Outcome of the Brac-HR workshop

The Organising Committee, 20/8/10

Preamble: Owing to the complexity of both the decision process for the HARMONIE strategy initiated by the ALADIN General Assembly (GA) and the scoping for the Brac-HR workshop, reporting about the latter in an exhaustive manner is a difficult exercise. The solution chosen here is the following:

- All the additional information (agenda, preparatory documents, key-note presentations, …) to the present document may soon be retrieved at the following web address:
  http://www.rclace.eu/?page=129
- The core of the 'long version' of the report is presented below in a stand-alone mode;
- Appendixes provide as ancillary information:
  - The list of participants;
  - The relevant part of the 'scoping document' that helped preparing the meeting, in line with the above-mentioned incentive of the ALADIN GA;
  - A short text describing the follow-on actions, as known at the time of writing the present document.

The Brac-HR workshop (BRain-storming on Advanced Concepts for High Resolution modelling) was held on the island of Brač in Croatia, 17-20/5/2010. This joint ALADIN-HIRLAM event hosted by RC LACE was born out of a decision of the ALADIN General Assembly at its 2009 Session in Istanbul. Besides the representatives of HARMONIE (24 of them) there was a much appreciated participation by three people representing ECMWF, COSMO and MO. The position papers, key-note presentations, working group debates and plenary sessions were very rich in novel or consolidated information and the debates animated but always tractable. The present document is the 'long' version of the outcome of the Brac-HR discussions. For maximum benefit, it should be read together with the ensemble of the preparatory position papers and the six sets of viewgraphs of the key-note lectures. On request, the consolidated notes of the working sessions can be distributed. A 'short' version of the product of the debates will also be prepared, mostly (but not only) for the benefit of people not present in Brač but nevertheless interested in the Brac-HR outcome.
The present document is organised in three parts between areas of consensus, topics of disagreement and synthesis. There are however many cross-references between the sections, reflecting the complexity of the issues at stake, this being another confirmation of the fact that such a brainstorming effort was highly needed.

A) <u>Areas of broad consensus</u>:

a. Important issues

- At high resolution, there is a need to consider things more and more from the stochastic point of view, for modelling as well as for interpretation, whatever tools are used to reach each part of this general goal.
- Indeed, given that we generally cannot expect to get a deterministic forecast of timing, location, intensity and structure of intense events on the basis of the official resolution of the model (2 δx), one may go for that towards larger scales, beyond the effective

resolution of the model (‘n’ δx with ‘n’ between 4 and 10, say). Then the details below what can be considered as ‘deterministic’ are partly ‘stochastic’, except in case of large-scale and/or surface forcing strong enough to produce useful small-scale information despite the back-influence of smaller scales on larger ones. This is even true without the need to run an ensemble of forecasts. This angle of view has of course also consequences for the interpretation and for the validation of the raw results. More generally, while at larger scales one witnesses a convergence between data assimilation and predictability aspects, it is rather unclear whether or not this will happen for ‘convection permitting’ scales.

- The increased sophistication for process description should probably touch primarily the microphysics of precipitation and the cloud-determination scheme(s).
- Even if not necessary linked with intense events, correct forecast of phenomena like fog and gusts is nevertheless an important target.

- There are topics concerning what is (too simplistically) called ‘physics-dynamics interface’ (PDI) which require additional work. A short list can be the following: (i) energy conservation by the parameterisation part of the model time-step; (ii) projecting the heat sources/sinks also on pressure, without falling in the pitfall of a solution describing only the non-adjusted part of the process observed in nature; (iii) organisation of the full model time-step between ‘dynamics’, ‘lateral diffusion’, ‘slow physics’, ‘adjustment physics’ (for instance the vertical mixing has to be incremental and implicit); (iv) starting a change of paradigm by looking together at all cases of irreversibility (physical 1D & 3D as well as numerical).

- Concerning surface handling issues, there is a clear need for some additional organisation effort. This should principally touch the land surface data bases and the methodology for extracting from them the most relevant information, as independently as possible from the subsequent various modelling choices. ECMWF could play a leading role in such a coordinated effort (going by nature beyond HARMONIE), for the surface properties that can be defined independently from the surface models.
- The use of surface ‘tiles’ is probably raising more and more problems as all types of model resolution increase. On the one hand the distinction between the various types of surface is likely to enhance the realism of a more and more important surface forcing. On the other hand, while lower and lower heights of the lowest model level make the hypothesis of a homogeneous atmosphere above an inhomogeneous surface less and less realistic. For various reasons, it may be attractive to have a finer grid for the surface than for the atmosphere, provided the surface scheme is completely externalised. This proposal, to be studied with attention, is of course strongly linked to the issues mentioned in the previous bullet.

- The Digital Filter Initialisation (DFI) business (absolute, incremental, penalty-function-type, scale-selective, etc.) seems to obey the same rules at high resolution than at lower ones. The difference comes rather from the variables to be filtered but the methodology is in principle scale-invariant. It should also be noted that the spin-up problems become more acute with rapid update cycling (this pushes for initialisation) but that the risk of mishandling important structures becomes bigger if one is compromising too much with the DFI set-up (this calls for extra care). In short, it is as helpful to have initialisation at high resolution as before, but care must be taken to have a completely correct set-up.

- Comprehensive tests are needed to choose the best nesting strategy for each particular practical forecasting problem. Additionally, since no tests can ever be a hundred percent comprehensive, some spectral analysis of the sampling problems at the boundaries should be taken into account to prepare the future systems for rare but extreme cases. The timeliness of the initial conditions' refreshment should also be considered in such studies.
- The orography of the area of 'coupling' should reflect as much as possible the characteristics of the one of the coupling model.

