.-I. Yano, F. Vana, L. Bengtsson, F. Bouyssel, R. Brozkova, B. Catry, I. Bastak, T. Kral, D. Banciu, P. Bechtold, R. Fournier and others [in the ALARO and COST ES0905 teams])

This document must be considered as an individual contribution, less balanced in its conclusions (but using the same analyses, of course) than the key-note lecture that I shall deliver on the same topic, on the first day of the workshop.

The heavy tendencies that I detect in the current evolution of the parameterisation trade are:

- more and more separation between handling basic physical processes (radiation, micro-physics of clouds and precipitations & local turbulence) on the one hand and describing (semi-)organised-plus advective transport of many items on the other hand;
- an increased concern for a clean definition in terms of "which parameterisation does what" on the continuous scale going from purely random turbulence to fully organised precipitating convection;
- the search for solutions that should be, as much as feasible, scale independent in their results and even, if possible, in their justifications;
- the growing awareness that just betting on increases in resolution and on more and more complex descriptions of basic processes will not deliver a stable and reliable solution to all problems associated with the challenge of NWP at high resolution;
- the emergence of an answer to the dilemma just above on the basis of three non-traditional items: (a) special care for the handling of 'lateral' (mesh to mesh) aspects; (b) reliance on a vast increase in the number of prognostic variables; (c) inclusion of stochastic considerations in the framework of each individual forecast (still 'deterministic' in the old meaning of the term).

Even if the trends are perhaps not as heavy as the above-mentioned ones, considerations concerning the logistic of parameterisation computations are in my opinion also evolving:

- The search for interoperability through modularity seems to simultaneously quit the two extremes of "plug compatibility" for full packages and of "processes' itemisation" at the lowest level of detail. It rather tries to identify comparable entities between various solutions/proposals at an intermediate level. Whether this evolution shall yield positive results remains open.
- The unexpected novelty when reaching the so-called 'convection permitting' scales is probably not the mitigated degree of success of 'resolved-type' solutions, but rather the importance of two devil's details. Those are namely, (i) the dynamics-physics interplay within the time step (and not only for the pure thermodynamics or for the sole vertical aspects, as shown in the study of Piotrowski et al.) and, (ii) the details of the representation of multiphasic flows. Even if there is a strong resistance to translating such facts into more advanced interfacing algorithms, future developments will have to care if one wishes to improve all other aspects on a sound basis.
- The tendency to increase the number of prognostic (and even 'historic') variables requires some evolution of the environment of physical parameterisations. Here the constraints originating from the dynamical core and from the code structure cannot always be put aside. In the future, the design of physical packages (or at least of their interfaces) will be more influenced by such constraints than by internal considerations.

Going now in some details for each 'classical' parameterisation topic, I'l try to evaluate and/or foresee a few specificities, not fully covered by the above general considerations:

- For basic physical processes like radiation and micro-physics, two opposite trends are at work. (I) correctly simulating the interplay between micro- and macro-physical aspects (i.e. what the model 'dynamics' will retain from the hyper-detailed forcing) requires relatively cheap algorithmic solutions, something partly at odds with evolutions in the climate-simulation trade. (II) the geometry of the intra- and inter-grid-point cloud scene is steadily gaining in importance and this fosters growing computational costs. My guess is that, at least for high resolution NWP, the answer to this contradiction can only come from solutions with a two-level-of-complexity solution for intermittent and/or scale-filtered calculations. Additionally, one will have to avoid entering the area of diminishing return for additional complexity in the processes' description.

- It is generally thought that the need to parameterise sub-grid mountain effects is 'behind us'. However we are not sure that partly 'resolved' partly 'cut' gravity waves will not saturate the dynamical-physical interplay in areas of strong orographic forcing. I believe that one should take more care not to leave here a void space between resolved and parameterised motions.
- Something similar is likely to be happening in the absence of any parameterisation of actively precipitating convection. Even when the main effect is captured by the 'resolved' part of the algorithm (dynamics + mesh-wide activation of microphysical calculations), fluctuations are likely to try and develop around this 'basic state' and, if allowed to influence the large scale flow, their impact might be higher than that of non-precipitating forms of convection. When the latter is allowed in the model and the former forbidden, it is difficult to foresee how and how much the true signal is distorted, but this surely happens, and of course with negative consequences.
- The main basic difficulty when one therefore tries to keep describing manifestations of precipitating convection at scales beyond the 'grey zone' is of course the constraint of installing return currents for the drafts within the considered grid-bow while their equivalents in nature do act at far longer distance. The concept of separation between microphysics and transport (MT), used in the 3MT ensemble of schemes, brings in my opinion the most appropriate answer to that question. There is now even a proposal (FP-MT, with FP for 'fully prognostic') to extend the idea to something like a destructured super-parameterisation set-up, in order to reduce the closure problems of all kinds of organised sub-grid transport to the prescription of two-by-two entrainment-detrainment contributions. This quite attractive longer-term and ambitious avenue will probably generate all sorts of unforeseen practical implementation problems, but it should surely not be discarded on that ground.
- The area of basic turbulence computations is a bit apart in this set of considerations. One may indeed consider here that the evolution of upstream concepts has been very rapid in the past years (recognition of the 'No Ri(cr)' fact, of the importance of the 'conversion' TKE ⇔ TPE at all stabilities and of the role of anisotropy, as well as emergence of the QNSE alternative to 'Reynolds averaging' methods). So the main issue is, in my eyes, how quick and how well NWP parameterisations will digest this new corpus of science and make positive use of it, in relation with all their other constraints. Paradoxically, it is for high stability situations that the success or failure of this enterprise will best be judged, even if it is where the related fluxes are smallest.
- Somewhere between the last two bullets, we are left with the mixed bag of issues often put under the generic name of 'shallow convection'. Here the situation is far less clear-cut. If one leaves aside the very expensive recast of all turbulent motions in a mass-flux-type formalism, two methods are competing for best describing the juxtaposition of random and quasi-organised turbulent motions. Either the 'classical downgradient' turbulent framework is left just to treat fluxes that cannot be reproduced by a mass-flux system similar to the one of deep convection, or this framework is consistently extended to capture the non-local character of developed turbulence. I'm personally betting on the second approach, on the ground that the interaction with moist phenomena is more logically parameterised there, but this argument is not a too strong one.

I hope to have, at least partly, convinced the reader that we dearly need changes of paradigms when preparing the evolution of the current parameterisation packages. We are not anymore at scales where any increase of resolution and some retuning work, maybe common with climate-simulation experts, was a guarantee of progress. But we are, by far, not in the situation to merely plug a LES local set of algorithms in a NWP-oriented dynamical core set-up. The associated challenges are rather frightening and, for me, should be addressed in a spirit of both wide exploration of potential research-type solutions and clear NWP orientation of (pre-)operational concretisations.

Scientific issues to be faced with in the forecast model during the coming years

Lisa Bengtsson-Sedlar, 2010-04-19

Stochastic component and communication between grid-boxes: It is desired to address uncertainties which are arising from sub-grid variability of deep convection, both within the deterministic forecast model, and within Ensemble Prediction Systems (EPS) developed based on the deterministic model. This can be addressed using a stochastic component, used in order to “stir things up”.

Another way to impose stochastic physics is to address the spatial and temporal scales of atmospheric motions underlying these uncertainties, such as deep convection. One idea is to use a self-organizing system, based on a cellular automaton, in order to generate stochastic “patterns” on the scales of deep convection, and in a dynamical sense insert stochastic physics on scales larger than the truncation scales. Another advantage of a cellular automaton is that it can help to organize convection in a way which is a challenge in “column based” parameterisations, by communicating across model grid-boxes.

A challenge arising from unresolved processes helping to organize convection in the PBL, as well as gravity waves and cold pool dynamics.

Another approach to highlight “communication” may be to take advantage of schemes such as Semi-Lagrangian Horizontal Diffusion (SLHD), which is a non-linear scheme where the diffusivity is based on the atmospheric motions. Both of the above mentioned ideas (where the latter of course is already developed and well established, whereas the first approach is still under construction) can also work as as a supplement to, or basis for, 3D turbulence.

Aspect ratio: Horizontal resolution vs vertical resolution. Much attention is put on increased horizontal resolution while keeping the vertical resolution the same as the “synoptic” scale models, yielding anisotropic eddies. More vertical levels in the PBL will need to be addressed in the forthcoming year(s), especially if ideas of 3D turbulence will be realized.

LBC-coupling: As our horizontal resolution increases, the domain size decreases, and the influence of the LBC becomes increasingly important. Focusing on physics, what is the repercussion of coupling a model in which the convection is resolved with a model in which convective updraughts are parameterized?

