Coupling, nesting and initialization

Some brainstorming

P. Termonia

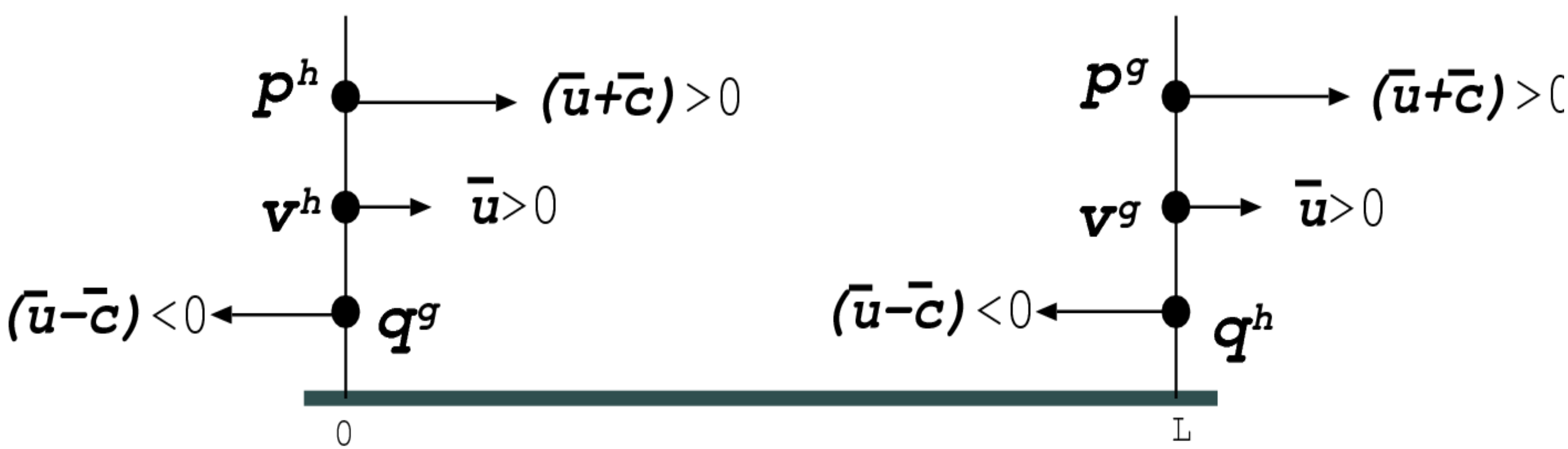
BRAC-NH

Content

- ***Open and well-posed lateral boundary conditions***: a frustrating business
  - Status (quo?)
  - The way out: a concrete proposal
- ***Nesting***: always know WHAT you do and WHY you do it.
- ***Spatiotemporal content of our forecasts***: what do we learn from initialization
- ***Physics-dynamics coupling***:
  - Why the $%^& does ARPEGE/AROME/ALADIN/HARMONIE work?
  - And to continue, some reflection about tunings, equilibria and steady states related to the reversible ↔ irreversible standpoint (no answers here)
- **Elements for consideration**

The idea is to impose the incoming characteristics

- Shallow water equations: impose the characteristics
- McDonald: implemented LBC in the hydrostatic model as multilevel slices, imposing also the characteristics



Open and well-posed LBCs within HIRLAM/ALADIN: A. McDonald (non exhaustive list)

[50] McDonald, A. 2000. Boundary Conditions for Semi-Lagrangian Schemes : Testing some Alternatives in One-Dimensional Models. *Mon. Wea. Rev.* **128**, 4084-4096.

[51] McDonald, A. 2002. A Step toward Transparent Boundary Conditions in Meteorological Models. *Mon. Wea. Rev.* **130**, 140-151.

[52] McDonald, A. 2003. Transparent Boundary Conditions for the Shallow-Water Equations : Testing in a Nested Environment. *Mon. Wea. Rev.* **131**, 698-705.

[53] McDonald, A. 2005. Transparent lateral boundary conditions for baroclinic waves : a study of two elementary systems of equations. *Tellus* **57A**, 171–182.

[54] McDonald, A. 2006. Transparent lateral boundary conditions for baroclinic waves II. Introducing potential vorticity waves. *Tellus* **58A**, 210–220.

[55] McDonald, A. 2006b. The 'Lorenz' grid comptational mode : implications for transparent boundary conditions and vertical two grid noise. *HIRLAM Tech. Report* **67**, 27pp.

Status in 3D model, McDonald

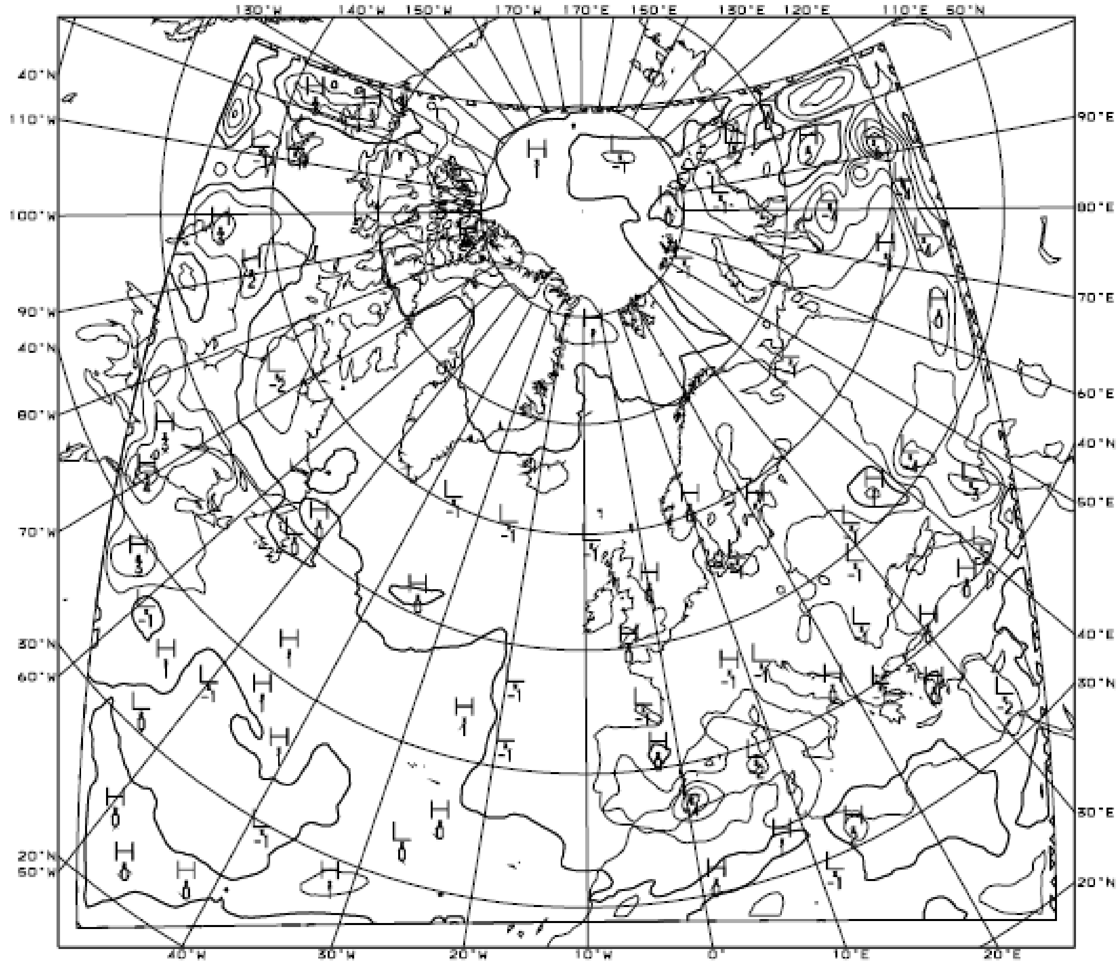


Figure 18: *The difference between the 'REF' and 'CHAR' forecasts of surface pressure at 48h displayed over the whole of the integration area.*

Verification with respect to ECMWF analyses

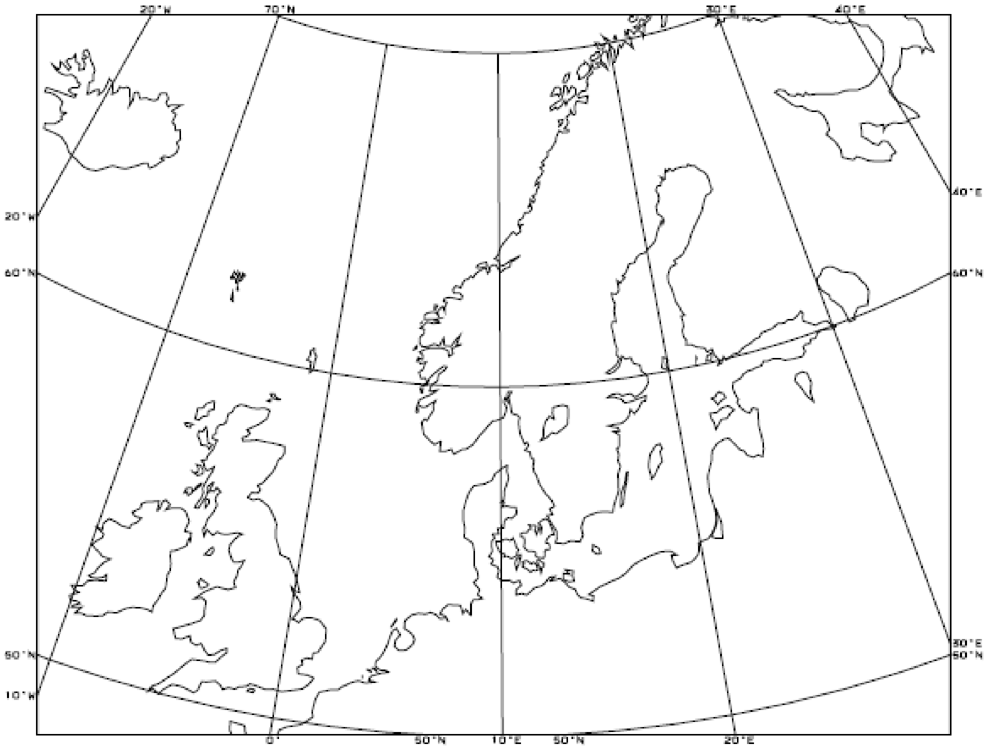


Figure 17: *The verification area*

	12h	24h	36h	48h
REF(V)	1.09	2.07	2.84	3.42
CHAR(V)	1.11	1.90	2.55	3.05
REF(F)	1.13	1.63	2.22	2.43
CHAR(F)	1.23	1.79	2.36	2.63

TABLE 2. Evolution of the root mean square c forecast and the confirming ECMWF analysis. T verification area shown in Fig. 17, and the symb shown in Fig. 18.