- One should try to harmonise more the use of internal (e.g. DDHs, i.e. box-type run-time model diagnostics) and external (e.g. comparison with Large-Eddy-Simulation [LES] results) diagnostic tools, along the lines of what already exists in the Meso-NH LES diagnostics.
- One should go more and more for radar and satellite data when verifying high resolution forecasts.
- An increase in the quantity and the quality of the exchange of information within HARMONIE concerning the results of dedicated tests is dearly needed.
- International comparison studies should be reinforced, with the ambition to apply the results to the broad areas of HARMONIE activities (better diagnostics systems, links with other research communities, influence on the NWP applications, further R&D efforts).

b. Structuring issues

- The exact scale of disappearance for the need to parameterise some phenomena is still not yet known well enough. This is true for gravity waves (originating from mountain forcing and/or other sources), precipitating convection and non-precipitating convection, in this order of increasing resolution for each 'grey-zone'.
- In the first of the three above cases, the search for a 'physically sound' simulation seems hopeless. Some dynamical schemes (e.g. semi-implicit) may well alter the frequencies of the gravity waves, but since the latter are anyhow permanently generated by all imbalances in the model (physical and numerical) and at the wrong scales, it should not matter so much. More important may be the parameterisation of the wave ⇔ turbulence interaction. Here the Quasi Normal Scale Elimination (QNSE) approach (see below) may offer a new and useful track, provided it is considered as more than just a retuning of two stability dependency functions in an unchanged turbulent-parameterisation framework.
- In the second of the three above cases, the issue is delicate because of the difference in scale between 'clouds' and 'updraft' (this is the reason for distinguishing 'convection permitting' and 'convection resolving'). Some new concepts (see below) should be tried to evaluate how to minimize the inherent contradiction coming from this 'double scale' problem (currently existing for both 'parameterised' and 'resolved' approaches).
- In the last of the three above cases, we are (for NWP activities) not yet in view of the 'grey zone' for 'shallow convection' (mesh-sizes of the order of 100m), even if parameterisation methods will surely need to evolve with increasing resolution (horizontal and vertical).
- One should consider the onset of convection as one of the most fundamental problems to solve. Currently both 'parameterised' and 'resolved' approaches do lead to a too

intense initial activity, for different reasons. In the first case we do not (yet?) know how to correctly 'parameterise' the non-instantaneous aspect of the interplay between forcing and response. In the second case, there is no way to 'resolve' the manifestation of the positive feedback that exists in nature in a very controlled way (control happening indeed via sub-scale aspects).

- As resolution increases we encounter new types of problems. This is most likely linked to the fact that we are starting to resolve turbulence, i.e. going towards higher and higher amounts of energy in nature at the scale where we set our effective model resolution. A recent paper (Piotrowski et al., Journal of Computational Physics, 2009) offers an interesting interpretation of the associated challenges in terms of matching the 'model' and 'nature' representations of the anisotropy of the flow. In a simplified configuration, although probably relevant for more complex NWP-type situations, the authors demonstrate the sensitivity of model behaviour on the setup of the irreversible part of the dynamical core as much or even more than on the physical forcing. The key parameter seems to be the link between the anisotropy of 'friction' with respect to the anisotropy of the grid-mesh. This finding automatically leads to give far more importance than up to now to the 'lateral' part of the irreversible behaviour of our models. But it also indirectly indicates that more 'memory' and more 'stochasticity' will be needed in the future in order to prevent additional difficulties when reducing mesh sizes. This holds for both horizontal and vertical aspects. In fact, in the latter case, what is useful for better describing processes happening within thin atmospheric slices is detrimental by increasing the intrinsic anisotropy of grid boxes.
- The main difficulty with the above 'brief' for improving the situation (probably currently already sensitive to such items) shall probably be to find the best possible interplay between various ingredients. Those are forcings, stochastic modulation of them (by cellular automatons (CA) [cf. infra], stochastic physics or other backscatter techniques) and control of the whole impact by more sophisticated representations of lateral mixing.

- The necessary high level of complexity of dedicated surface schemes should not hinder flexibility. Staged solutions should be a necessity, if one wants to preserve the variety of applications needed to remain compatible with the high level of investment in surface schemes' development.
- More generally we need (with a long term view for implementation but a short term one for starting) a well-defined set-up for R&D constraints for the surface schemes' development and the associated data handling.
- This is even more necessary since the externalisation of surface schemes is a strong driver for common work with Academia, while we wish as transversal a use as possible for the fruits of the resulting collaborations.
- The 3D effects of radiation (sloping lower boundary condition as well as shadow effects of the clouds) ought to be first taken into account in the surface computations (this is not so difficult, in principle). This should happen well before we may envisage a sufficiently economical way of dealing with the full 3D complexity of the problem.
- The 'sea aspects' (open and frozen water alike) of the surface handling should receive an increased attention.
- Sophisticated surface schemes should be supported by corresponding developments in surface assimilation methods and availability of the data, for instance for snow and soil moisture.

- Following some reassuring experimentation and the lack of success of alternative proposals, the terrain-following vertical coordinates seem to have 'recovered' from a period where their use at high resolution was judged problematic. Of course, there is still room for improvement of modelling (e.g.) deep valleys, by working on other aspects of the models.
- One may roughly say the same from the so-called 'Davies' lateral boundary relaxation scheme. Here the issue is rather that 'transparent boundary conditions' in a compressible framework seem to be a tantalising challenge, to the point that it would probably be better to imagine LBC-related errors as part of a stochastic process rather than deterministic quantities quasi-impossible to evaluate. The magnitude of such errors is however also difficult to estimate.
- The unavoidable mismatch between some of the formulations within the coupling model and their equivalent within the coupled model is a key problem. The obvious (expensive) solutions are larger integration areas and/or variable mesh strategies. Keeping a staged approach with two LAM applications helps diminishing the scale-related side of the problem (not the one of change of number and nature of prognostic variables, though) but may exacerbate other difficulties. In any case the use of so-called scale-independent parameterisation sets may also help reducing some of the impact of the mismatch. The relevance of these issues in each NWP centre depends on the LAM size and resolutions that can be afforded locally, i.e. it depends on local computing resources.