Some scientific issues for km scale modeling

László Kullmann (Hungarian Meteorological Service)

Downscaling and lateral boundary conditions: In my opinion intermediate models will be still needed to bridge the gap between the global and convective-permitting models. There are various reasons behind that: due to the small HR domain size (including small coupling zone) the impact of lateral boundaries will be more emphasized and at the same time it will be essential to increase the coupling frequency (which can be realized if the intermediate and the HR models are running at the same location). The dynamical and physical consistency between the intermediate and HR model will be more important than it is the case between the global and intermediate model. This fact also corresponds with the small domain size of the HR model (since the inconsistent information between the HR model and the intermediate one will be propagated faster through the lateral boundaries). Additionally, it will be important to have multi-scale physics to have consistent physics between driving and HR model (to describe the larger scale processes in the same way). This may also help to reduce the spin-up of the model. Finally even if the number of available observations is the same in the high and low resolution model it is important to have DA in the HR one (again because of consistency, i.e. to use background covariances calculated for the HR model).

Importance of EPS: The importance of EPS will be increasing since at km scale the forecast will have more noisy features due to the lack of correct knowledge in the initial conditions and the improper description of the small scale physics. Here again multi-scale physics is important to have consistent high resolution deterministic and lower resolution EPS system. (With the EPS system we describe the behavior at larger scales while with HR deterministic we precise the smaller scales.) Therefore the high resolution physics packages should include some stochastic aspects (see also below) in order to address the uncertainties related to the description of physical processes. On the other hand since the small scale processes are less known the use of multi-physics or stochastic physics in EPS is a key issue.

Spin-up: When going to HR the importance of very short range forecast is increasing (small domain, more noise prevents to have longer forecast time). It is then crucial to have smaller spin-up time. Maybe in the assimilation some balance condition could be prescribed to reduce the spin-up.

Small scale physics

With increasing resolution we will have to describe more processes but they are not known precisely. Some kind of probabilistic approach (internal, not EPS) may be used (like the difference between molecular dynamics -where we try to describe the interaction at particle scale- and statistical physics where we only give the probability of a macro state) instead of trying to describe explicitly these processes.

The importance of real 3D physics is increasing which will need code reorganization.

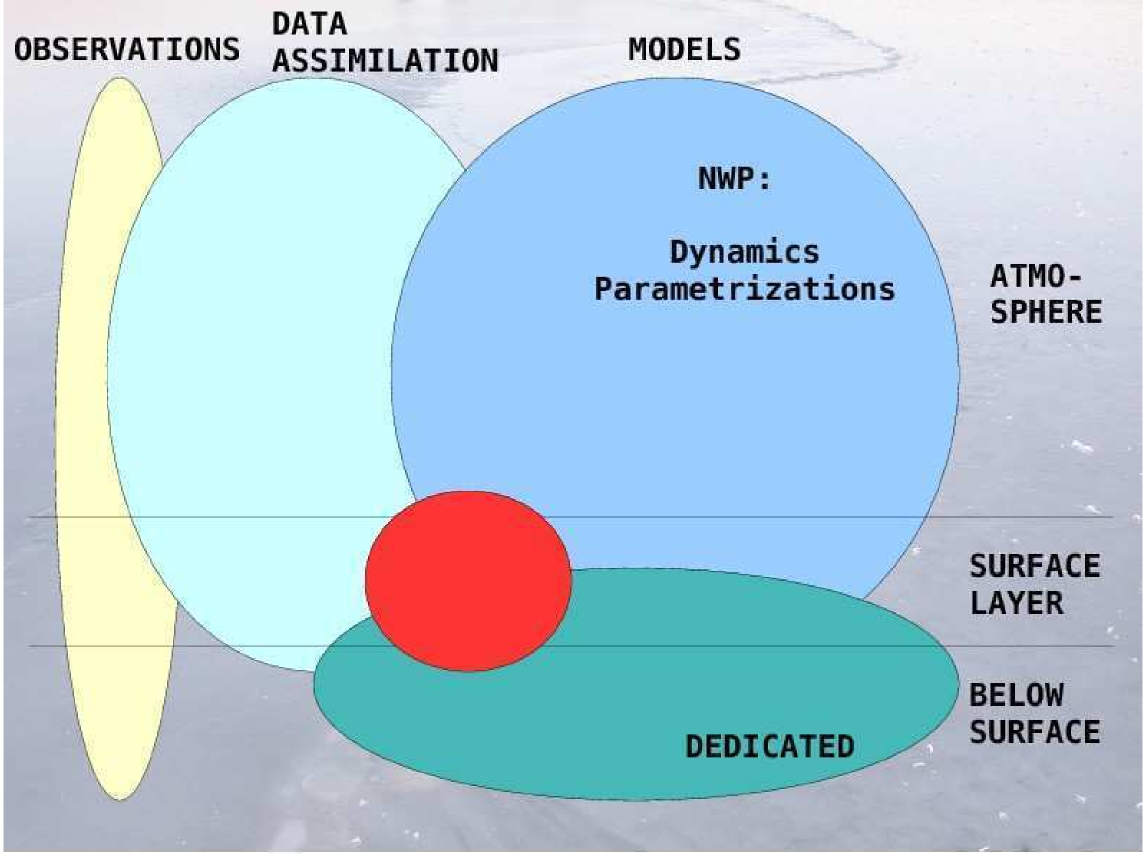
Surface processes: More tiles should be taken into account (high/low vegetation, different urban tiles, etc.) to be able to better model surface processes. There may be a problem of interaction with atmosphere with tile-average fluxes when lowest model level is close to surface (the flux is representative to the specific tile and not the average).

Modularization of code: More and more processes have to be described with different solutions. Solution may be case dependent (depending on location of domain, driving model). To be able to choose among different solutions, code have to be modular but it is better to have this modularity at higher level (larger packages) to avoid inconsistency.

Verification: To be able to verify the small structures of the high resolution forecast it is very important to incorporate more observations for the verification. Especially at higher level where now only TEMPs are taken into account. It is also important to move from point-to-point verification towards structure verification (like SAL).

Comments around the surface for the Brac workshop 17-20.5.2010 Laura Rontu 3.5.2010

1. About modelling of the surface-atmosphere interactions



Red circle: we are here.

The outcome of a fine-resolution NWP model is the three-dimensional state of the atmosphere at any instance of time within the forecast range of O(day), i.e. surface pressure, temperature, humidity, wind, hydrometeors (+ radiation fluxes, momentum fluxes due to waves and turbulence, turbulent heat and moisture fluxes). What about the surface state - snow, ice, water, vegetation, soil temperature, soil moisture etc? A NWP model itself needs it for description of the atmosphere-surface interactions. Thus, we are able to handle the changing surface properties at the levels of physiography description, data assimilation and forecast. Who else analyses, predicts, needs the surface state? - the satellite applications, dedicated soil/ice/snow/hydrology models ... (SURFEX is, in fact, a collection of dedicated models with an advanced interface to the atmospheric model.) Is the surface for us a by-product only? Do the dedicated models handle it better in data assimilation and forecast? These models need atmospheric state for their input, most often provided by a NWP model. Should the fine-scale NWP models develop towards fine-scale and short-range earth system models, which provide forecasts for a wide range of applications or should we limit our activities strictly to the atmospheric processes? What are the consequences of this choice, i.e. how should the interactions shown in the figure be handled in a coupled or an integrated mode? Perhaps, let all flowers (approaches) blossom while understanding the interactions, alternatives and possibilities?

2. About atmosphere-orography interactions in different spatial scales

Surface elevation, orography, as a lower boundary condition for an atmospheric model, has the pleasant feature of not changing in time. However, all temporal and spatial scales are present in the atmosphere, thus also in the interactions between these two. Orography may be the only element of forcing of the atmospheric processes which has always been treated by both the resolved dynamics and different subgrid-scale parametrizations.

It is commonly stated that with an improving horizontal resolution the explicit dynamics of the NWP models will need to treat increasingly steep slopes, over which the numerical solution of the basic equations is complicated or impossible. Is it indeed inevitable to enter this problem? Why should we attack the atmospheric flow at high-resolution with a straight-forward solution of the full equations of dynamics by including the finest-resolution details of orography, like the steepest slopes, in the vertical coordinate? Do we expect that the air flow indeed strictly follows all the peaks, slopes, gaps and valleys which the contemporary and future satellite measurements are able to resolve (a resolution of 10-100 metres is available almost globally nowadays)? Do we hope to solve the whole spectrum of orography-related waves and turbulence and their interactions in atmosphere automatically, with brute force and in all scales simultaneously?

