ALATNET WORK

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Part 7: Data assimilation, Variational computations

1 Introduction:

The aim of this (rather not formal) report is to inform You about therecent achievements of my study related to the problem of turbulent fluxes and forecasting rapid cyclogenesis. The second aim is to have some kind of documentation, because the study spread towards several directions, sometimes altering from the original project of my thesis.

Basically, I can divide my work during this period into three categories:

1. Computation of further diagnostic parameters with respect to theturbulent transport and cyclogenesis relationship
2. New formulation of the mixing length profile allowing concave shapes of K- coefficients in cyclonic situations.
3. Geostrophic adjustment of the CSI formulation for turbulence and shear-linked convection, as well as retuning of the modified Richardson number (Ri_p) in the case of symmetric instability by changing its critical value (RICUT)

The theoretical basis for this work was described in the project of my thesis (available on Delage on the following adress: /cnrm2_a/mrpe/mrpe685/PROJECT, where You will find a set of Word documents.

All executables have been created and all experiments have been done on the cycle 25t1_op4.01 for ARPEGE and for ALADIN based on this cycle.

2 Part 1: Diagnostic parameters with respect to turbulence and cyclogenesis.

The idea of a new diagnostic approach for the turbulent fluxes and cyclogenesis relationship comes from the formulation of the effects of turbulent diffusion in vorticity equation and quasigeostrophic omega equation (see the equations 2.8 and 2.11 of the project). The key parameters should be here the rotation of the friction force and the variation of this parameter with height (related to vorticity and vertical motions, respectively).

This parameters should tell us, where the parameterisation of turbulent fluxes of momentum will have a cyclogenetical and where an anticyclogenetical impact and of which scale will be this impact.

Actually, I was motivated also by the fact, that my experiments with DDH on limited areas gave mostly ambiguous results (influence of the advection from the neighbourhood) and were unuseful in the case of small mesoscale cyclogenesis.

From the technical point of view, this kind of diagnostics CANNOT become operational, at least not easily, because it requires computaton of horizontal differences of several parameters in the grid-point space. Hence I was able to do it only for ALADIN and to compute it on one processor.

The computations are switched on with the key LHDDIA (.T. or .F.) in NAMPHY. The selection of the parameters is done by the switch TUNE (from 1 to 20) or by the switch SIMP (from 1 to 7) in NAMPHY0, that selects the kind of friction force used by the experiment. According to theoretical assumptions stated in the project of thesis, I have decided to compute the friction force in the case, when bulk formulation or Ekman spiral would be used (see for instance the equation 2.3).

The outputs will concern following diagnostic parameters:

1.-6. Horizontal and vertical turbulent fluxes (Jxx, Jxy, Jxz, Jyx, Jyy, Jyz) selected by the parameter TUNE=1. to TUNE=6.

In the same time the fluxes represent the components of the friction force when K-theory is considered.

7.-12. Tendencies from horizontal turbulent diffusion for u and v components of wind, tendencies from the vertical diffusion, ratio of the horizontal and vertical tendencies.

13.-15. Changes of absolute value of wind velocity due to horizontal components of turbulence, due to vertical components of turbulence after one t-step, original value of the velocity.

16.-17. Zonal and meridional components of the friction force (for bulk, gradient or Ekman spiral formulation)

SIMP=1 Friction force not including shallow convection and antifibrillation parameterisation

SIMP=2 Full (operational) formulation

SIMP=3 Bulk formulation (computed with velocity of the lowest model level) Drag coefficients were computed according to present ARPEGE/ALADIN parameterisation

SIMP=4 Bulk formulation (computed with velocity of the present model level)

SIMP=5 Bulk formulation after Bluestein's textbook (see also the project of thesis, eq.2.3 and 2.4) with simple computation of the drag coefficient

SIMP=6 The same, but with operational Cd coefficient.

SIMP=7 Friction force computed from the Ekman spiral (parameterisation SIMP=5.-6. need to define the depth of the friction layer, parameter FLHD, that is now simply tunable)

18.-19. Rotation of the friction force and the ratio of this parameterand the divergence term in the vorticity equation

20 Derivation of the rotation of the friction force by pressure

Some parameters (as rotation of the friction force and its vertical derivation) are optional for p- or z- system (switch LPDDIA=.T. or .F.).