- One important aim for designing evolutions of a validation system should be to get Academia truly interested in using it.

- The item concerning the future of the dynamical core is the one where there are the biggest divergences of opinion within HARMONIE (as well within C-SRNWP + ECMWF). It is also clear that there is a strong link with the problems of an incomplete validation system (in a partial and unstructured ensemble of results it is easier to find out the sub-set that supports this or that scientific orientation). In short, there is consensus that deep divergences exist and that we may not have all the information available to treat the problem mainly at the scientific level.
- The methodologies to try and overcome the above-listed difficulties are known. Simplifying a bit they may be named 'emergency tests', 'production tests' and 'replacement tests'. There was however no point in discussing much their relative merits as long as the medium- and long-term aims of the studies concerning the dynamical core are not sufficiently consensual (otherwise each type of tests will become instrumental to one of the competing strategic proposal). There was indeed an implicit agreement during the workshop to avoid going to this 'methodology' part of the topic without any sufficient 'safety-net' about the aims. At the same time it is important to seek a good balance between preserving the historical strengths of the ALADIN community and maintaining an open mind for the fact that methods used in other NWP communities may provide solutions for any of our problems[1].
- The problems related to the dynamical core are not isolated from those concerning other topics (in particular PDI, 4D-Var data assimilation, and computer scalability which is a key problem in some NWP centres). But their specificity is the 'central' characteristic of the dynamical core that makes in any case a 'strong bet' to decide on any evolution and/or replacement for it. One is speaking of orders of magnitude of 10

[1] e.g. for the dynamical issue, importing in the past the ideas of periodicisation, of DFI and of hydrostatic pressure vertical coordinate has turned out to be very beneficial

year and of 100 person x year once all consequences (on PDI, on data-assimilation and EPS, on post-processing, etc.) are taken into account. For the same reasons, neglecting dynamical core issues will carry a huge penalty if their treatment is postponed until it is forced by external constraints, such as an improvement in scalability required by computer technology. The details of the links with PDI can be inferred from the above and will be mentioned again later. For the 4D-Var business, the problem is the one of the tangent-linear and adjoint (TL/AD) model. In the 'incremental'- or 'regularised physics'-spirit the dynamical cores may differ between the 'forecast' model and the 'perturbation model' but the data structure ought to be the same (or as close as possible) between both. This means that separated evolutions of the dynamical core are possible but not any of them, if one wants to avoid a complete system rewriting. The issue mentioned in this bullet has to do with the Object Oriented Prediction System (OOPS) concept, but on a longer time-scale than the one of the initial step of the project, as set by ECMWF.

c. *Innovative (at least potentially so) issues*

- CAs are a tool with potentially all the needed characteristics to help minimising the detrimental impact of the 'double scale' convective problem. They keep their own 'memory' (albeit in 2D only), they can have a tunable amount of stochastic character and (thanks to their higher horizontal resolution) they encompass a specific way to describe lateral communication between adjacent grid-boxes.
- Addressing the convective onset problem in the spirit of 'parameterisation' leads to the new concept of 'virtual unresolved updrafts' (VUU): one considers that the 'deep convective parameterisation' should treat, not the whole convective event, but only perturbations around what the 'resolved' computation anyhow 'sees', at its own mesh-size-imposed scale.
- The difficulty in the above approach is linked to the ambiguous role of 'entrainment' (measure of the VUU intensity and closure for the full budgets of conserved quantities at the same time). In a quite long term perspective, the FP-MT (Fully Prognostic Microphysics & Transport) proposal possibly offers an attractive (but expensive) solution to this problem. On the other hand it remains to be seen how the 'self-selective' aspects of the competition for 'sub-grid-space' between various 'modes' will lead to realistic equilibrium solutions in FP-MT. This is especially true given the maximum of physical independence (radiation, microphysics, turbulence) given to these 1D-vertical entities (the modes).
- The recently proposed turbulence theory named QNSE places anisotropy and waves at the heart of its computations. As such it delivers solutions that tend to differ a good deal from those of the classical Reynolds averaging method. It should be investigated with well prepared tests whether this novelty significantly influences the behaviour of a model. This is especially necessary since QNSE does not offer many degrees of freedom for its implementation: both the interfacing constraints and the basic numerical values are given as absolute. They cannot in principle be subject to tuning.
- Apart from the various above ideas on how to better parameterise the 'moist turbulence + convection' ensemble in direct link with its 'dynamics + resolved phase changes' counterpart, we have a difficulty to assess the consistency of all aspects of the complex schemes needed for such a huge challenge. We know that residual instabilities may survive the end of the time step and thus trigger unrealistic behaviour at the beginning of the next one (this also has to do with the above-mentioned problem

of the PDI time-step organisation). We also know that the diagnostic conservation laws are written differently for advective and diffusive transport mechanisms, even if their separation is arbitrary in any model. Following some recent discoveries there is however hope that a new 'moist potential temperature' will soon help reducing the said difficulties. With a bit of extrapolation one may also expect to better separate the 'cloud' and 'turbulence' aspects of the parameterisation schemes, on the basis of this new quantity.

- There seems to be a need to parameterise 3D turbulence, but rather in a 2D+1D approach than with a full (LES-like) integrated scheme. Indeed, even if they must interact, the distinction between the horizontal and vertical impacts will come far more from the anisotropy of the mesh-geometry than from the one of the exact formulation leading to the various diffusive fluxes (cf. supra). It should be noted here that QNSE offers, additionally to the relative dependency of vertical exchange on vertical stability, an estimate of the relative dependency of horizontal exchange on the same quantity. This may be an additional argument for accepting the above-mentioned risk of 'rigidity' associated with a QNSE-based parameterisation. Mesh anisotropy is taken into account in more classical 3D turbulence schemes, but in a different way from QNSE.
- The stencil of the semi-Lagrangian part of the dynamical computations (provided of course that the latter are not Eulerian) seems to be well adapted for this kind of 2D+1D parameterisation. This property is already used in a 2D framework for the so-called SLHD (Semi-Lagrangian Horizontal Diffusion) lateral mixing. Given the flexibility of triggering SLHD on the basis of various criteria, there is a natural possibility of fruitful extension. SLHD is furthermore a small first step in the direction of integrating more and more all irreversible aspects of the model's time-step.