	12h	24h	36h	48h		12h	24h	36h	48h
REF(V)	0.80	0.97	1.17	1.30		4.59	5.45	5.73	7.70
CHAR(V)	0.80	0.97	1.16	1.34		4.59	5.47	5.77	7.95
REF(F)	0.75	0.96	1.15	1.26		3.51	4.49	4.95	6.29
CHAR(F)	0.78	0.98	1.22	1.37		3.78	4.94	5.66	7.03

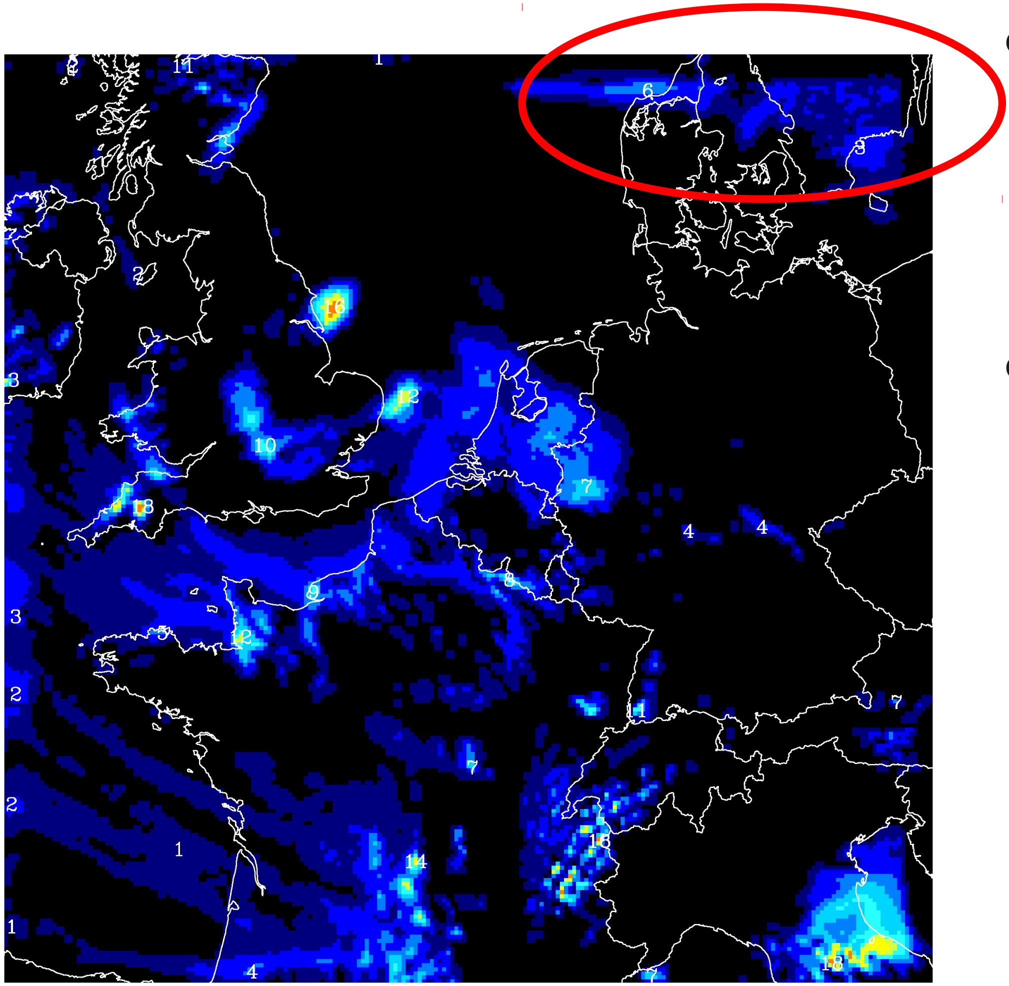
TABLE 3. Same as table 2 except this tabulates the errors for the temperature at model level 15

Conclusion

at level 15 are listed in cols. 6-9 of table 3. It is fair to say that REF and CHAR are approximately equally accurate over the verification area, but that the CHAR forecast becomes more and more inferior to REF forecast as the forecast proceeds when the error is measured over the full integration area.

- From A. McDonald, 2004: transparent lateral boundary conditions for HIRLAM. The final shallow water tests and a first multi-level implementation, HIRLAM Technical Report.

Of course the target is the gray zone + resolved scales



- Problem with deep convection trying to leave the domain
- So we need to continue the job beyond hydrostatic and characteristics

And further, Euler Equations?

- The gravity waves do not propagate along the characteristics, so we should consider the wave solutions
- For this you have to assume that the frequency is either larger then the Brunt Vaisala frequency (accoustic wave solutions) of smaller (gravity) to derive the conditions. So one has to choose. For instance, impose the correct gravity waves and hope it will work well for the accoustic ones or vice versa
- This problem is not solved yet

``Known pitfalls” to which we don't have solutions yet

- The problem of the **drift**
- The **corner** problem
- The problem of the **truncation of the trajectories**
- There may be a **critical level of spurious gravity waves** where the well posed LBCs break down
- The “working” solution of Aidan is mathematically not sound. The **hydrostatic equations are not well posed**. Aidan had to do some ``tweaking'' to make it work

So for methodology this implies

- That the only way to make progress is to proceed from simple models to more complicated ones (i.e. include progressively the pitfalls and master them)
- If we would ever succeed in mastering all the mentioned problems, it will take a long time, probably not in the time period 5 to 10 years.
- Additionally, almost no one is currently, within the two communities (HIRLAM/ALADIN), working on the formulation of the LBCs. (Compare that to data assimilation). One could say that the success of the Davies scheme (and the fact that the community was not forced to work on it) created a serious weakness today.

Spectral models?

- To have stable open boundary conditions we need to impose them within the Helmholtz solver, but there we are in spectral space
- Two ways have been investigated (thesis of F. Voitus):
  - Impose them within the iteration of the *iterative scheme*. Problem: have to iterate anyway, even if it is not necessary for stability reasons.
  - *Extrinsic LBC's*, i.e. compute them outside the dynamic core.

Results: iterations with trajectory truncation

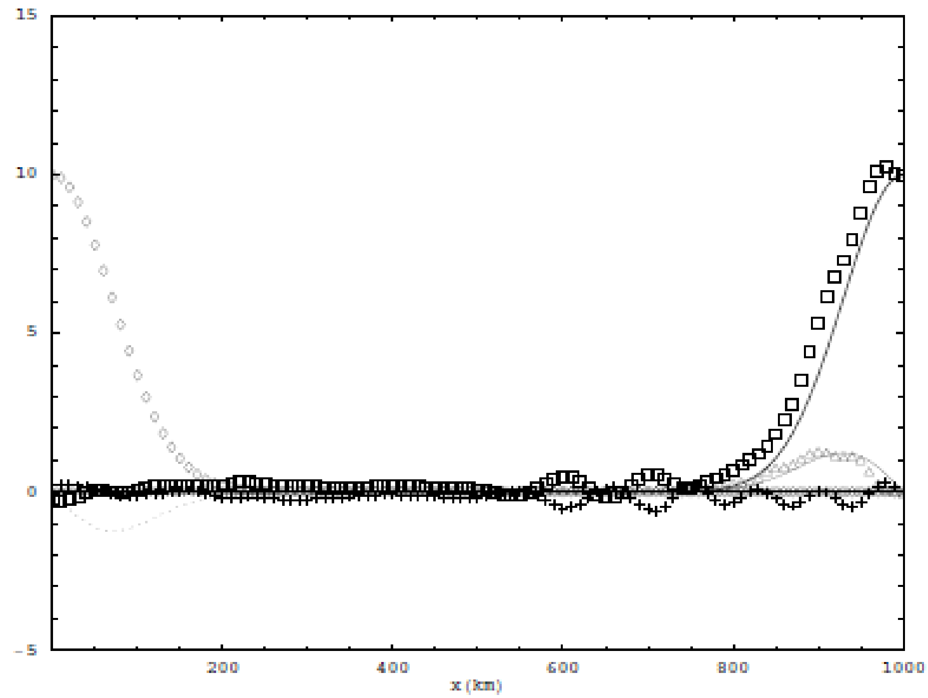


Figure 5: *Advecting solution of Eqs (1)-(3) with initial condition given by Eqs (45) and* p*,* q *and* v *are imposed at boundaries, using 'trajectory truncation method'. At time zero* Φ *is shown by the diamonds,* u *by the '+', and* v *by the dot line. The analytical solution at time* T *for all three fields are shown by symbol-free lines. The result of integrating with* $\Delta t = 416.667\, s$*,* $\bar{c} = 300\, ms^{-1}$*,* $\bar{u} = 100\, ms^{-1}$*,* $\Delta x = 10\, km$*, and* $N_{iter} = 4$*, is displayed at time* (T) *as squares for* Φ*, '×' for* u*, and dots for* v *(the E-zone has been dropped)*

The problem of the trajectory truncation: well-posed buffer zone and substepping

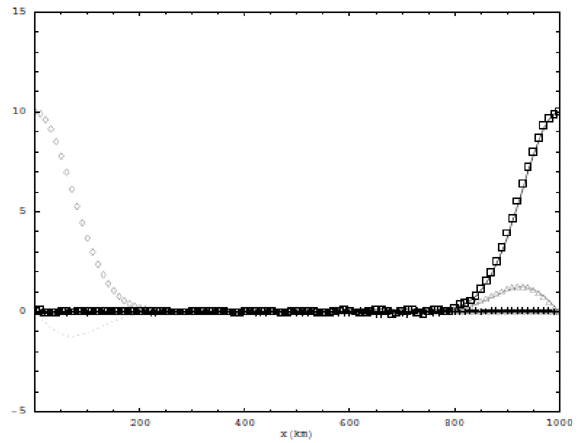


Figure 6: *Same as Fig. 5, but using 'well-posed buffer zone' on the boundary instead of 'trajectory truncation'*