There will always be unresolved scales of orography (let alone the forest, ice, snow and such things that cover the surface also over the mountains). In the subgrid-scale, processes of different scales and physics may exist. We cannot expect to resolve with the basic equations waves forced by features of orography smaller than about five gridlengths. Such features can be filtered away without doing harm to the accuracy of the results. In a kilometer-scale model, this means that the terrain-following coordinate will see obstacles of O (10km). This will be well sufficient to predict the bora wind here in the lee of Velebit mountains, along the Adriatic coast and islands with the three-dimensional explicit nonhydrostatic equations. In a model with a horizontal resolution of one hundred metres, we can hope to handle explicitly large scale eddies, rotors and other kilometer-scale features in the lee of the mountains, at the border of land and sea.

However, the concept of subgrid-scale becomes somewhat problematic when processes due to the features larger than grid-size but beyond the resolution of explicit dynamics (ca. 2-6 gridlengths) will need parametrization. In the present HIRLAM, orography-related momentum fluxes are related, in an approximate way, (1) to the smallest-scale form drag, assumed to originate due the smallest-scale (< 2 gridlengths) orography features and parametrized as some kind of turbulence; (2) to the mesoscale mountain wave processes due to the intermediate-scale obstacles (2-6 gridlengths); (3) to the resolved-scale waves handled explicitly in the framework of the terrain-following coordinate system. Picking such domains and corresponding physical processes from the continuous spectrum of orography-atmosphere interactions is of course a simplification, but hopefully a consistent one. In the kilometer-scale models, a parametrization of the mesoscale mountain wave generation will probably not be necessary. Instead, an advanced parametrization of three-dimensional wave-turbulence interactions, i.e. breaking of the resolved-scale waves, should be developed and applied (taking Bougeault-Lacarrere, 1989, as starting point?!).

Parametrization of orography-related momentum or radiation fluxes requires proper statistics of the (subgrid-scale) surface elevation, describing the variation of the height and directions of the slopes. For the kilometer-scale models, such statistics should be derived from the highest-resolution digital elevation data available (e.g. the relatively recent SRTM or ASTER data sets). Naturally, the filtering, scales and statistical parameters of the orography should be consistent between the surface and atmospheric dynamics, parametrizations and data assimilation.

Contribution of Mariano Hortal

At this stage I only want to remind everybody about two very basic concepts which should not be forgotten.

The first one is the effective resolution of a numerical model: We cannot expect to forecast properly features of a spatial scale smaller than about 6 times the nominal resolution of the model. This has implications in

- Verification
- Academic studies (it is wrong to test the behavior of the model with features covering just one grid point)

The second one is the order of the schemes to use: The discretization introduces an error with respect to the analytical solution which decreases when the resolution is increased. The decrease is more pronounced if the order of the scheme is higher. The question is: what is more efficient in decreasing the discretization error?, to increase drastically the resolution? or to increase it less drastically using a higher order scheme?. My opinion is that it is more efficient to use a higher order scheme. But having said that, all present models use second order in time. Should we think about higher order in time schemes? Perhaps finite elements in time?.

Open views on strategic issues for high-resolution NWP dynamical cores

P.Bénard and C. Lac 04/05/2010

This is a working document for feeding the discussions about strategic issues on dynamical cores in Brac-HR meeting. This does not reflect the personal views or opinions of the two authors, but is rather a compilation of all avenues considered during preparatory meetings at CNRM, among a wide community (not necessarily from the NWP world). Since Brac-HR meeting is a brainstorming and therefore, nobody should come there with a closed strategic program.
The following prospective assumes a continuation of all existing international collaborations, eventually leading to a high level of code-cooperation. This means that a reasonnable consensus must be sought between ALADIN, HIRLAM and ECMWF partners about strategic issues.
Although it might seem a long period of time for managers, a ten-years term is now a quite short period when speaking about substantial evolutions (or replacement) of dynamical cores. This mainly comes from (i) the complexity of the equation systems used at high resolution (e.g. compressible systems); (ii) the difficulty to find a happy compromise between a high level of accuracy and an optimal efficiency; (iii) the complexity brought by the wishable ability to exploit the capabilities of various computing machine architectures, including massively parallel ones. Consequently, it is not uncommon, in NWP research services all around the world, to observe preparatory works lasting more than ten years in order to built or deeply revise the dynamical core of their high-resolution models. This remark is made even more accute when considering the general context of decrease for the budgets allocated to public services.

1 Context

The current high-resolution dynamical core (Aladin-NH) delivers robust, efficient and reasonably accurate capabilities. However if no modification is made in the dynamical core, the three latter properties might become problematic when passing to hectometric scales on massively parallel computers. Moreover, some specific problems in the current forecasts of AROME (during convective events) are suspected to be related to weaknesses already manifesting themselves in the dynamical core (e.g. non-conservation of the semi-Lagrangian scheme in strongly convergent/divergent areas).
All three general properties (robustness, efficiency and quality) may be endangered when increasing the resolution. Here are the most commonly reported sources of problems at high-resolutions :

- Robustness could become more problematic for flows with a chaotic wind field at grid-scale, making it difficult for the SL scheme to accurately determine the parcel trajectories. Besides, large nonlinearities [with respect to the linear system of the semi-implicit (SI) scheme] might dramatically reduce the stability domain of the system : for instantce, very large static stability conditions; very large domains; and steep slopes, particularly during strong wind conditions.
- Efficiency could decrease significantly if poorly scalable algorithms are used on massively parallel architectures; and also, if the only solution to restore robustness when facing a stability problem is to decrease the time-step more than proportionnally with respect to the grid-mesh.
- The quality of the forecasts might experience some degradation at high-resolution if specific processes are not properly addressed. This includes the consistency of the interactions between diabatic and dynamical processes, excessive diffusivity or non-conservation in the semi-Lagrangian scheme, possible spurious circulations around steep slopes, etc. Also, conservation, monotonicity and capability to maintain sharp gradients (low numerical diffusion) will be essential properties for the transport of passive scalars in the context of two-moments microphysics and aerosol modelling.

In a more general context, the current emphasis of high resolution nonhydrostatic mesoscale models is on high-order explicit and non oscillatory finite difference advection schemes.
Most of the proposal below specifically tend to answer to one or several of these possible concerns. It is seen that, in opposition to past years (decades), no issue is here considered as taboo, in order to stimulate debates.

2 Open issues for prospectives

2.1 Short-term prospective

The short-term program is to improve the conservation properties of the SL scheme in convergent/divergent areas. The minimization of numerical diffusion intrinsic to the semi-Lagrangian scheme is an important issue : a recent study has shown that numerical diffusion inherent to semi-lagrangian in AROME introduces excessive dissipation that reduces the model's effective resolution. Also a progress in the formulation of diabatism is needed whatever is decided for the dynamical core, since the status of diabatic sources is not completely clear in the current version. This last point may require a clarification of the diabatic formalism in the physics package itself, in the physics-dynamics interface, and in the dynamical core as well. If these tasks are completed, the current dynamical core is assumed to deliver an acceptable level of service for several years, for scales down to sub-kilometric ones (e.g. half-kilometer).

2.2 Scalable specific algorithms for spectral transforms

The spectral technique has proven its high-efficiency in the past (in conjunction with SI-SL time-stepping algorithms), but suffers from some limitations. First, it is inherently non-local and hence does not easily lend itself to massively parallel architectures. The transposition process during the spectral transforms restricts the scalability.
If restricting the scope to this scalability problem, a possible solution could be to investigate Fourier Transform (FT) algorithms in a specifically scalable way. This could be reached by moving from "fast but unscalable" FTs, to "slower but scalable" FTs. This assumes that the lack of scalability of the current transforms essentially comes from the non-local character of FFTs.
Making FT scalable would allow to ensure the efficiency of the system in the future, but would not address other limitations linked to the use of the spectral technique.

2.3 Grid-point algorithms

Another limitation of the spectral technique is that it makes it difficult to include orographic terms in the semi-implicit treatment. Besides, the problem of large deviations between the actual state and the reference state is increased with higher resolutions. For instance, very stably stratified layers may appear at high resolutions. Another difficulty is to introduce 3D turbulence with the spectral technique if it is shown that the correspondant effective resolution requires 3D turbulence. All these limitations might potentially endanger the robustness of the system at hectometric scales.
As a response to these risks, it could be considered to renounce to the use of the spectral technique. However, this change would be made difficult if the current A-type unstaggered grid is not an appropriate choice for grid-point models. Traditionnally, such an "A-Grid" is not recommended, but it is not sure the reasons for this are valid for high-order grid-point schemes. Moving to a C-grid (for u and v wind-components) would represent a much bigger job.
Basically, returning to a grid point model would need to write grid-point algorithms for derivative computations, and to import a highly scalable iterative solver for inverting the 3D operator of the semi-implicit problem. It must be stressed that the convergence of these solver is sometimes poor when the reference state of the semi-implicit scheme is too complex.