- Concerning the representation of all surface types, more research is needed for finding a replacement to the 'roughness length' concept, a step that would allow conjugating more realism with more flexibility.
- Contacts should be sought with the mountain meteorology community in order to jointly explore the problem of model's closed-valleys. In nature valleys lead either to a lake (i.e. increased surface thermal inertia) or to a flat-land area and then to sea. In models on the contrary they may be 'enclosed' by the neighbouring points' orography, a situation leading to a frequent damming of cold air, which can presently only be counter-acted by additional (and targeted?) lateral mixing.
- Another problem which becomes more and more specific as the horizontal resolution of models increases is the one of areas which are land at low tide and sea at high tide.

- Generally speaking one should aim at representing in the parameterisation trade all known sub-grid orography non-represented dynamical effects.

- There are several general directions of further evolution for the validation/verification business that received a broad support at the general level but not when the dimensioning details were at stake. The workshop being focused on modelling aspects, its participants were not necessarily specialists of verification issues. One shall first list here the consensus points: (i) need for innovation in the verification scores themselves (letting the specialised community make the job of sorting out the good ones out of a vast list of proposals); (ii) need to create a more specific model-comparison environment; (iii) need to verify and/or validate with the thought in mind that higher and higher resolution forecasts are more and more stochastic (by nature and even maybe by design). The areas of divergence in appreciation will follow later on.

B) Areas with divergence of opinions:

Simplifying a bit in order to make the situation more understandable, the discussions at Brac-HR have revealed three areas where no consensus emerged:

- the detailed priorities for the necessary evolutions of our diagnostic-validation-verification ensemble of tools;
- the main aims of an evolution or substitution of our dynamical core, if the latter would prove structurally unable to face the challenges of science and/or of High Performance Computing (HPC) technology in the future;
- owing to the various choices available for making evolve other less 'central' parts of the code (themselves in strong interaction with the dynamical core for determining the model's behaviour) the degree of emergency and/or of priority of 'taking a bet' about the said dynamical core issue.

All three items of divergence are different in their roots and manifestations. The first one is a classical problem of optimisation of too scarce manpower resources for a topic where transversal and local priorities are more often diverging than coinciding. The second one corresponds to a multi component optimisation procedure where today's truth of two rather opposite clustering of choices may or may not match what scientific results and computer manufacturers will deliver us as working environment in about 10 years from now. The third one is more about the way in which NWP operational environments should evolve, either progressively under a constant level of pressure or intermittently on the basis of windows of opportunity. In continuity with a joke made during the discussions, the choice may be here caricatured between 'doing things (a bit) wrong in a disciplined way' and 'doing things right in a (partly) uncontrolled way'.
The three items are obviously ranked here in increasing order of importance and the ways to potentially treat them also strongly differ. Let us now see each of them in more details.

a. Diagnostic-validation-verification

- We lack an overall strategy for this part of our activities: (i) no 'best score(s)' concept emerges; (ii) 'fuzzy' verification methods are progressively replacing the 'point-wise RMS' ones but the offer of novelty is wide and we have to wait for more specialised groups to pick-up the proposals that deserve surviving in the long term; (iii) the LAM inter-comparison is by nature far more delicate than that of global models (issues about geographical areas, nesting strategies, synchronisation of coupling, quality of the LBCs, …); (iv) there is a limited interest in doing heavy validation of 'static' model configurations (see the past failure of the 'COMPARE' project to foster convergence and/or selection of scientific solutions); but it is impossible to do all relevant tests for evolving configurations where major deficiencies are often corrected soon after being revealed by partial prior efforts. Some specific weaknesses, regarding convection in particular, are long-standing and they do warrant coordinated, targeted validation efforts.
- Contrary to what happens for global modelling (or for larger scales) there is an inherent contradiction between two sources of information. The first one uses 'integrated scores' that help monitoring progress via long time series and via inter-

comparison of competing solutions. The second one relies on specific scores meant to closely follow what forecasters and users require in terms of absolute quality of high resolution NWP products on the other hand. The former are necessarily reductive while the latter tend to target more and more extreme conditions, without enough sampling to be representative. And doing both types of verification or inventing compromises is of course much time-consuming.

- There is no consensus on what should be a correct reference for any scoring system:
    - ECMWF scores may be considered but, owing to the scale gap, they are more an independent information than a benchmark for the most relevant products of high resolution NWP;
    - Synthetic data (INCA-type [Integrated Nowcasting through Comprehensive Analysis]) have the opposite pros and cons (well adapted to local conditions, but far less 'safe and consistent' than what the ECMWF machinery engineers);
    - Past series of scores are less representative than at larger scales because the time needed to truly evaluate the benefit of an operational evolution is very long (too long for accepting to delay the next steps, often); it may well be impossible to demonstrate improvements to smaller-scale modelling systems without running long and expensive validation experiments;
    - Case studies' outcomes are a kind of compromise but their representativeness may always be put in doubt in fine;
    - Idealised model tests (quite underused) are more objective but they go quite away from the day-to-day forecasting worries and they are by nature more targeted towards this or that aspect of the whole modelling system.
- EPS-type scores have to be adapted to high resolution characteristics (it is important to do so, knowing that our presentation of high resolution products will be more and more probabilistic). But, as said earlier, a stochastic view of high resolution results can be obtained without the 'ensemble' tool. How then objectively comparing various probabilistic outcomes?
- Last but not least, 'meaningful diagnostics' are currently a very relative notion in our community, owing to the divergences of opinion that will be reported in the next sub-sections. One can probably agree that we mainly had in Brac-HR a thrust towards very selective argumentation, each 'camp' believing of course that this is not its own case!