Originally, I started to code this part inside the ACCOEFK routine, using many parameters, that were already developed. However, in the future I will prepare a "cleaner" version and perhaps I will add some parameters with respect to static stability tendencies (expressing the influence of turbulence transport of heat and moisture). The geostrophic wind and the components of the Ekman spiral are already computed in a new routine called ACGEO, called before ACCOEFK.

Actually, quite a lot of work has to be done to improve the computations (now it is in the phase, that it "works", however, I did only few experiments). From the numerical pointof view, I will have to change the system of the horizontal derivations (I started with centred differences, but for a synoptic scale comparisons this gives very noisy results, above all in the area of mountains). Further, I had to do transformations from eta- to z- and p- system (most of the theoretical equations use these systems). I consulted the scheme for the transformations with Karim Yessad, Pierre Benard and Filip Vana as well, because I did this on full levels (inspired by the schemes used for precipitation fluxes in CPTEND and computation of humidity convergence). From the physical point of view, there are also smaller or bigger uncertainities in the way of computing the bulk formulations or even the Ekman spiral (coefficients dependent on the latitude). Most probably, I will try to formulate a very trivial environment with barotropic atmosphere for the validation and comparison of the parameterisations (especially for the Ekman spiral one).

3 Part II: Adjusted formulation of the mixing length profile

The origin of this idea came from the comparison of the bulk and K- theory formulas, with respect to impact of the resulting friction force on cyclogenesis (based on vorticity and omega equation). While the bulk formulas act mostly via simple derivation of the exchange coefficient (decreasing with height), the classical K-theory gives more complicated solutions and the second derivations of K with height become important. This is simply the consequence of the baroclinic character of the K-theory in contrary to the barotropic manner of the bulk formulas (not dependent on wind shear). However, there should be at least some common consequences (e.g. Ekman pumping or suction, that is usually observed in the cyclones and anticyclones of synoptic scales). Thus, experiments in a simple barotropic atmosphere should tell us how much are the effects of Ekman pumping supported by the current parameterisation.

Nevertheless, we can already expect, that a correct solution might distinguish between the shape of the mixing length profile for a cyclonic or anticyclonic environment (see chapter 2.1. and the discussion to chapter 2 in the project of thesis). According to this preliminary expectation I supposed, that a concave profile for the mixing length in the PBL might be possible while supporting the transport of the momentum in the areas of cyclones.

Originally, it was proposed by J.-F.Geleyn to modify the geopotential to obtain a concave profile in the bottom part of the PBL (in the current scheme the profile of mixing length can be here only convex and tuned by the shaping function dependent on BEDIFV and UHDIFV parameters). However, I realised, that a linear adjustment of geopotential would not give the profile with desired properties (I proved it for a general function for mixing length and in the case of second derivations we get always squares of the multiplying factors, hence the switch from convex to concave profile and vice versa for linear modification of geopotential is not possible).

The new proposal was to modify completely the shaping function and to introduce a default concave profile for the bottom PBL, convex profile for the upper PBL and slightly concave one for the free atmosphere. The function having all these properties is the so called Hyperbolic secant, or :

y = 2 / (exp{x} + exp{-x})

Ofcourse, for our aims, we have to modify it and to retune it. We do it via setup of several parameters and we get:

f(z) = a + 2 / [exp{b*(z - H)} + exp{-c*(z-H)}] , where

a (BAA) gives the assymptotic solution of the function for infinity

b (BBB) changes the profile in the upper part (smaller b makes less concave profile with slower approach to the assymptotic value)

c (BCC) changes the profile in the bottom part, bigger c gives more concave one

H determines the height of the maximum of the f(z) function and is computed as: BHH / ZSECH,

where ZSECH = 2 / (exp{1/SQRT2} + 1/exp{1/SQRT2})

actually, H replaces the parameter ZEDIFV of the operational shaping function

To get the shaping function to the interval <0,1>, we divide it by a norm d (parameter BDD), that is simply the maximum value reached by the function. I got it usually experimentally, anyway it is kind of formal adjustment. However, I had to touch also the original function of Blackadar, other wise the mixing length would very slightly grow in the high levels (stratosphere). This unwanted effect was eliminated via adding a multiplicative factor to the denominator of the first term of the mixing length formula, via parameter n (BNN). It makes, that the mixing length will very slowly descend in the top of our atmosphere (although the assymptotical properties for infinite heights are definitively lost).