2.4 Eulerian schemes

It is sometimes suggested to come back to Eulerian advection schemes, in order to get rid of non-controlled diffusivity and conservation problems linked to SL schemes. Currently, it is not possible to run our NWP models in Eulerian mode, because some prognostic variables have no spectral representation (e.g. moist variables, TKE,...). In order to run in Eulerian mode, a grid-point derivative algorithm would be needed at least for these variables. However, by construction, these "non-dynamical" variables are not subjected to semi-implicit corrections, hence a "hybrid model" with SI-variables still treated in spectral, and explicit-variables in grid-point could also be considered.

2.5 Eulerian grid-point schemes

The two previous options could be mixed, in order to come back to an Eulerian grid-point model, still in the semi-implicit context. As stated before, if this is possible in the A-grid formalism, this would make the task much easier. Such a combination would allow to adress the problems of scalability, non-controlled behaviour of SL schemes, and would maybe allow a safer treatment of orographic terms and large nonlinearities of local states compared to the reference SI state.

2.6 Eulerian grid-point explicit schemes

This strategy would imply a convergence with e.g. WRF (or similar models) paradigms. The idea behind this is that the system of Euler Equation treated in this formalism is then local (or almost), and simple algorithms are supposed to guaranty controlled properties (conservation, no-diffusivity, etc.). It should be noted that some advanced versions of WRF use an implicit treatment of the vertically-propagating part of elastic waves. However this only introduces a slight non-locality, along vertical direction. Provided such simple algorithms can work in NWP without being polluted by various ad-hoc modifications and fixers, the accessibility of the code would also be increased. In the opposite case, the advantage of simple schemes would be less clear. For instance, it is not clear if explicit time-splitting approaches finally result in clearer code than SI approaches.
Choosing this strategy would mean a complete rewriting on the dynamical core. This is an extreme way: the resulting strategy would then become almost exactly opposite to our current one. Hence, this would need a complete change with respect to our current "NWP culture", which has been progressively shared through years, between all of our NWP partners research groups. On the other hand, it will avoid a singularity position compared to the high-resolution modelling community.

2.7 Unexamined points

- Should we use anelastic systems;
- Issues linked with terrain-following approach;
- Should we mix the diabatic and adiabatic parts of the model inside the time-step.
- Can we make the dynamical code more modular (following OOPS)
- AOB (as deep atmosphere, oblated spherod, multi-phase fluids...)

3 Conclusion

For time being, we have a system which is able to work properly for NWP at current scales, and quite safely down to e.g. 500 m resolution. Experiments with such sub-kilometric resolutions are commonly performed without major problems, but it still needs validation in academic and real cases. Perhaps current problems of non conservation of the SL scheme at 2.5km resolution could become flagrant at higher resolutions.

Hence the current dynamical core can be expected to go on fulfilling its NWP capability for some years, provided short-term improvements are brought in the system. Similarly, at short-term, the problem of the lack of large-scalability is not expected to adversely affect the performance of the system in a too much dramatic way.

Therefore, all the paths explored above should initially be viewed as research paths rather than development paths. To illustrate this, assuming that the migration to an "as simple as possible" dynamical core is priviledged (implying a convergence towards e.g. WRF paradigms : "almost-explicit", Eulerian, grid-point...), this would require a preliminary and careful examination of pros and contras for such a revolutionary change. This, in particular would imply a very detailed intercomparison exercise on a common platform, in order to check all aspects of the change in a comprehensive way. The Meso-NH research model could be an element of the intercomparison exercise (in addition to the fact that Meso-NH physics is similar with AROME). as high-order advection schemes (WENO and PPM) combined to RK3 time-splitting explicit scheme begins to work and shows large improvement of efficiency and accuracy (compared to centred advection schemes and leap-frog) Ultra-simple dynamical cores certainly have advantages in accessibility, and probably easier to scale-up, but before to embark for such a different strategy, it must be check carefully that it is really beneficial in a NWP framework, when all points are considered.

A last point for this conclusion deals with human aspects. A complete change of strategy (compared to the current one) would virtually imply a new and far-reaching "project". Being stated that a new "culture" should be built among the research and development teams, and given the time-scales needed for all this, it is likely that the community would dramatically need an infusion of new blood, with full-time new, motivated and young scientists holding this project as their own personal project, that is, as a milestone in their career. It seems problematic to completely ignore this issue when speaking about strategy of dynamical cores.

Position paper for the Brač-HR meeting

L. Gerard and P. Termonia

Physics

General approach

It is now realized that NWP at km scale is very problematic. What are the equilibria at km scale? What is the slow manifold at km scale? In Cracow, Jean-François Geleyn raised the point that we may have to move from a paradigm based on the physics ↔ dynamics approach toward one based on the distinction between reversible ↔ irreversible processes. The question is to have a scientific handle on that. If not, modeling at km scale will be merely a mastering of compensating errors; we have currently no systematic methodology for it (other than trial-and-error tuning).

Question: Will we be stuck to mastering compensating errors? In that case should we look for a theory for tuning? Does it even make sense to look for it?

Is there something to be learned from fibrillations? One could say that the situation is similar in the sense that also here we have unrealistic behavior, but it doesn't make the model blow. The way to handle the "unrealistic behavior" is to control the perturbations around a steady state (Kalnay, Kanamitsu). The steady state in the model is known (see Bénard et al.) even though the model is never in it. But it can be used to control the model. The upshot is that the steady state, adding zero tendency, is neutral with respect to the dynamics. By splitting the problem in the steady state plus perturbation, the bulk of the physics-dynamics interaction is already treated in a trivial way.

Question: What is the distinction between equilibrium state and steady state? How to define the steady state? Are they the same for deep convection and shouldn't we focus on steady states more (as we did for the diffusion equation) ?

For deep convection one usually thinks in terms of (quasi) equilibrium. In physics, an equilibrium is usually defined by a lowest state in some sort of background potential. It can be stable or unstable. A steady state is defined as having zero tendency (d/dt=0). In the case of geostrophic balance and hydrostatic equilibrium, steady-state and equilibrium are the same.

The distinction between steady state and equilibrium becomes important at high resolution/small time steps and when developing multiscale solutions. Having the tendency of a physics parameterization equal to zero does not mean we are in equilibrium in a gray zone parameterization, since the dynamics also takes part in the equilibrium.

Question: is it possible to formulate multi scale parameterizations as perturbations around/ transient state towards a (time-evolving) steady-state solution.

- 'Transient state towards' implies the use of prognostic variables.
- In the Alaro-1 approach with virtual subgrid updraft (Gerard, WGNE Blue Book 2010) , this steady-state is represented by the combination of a resolved updraft and the subgrid steady state (corresponding to maintaining current forcing indefinitely) of the virtual updraft.
- If some kind of numerical (3D) resolved diffusion scheme could produce the so-called neutral steady state, the combined effect of all the parameterizations would provide the 'perturbation' to it.

Claim: Steady states have to be defined in a clear manner, the convergence days actions are

necessary and should be finished.

The definition of the steady state corresponds to a vanishing tendency. The question is ``where in the model"? For instance in the case of the turbulence scheme this is defined between the physics parameterizations and the dynamics. As said in the case of the gray zone this is not so clear. Also this makes a difference when we use parallel or sequential physics. In the spirit of the physics-dynamics interfaces this definition should, ideally, be made with respect to the interface.

Other important issues

- Multiple interactions between parameterizations (e.g. radiation/ convection (deep and shallow) / microphysics/ turbulence...).
- Better understanding of entrainment in shallow and deep convective clouds.
- Specific treatment of fog, low clouds, cirrus clouds, internal downdrafts /evaporative instability, work on the diurnal cycle of clouds...
- 3D short-time effects in physics, especially radiation (shadowing by orography / by clouds), or effects of density currents for deep convection enhancement.
- Stochastic inputs to parts of the physics.
- Two-moment cloud water distribution for microphysics, use of aerosol data.
- Use of (e.g. monthly-) observed surface cover data for the surface scheme.
- Availability of adequate and effective validation tools for the parameterizations.

Lateral-boundary conditions

The situation on LBC's is a paradoxical one. The Davies scheme and damping schemes (such as for instance the PML approach) have been exceptionally successful, and there has never been an explicit pressure to put much effort in finding improvements. As a consequence, not only do we not have competing replacements, we also do not have sufficient mathematical insight to formulate so-called open well-posed LBC's. So the strength of the Davies scheme created our current weakness in the numerics at the lateral boundaries.