All these points indicate the need and importance to put order in this part of our activities, but on the basis of a complete and consistent analysis. The aim would of course be to define and build a new integrated system as free as possible of all above caveats, on the basis of a few compromises, but not too many of them either. For example, if correctly employed (i.e. for treating phenomena really remaining 1D-vertical in their manifestations), it is important to develop a 1D version of the relevant 3D model, a step that also enables enhancing collaboration with other communities (GCSS-type) and between sub-groups of HARMONIE involved in R&D about parameterising the above-mentioned phenomena.

b. Aims for the future dynamical core

- That there was 'agreement about the fact of having in depth disagreements' is extensively recalled in the last three bullet of sub-section A-b (and consequently there is unfortunately nothing specific about 'dynamics' in A-a & in A-c).
- Additionally one may say: (i) that the discussions were extremely random (it was impossible to much structure them sub-theme by sub-theme [like spectral, semi-

Lagrangian, semi-implicit & 'others']); (ii) that they rather kept drifting back to links with other topics, especially those concerning the 'physics–dynamics interfacing business' (as short-hand for something a bit wider, which was another important topic of the workshop, see above in section A).

- The former handicap came mainly from the fact that two 'extreme solutions' immediately appeared as 'compact, consistent and easy-to-sell'.
    - The first one is the current ALADIN-NH dynamical core as implemented in AROME, tried at ECMWF as a compatible extension to the Hydrostatic Primitive Equation operational IFS. Its main characteristics are 'spectral', 'semi-Lagrangian' (SL) and 'semi-implicit' (SI). In view of a recent analysis performed at ECMWF the barrier of 'weakness in scalability' is still quite far away for this configuration. In NWP centres that try to complement global model products, the emphasis is on shorter production schedules than in global centres, so that high resolution regional models will tend to hit the scalability barrier earlier than global models. This is even more true for planned 'NWP for nowcasting' applications. There are indeed a few theoretical limitations in stability of the SI and in conservation-potential for the SL-transported quantities. But, within the type of 'safe' application chosen e.g. for AROME with only 20% of the ultimate potential in terms of length of the time step (measured on pure dynamical considerations), they probably are of little influence, is one is willing to sacrifice some forecast accuracy in favour of better numerical efficiency. And there may exist incremental-type solutions in store for improving those aspects in the future.
    - The second one is a solution that offers the exact opposite for each of the three 'key' characteristics mentioned above: finite differences for the horizontal discretisation, Eulerian advection and iterative-split algorithms (on a Runge-Kutta [RK] basis) for the time-marching scheme. Pushed to the extreme of its logic in a compressible equation system with vertical-only control of acoustic waves, this combination offers the unique advantage to be '3D-solver-free', a guarantee against any big scalability problem for future HPC systems. The purely scientific pros and cons are far more difficult to establish (see below). In technological terms, this solution is of the same family as WRF (the US community system) and as COSMO and close to what would be a Meso-NH having abandoned the anelastic approximation.
    - In the rare moments where the discussion was focussed on one single issue, it appeared that merits were usually on both sides: (I) when coupled with SL, spectral methods may use the so-called 'linear grid' and thus get free of Gibbs-type problems while hardly computing any non-linear term on the basis of the spectral decomposition; on the other hand the non-locality of the few still spectral-type terms may ultimately meet its limits with very strong orographic slopes; (II) Eulerian methods, provided the time marching scheme is made fully compatible, ensure better conservation properties; but, if kept to a limited degree of complexity, they induce some dispersion in the spectrum of the transported variables (this is why sometimes SL is kept for the passive-tracer-type variables like hydrometeors; note however that DWD has demonstrated an adverse impact of using SL for humidity advection); (III) SI is modifying the frequency of the waves it controls but those are anyhow mainly contributing to the 'noise' and not the 'signal' of the model; RK computations let the waves travel freely but they introduce a more complex interplay with other parts of the time-scheme.

- In fact, while it would be stupid to dispute that there are nowadays two rather opposite 'clustering' of characteristics, it is relatively easy to show in each case partly contradicting constraints for the march towards higher resolutions. Hence the debate about how to anticipate the situation in 10 years time might equally well be taken in a 'block against block' spirit or in a more 'analytical' way. Unfortunately this was hardly debated at Brac-HR, for reasons which will now be exposed.

- The latter handicap came from a surprising (with respect to the preparatory position papers) raise of the stakes on the issue of a possible need to 'change dynamical core' already with respect to the present situation (i.e. at 'convection permitting' resolution). It is a fact that convection-permitting forecasts are sensitive to a change of dynamical core, so it makes sense to explore what can be gained by working in this area. Of course, scientific testing of dynamical core changes must not be construed as a decision to ditch the existing core, which would require some careful weighing of the costs vs. the benefit. A naive observer might think that this amounts to risk an escalating process in which any non-dynamical proposal for improving the present situation would be deemed redundant because not necessarily compatible with the yet-to-come new dynamical core. Symmetrically, it would be absurd to forbid any long-term thinking on the dynamical core on the grounds that it might have implications on other, shorter-term tasks. Hence, it is not surprising that the debates kept coming back to other possible interpretations of the currently scrutinised weaknesses (see below). This 'divergence' of the discussion away from the pure dynamics issues was necessary, given that the consequences of a too early decision taken on the basis of a possibly too narrow evidence would be very heavy, for everyone involved. Like said during the debates, 'you can only bet what you have'.