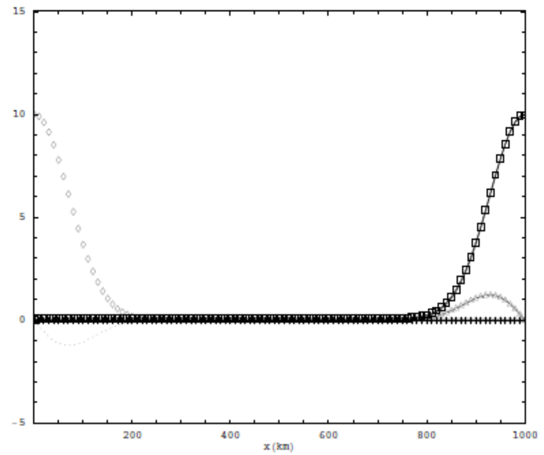
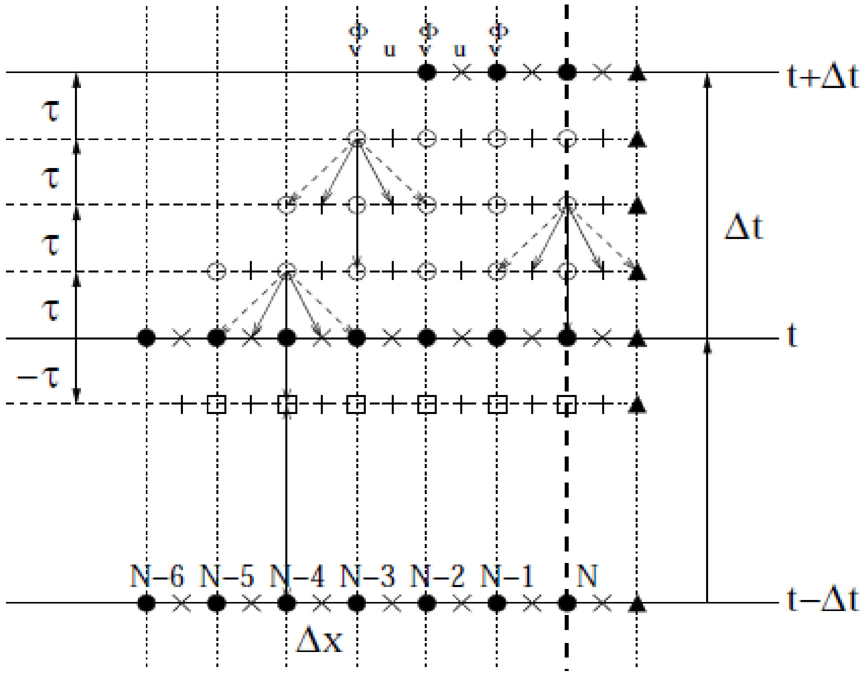
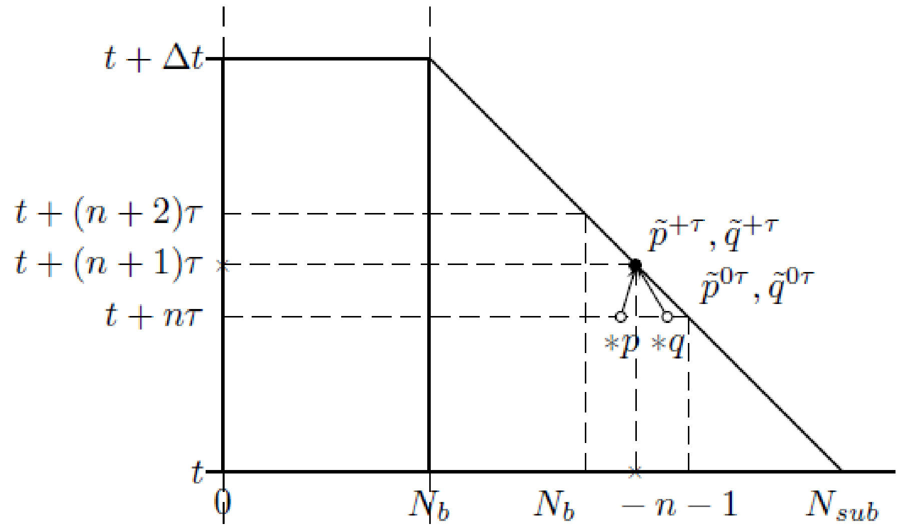


Figure 7: *Same as Fig. 5, but using 'substepping inflow scheme' on the boundary instead of 'trajectory truncation'*

Extrinsic LBCs





- Code a dynamic core that computes the time step parallel to the 2TL SISL scheme. Use an explicit gridpoint scheme, so imposing the LBCs is easy (you don't have to mess within the Helmhotlz solver).
- Termonia and Voitus, 2008: ***it doesn't matter what scheme you use for the extrinsic LBCs!***
- Additionally, if we write this scheme in the basis of the characteristics and use a foreward/backward scheme, it is stable (the SW) and we do not need substepping (but substepping helps to solve the problem of the trajectories).

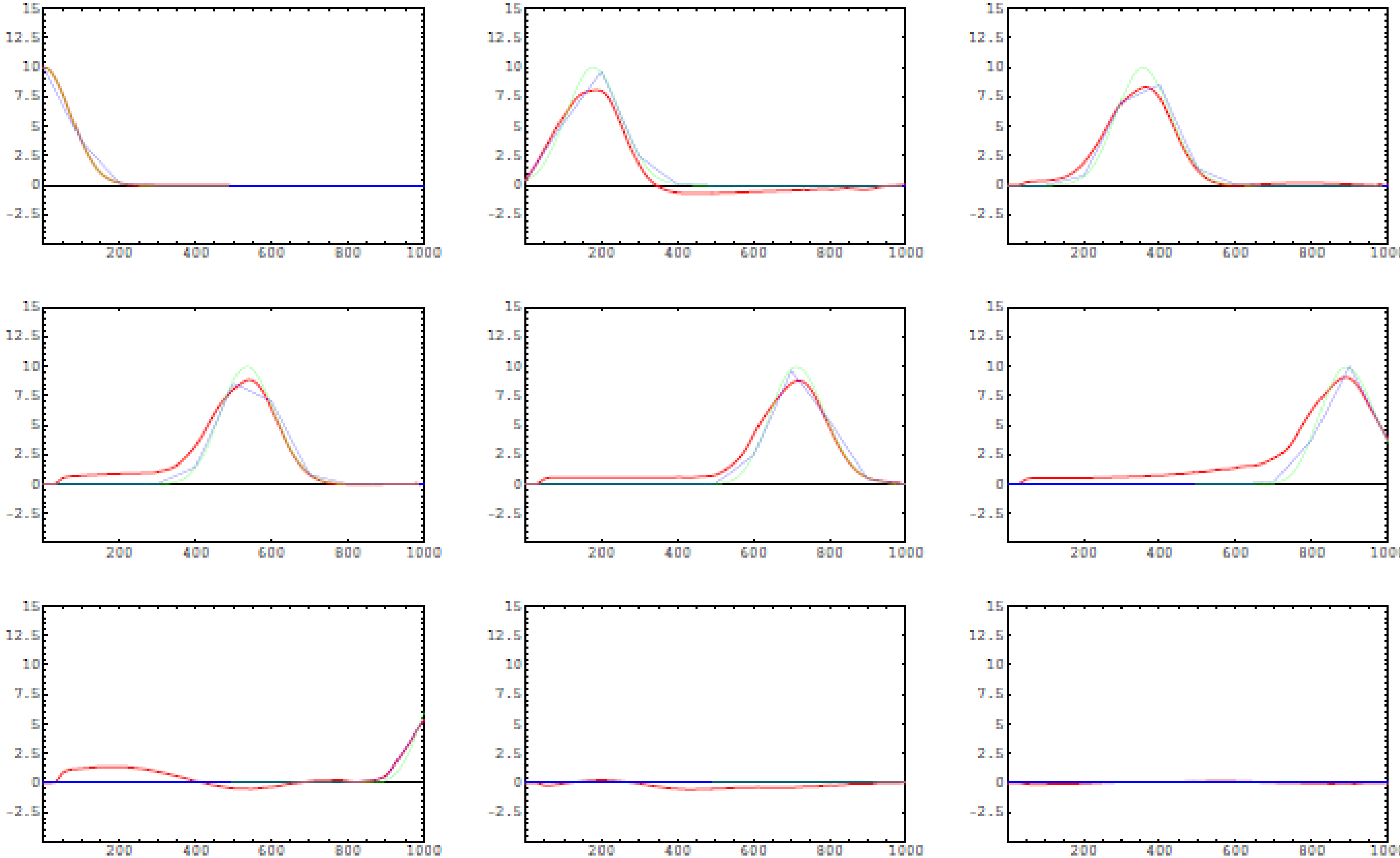
So on top of the list of pitfalls identified in Dublin

- In 3D we only have Aidan's tests with a multilevel implementation of the characteristics. When going to NH (i.e. high resolution) we need well-posed LBCs for the *Euler equations:* the solution is not known
- *In the spectral model things are even more complicated*. The iterative solution puts constraints on the use and the extrinsic LBCs still exhibit some problems with the application of some operator (the so-called **Q** operator)
- So this has become a ***frustrating business***.

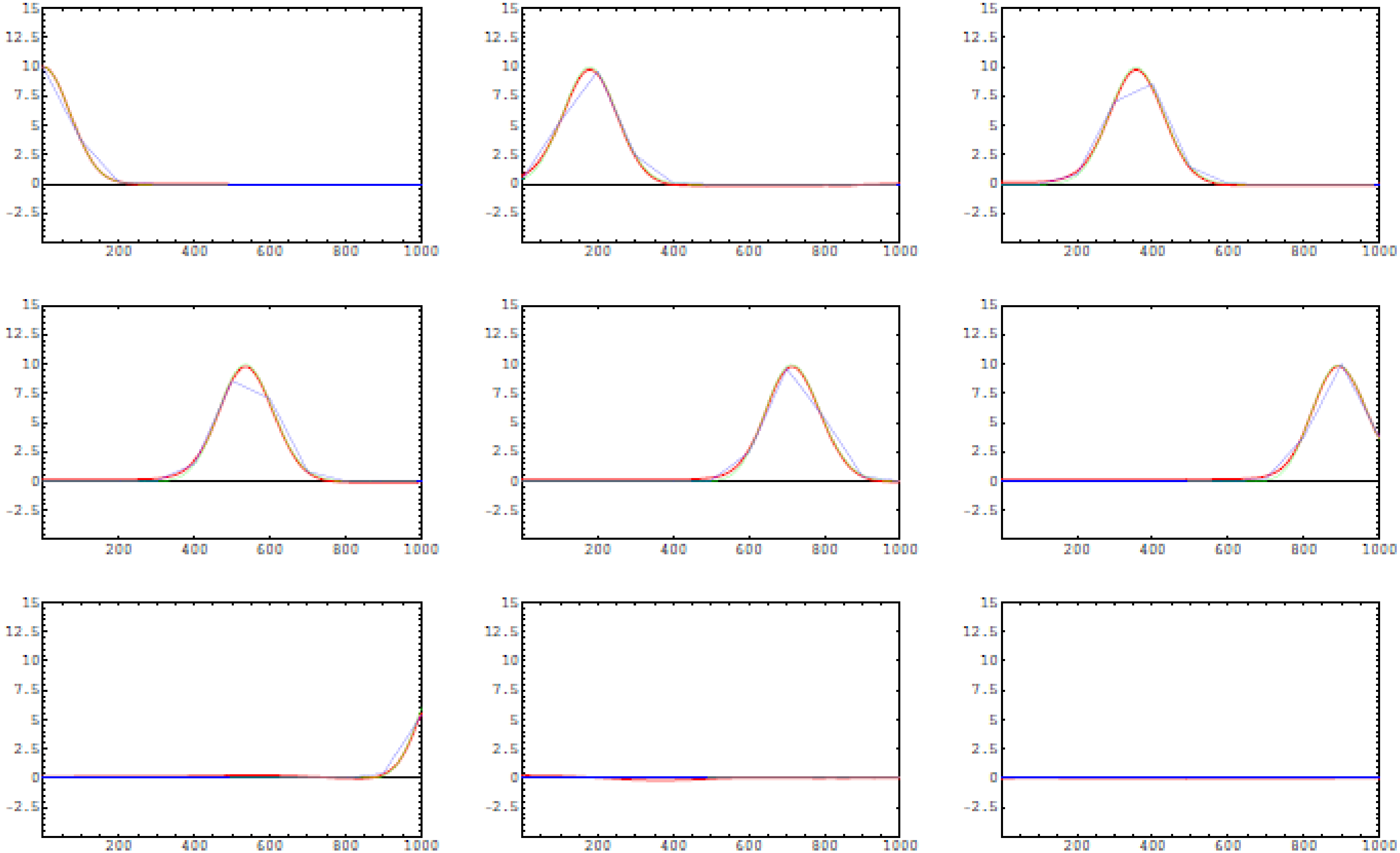
The way out of the dilemma

- Nevertheless we learned something.
- We know what we should impose: we can compute it (the characteristics) to some degree of accuracy. At least Aidan's work shows it can lead to something that is roughly equivalent to Davies (be it sometimes less good).
- Can this information be used?