The status, within the HIRLAM/ALADIN consortia, is now :

- The work of A. McDonald, who managed to run the hydrostatic model with some kind of well-posed LBC's. However, there were still many open issues: the corner problem, the tweaking of the vertical discretization, going to NH, ...
- As far as spectral models are concerned, the research has been limited to simplified model tests.

Now it is planned to study the formulation of well-posed LBC's for the Euler equations. In that case, the signal is incoming/outgoing via the wave solutions and it has been observed that one is forced to make a choice between the gravity waves and the acoustic waves.

Lately Termonia and Voitus investigated whether the gained knowledge is useful as a perturbation of the LBCs in a sense of an ensemble forecast. Indeed, one uses what the theory would tell you to impose to construct ``intelligent" perturbations of the large-scale fields coming from the host model. The perturbations are then applied under a Davies relaxation. So the scheme is anyhow always stable. But this generates some spread that is representative for our current knowledge of model error at the lateral boundaries that could be used in an ensemble system.

It has been shown that one can control the problem of temporal resolution in the coupling (at the lateral boundaries or within the context of spectral nudging), by means of a high-pass digital filter.

The situation can be substantially improved by so-called boundary-error restarts, with some localized grid-point nudging. It is interesting to note here that such restarts can also be seen as a parallel run to the uninterrupted one, providing an indication, or some ``spread", of the error originating from the temporal resolution problem. Again this points in the direction of a probabilistic approach for LBC problem.

Claim: the future for LBC's will be probabilistic.

It was observed that DFI may filter out part of the meteorological signal of the Lothar storm. This was explained by a Doppler shift of the time frequencies. It was also stated that this problem may become more serious when increasing the resolution of the model. To understand this, one has to analyze the frequency spectrum in the space and the time domain. So far, most analyses in NWP are carried out either in space or time. When going to km scale we lack understanding of the space-time aspects and have no tools to test the interplay between space and time.

Claim: we need more filters in the space-time domain as diagnostic tools.

Concerning the downscaling strategies (size of the domain, which coupling updates) the operational setups are determined by brute-force testing. However, such test periods may exclude the worst cases (e.g. the Lothar storm occurring in the coupling files exclusively before and after the Davies zone) so it is never clear whether these tests are exhaustive enough not to exclude the most important cases that might happen in your future operational setup. It has been shown that more system understanding and control over these problems can be gained by applying a diagnostic within the forecast of the host model.

Claim: concerning downscaling strategy one should use/develop more diagnostic tools to be used operationally.

Scientific Issues for New Resolution

CHMI and Comenius University in Bratislava development team

Projection of diabatic forcing on prognostic variables in case of fully elastic equations

Recently this issue was studied because of surprising (in fact bad) results when trying to project heat on both temperature and pressure – a degree of freedom offered by the fully compressible equation system of ALADIN NH dynamics.

The traditional approach is to split physics and dynamics tendencies into two steps: in thermodynamic equation the temperature tendency due to heat is treated locally and the temperature tendency due to conversion term (mechanical work of gas) is treated in the dynamics. It means that conversion term does not get any direct diabatic contribution. But there are two ways to write down the thermodynamic equation – with the conversion term expressed as tendency of pressure (in which case pressure is kept constant in physics time-step) and with the conversion term expressed as tendency of specific volume, i.e. density (in which case density is kept constant in physics time-step). This can be done in general both for HPE system and fully compressible system. For the latter however the choice of keeping density constant within the physics time-step yields the expression to compute some diabatic evolution term for the pressure departure from the hydrostatic state, one of the two additional prognostic quantities in Laprise's system.

To keep pressure constant in the elastic model means to arrive directly at the final state of the adjustment due to heating. To keep density constant corresponds to what happens in the very first moment of the adjustment, but it also leads to erroneous model results, like if the correct final state was never reached. Each solution thus represents in a nut-shell some extreme, arbitrary choice.

Fort the time being, it is rather clear that the solution with no diabatic impact on pressure is preferable. When however moving to new resolutions, it would be interesting to see how to describe the transition stages between both solutions within the dynamical adjustment process, something in principle allowed by the fully compressible equations. This will however require finding how to best eliminate an intrinsic degree of freedom, short of keeping either 'p' or 'ρ' steadily constant when heat source/sinks are taken into account.

Turbulence – new understandings and challenges

The theoretical understanding of basic characteristics from turbulent flows has evolved quite strongly in past ten years:

- Contrary to previous theoretical results, there is nothing like a critical Richardson number in the stable regime, beyond what turbulence would vanish.
- The conversions between Turbulent Kinetic Energy (TKE) and Turbulent Potential Energy (TPE) are a key element of the turbulence influence on resolved flows. They

must either be made explicit or be parameterised, considering the sum TKE+TPE as some invariant.

- At all stabilities, the turbulent flows are a mixture of random and of quasi-organised motions. This has two important consequences, which one would even like to be able to combine:
    - one may capitalise on recent advances in describing Third Order Moments (TOMs) in a limited number of terms but still with a solid theoretical background; such TOMs terms are in fact a generalisation for a mass-flux formulation of the consequences from skewness in the statistics of turbulent motions; such a TOMs additional contribution can nicely complement the classical ‘downgradient’ second order terms, if one elects leaving the pure mass-flux equations to precipitating convection;
    - The recently proposed alternative to the Reynolds formalism, named QNSE, directly treats waves (and anisotropy) within a compact formalism; it might become a reference or even an alternative for the above-mentioned second order terms.
- The common handling of ‘moist’ turbulent effects and of their statistical links with cloud-microphysics and radiation appears creating at least as many problems as it solves.

Inside the so-called ‘TOUCANS’ component of ALARO, we are trying to create a long-term-oriented framework for a possible concretisation of all above tracks.

In order to give a chance of success to the assembling of those various steps, the scheme must also care for specific algorithmic issues. In this perspective our current proposals are:

- having a time-discretisation algorithm for the evolution of TKE and of ‘diffused’ prognostic quantities, inherited from the so-called p-TKE framework and allowing in principle maximum stability plus accuracy and minimum impact of the vertical staggering problems;
- joining the considerations on ‘No Ri(cr)’ and on TKE+TPE conservation for obtaining a rather universal choice of relevant degrees of freedom for the stability dependency functions of this algorithm;
- integrating as an option the full treatment of anisotropy into this ‘merged’ concept;
- offering various existing formulations for the characteristic ‘length scales’, with care taken for the link to moist aspects and for the continuity with the Monin-Obukhov formalism near the surface;
- using an a-priori definition of the moist Brunt-Vaisala frequency to try and (i) parameterise shallow convection and (ii) unify the various moist considerations within turbulent computations; the choice of this ‘definition’ will initially be quite heuristic but we hope to subsequently work on the basis of some more solid theory;
- preparing for an extension towards lateral mixing, in a manner compatible with some 3D turbulent concept and with the already existing Semi-Lagrangian Horizontal Diffusion (SLHD) technical framework.

Position paper for the Brac-HR meeting

J. Vivoda

Dynamics

My statements are based on experiences with NH dynamical core model ALADIN/AROME. My experiences are based on very high resolution idealized test runs. Here are the topics for discussion:

System of equations

The acoustic modes in fully elastic system of equation are potential problem for stability of time-stepping algorithm. During last years we showed that the correct choice of prognostic variables and “more implicit” schemes are sufficient to control stability of these modes.

There is no need to filter out these modes with anelastic approximation, because the complexity of system is not significantly reduced when compared to elastic system of equations. What would be the benefit of anelastic approximation when compared to full system in such case ?

In order to build efficient “more implicit” scheme the proper choice of prognostic variables must be chosen. There exists unproved hypothesis that the D3 term shall be linear combination of prognostic variables.

Time-stepping

We do expect that convective scale kilometric models will be used for very high resolution probabilistic forecast. The efficiency of time stepping algorithm shall be the most important aspect to be considered in order to make it feasible on medium-size domain with sufficient number of ensemble members.

As I mentioned in previous paragraph, the “more implicit” schemes are capable to control stability of acoustic modes. It is not clear what is the best choice for “more impliciteness”:

- iterative solution of complex implicit system (nonlinear system or linear system with orography in reference state),
- iterations of non-linear residual with simple very efficient solver (linear system with flat orography), this is only choice for spectral solvers.

Semi-lagrangian (SL) advection treatment has to assure conservative properties of advected quantities as still more prognostic quantities appeared in the models. SL schemes are very promising as they allow controlled 3D filtering of signal via proper choice of interpolators.

Eulerian advection treatment in my view is still not appropriate as significantly limits the time step and via these terms numerical oscillations enters the solution at discontinuities (see last paragraph).