As could anyhow have been expected, the 'debate of experts' did not lead to any firm conclusion on the purely dynamical topics. In itself this would not be a problem if some continuity would have been established with other connected issues [(A) definition of a methodology of tests, on the basis of the classification in three categories explained higher-up, and with some 'blending' of tools whenever needed; (B) starting to explore the issue about future scalability constraints; (C) extension of the concept of PDI to something wider]. It was alas not the case, as priority rightly remained on the issue of how to best track and explain the currently witnessed problems.

c. *Relative priority to be assigned to the dynamical core issue*

Items in subsections A-b, B-a and B-b already set most of the scene for this topic. The initial trigger was a discussion about the effective resolution of the model configurations. If defined as the scale at which the kinetic energy spectra change slope near their Nyquist limit, AROME would be, at all physics and PDI items unchanged, ~1.5 times less selective than Meso-NH. Hence the 'effective competitiveness' of both models could be deemed rather equivalent. Furthermore the problems encountered near the highest resolution of AROME (mostly documented by HIRLAM people) could then in some sense be attributed exclusively to dynamical features, in particular to the semi-Lagrangian advection's slightly dissipative and non-conservative character. In the evolution of the discussion it became from time to time apparent that the above 'triggering' argument may not be as central as initially presented. The core issue then appeared rather to be the definition of the respective borders between the 'dynamical' and 'physical' parts of the model and some "no man's land" in between them.

Whether the definition of the latter should be 'frozen' until issues about the dynamical core would be solved or it should evolve under the pressure of new understanding of the high-resolution challenges, indeed sometimes appeared as the true bone of contention.

The position of priority and urgency to the 'dynamical core issue' was hold by a minority of participants (the delegation of Meteo-France and [for the purely scientific aspects] the COSMO 'observer'). It was also rather monolithic in its expression (these two qualifications are not a judgement of value; they just reflect the facts of the debates). Hence, after having explained this position in the previous paragraph, it is simpler to now list all the counter-arguments raised at Brac-HR by some participants.

- Several independent results point out that the source of problems with the current AROME behaviour cannot be only attributed to alleged weaknesses in the current dynamical set-up. Those results have to do respectively with: (i) the lack of sub-grid representation for the distinction between 'permitting' and 'resolving' convection; (ii) the non-conservation of energy by the parameterisation set (strangely downplayed with respect to the non-conservation properties of the SL scheme); (iii) the too early kick-off of some occurrences of convective activity despite hopes put here in the resolved method.
- Much scientific evidence (about which there was consensus in other working groups) points towards a need to enlarge the scope of the above-mentioned "no man's land". In the case of maintaining suspicion, priority and emergency on the sole dynamical core issue, many occasions to prepare a far better system in the longer term would probably be lost. And this would happen if these issues are considered as merely 'technical' and if one regards this approach as being one-sided.
- The key argument in favour of a dominating priority for the dynamical issue takes things 'externally' and 'in block', which does not allow to estimate whether partial evolutions might be sufficient to reduce or solve the scale-selectivity problem in AROME. For instance it is known (and was shown again in Brač) that the model's reaction to choices concerning the lateral diffusion is highly non-intuitive at the finest scales in unforced convective situations. This should plead for a more nuanced interpretation of the model's effective resolution than the one coming only from the analysis of kinetic energy spectra (the issue of 'signal vs. noise' is here in principle what should help to look at the results).
- Also, the 'all or nothing' specificity of the 'purely dynamical' proposed approach does not allow to select what you would like not to put first at risk in an evolution of the dynamical core, i.e. what has deep consequences on the link with the data assimilation schemes (see the discussion about this issue at the end of subsection A-b).
- Is it anyhow right to put in the balance the essential and structural link with IFS (ECMWF will not follow any step away from the current choices for at least a few years to come) at the same level as the limited amount of information supporting the suspicion against the behaviour of the ALADIN-NH dynamical core at the AROME current resolution? This issue is crucial because the link with IFS goes far beyond the mere sharing of the ALADIN-NH dynamical core for operations in AROME and research at ECMWF. It also has to do with code efficiency, hybrid parallelisation, the expected benefits of OOPS, some aspects of data assimilation and, last but not least, the possibility to easily test new ideas about parameterisations and/or 'extended PDI features' in the context of a global model, i.e. under a larger number of geographical configurations. As mentioned earlier, a future extension of OOPS would make it easier to use different dynamical cores without endangering the software link with ECMWF.
- Even beyond that, is not the drain of human resources which would be associated with a radical change of course concerning the dynamical core incompatible with the need to improve the quality of the HARMONIE forecasting systems in the ~5 years to come?

Synthesising the above set of bullets, some Brac-HR participants were of the opinion that characterising all aspects of the debate under the simple opposition between 'dynamics' and 'the rest' is a gross oversimplification. There are specific parts of the dynamics which may contribute to the problems presently witnessed (the evidence for this is contradictory at the moment, something that was not debated enough at Brac-HR). But extending this analysis into a wish for a total overhaul of practically all components of the system without strong evidence for the need of it, or certainty of the alternative(s) to perform better would be a response which would be out of proportion in view of the limited resources available.

Needless to say, on the spot, neither of the 'parties' could convince the other one and the debate mostly ended in a kind of deadlock.

C) <u>Attempt at a synthesis for future steps</u> (in particular concerning the scalability issue that was hardly touched during Brac-HR):

Part of the problems encountered in Brac-HR had to do with confusion between the various priorities and time-scales of our actions. Further steps should introduce a better methodology for distinguishing between emergency actions, medium term production issues and long term investment.