The final form of the mixing length yields:

lm;h = [kappa * (z + z0;h) / (1 + kappa * (z + z0;h)**n / (ALMAVc;h))] * [a + 2 / (exp {b*(z - H)} + exp {c*(H-z)})] / dd

where ALMAVC and the ALMAVH are in this new formulation the assymptotic parameters for the mixing length of momentum and heat, respectively. The switch to the new profile is done only in the case of cyclonic vorticity by using LSECM=.T. for momentum and LSECT=.T. for heat.

3.1 Results:

The new shape of the mixing length was at first tested and compared with the operational formula outside of the model, in the situation with entirely academical expression of the K-coefficient (with constant wind shear and for Richardson number equal 4.5). I have selected such setup of parameters, that the PBL maximum of the mixing length was smaller or equal to the operational one in cycle 25t1op401 and I was changing the shapes of the curve with aid of the b and c parameters (BBB, BCC).

After I tested around 10 setups on the ALADIN model on the case study of the 20.7.2001 Adriatic storm (LACE with 12 km resolution) and on the famous 20.12.1998 storm (ATLA with 33 km resolution).

In both cases I have estimated a significant impact and I was able to make further conclusions:

The best setup for both cases (false cyclogenesis and the 1998 storm) was:

BNN=1.1

ALMAVC=300

ALMAVH=120

BHH=1

BAA=0.25

BBB=0.1

BCC=2.

BDD=1.89503

(see the attached figures 1. - 5. for the profiles of the mixing lengths and K- coefficients)

This forecasts a less deep Adriatic storm by 1 hPa (looks funny, but remind, that a change for the ECMWF initial conditions changed it only by some fraction of hPa as well as the majority of the experiments on physics) and deepens the 1998 storm by 7 hPa comparing the pressure in the centre. (see Fig.7 and Fig.10)

The latter result can be considered as a succesfull forecast, because You can see a deep cyclone structure instead of shallow pattern of low in the original forecast.

After selecting this profile as a basis for the future experiments (called USU8) I realised, that:

1. More concave profile in lower PBL (BCC>2) brings worse results on both cases than USU8
2. Less concave profile and bigger mixing length in the free atmosphere makes even better forecasts than USU8 (BBB<0.1)
3. Smaller ALMAVC (150) and ALMAVH (60) are definitely not improving the results comparing to USU8 but still are better than the reference operational setup !
4. Setting LSECT=.F. gives worse results than the operational one for the Adriatic storm, in the case of the 1998 storm it is almost the operational result (but still forecasting the storm).
5. Setting LSECM=.F. but LSECT=.T. gives surprisingly good results for the adriatic storm (improvement by almost 3.4 hPa - see Fig.8) However, by the 1998 case we get a bit worse results than USU8 and the trajectory of the storm is changed more towards south.

3.2 Conclusion:

It seems, that changes in the shape of the mixing length profile are important both for momentum and heat, if we want to get satisfying forecast. Originally, I believed, that a more cyclogenetic profile for momentum will allow us to keep the original profile for the heat (LSECT=.F.) but I was wrong. Definitely, the amount of heat exchange in the PBL and in the free atmosphere is crucial for both rapid and false cyclogenesis. Unfortunately, in the former case, this leads to not realistic decrease of static stability at the top of the PBL. In the latter case, the decrease of the static stability can be positive while probably decreasing the depth of the lower PV anomaly in the storm environment. In any case, we can find a compromise, that gives better results as the reference operational setup with reasonable maximum of the exchange coefficients in the PBL and smaller Richardson flux numbers . Actually, we can think about higher ALMAVC and ALMAVH (cyclonic environment should allow higher mixing lengths as the anticyclonic ones), but I am not sure, if this would lead to better scores, remembering some tests on the mixing length done in the March 2001.

4 Part 3: Adjusting the parameterisation of shear-linked convection and the conditional symmetric instability parameterisation for turbulent fluxes

The aim of this study was to introduce an improved formula for ZATSLC (derivation of the moist static energy with height and divided by g) in the ACCVIMP routine for parameterisation of convection.