Choice of vertical coordinate and horizontal representation

Is terrain following coordinate appropriate when treating 3D turbulence (horizontal diffusion along constant vertical coordinate surfaces) ? Is there simple fix for this ?

This choice seems to be crucial for spectral models, because spectral method requires fields to be continuous on the whole model domain. This is apparently not true for non-following coordinates. This would require to use high-order derivatives computed on local stencils (finite difference method, horizonal finite elements, high order compact differences …). What would be consequences on model stability and efficiency of “more implicit” time stepping in such case ?

Derivative and integral operators

When going to very high resolution the model fields contain discontinuities (regions of very high gradients and “shock” like features). Traditionally the oscillatory methods are used in order to compute derivative resp. integral operators. Therefore Gibbs phenomenon is present at discontinuities. Numerical dissipation must be applied to get rid of such numerical noise. The usage of high order non-oscillatory methods shall be considered in the very high resolution models.

Scalability of dynamical core

Future petascale high performace computing systems are expected to contain thousands of cores. The prediction problem must be capable to be scalable on such systems. This fact prefers explicit schemes (preferably high order) with small stencil operators. Will this scalable “brute force” dictate future development of dynamics because the communication will become more expensive than model computation ?

MAJOR SCIENTIFIC ISSUES FOR KM-SCALE NWP MODELS

Sami Niemelä – FMI
Brac workshop 17.-20.5.2010

1. *Convection*

One of the main scientific issue related to km-scale NWP models is the treatment convective processes. We are entering the region where the formerly unresolved processes start to be present in the scales of the model grid. We know that in km-scales traditional schemes are not necessary valid in terms of assumptions made within them, although they might produce satisfactory forecasts. Moreover, we have experienced that km-scale models without deep convection parameterisation face severe problems related to too intense convective flow structures and precipitation. There are probably several interplaying issues affecting. (a) In order to simulate the onset of convection, harmonised parameterisation for boundary layer and shallow convection processes are needed. (b) Some parameterisation for updraft and downdraft part of the convection might still be needed until we reach the scales O(100m).

2. *3D-physics*

In km-scale we may start to reach the applicability of parameterising the physical processes only in vertical. 3D turbulence processes might have role in initiating the convective activity. Also the 3D radiative effects become increasingly important especially in the regions of variable topography.

3. *Microphysics*

Microphysics has an important role in the development of the convection. It is essential to include the whole water cycle with phase change processes as accurately as possible. Moreover, the following step would be the inclusion of droplet number concentration as prognostic variable (2nd moment schemes), and possibly even “sectional schemes” paving the way towards modelling of the droplet size distribution. These type of schemes are computationally very expensive. However, if they lead to more physically based (and better) fog and visibility forecasts, the extra cost would be justified.

4. *Data assimilation*

One of the major issues is the initialization of a model at very fine scales, since the assimilation system should be able to deal with highly non-linear processes, such as microphysics. What are the methods that can handle non-linearity and take into account the model error? 4D-VAR, ensemble Kalman-filter, or something else? This issue is clearly the crucial component of the km-scale NWP system. Moreover, for deterministic prediction of mesoscale weather systems, it is essential to have accurate initial conditions for both balanced and unbalanced parts of the wind field as well as for three dimensional distribution of water vapour. This sets an additional requirement for mesoscale observing networks and the usage of such observations (e.g. radar and satellite observations).

5. *km-scale EPS*

Can we deterministically forecast the features that our km-scale models are able to simulate? In principle we should be able to do that, but only if: (a) we can assimilate the mesoscale structures of the initial state, (b) model's response to this initial state is realistic and (c) the focus is on (very) short time scales. However, it seems inevitable that atmospheric phenomena with a short life time (e.g. convection) need a probablistic approach when lead times exceed 12-24 hours. There are many questions to be addressed. What are strategies for alternative initial states and lateral boundary conditions for LAMs (e.g. downscaling global EPS, LAM singular vectors, LAM breeding, ETKF)? What are strategies for alternative model and lower boundary formulations (e.g. multi-model, multi-physics, stochastic physics, all of them)? What should be the balance between ensemble size vs. grid resolution for predicting high-impact weather? How to treat ensemble calibration and how to extract user-relevant information from short-range EPS?

6. *Code scalability and performance*

Massively parallel HPC solutions with 10^4 - 10^6 computational cores set strong requirements for code maintenance (both model and assimilation) in terms of scalability. Furthermore, new type of computational processors, such as GPU's, have already shown some promising speed-up. However, in order to take the full advantage of the new developments, the present code architecture could need some serious re-designing.

7. *Verification*

The recent advancements in the field of spatial forecast verification methods (e.g. CRA [Ebert and McBride; 2000], FSS [Roberts and Lean; 2007] and SAL [Wernli et al.; 2008]) should form a foundation towards high-resolution forecast verification methods development. The issue of forecast performance as function of spatial and temporal scales has to be addressed. Verification of extreme weather events and severe weather warnings, which have a high impact on the society deserve a specific focus with regards to methods taking into account uncertainty associated with verification measures per se as well as the underlying observational uncertainty (e.g. radar vs. in situ precipitation observations). The enhanced use of remotely-sensed observations is required in areas with sparse traditional observational coverage.

Ebert, E. and McBride, J., 2000: Verification of precipitation in weather systems: determination of systematic errors. *J. Hydrol.*, 239, 179-202.

Roberts, N. and Lean, H., 2008: Scale selective verification of rainfall accumulations from high-resolution forecasts of convective events. *Mon. Wea. Rev.*, 136, 78-97.

Wernli, H., Paulat, P., Hagen, M. and Frei C., 2008: SAL—A Novel Quality Measure for the Verification of Quantitative Precipitation Forecasts. *Mon. Wea. Rev.,* 136, 4470-4487.

Contribution of Siham Sbii (with help of Nourredine Semane)

1. Is there a specificity of LAM vs. global models in the march towards higher resolutions?
Probably Yes, especially in the case of the physical processes which still need to be parametrized even in high resolution.

2.What to do about the very tough issue of lateral coupling in operational NWP?
Working with more than one direct coupling: going to higher resolution step by step because I think that some small scale processes should not be linked to what happens at global scales.

3. More generally, is there a chance to see a consensus emerge on the best downscaling strategy? Or will empiricism continue to dominate here?
I think It will continue to depend on the fundings allocated for that, that's why a single concensus is not possible in my point of view.

4. Since there is a good chance that every topical solution for resolution-related problems will now be associated with a new problem, should increase in horizontal resolution continue to be presented as the main way out of 'parameterisation problems'?
No, The spirit of parametrisation is not only related to subgrid physical processes which could be resolved with high resolution models, but also it's linked to other issues especially interfacing with dynamical core.

5. What are the links between the march towards higher resolution and data assimilation at scales where the slow manifold ceases to be constrained by geostrophism/non-hydrostatism? Is a strong increase (in relative terms) of the quantity and quality of the observations the only way to avoid being de facto restricted to longer effective time-space scales for the model's initial state?

6. What about 'initialisation' at high resolution, where the distinction between signal and noise starts to be blurred (this also has to do with the previous issue and with the one five bullets down)?

7. What will be the relative importance of EPS versus high resolution deterministic forecasts as resolution further increases?
I think if EPS is only based on perturbations of the initial states, it will be difficult with high resolution to have a good probabilistic diagnostics.

8. What is the NWP cost-effectiveness of enhancing the description of more and more physical processes, especially if their interplay with the core characteristics of the dynamical host models is more and more difficult to grasp and to control at the level of discretised equations?
I think that an additional effort should be done to optimise the physical part algorithms and thereby ensure high cost-effectiveness.

9. Concerning the stochastic aspects, what are going to be the relative weights of their internal (Self Organised Criticality) and external (Ensemble Prediction Systems) importance for the R&D concerning the modelling part of high-resolution NWP?

I think here only statistical approches are useful to deal with the stochastic aspects, this idea is also linked to the new verification and validation methods which should be developped.

10. What are the real 'boundaries' of the various 'grey zones' that we shall be confronted with? Is the question anyhow pertinent or should the search for multi-scale solutions become a standard priority?
I think that a search for mutiscale solutions is less difficult because the scale boundaries are not evident to specify, for example when we used 10km for the Hydrostatic models as a threshold we found later than even at 9km or less, these models still be in a good agreement with non-hydrostatic ones.

11. How are 3D aspects of the physical forcing going to influence the evolution of models? Shall they stay purely phenomenology-driven or is their algorithmic aspect going to be the dominating aspect as we start resolving turbulent motions in a 2D/3D relatively arbitrary mix?
I think that the forcing is not only phenomenology-driven, because some physical processes are not completely subgrid phenomena.