The material collected on the Brac-HR occasion is very rich (the above report is 'selective' [yes, yes] and interested people are encouraged to look at the preparatory position papers, at the key-note presentations and at the detailed reports of working groups and of plenary discussions). In the areas of broad consensus, plans can be prepared on the basis of what has been collectively produced. In the areas of disagreement, the situation is rather contrasted:

- In the 'diagnostic-validation-verification' case, we know the constraints and the need to establish priorities as well as finding a few compromises. Since the topic touches a wide community (R&D, operational and end-users), we are now in need of 'synthetic additional expertise'.
- For the aims of the (possible) evolution or revolution of the dynamical core, we should find a way to disconnect the scientific topics from our third (and big) 'worry'. This would then allow treating the methodology issue (where opinions are hopefully less contrasted) as well as the one about scalability. For the latter, three important things were discussed during Brac-HR and should be kept in mind:
    - At a higher level than that of the meteorological community, initiatives are starting (or ongoing) to find ways to optimise various types of algorithms on massively parallel machines. The level of success of such initiatives will strongly condition the way in which we (the C-SRNWP community + ECMWF here) shall arbitrate between what will in any case be contradicting requirements.
    - The 'EPS' issue conditions the importance of the scalability issue (independent model runs are automatically parallelised).
    - The scalability issue is also technology-related. Even with the current model set-ups, the manifestations of 'saturation' will be seriously lessened in case of true 'two level architecture' (number of nodes ≠≠ number of cores) and/or of good quality networking between the computing units.

  It is not clear at this stage whether these are strong enough reasons not to devote some effort on improving the scalability of our models. Obviously, Brac-HR being a meeting of scientists, rather than of computer specialists, it could not be expected to conclude on the relative priorities between technical and scientific issues.

- For what could be qualified as 'Brac-HR deadlock', only two proposals seemed to offer some hope to introduce more science in the debate: (i) to perform the Weismann-Klemp standard dyn+phys idealised test in as many combinations as feasible; (ii) to already explore, even before concretising longer-term investments, the potential of what can be put under the generic notion of "extending the no man's land's scope"; this may mean issues about conservation properties, about organisation of the time-step, about QNSE and about heat sources/sinks projection, as well as first trials with CAs and with 'turbulence-linked input' to SLHD.

Concerning the previous paragraph the two advocated types of study may be scientifically useful but will most likely not solve the core issue, given the broader implications of the latter. One possible way out would be to start by a scientific rehabilitation of 'physics dynamics interaction issues' (as an extension of 'interfacing'). Those would cease to be considered as a mere technicality between two 'noble' activities linked to dynamical cores on the one side and to phenomenological parameterisation steps on the other side and would capitalise on the above mentioned 'first steps'. This more synthetic approach could not be seriously discussed at the workshop, since there was too much confusion between its shorter- and longer-term consequences. Yet this might be a fundamental aspect of modelling at high resolution in the coming years.

Appendix I

List of participants

AUGER Ludovic	Météo-France, Toulouse
BARKMEIJER Jan	KNMI, De Bilt
BAŠTÁK ĎURÁN Ivan	UKB, Bratislava
BENGTSSON-SEDLAR Lisa	SMHI, Norrköping
BOUTTIER François	Météo-France, Toulouse
BOUYSSEL François	Météo-France, Toulouse
BROŽKOVÁ Radmila	CHMI, Prague
DAVIES Terry	Met Office, Reading
GELEYN Jean-François	ALADIN Programme Manager, Toulouse
GERARD Luc	RMI, Brussels
HANSEN SASS Bent	DMI, Kopenhagen
HORTAL Mariano	AEMET, Madrid
KÄLLEN Erland	ECMWF, Reading
LAC Christine	Météo-France, Toulouse
KLARIĆ Dijana	RC LACE Programme Manager, Zagreb
KULLMANN Laszlo	HMS, Budapest
MAJEWSKI Detlev	DWD, Offenbach am Main
NIEMALÄ Sami	FMI, Helsinki
ONVLEE Jeanette	HIRLAM Programme Manager, De Bilt
PRISTOV Neva	EARS, Ljubljana
RONTU Laura	FMI, Helsinki
TERMONIA Piet	RMI, Brussels
TIJM Sander	KNMI, De Bilt
VÁŇA Filip	CHMI, Prague
VIVODA Jozef	SHMI, Bratislava
WITTMANN Christoph	ZAMG, Vienna
WANG Yong	ZAMG, Vienna

Appendix II

Scope of a 'Kick-off brainstorming meeting about future LAM direct modelling' to be held in Brac (Croatia), 17-20/5/2010

J.-F. Geleyn, J. Onvlee, D. Klaric, P. Termonia and C. Fischer, 2/3/10

"(*Concerning the problems in HARMONIE integrations at high resolution detected by HIRLAM*) PM, CSSI chair and French LTM agree that 'physics' for high resolution NWP (in fact we are speaking here about parameterisations, dynamics and physics-dynamics links, including interfacing issues) is a complex set of topic .../... Despite accumulation of papers and new experimental results, we are working in new territory: problems need to be tackled from several angles but with the constraint that coherent solutions need to come out; some studies had no application for years but prove to be useful now; how to find an equilibrium between research and applications, lack of observation data at small scale, .../...
Alain Ratier proposes a strategic workshop early 2011 to address issues where we need diversity of approaches. HIRLAM PM agrees but LACE PM thinks it should be organized earlier.
On PM's proposal, GA decides a two-stage process: a kick-off brainstorming meeting will be organized in May 2010 in Croatia, prepared by ALADIN, HIRLAM and LACE Programme Managers, CSSI chair and French LTM: experts on theoretical aspects from ALADIN/HIRLAM/ECMWF will be invited, in the fields of higher resolution modelling (i.e. all forecast model aspects including the links with data assimilation and EPS but not more on the latter aspects). PAC will then study the conclusions/proposals of this workshop and prepare a scientific vision workshop for early 2011."

The above extract of the Draft Minutes of the 2009 General Assembly (GA) of ALADIN Partners (held in Istanbul last November) clearly sets the background and aims of the meeting that will take place from early Monday until Thursday lunch time in Brac (near Split), on the dates mentioned in the title of this note.