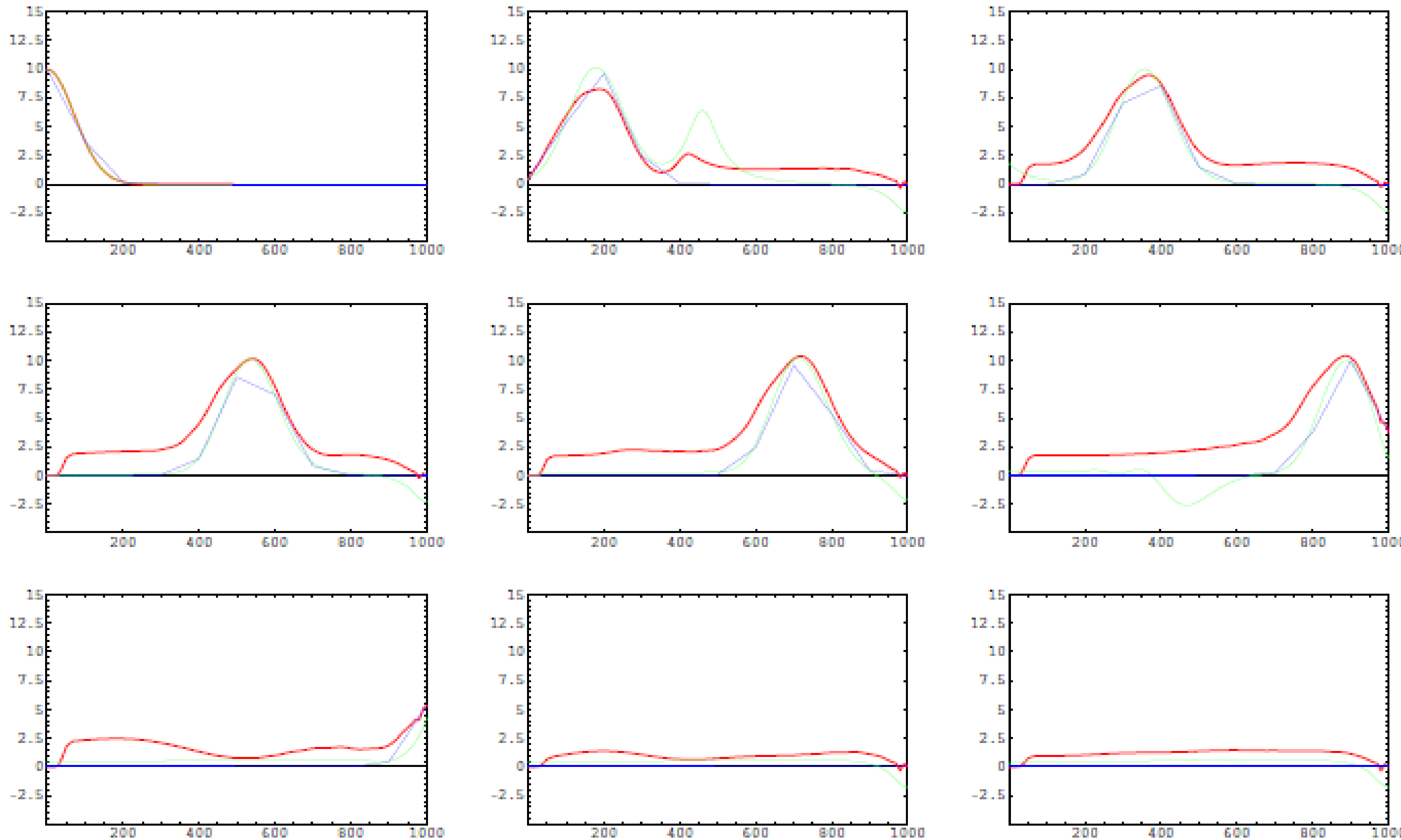
Davies, no orography



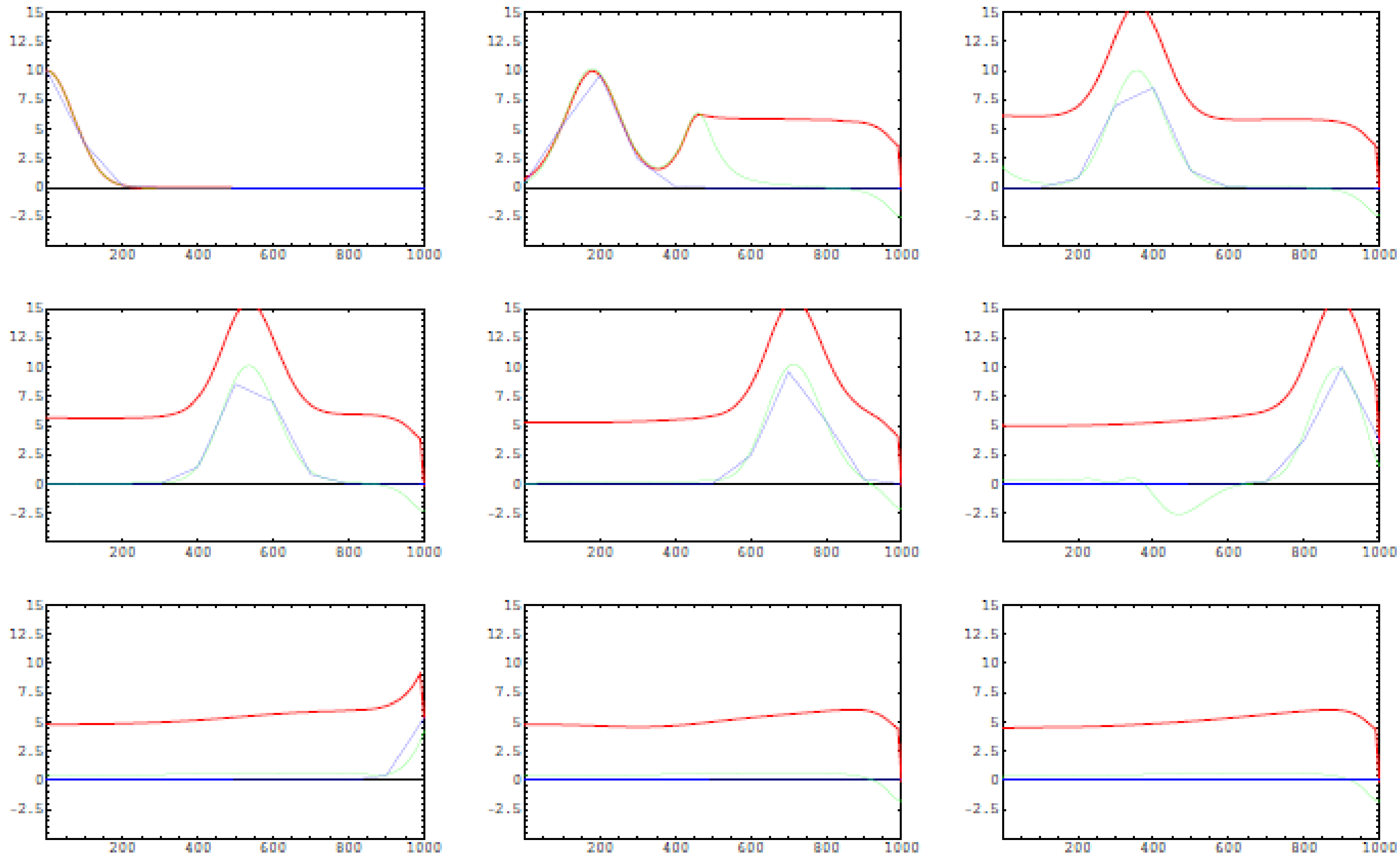
Open boundaries, no orography



Davies, with orography

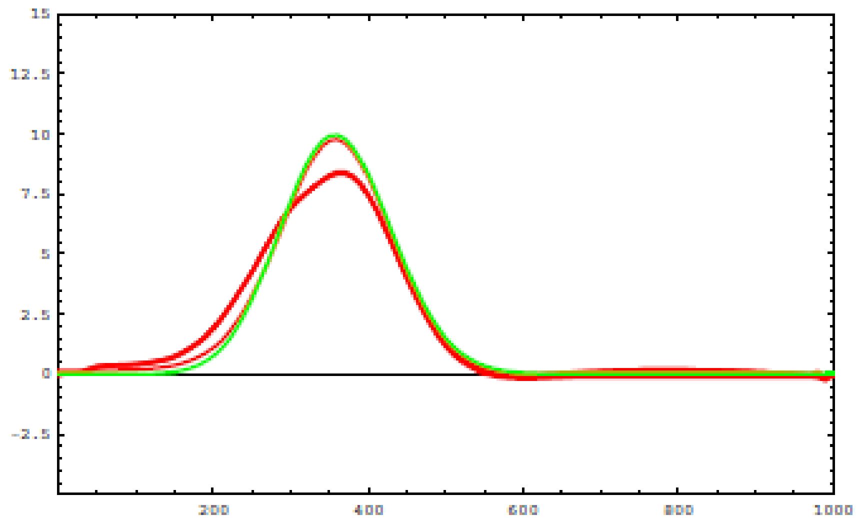


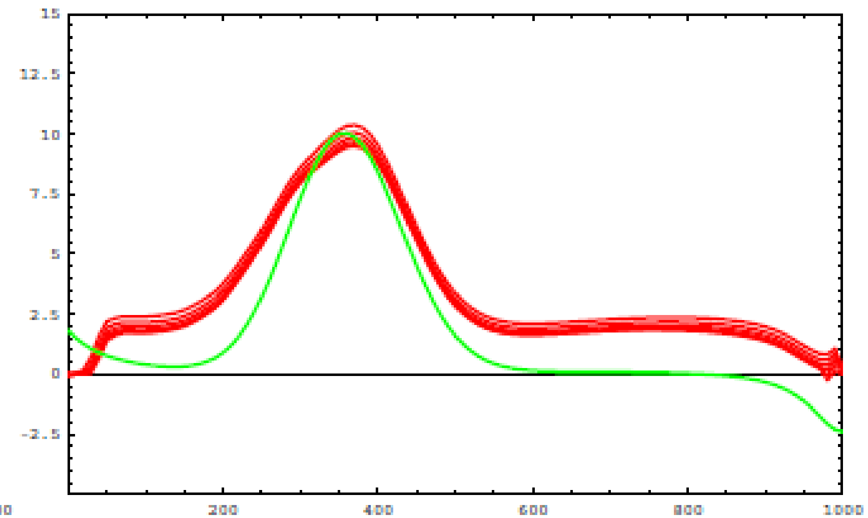
Open, with orography



An ensemble, orography

$$X_{perturbed} = X_{global} + \sigma\,(X_{open} - X_{global}) = X_{global} + \sigma\,\delta X$$
$$X_{coupled} = (1-\alpha)X_{timestep} + \alpha\,X_{perturbed} \quad (1)$$