12. More generally, shall not we soon be forced to stop using the 'absolute' separation between 'dynamics' and 'physics' and consider as core issue the interplay between all forcings and the truly reversible part of the equations?
I think yes, because that separation will not continue to be based on phenomenology aspect only.

13. How much additional prognostic character should we aim at for solving with the most possible consistency the problem touched in the four preceding bullets?

14. How to evaluate and better control the interplay between micro-physical and macro-physical atmospheric representations?

15. At the code level, which level of modularity should preferentially be sought (whole packages, integrated algorithms or individual processes)?
I think whole packages, because it is difficult to treat separately individual processes.

16. Which medium-term strategy concerning the surface issues (oceanic, classical, urban, etc.)? Is not here the main problem the one of 'software integration vs. generality of options' than the one of 'modularisation with respect to upper air computations'?

17. Which of the above-mentioned issues might be beneficial in reducing the spread/uncertainties in climate projections?

18. How does all the above interacts with the evolution of High Performance Computing?
The High Performance Computing is offering more and more possibilities to optimise and to run that kind of algorithm complexity, but it still depending on the fundings and on the strategy of each country.

19. What are the necessary evolutions of the validation, assessment and verification tools needed to progress towards higher resolution?
The evolution in verification should be in parallel with the DA evolution, in order to use the maximum of high resolution data in verification.

20. How to convert known present weaknesses in results into plans for as transversal as possible progress in the modelling part of NWP?

21. What about more specific issues:

- Characteristics of the generic equations (thin vs. thick atmosphere; conservation laws; compressibility and influence of the heat sources/sinks; …)? Should the latter aspect interact (or better not) with parameterisation considerations?
- What about parameterisation terms in the budget of the vertical momentum component?
- Choices for the time discretisation scheme?
- Choices for the vertical functional representation and discretisation algorithms?
- Choices for the horizontal functional representation and discretisation algorithms?
- Problems of intermittency, cost-effectiveness and cloud scene representation in radiative computations?
- How to choose the level of complexity of cloud-precipitation micro-physical schemes to avoid a diminishing return syndrome?
- For sub-grid aspects, where to set the 'model' gap between organised and unorganised motions? How to ensure consistency of the related choices with the above-mentioned radiative and micro-physical considerations?
- Choices for the various aspects of the closure assumptions for convection and for (moist) turbulence equations?

Since the parametrization will not be only phenomenology-dependent, the closure assumptions have to be reviewd.

- Which links between moist physics and momentum transport by all phenomena (resolved, vertically parameterised, 3D minus 1D)?

- Choices for the representation of orography (resolved vs. subgrid; control of generated waves; …)?

- Choices for the huge problem of the surface-atmosphere interplay (model and [specific] DA issues)?

Issues for high resolution modelling of convection, especially from the point of the physics and dynamics

Sander Tijm, KNMI

Issues Brac

1) At what resolution do we start to resolve the deep convection properly?
At the moment the different columns within the convection permitting AROME live more or less independent lifes, with the connection being provided through the advection terms and the horizontal diffusion. The model does not really know what resolution it has, giving similar convective structures for resolutions of 500 metres or 2500 metres. Clearly this should not be the case, but how can we get the correct signal of convection regardless of the horizontal resolution that is used?

There are probably 5 or 6 subjects that need to be studied together to get a model that behaves correctly, that gives the correct convective signal independent of the resolution of the model. These are (as far as I see them):

- the settings and type of horizontal diffusion: Poitrowski et al have shown a very high sensitivity of the way the model resolves convection depending on the diffusion settings. Therefore these need to be studied
- effects of the dynamics like the semi-lagrangian scheme, which may give rise to spurious moisture sources like seen in the experiments of Sylvie and in the comments of Detlev Majewski.
- 3D turbulence will have a similar effect as horizontal diffusion and therefore will be important to study in connection with the deep convection activity and the dependence on the resolution
- the microphysics behaviour. If the vertical velocities are not correct, do not give the correct average behaviour with weaker vertical velocities for coarser grids, then there will be a too large production of the quick species like graupel and hail, causing the convection to start up too quickly, be too active with too high precipitation intensities and also die down too quickly. Note that we may need to make assumptions on the distribution of the moisture in the grid boxes that are convectively active to generate more slow hydrometeors (snow and cloud ice) at the expense of the quicker hydrometeors (hail and graupel). We may also think of subgrid variability for processes like evaporation, which shows to have a very large impact on outflow characteristics.
- should there be some brake on the vertical velocities in the

model, depending on the horizontal resolution of the model, to force the correct average distribution of the vertical velocity. Changing the fall speed of the hydrometeors, which increases the water loading in the updrafts and reduces the updraft strength has shown to have a significant impact on the convective structures.

- is there a role for a deep convection parameterization in all of this, and up to which resolution do you need such a scheme? The reduction of the buoyancy from a deep convection scheme may provide the brake that we need on the vertical velocity, in combination with the more widespread precipitation release of a deep convection scheme compared to resolved convection

Most important of all is what do we think are the main issues in all of this, which will have a first order effect and which will be less important. That should determine where we put the main emphasis in the research developments.

2) What resolution is enough for deterministic forecasting, when should we be looking more at ensembles for convection permitting models. How to handle the second wave of convection, initiated by the outflow from the first one. Can that still be handled in a deterministic sense or is deterministic modelling impossible after the first convective cycle?

3) Physics in the coupled and coupling model. Ideally they should be as close as possible to reduce the spinup in especially the boundary layer structure and cloud microphysics. Experiments with nesting HIRLAM with ECMWF physics in ECMWF show that the impact is huge, spinup can take as much as 500-1000 km for large scale precipitation if the physics packages differ significantly, these effects are similar for high resolution models, the deep convection may take such a large time/distance to spin up which has an impact on the ability of the model to represent the deep convection and has its consequences for the size of the model domains that we use.

Contribution by Erland Källen, ECMWF

Here are some points that will be relevant for ECMWF in the coming years:

We are aiming for a horizontal resolution of about 10 km globally in 2015, we are quite confident that we will reach this target. We also think that the spectral technique will continue to be competitive for global modelling, it remains to be seen if we also need to go non-hydrostatic by 2015. One important area of research in model formulation is the spectral transform technique, we hope to develop faster Legendre transforms. For vertical resolution we will implement a higher vertical resolution later this year or early next year, the number of levels will increase by approximately 50% from the present 91 levels. In the five year perspective we also see a further vertical resolution increase, maybe towards 200 vertical levels. I see this also as an issue for the LAM community, why shouldn't an increased vertical resolution go hand-in-hand with an increase in horizontal resolution?

Concerning data assimilation we are developing hybrid variational and ensemble assimilation techniques. A first version will be introduced already this year, at first the ensemble will only be used to initialise perturbations in the ensemble prediction system. Later on the ensemble produced variances will also be used in the 4DVAR system. In the future also co-variances from the ensemble system will be included in the variational analysis, this is still research to be done. When going to higher resolutions the analysis technique will be crucial to really benefit from the potential of using higher resolution observations. However, there is a trade-off in terms of resolution: We can have a high model resolution or a high resolution in terms of ensemble size in the data assimilation. Both aspects will help to better resolve small scale, intense atmospheric phenomena.

In model physics development work we will have an emphasis on moist physics. We are very much aware of the "grey zone" problems. We see no way around these problems, we must attack them now in order to be ready for the foreseen resolution increases. Here we very much rely on interaction with the LAM community, I also believe that interaction between parameterized physical processes and numerics will continue to be important. We are likely to continue with semi-Lagrangian, semi-implicit time stepping schemes for efficiency reasons and the interaction between parameterized model physics and numerics should be investigated in this framework unless someone comes up with a better idea that is efficient enough.

Another aspect of parameterized physics is so called stochastic physics. We are now using stochastic physics both to represent the probabilistic nature of atmospheric motions in the ensemble prediction system as well as generating different members in the ensemble data assimilation system. This means that stochastic physics represents both short time scale uncertainties as well as uncertainties resulting from large scale flow developments. In the future there will be a closer coupling between stochastic physics ideas and more traditional parameterization research. We also think that in this area we need explicit, very high resolution modelling to guide the ideas developed in stochastic physics. The tropics is the geographical area where I believe that this coupling is most important.

Physical processes related to the boundary layer and topographic forcing continue to be relevant for high horizontal and vertical resolution. How should the vertical resolution be distributed to most effectively handle stable boundary layers and stratified clouds?

Well, these are some of the issues we are considering in our present strategy update. I hope to be able to contribute to the final discussion at the Brac meeting on Thursday 20 May, coming on Wednesday 19 May I would also be able to take part in some group discussions to get a feeling for what has been said in the first days. However, it remains to be seen if this is logistically possible.