Further aspects guiding the preparation and content of the meeting, decided (or precised) since GA, are the following:

- The time scale of the prospective effort is in the 5 to 10 years range.
- The core part of the discussions shall address various aspects (atmospheric and surface-linked) that are felt crucial for the progress in numerical weather forecasting at the scales of a few hundreds of meters to a few kilometres. A more detailed description of what this means in practice will be prepared during the further steps leading to the meeting in Brac. Let us just state here:
  - that the aim is clearly to take a NWP view of the challenges, without forgetting theoretical aspects about processes (as observed in nature or modelled by specific upstream research tools), but always with the NWP specific application constraints in mind;
  - that, for aspects relevant to interfacing issues (concept taken here in a broad sense), the gathering of May 2010 should try to go in the path of the positive momentum acquired in September 2008 during the so-called 'Convergence Days' in Toulouse;

- that the local organisers make their best to create a fine, stimulating and peaceful environment for the forthcoming discussions; it is hoped that this shall help ironing out any imprecision about the scope of the meeting that might still exist at the end of the preparation steps (hopefully none, of course).

- The 'variational' and 'EPS' aspects will be treated only with respect to their interaction with the deterministic atmospheric model component of the full NWP system, always with a flavour of going to high resolutions (*anything* shorter than the generally admitted limit for hydrostatic modelling, i.e. about 7-8 km of grid mesh[2]). For data assimilation (variational mostly), this means that one wishes the "best" possible direct propagation model reducing the possible errors (both mean and second-order) of the so-called trajectory and of the modelled counterpart of an observation (for the computation of the innovation information). For EPS, this means at first place to provide a model component providing the most realistic "control" run (if the latter does exist) but also the "best possible" ensemble mean ("climatology" of the ensemble). At second place, one would also wish the direct model to provide some range of variation coinciding with the observed variability of the atmosphere (probabilistic "resolution" of the ensemble).
- The core limitation to the brainstorming aspect of the meeting will be to stay within the current broad structural advantages and limitations of the IAAAAH[3] common code. It will thus be assumed that steps for optimisation currently undertaken (especially, but not only, at ECMWF) and the OOPS project (if concretised) will maintain (or even reinforce) this code's label of excellence in efficiency, in flexibility of use and in potential for generalisation to new scientific solutions. Conversely, if issues would arise where a "risk" of breaking the current (and near-future) IAAAAH framework can appear, then these issues should be listed with a minimal notice on the nature of the risk and whether a further survey of such item should be ensured.
- The Brac brainstorming event is intended as a first step only in a strategic planning process; the delivery of a more concrete 5-10 year plan is expected to take place at a later stage (after a second meeting in 2011). Nevertheless, the discussions in Brac may in some cases consider some broad issues touching the conversion of concepts into plans (critical mass considerations, cooperation methods, intermediate steps, …).
- The participation within the HARMONIE community will be upon targeted invitation, in order to find a balance between gathering a maximum amount of expertise and keeping the meeting to a reasonable size, in order to be efficient.
- Additional invitations, on top of the participation of ECMWF, will be sent to the Coordinator of C-SRNWP and to one person each for the COSMO and Met-Office Consortia.
- Participants are invited to report about their possible participation (or simply knowledge) in other cooperative projects tackling aspects that would be included or partially match the themes of this brainstorming meeting. For instance, there will be a partial overlap between our brainstorming exercise and the efforts of the recently launched COST ES-0905 action (*Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models, led by J.-I. Yano from CNRM*). This will be especially true for the action's annual specialised workshop, this year on 'Increasing resolutions and parameterisation', which will have previously been taking place in Warsaw.

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[2] this definition of the target for resolution is given in order to cover the expected targets within the whole IAAAAH community, not presuming (nor imposing) the specific targets of any particular Center or group of Centers

[3] IFS/Arpège/Aladin/Alaro/Arome/Harmonie

Appendix III

Some information about the follow-on actions of Brac-HR (compiled by the ALADIN PM)

A) The second workshop, part of the path set by the ALADIN GA on the way to some update of HARMONIE strategic documents is still planned for early 2011 and shall take place in one HIRLAM Country.

B) At its 6th Session, in Bucharest on 3-4/6/2010, the ALADIN Policy Advisory Committee (PAC) discussed the outcome of Brac-HR on the basis of a draft of the present report and of verbal accounts of the four people present at both meetings. In short, PAC:
   a. Recognises the value of progressive development (concerning issues with strong potential for either bifurcations [e.g. dynamical core] or half-competing / half-complementary solutions [e.g. simulation of convective motions]). Use of a strategy of 'replacement tests' (like in the example of the late position paper to Brac-HR submitted by the HIRLAM and ALADIN PMs) should be a key tool for that, without excluding the use of other types of tests in well targeted circumstances.
   b. Asks that positive aspects of the Brac-HR outcome which are on the right time scale should find their way towards the 2011 Workplan (plan to be common this time to ALADIN and HIRLAM).
   c. Insists on scalability becoming a transversal concern in all related scientific matters.
   d. Would like to see the many issues (some of them probably critical for future progress) currently loosely defined under Physics-Dynamics Interaction (PDI, *not meaning here any more Physics-Dynamics Interfacing*) being treated in a more coordinated and hence more constructive way.
   e. Asks a group composed by the respective members for 'dynamics' and 'physics' of both HIRLAM Management Group and ALADIN CSSI to prepare proposals in view of making up for the absence of consensus at Brac-HR on issues related to their area of competency, on the basis of the above statements, when appropriate.

C) Two topics raised (directly or indirectly) at Brac-HR remain inconclusive after the PAC Session:
   a. Concerning diagnostic, validation and verification, PAC concurred that it is an important topic, but without making any concrete proposal (it simply acknowledged the need of some more input for preparing an evolution).
   b. The consequences, for the code evolutions, of all types of other decisions have been left as a pending issue.