An EPS approach

- Use
  - Aidan's work as a sort of proof of concept: this shows that imposing the characteristic is, at least in the hydrostatic model, not entirely crazy.
  - Extrinsic LBC: the details of the numerical scheme that compute the incoming and the outgoing signal is irrelevant
- And use the incoming and outgoing parts as perturbations in an ensemble system
- This corresponds to adding perturbations corresponding to ``model error'' in this case of the LBC's
- Then the fact that the pitfalls are not solved, is not important, we only estimated the model error with respect to our stat-of-the-art knowledge of LBC's

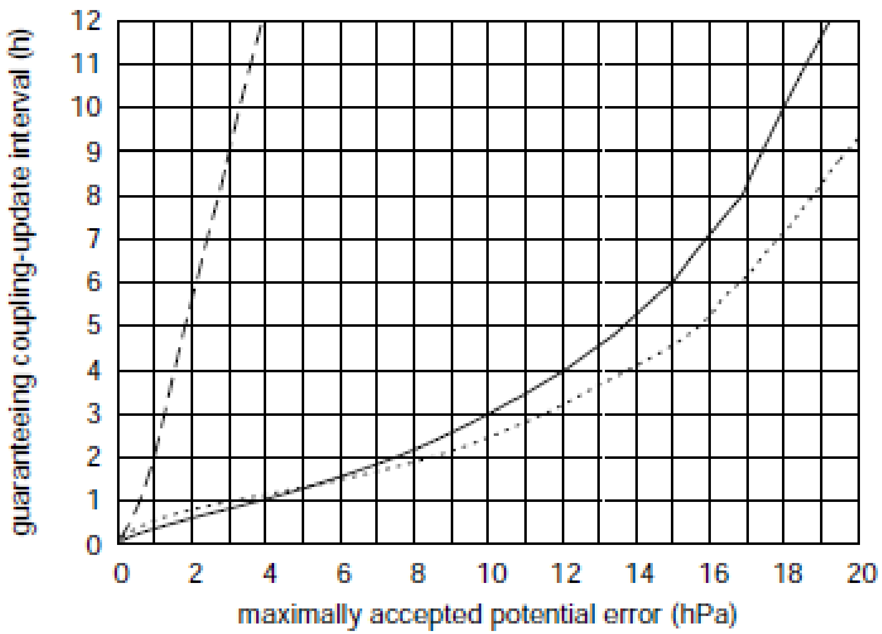
Proposal for a plan

- Develop a ``local'' dynamical core, e.g. on A grid with Galerkin finite elements … to be discussed.
- Don't use this ***within*** the model, don't couple to physics, but only compute the incoming and outgoing modes, as best as you can. Do this in an equivalent way as Aidan did in HIRLAM
- Use these modes to **cook up perturbations** (ideally they should only contain the outgoing modes) of the boundary fields and put them ***under*** a Davies relaxation within the members of an LAM EPS.
- Then ***continue the research*** (the Euler equations, pitfalls, …) as planned and try the master them
- This changes the valorization of the research. Instead of embarking on a dead-end adventure, progress in LBCs will serve to ***reduce the spread*** in the ensemble. Ideally, ***long-term*** research would then converge to exact open LBCs.

Nesting:

Know what and why

What coupling interval should we use?



Termonia, Deckmyn, Hamdi, 2009, MWR

- *But in normal cases 3h is OK.*
- *To have a guarantee that we never make an interpolation error of more than 1 hPa we would need to couple with about* **15-20 min** *intervals!*
- *With 1-h intervals we could get 4 hPa.*

Interpolation and MCUF

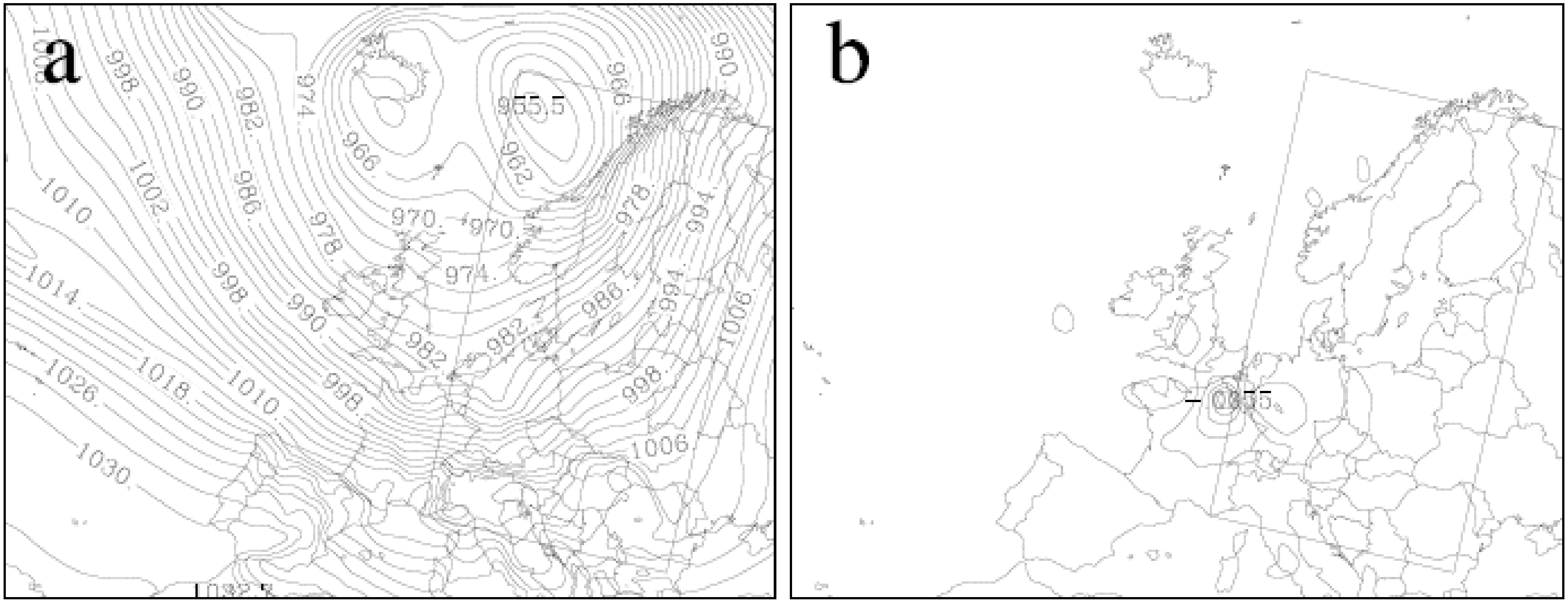
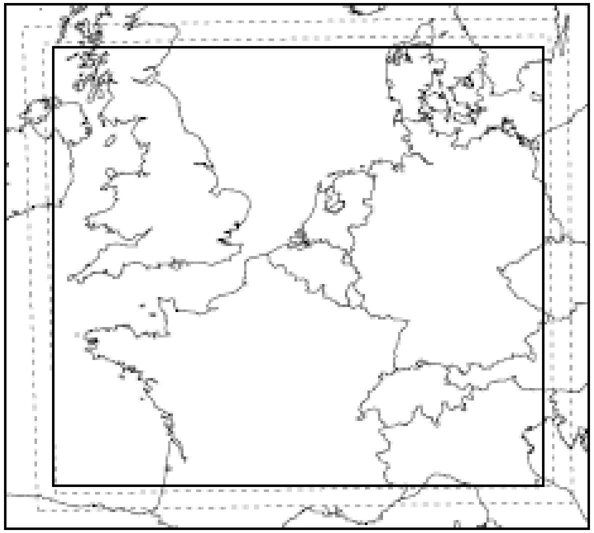


Figure 2: An illustration of the temporal resolution problem: (a) The temporal interpolation between the 0600 UTC and the 1200 UTC of the 40-km resolution host-model output, taken in the middle of the time interval, i.e. 0900 UTC and (b) the high-pass filtered logarithmic surface pressure (Termonia, 2004), MCUF field at 0900 UTC with a 3-h cutoff.