Contribution for the Brač-HR meeting

Neva Pristov, EARS, 5.5.2010

Few issues for high resolution modelling are selected. Last two are probably not totally in line with main subject but they should not be forgotten.

Physics

Moving to higher resolution is requesting enhanced description of more and more physical processes. It can happen that schemes developed at one scale may no longer be valid and the schemes that gradually change with scale will be required (not only for convection). We can think also about stochastic approach which might be able to combine the advantages of deterministic and ensemble approaches.
One of the question is also what kind of consequences will the better representation of steep orography have on the numerical schemes and on the physical parameterization schemes.

Surface

For describe the interaction between surface and atmosphere
Better description of orography and coastlines is not enough, improved description of physiography and the change of surface conditions (snow, ice, …) will become more and more vital.

Validation, verification, evaluation

For this purpose a good diagnostics environment and validation tools are essential, new verification tools and additional observations are needed. It is valuable to show, where we can expect added value (with the respect to current systems at few km scale).

Information delivery

Not only the high-quality modeling system but also the ability to provide the relevant information to forecasters and other end-users is important. To design a system how to move/present information from the model to the users would be very useful.

Link to other models

Also other areas are showing interest for the high-resolution models. There is high potential for coupled atmosphere-chemistry models, coupled ocean-atmosphere models, coupled atmosphere-hydrology models. The model quality can also improve from feedbacks between these miscellaneous processes, for example: the impact of aerosol on radiation, microphysic. We can expect that new satellite measurements and other remote sensing will provide additional data which can improve the analysis of additional variables. While designing whole system we should also not forget link to the nowcasting system.

A proposal for an 'internal' and flexible approach to the challenges posed by future high-resolution NWP perspectives to the current ALADIN-NH dynamical core

Jean-François Geleyn and Jeanette Onvlee, 15/5/10

After reading the twenty received position papers and acknowledging the fact that half of the key-note presentations are going to touch the 'dynamics' topic in some broad sense, the Brac-HR organising committee made it a transversal issue, present in five out of the eight foreseen parallel sessions. It is thus very likely that some further crystallisation of part of the debates will concern the long-term future of the ALADIN-NH dynamical core.

Only one however of the position papers (the one by P. Bénard and C. Lac) treats exclusively of this central issue. As wanted by its authors it reflects in an open and direct way the results of preparatory discussions. However, in our opinion, the document does not touch enough upon the past scientific legacy of the current ALADIN-NH dynamical core (DC) and in particular the IFS-ARPEGE-associated spirit of flexibility with respect to a large number of choices. In the past ~10 days, we have been considering this aspect and its consequences for a scientifically sound and flexible way forward.

Indeed, if the issues at stake for the future do touch the 'solver', 'semi-Lagrangian' and 'semi-implicit' issues (and we believe they may), it would be fair to first remember that the 'SI' and 'SL' have been and (to a large extent) still are 'options' in the ALADIN-NH code. Additionally, the use of the spectral technique outside the 'solver' is now intentionally limited to a strict minimum of computations, unlike earlier on.

We therefore had the idea to discuss with a few people, acknowledgeable for their expertise in DC issues and/or in science policy, what could be a scientific plan capitalising on such assets rather than considering the ALADIN-NH as some sort of compact frozen concept. This exercise led to two broad findings. First, choosing this angle of view can lead to some rather innovative perspectives, albeit with associated R&D risks, not more and not less than in any similar case. Second, beyond its scientific aspects, it surprisingly offers a quite attractive way to envisage a flexible operational future. Not only would the high resolution challenges (hopefully) get each a specific answer. But also operational applications still at lower resolution would not automatically loose their efficiency because of a too exclusive and too compact 'new' solution. In some sense the proposal outlined below cares for 'ascending compatibility' on the same foot than 'purpose-driven innovation'. Seen from an even broader perspective it could be a way to define a super-optimisation problem when taking simultaneously into account: accuracy of the DC vs. its computational cost; length of the time-steps; scalability of the communications and computations for the solver (if any); progress in hardware (the existence of two true levels of parallelism, one with shared memory, diminishes the importance of the previous item).

The above-mentioned discussions happened in a rather short lapse of time, did not however reveal any basic weakness in this way of attacking the problem, but could of course not lead to a fully fledged draft-plan. What will be explained below must therefore be taken as a guideline for something (i) sharing the same concerns as the Bénard-Lac paper, (ii) complementary to the 'external-type' studies mentioned there as main immediate actions, (iii) surely less risky than the solutions vaguely anticipated in the said document, (iv) with the potential to foster some consensus around itself among the various HARMONIE partners and (v) scientifically motivating, at least we hope so.

The central idea behind the proposal is to alternate preparatory (quasi-)compulsory steps (PCS) and set-up of clean-comparison conditions (SUC3). They are presented here in chronological scientific planning order for an example, not completely scrutinised yet, but probably close to what iterations with specialists would eventually deliver. For people with some knowledge of DC issues, it is probably better to read each time the SUC3 (in bold-face italic, the interesting but not crucial side-issues being in italic only) before the related PCS that precedes it. Indeed the latter are basically consequences of the former.

PCS-A: Changing the computations so that the semi-Lagrangian advection of the wind components is slightly modified in order to retrieve an 'equivalent-advected divergence', complemented by a direct advection of absolute vorticity (so-called SL-AV technique), still in fully spectral and SISL mode. In principle the meteorological impact of such a rearranging of calculations should be small.

SUC3-B: Still being Galerkin and on the A-grid, introducing finite volumes as an alternative to bi-Fourier modes. Making the SL-AV interface compatible with both choices. In this way, the issue about the scales where it is claimed that we need more 'locality' at high resolution (than with a spectral model) could be attacked with a tool delivering 'clean' tests about this issue and about it only (no 'apples and bananas', so to say).

PCS-C: Allowing the optional suppression of trajectory recomputations when using the iterative solution for the SISL scheme (an already considered possibility). Furthermore, securing the absolute compatibility between the three-time-level and two-time-level codes, even for the SI-solving iterations.

SUC3-D: Coding again the (once existing in 1D-vertical only) option of non-interpolating semi-Lagrangian scheme (SL from grid point to grid point + Eulerian for the virtually remaining part of the 'full trajectory') as an alternative to classical semi-Lagrangian. Then, it is basically the length of the time-step that determines how the 'mixed' scheme reacts: for small time steps, one automatically falls back on a pure Eulerian scheme. This should (a) help sorting out (once again in the cleanest possible way) the open questions concerning the SL behaviour at very high resolution and (ii) leave in any case the 'full SL' option alive for people that would still find more advantage in longer time-steps than in improved scalability on their HPC platforms. *Another advantage of this way of proceeding is that it allows considering 'SL conservativity' in the work-plans without investing in a potential 'cul-de-sac'. The question of what to do for the passively transported species is also neutralised, most options being left open for their specific case.*

PCS-E: This part is still 'in construction' (see below).

SUC3-F: Using the so-called 'beta=0' option to switch off the 3D SI scheme and coding a 'beta_V option' for the vertical treatment of acoustic waves only (on the basis of a reduction of exactly the same structure equation, *NH-VFE included if we would solve this problem in between).* ***This corresponds to ideas 'à la WRF', but in a controlled environment that we know to be excellent for the rather delicate stabilisation of the 'Euler-equations + SI' (see for instance the d3<=>d4 issue, which should offer two possible ways for applying this 'F-part' of the proposal, hence with increased chances of success).***

The reason why the E-part is left open is the following. Imagining that the only solution worth pursuing in the long term would be the 'local + Eulerian + vertical-only SI' one, the proposed set up would contain an important amount of unnecessary overheads for this particular case (since it is in principle the only one without need of a 3D solver and that our data-flows would still cater for the possible presence of such a solver). This situation would rapidly call for steps making these overheads optional and/or redundant. A return to step E (void like now for a pure scientific exploration) would become necessary, provided one would still like both avoiding a recoding from scratch and keeping the benefits from the two preceding SUC3 steps. But this requires a more careful analysis, probably impossible to perform before we precisely know where steps A, B, C and D would have led us.

The above is de facto a 'late contribution', but the topic is clearly important and we believe that it is not too late to be taken as part of the items to be discussed. The proposal's claimed characteristics of 'targeting', 'attractiveness', 'feasibility', 'consensus-building' and 'long-term orientation' ought to be scrutinised and debated, even if presented here in a bit unfinished shape (despite some last-minute cross-checking). In fact, for us, the new angle of view just covers in emergency a gap left open by the rather 'half-proposal, half-regret' character of the only position paper concentrating on the DC-issue. So we make use of our prerogative of members of the SOC to bring in the present note at this late date, with apologies.