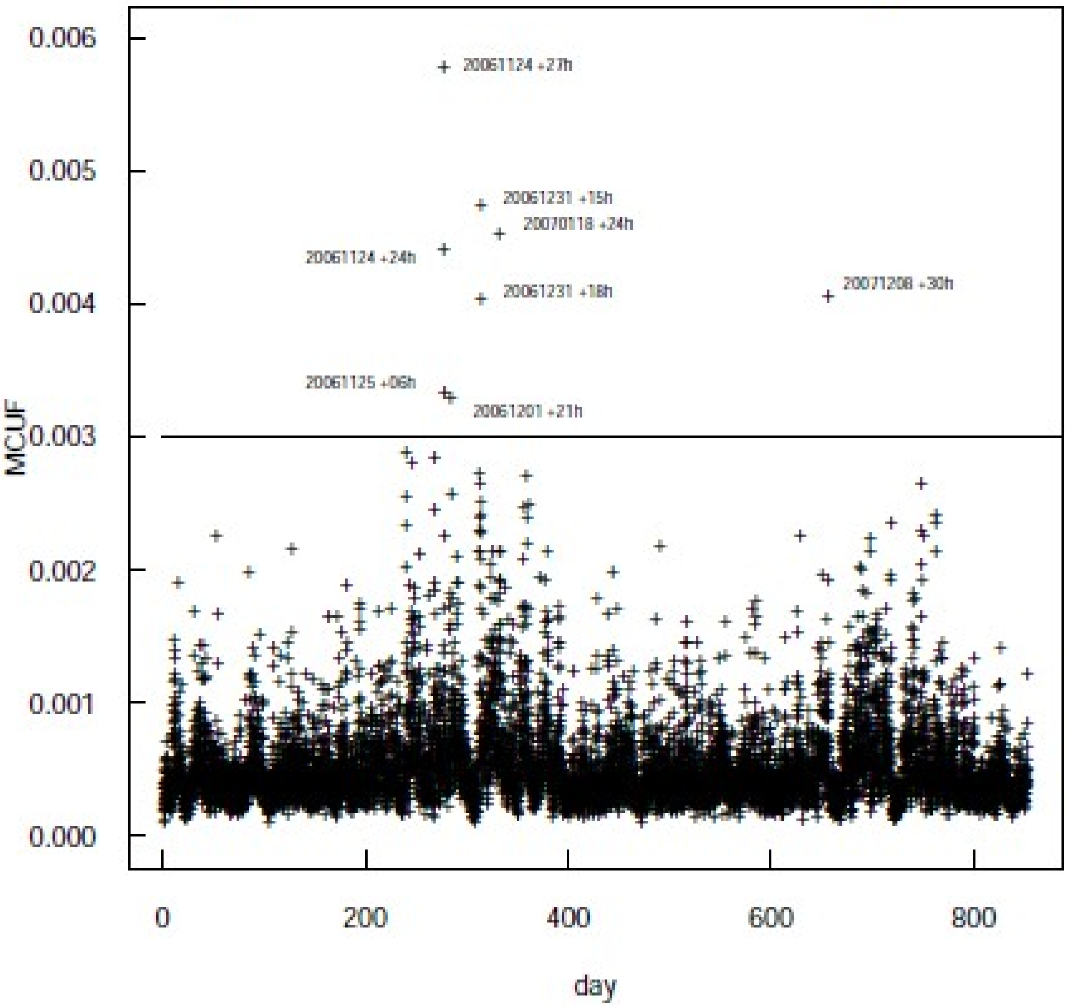
MCUF

This MCUF field is operationally computed in ARPEGE and written to the coupling files of the ALADIN models. We considered it in the frame (solid line) covering the Davies zone (dashed),



The maximum MCUF in the frame

in the period 21 February 2006 – 30 June 2008



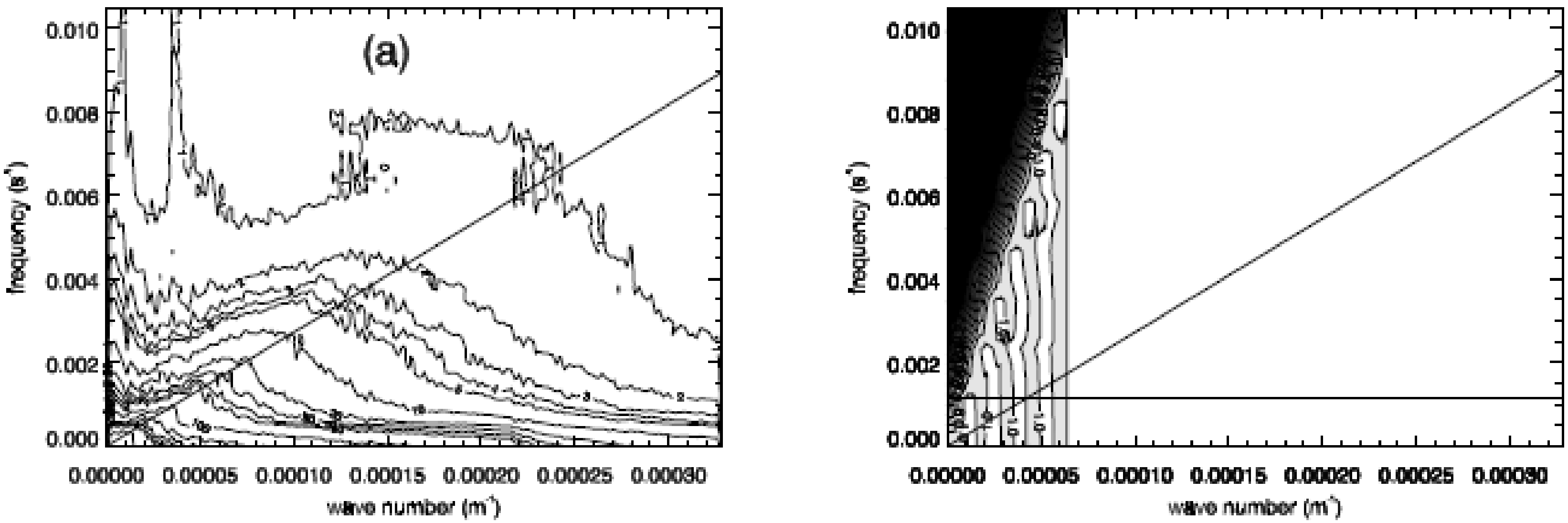
Let us consider a threshold value of 0.003. Then we had 8 alerts.

Going to km scale

- I am not going to repeat what you can do in case you detect a boundary error
- However this monitoring allows you take action (WHAT) for a particular reason (WHY): ***Know what you do and why you are doing it***. (it practice this is an ideal, but it is worthwhile to state this)
- So far we have a way to detect sampling errors at the boundaries for incoming storms
- It might beneficial to tests for instance fields that are relevant at high resolutions, e.g. CAPE.

Filtering

Scale-selective low-pass windows



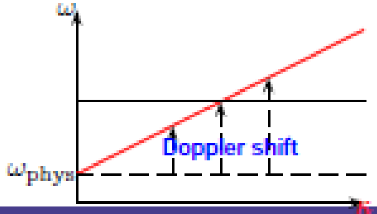
The *scale-selective* cut-off frequency of a low-pass Lancsoz filter:

$$\omega_c(\kappa) = \begin{cases} \omega_c^0 + \frac{\kappa}{\kappa_c}\left(\frac{\pi}{\Delta t} - \omega_c^0\right) & \text{if} \quad \kappa \le \kappa_c \\ \frac{\pi}{\Delta t} & \text{if} \quad \kappa > \kappa_c \end{cases}$$

The cut-ott period is $T_c^0 = 2\pi/\omega_c^0$ while the *slope* of the cut-off frequencies is $c = \pi/(\kappa_c \Delta t)$.

going to higher resolution

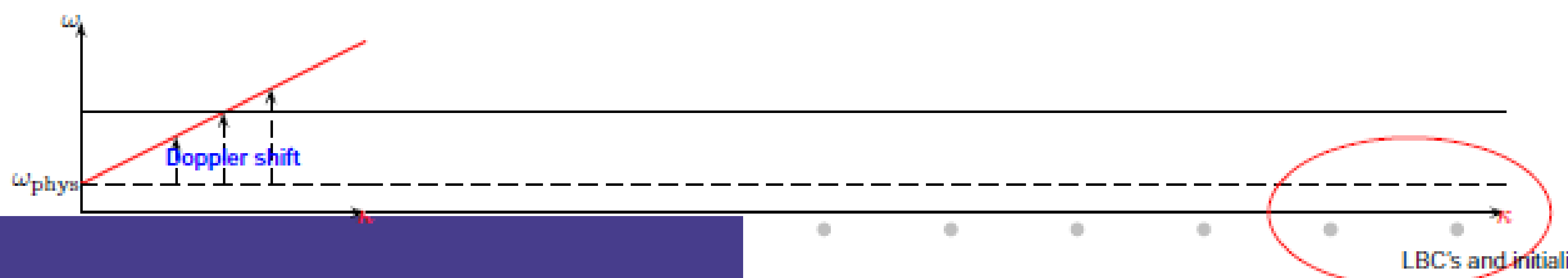
ALADIN$_{10\mathrm{km}}$ $\rightarrow$ AROME/HARMONIE$_{2\mathrm{km}}$: problems will start already with smaller ($5\times$) propagation speeds:



going to higher resolution

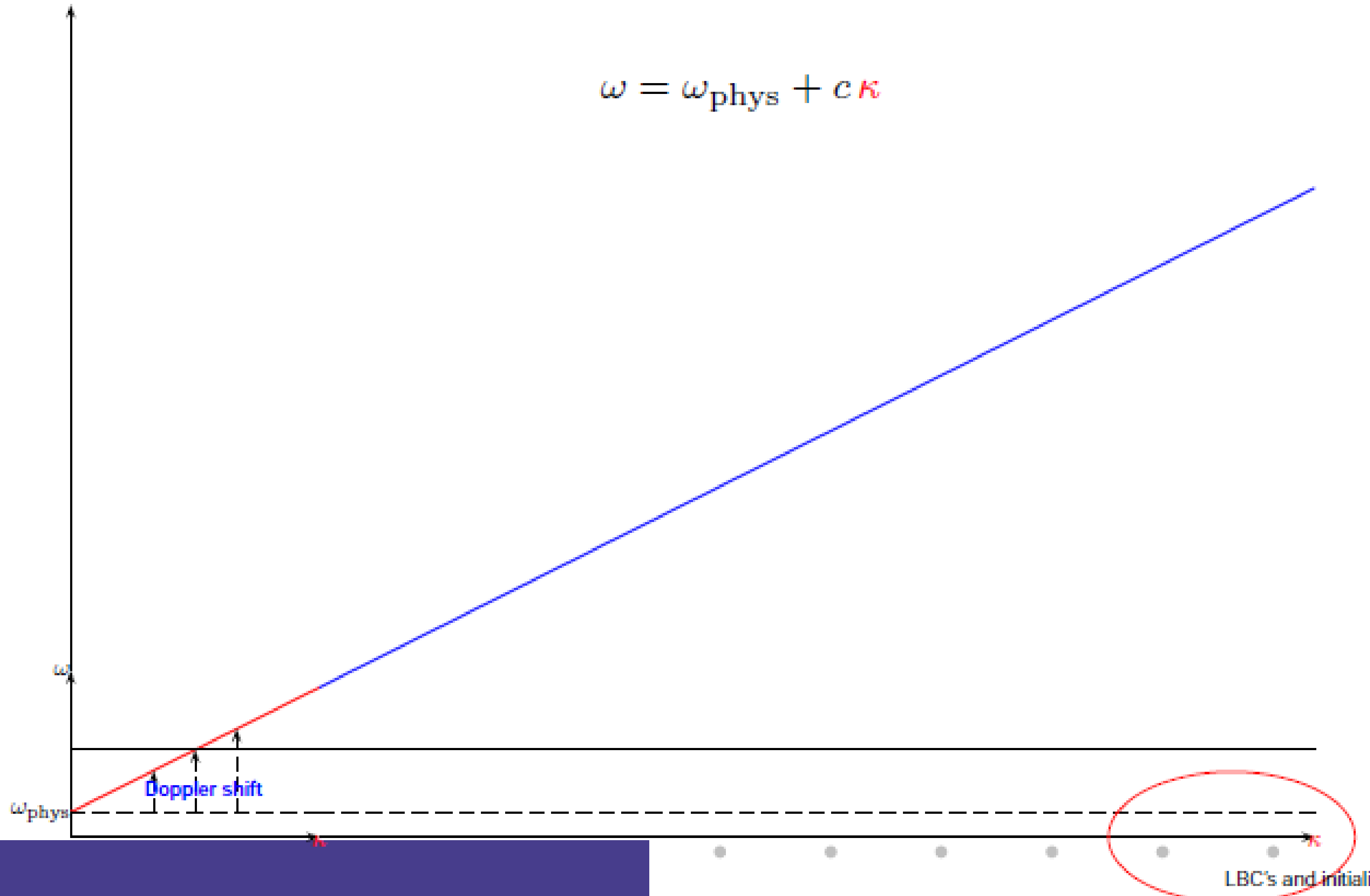
ALADIN$_{10\mathrm{km}}$ $\rightarrow$ AROME/HARMONIE$_{2\mathrm{km}}$: problems will start already with smaller ($5\times$) propagation speeds:

$$\omega = \omega_{\mathrm{phys}} + c\,\kappa$$



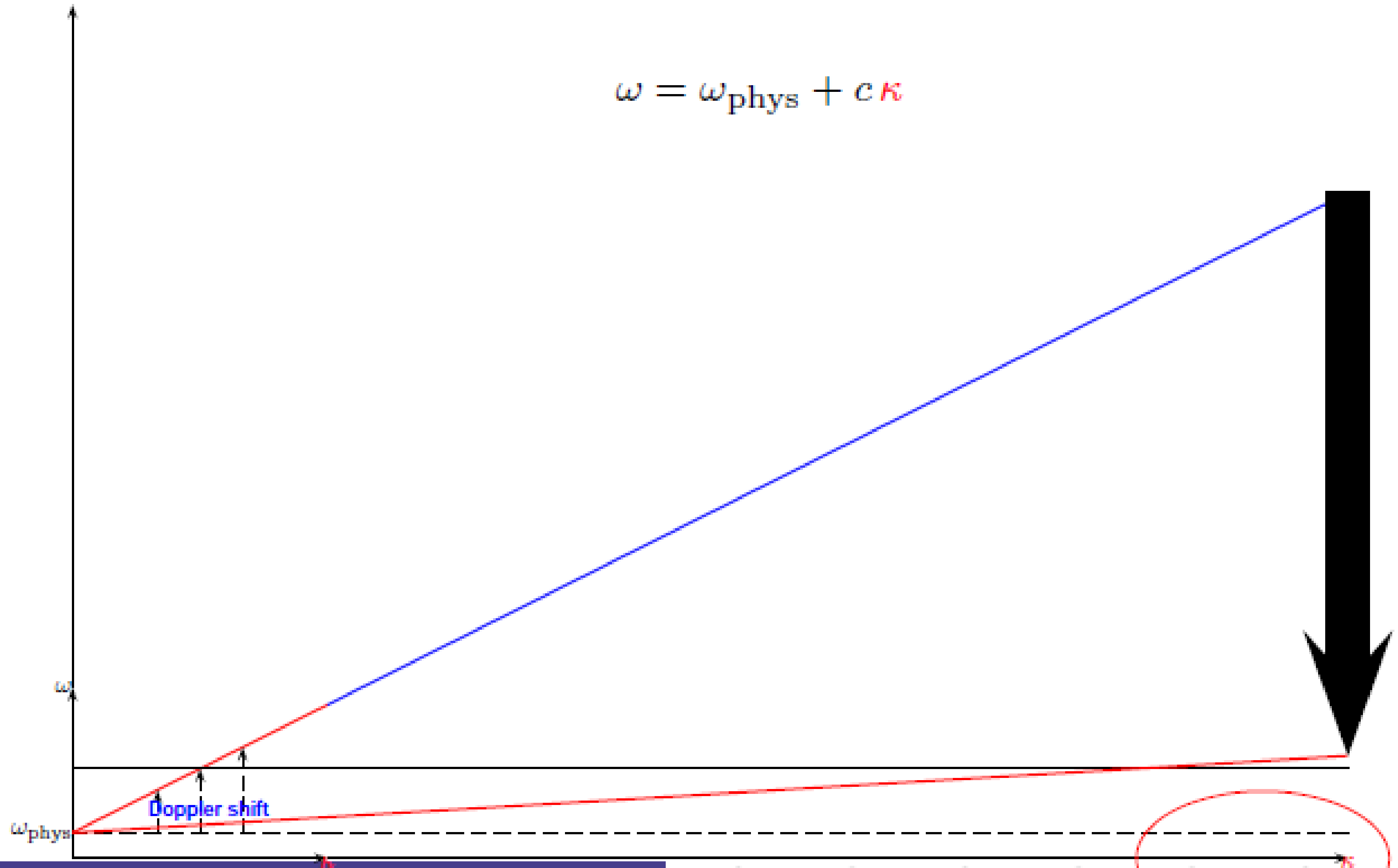
going to higher resolution

ALADIN$_{10\mathrm{km}}$ $\rightarrow$ AROME/HARMONIE$_{2\mathrm{km}}$: problems will start already with smaller ($5\times$) propagation speeds:



going to higher resolution

ALADIN$_{10\mathrm{km}}$ → AROME/HARMONIE$_{2\mathrm{km}}$: problems will start already with smaller (5×) propagation speeds:



What are we filtering today

And what will we be filtering in the future?

Time step organization

- Let us start for a ridiculously simple system, and treat it as we do in NWP:

$$\frac{\partial F}{\partial t} + U\frac{\partial F}{\partial x} + \mathrm{i}\omega F = -\sum_{\alpha=1}^{N-p}\beta_\alpha F + \sum_{\alpha=p+1}^{N} R_\alpha \mathrm{e}^{\mathrm{i}(kx+\Omega_\alpha t)}, \qquad (2)$$

Table IV. Simplified model frame for parallel interfaces based upon the structure of the model codes of the AAA models. The acronyms in the left column denote the type of space, spectral (SP) or grid-point (GP), in which the computation is performed.

SP	Derivatives	$\partial_x^r F_\Lambda = (\mathrm{i}k)^r F$
Inverse spectral transform		
	Physics (level I)	$\frac{G_\alpha - F_\Lambda^0}{\Delta t} = \xi_\alpha\phi_\alpha[F_\Lambda^0, G_\alpha], \quad \alpha = 1,\ldots,M$
	Coupling	$F_\Lambda^1 = F_\Lambda^0 + \Delta t\sum_{\alpha=1}^{M}\frac{G_\alpha - F_\Lambda^0}{\Delta t}$
	Interpolation	$F_D^1 = \mathrm{e}^{-\mathrm{i}kU\Delta t}F_\Lambda^1$
GP	Explicit dynamics	$F_\Lambda^{exp} = \left(1 - \frac{\mathrm{i}\omega}{2}\Delta t\right)F_D^1 - \frac{1}{2}(\omega - \omega^*)\Delta t\, F^{(0)}$
	Full TL first guess	$\tilde{F}^+ = F_\Lambda^{exp} - \frac{1}{2}\omega^*\Delta t\, F_\Lambda^1$
	Physics (level II)	$\frac{G_\alpha^{exp} - \tilde{F}^+}{\Delta t} = (1-\xi_\alpha)(1-\nu_\alpha)\phi_\alpha[\tilde{F}^+; G_\alpha^{exp}];\ \alpha = 1,\ldots,M$
	Coupling	$G_\Lambda^{exp} = \tilde{F}^+ + \Delta t\sum_{\alpha=1}^{M}\frac{G_\alpha^{exp} - F^+}{\Delta t}$
	Subtract first guess	$F^{fg} = G_\Lambda^{exp} + \frac{1}{2}\omega^*\Delta t\, F_\Lambda^1$
Direct spectral transform		
SP	Implicit dynamics	$F_\Lambda^{dyn} = \left(1 + \frac{\mathrm{i}\omega^*}{2}\Delta t - \Delta t\sum_{\alpha=1}^{M}(1-\xi_\alpha)\nu_\alpha\phi_\alpha^{imp}\right)^{-1} F^{fg}$

Table V. Simplified model frame for sequential interfaces based upon the structure of the IFS model. The acronyms in the left column denote the type of space, spectral (SP) or gridpoint (GP), in which the computation is performed.

SP	Derivatives	$\partial_x^r F_\Lambda = (\mathrm{i}k)^r F$
Inverse spectral transform		
	Physics (level I)	$\frac{G_\alpha - G_{\alpha-1}}{\Delta t} = \xi_\alpha\phi_\alpha[G_{\alpha-1}, G_\alpha];\ \alpha = 1,\ldots,M;\ G_0 = F_\Lambda^0$
	Interpolation	$F_D^0 = \mathrm{e}^{-\mathrm{i}kU\Delta t}F_\Lambda^0, \quad T_D = \mathrm{e}^{-\mathrm{i}kU\Delta t}\frac{G_M - F_\Lambda^0}{\Delta t}$
GP	Explicit dynamics	$F_\Lambda^{exp} = \left(1 - \frac{\mathrm{i}\omega}{2}\Delta t\right)F_D^0 - \frac{1}{2}(\omega - \omega^*)\Delta t\, F^{(0)}$
	Full TL first guess	$F^+ = F_\Lambda^{exp} - \frac{1}{2}\omega^*\Delta t\, F_\Lambda^0$
	Physics (level II)	$\frac{G_\alpha^{exp} - G_{\alpha-1}^{exp}}{\Delta t} = (1-\xi_\alpha)(1-\nu_\alpha)\phi_\alpha[G_{\alpha-1}^{exp}, G_\alpha^{exp}];\ \alpha = 1,\ldots,M;\ G_0^{exp} = \tilde{F}^+$
	Coupling	$F^C = T_D\Delta t + G_M^{exp}$
	Subtract first guess	$F^{fg} = F^C + \frac{1}{2}\omega^*\Delta t\, F_\Lambda^0$
Direct spectral transform		
SP	Implicit dynamics	$F_\Lambda^{dyn} = \left[1 + \frac{\mathrm{i}\omega^*}{2}\Delta t - \Delta t\sum_{\alpha=1}^{M}(1-\xi_\alpha)\nu_\alpha\phi_\alpha^{imp}\right]^{-1} F^{fg}$

The result (Termonia and Hamdi, QJRMS, 2007)

$$(\beta + \mathrm{i}\omega)^2 \left(\frac{F^{\text{discrete}}_{\text{steady}}}{R} - \frac{F^{\text{exact}}_{\text{steady}}}{R} \right) = \left[\frac{1}{2}\xi\omega^2 - \mathrm{i}\beta\omega(\nu-1)(\xi-1)^2 + \beta^2\{\mu_{1,I}\xi^2 + \mu_{1,II}(\nu-1)^2(\xi-1)^2\} \right] \Delta t + O(\Delta t^2). \tag{33}$$

$$(\beta+\mathrm{i}\omega)^2 \left(\frac{F^{\text{discrete}}_{\text{steady}}}{R} - \frac{F^{\text{exact}}_{\text{steady}}}{R} \right) = \left[\beta^2\{\mu_{1,I}\xi^2 + \mu_{1,II}(\nu-1)^2(\xi-1)^2 - \eta_1\xi^2 + \eta_2(\nu-1)(\xi-1)^2\} + \mathrm{i}\beta\omega\{(\nu-1)(\xi-1) - \eta_1\xi^2 + \eta_2(\nu-1)(\xi-1)^2\} \right] \Delta t + O(\Delta t^2). \tag{28}$$

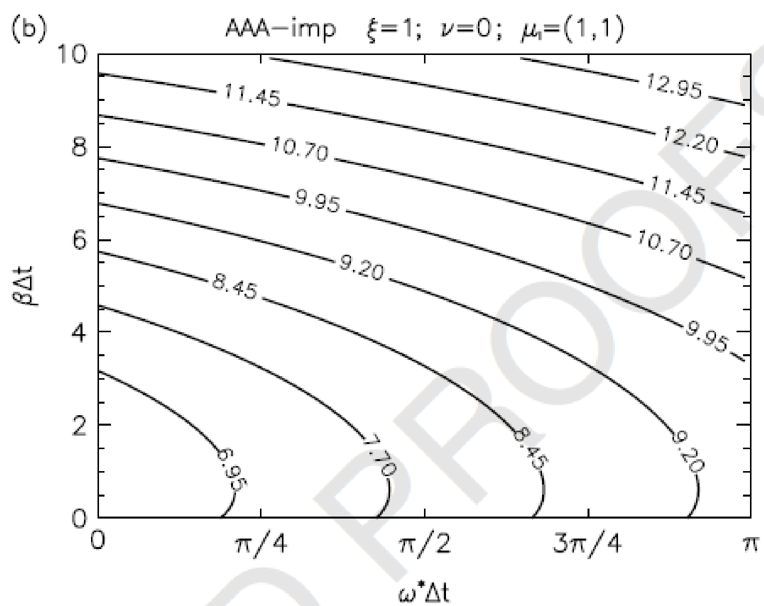


Figure 5. The ratio $|F^{+}_{\text{A}}/F^{\text{exact}}_{\text{forced}}|$ of the amplitude of the approximate forced response, calculated (a) with (32) and (b) with (34), to the exact one (Table 3).

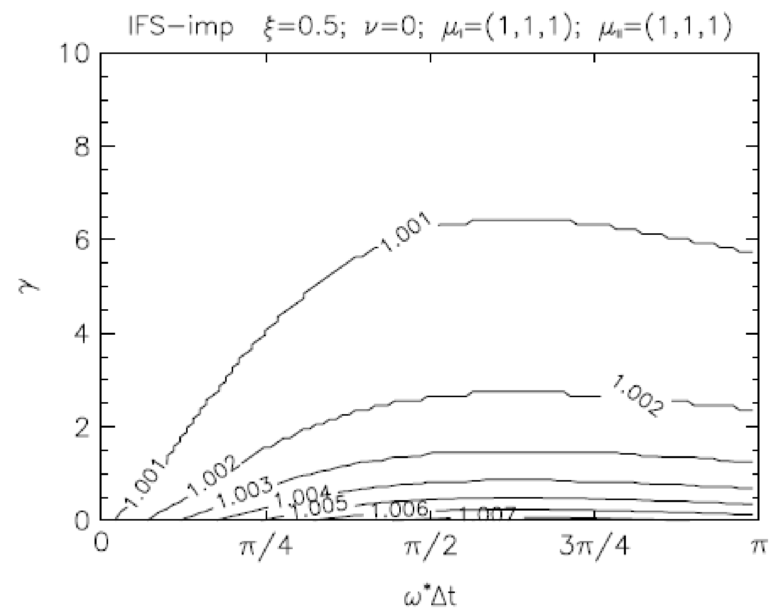
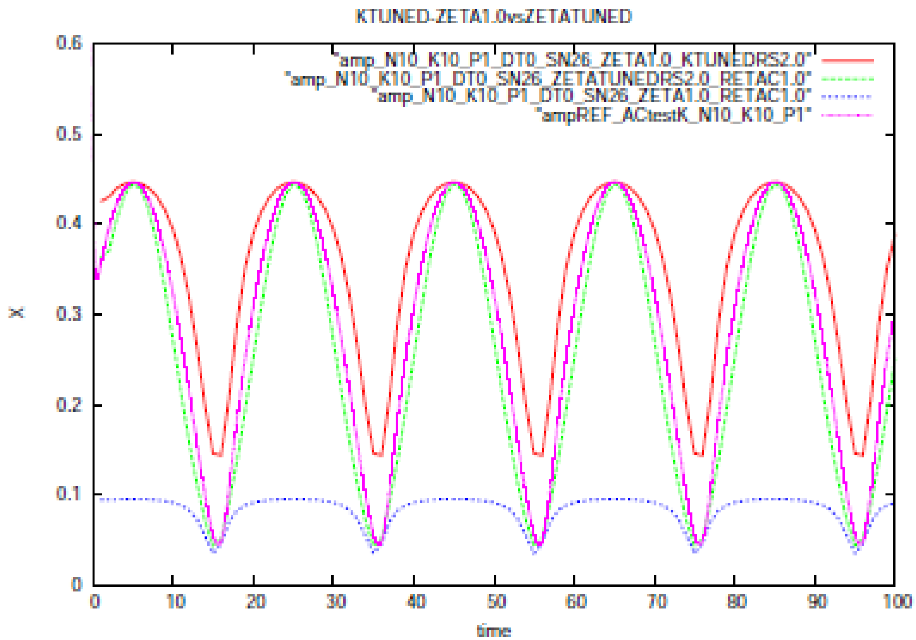


Figure 7. As Figure 6 but calculated from (43). ϕ^3 represents one of the remaining forcings (gravity wave drag, convection, or cloud).

What do we learn from this?

- If we would have known this before building, the AAAH system we would have chosen something else: **why the hell is it working**? The obvious answer is ***tuning***
- Is the fact that the steady state is not correct already an indication of more trouble?
- Now let us consider this at the level of physics parameterizatsions ...
- The main advantage of the steady state is that there is **NO physics-dynamics coupling!** (if your scheme produces the steady state the physics-dynamics interaction will be null, by definition)
- Within the physics-dynamics vs ***reversible*** ↔ ***irreversible*** problem it might be a good strategy to study steady states first! They don't add any problems, and if properly chosen they already represent the bulk of the contribution. And this is mastered in a trivial way.
- We know an example where we exploit the steady state successfully: antifibrillation, see Bénard et al., MWR, (2000)
- These techniques are based on stabilising perturbations around a steady state, following Kalnay, Kanamitsu. So ideally we can rely on linear stability analysis.

Tuning of physical constants vs. numeric of the steady state



- a) tuning, i.e. adaptation of K (in order to represent the correct steady state) (red)
- b) tuning of the decentering gamma (green)
- c) do nothing (blue)

$$\frac{\partial X}{\partial t} = -(KX^P)X + S\,,$$

$$\begin{aligned} X^* &= X + \eta S \Delta t \\ X^{**} &= X^* + D(X^{**}, X^*)\Delta t \\ X^+ &= X^{**} + (1-\eta) S \Delta t\,. \end{aligned}$$

$$D(X^+, X) = -KX^P\left[\gamma X^+ + (1-\gamma)X\right]$$

- In case of the vertical diffusion we adapt the ***decentering***
- In case of deep convection we ***tune the physics***.

Deep convection: some reflection

- After a long study the problem of deep convection in the gray zone for a long time, Luc (Gerard) arrived at a point where he considers perturbations. The idea is that at the resolved scale the perturbations should vanish.
- Can this be cast into a frame of steady states?
- Somehow we also have a forcing (stability and moisture convergence as a sort of equivalent of S) and a dissipation (lateral diffusion)
- At the resolved scale the output of the scheme has to be the steady state (no tendency), so one may adapt the diffusion to make it steady and this should deliver zero tendency, the result is the correct irreversible contribution. Higher up (gray zone and unresolved scales) the parameterisation is then expanded around this steady state.

Steady states vs. equilibria

- In dynamics they coincide e.g. geostrophic, hydrostatic.
- But in deep convection in the gray zone, this is impossible, because part of it is resolved by the dynamics

What is the steady state in parallel and sequential physics

- In the case of parallel physics steady state is the steady state of the physics you are parameterizing
- In sequential calling it always includes some physics that was preceding the one you are computing here.

ALADIN/ARPEGE: simplified

- "physics" computation

$$\frac{F^*_{A,1} - F^0_A}{\Delta t} = \Phi_1, \ldots, \frac{F^*_{A,M} - F^0_A}{\Delta t} = \Phi_M$$

- add + interpolate to departure point D $\Rightarrow F^*_D$
- "dynamics" computation

$$\frac{F^+_A - F^*_D}{\Delta t} = -\frac{i\omega}{2}\left(F^+_A + F^*_D\right)$$

SLAVEPP: simplified

- in the simplified 1D model

$$
\begin{aligned}
\frac{F_A^{exp} - F_D^0}{\Delta t} &= -\frac{i\omega}{2} F_D^0 \\
\tilde{D} &= \frac{F_A^{exp} - F_A^0}{\Delta t} \\
\frac{G_1^{exp} - F_A^{exp}}{\Delta t} &= \Phi_1[F^{exp}, \tilde{D}; G_1^{exp}] \\
\frac{G_2^{exp} - G_1^{exp}}{\Delta t} &= \Phi_2[F^{exp}, \tilde{D}; G_1^{exp}, G_2^{exp}] \\
&\vdots \\
\frac{G_M^{exp} - G_{M-1}^{exp}}{\Delta t} &= \Phi_M[F^{exp}, \tilde{D}; G_1^{exp}, \ldots, G_M^{exp}] \\
\left(1 + \frac{i\omega}{2}\Delta t\right) F_A^{+} &= G_M^{exp}
\end{aligned}
$$

Elements to consider for strategy

- Concerning LBCs I propose to put the gun at the other shoulder: treat it consciously as a model error, cfr. Model error in ensemble systems
- Nesting: know what you do and why you do it seems a worthwhile goal (even if in practice you can never fully achieve it).
- We have coded a diagnostic filter in the past. Perhaps we may extend it to study the content of the fields when going to high resolutions
- The study of more flexible time steps was abandoned due to lack of manpower. It is not clear whether will have manpower in the future, but this research on the time step organization is still relevant in my opinion. If it is restarted it should be carried out from the reversible $\leftrightarrow$ irreversible point of view.
- On a general level the definition of equilibrium, slow manifold and steady states is sometimes fuzzy. I tried to get some clarity in it. One may wonder whether the notion of the steady state may not become (more) crucial.

I touched the following issues from the topics of this meeting

* 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

* What about ‘initialisation’ at high resolution, where the distinction between signal and noise starts to be blurred (this also has to do with the issue five bullets down)?

* What will be the relative importance of EPS versus high resolution deterministic forecasts as resolution further increases?

* 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 the interplay between all forcings and the truly reversible part of the equations as the core issue?

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

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