In the current parameterisation the formula for shear-linked ZATSLC yields:

ZATSLC = (f / eta) * (1 / gamma) * [(du / d phi)**2 + (dv / d phi)**2]

where f is the Coriolis parameter, eta is the absolute vorticity, gamma is the vertical gradient of the web bulb temperature du, dv are vertical differences of wind, d phi is the vertical difference for geopotential.

However, the formula, that we originally get from the semigeostrophic approximation and 3-D expansion of the equations describing the CSI relationships yields:

ZATSLCG = (f / gamma) * [(dug / d phi)**2 / alpha + + (dvg / d phi)**2 / beta]

where the wind is replaced by the geostrophic one and alpha = f + dvg / dx beta = f - dug / dy - d (ln f) / d y

Hence we turn our attention more to the shear and curvature contributions to vorticity as it was pointed already in the article of Nordeng (1987)

The CSI is stopped in the case of strong anticyclonic shear and curvature (close to + or - f, where we would get a singularity). Hence safety parameters PUTCUT and PVTCUT are introduced (should be bigger than 0).The key LSLCG=.T. switches on the adjusted formula for the geostrophic wind. The key LSLCH=.T. gives the same formula with alpha and beta coefficients but the wind is not geostrophic (could be computed even operationally).

Results: I managed to test the new formula only in the case of the 20.7.2001 Adriatic storm. Generally, the results are improved with LSLCG=.T. and PUTCUT/PVTCUT=0.5 by almost 1.5 hPa. The LSLCH=.T. switch gives slightly worse results, but still better, than the reference forecast (more than 0.5 hPa as the reference with original formula for LSLC=.T.). However, it shows, that even an improved scheme of shear-linked convection is not able to cancel false cyclogenesis. The geostrophic wind used in the experiment is quite noisy in low levels, however, we can see, that the switch to the geostrophic formula is not so much important as we believed before. The PUTCUT and PVTCUT can go down to 0.0001 without having model blow-up.

I have introduced the geostrophic wind also to the scheme of dry/conditional symmetric instability to modify the Richardson number (see the project of thesis and the equation 6.4, where a geostrophic vorticity and geostrophic Richardson number is required.

The switch LCSIG=.T. for this approximation gives slightly better results if the scheme of conditional symmetric instability is applied (LDRYSI=LMOISI=.T.). However, we can speak about departures about 1 hPa maximum (even in the case of the 84 hour forecast of the 1998 cyclone).

Tuning of the original CSI modification of the Richardson number:

In the project of thesis You will find, that the modified Richardson number, that indicates the presence of the CSI (Ri_p < 0) might have both negative and positive values. However, the application of negative Ri_p except of Ri caused blow_up of the model after maybe 30 h of integration. Hence I was forced to put a lower boundary for the modified Richardson number (RICUT). I did the majority of the ALATNET experiments with RICUT= 0., that means, that if CSI is present, the turbulent fluxes will be parameterised in the same way as for neutral conditions (well mixed PBL). However, I realised later, that most of the CSI experiments done by Emanuel were expecting rather a bit stable character of CSI environment corresponding to Ri=0.25 (I spoke about this also with Claude Fischer). To make this, I tuned the parameter RICUT to 0.25 and I tested it on the Adriatic storm and 1998 storm case study.

For the adriatic storm increase of RICUT deepens the storm further by 1.5 hPa, however, the forecast of the mean sea level pressure is much less noisy, than my reference experiments (mostly in the environment of the mountains). For the 1998 storm the impact is huge and very positive, according the structure of the cyclone, much nicer field of MSLP etc. By further increase of RICUT to 0.5 I got the almost perfect forecast of the storm with realistic development during the 24 and 66h computation. (see the Figure 12)

The success of the latter experiment forced me to do the same in the ARPEGE model. However, for ARPEGE the results were more ambiguous and disappointing. Although I corrected the noise in the MSLP pressure and the position of the 1998 storm - the development is false: the storm in ARPEGE doesn't want to decay during the 24 - 66 h forecasting period when CSI scheme is applied. Moreover, the forecasted storm is too deep (by RICUT=0.25 it gives 986 hPa in the centre of the cyclone after 84 hours - see Fig. 15).

And ... further increase of RICUT has the opposite impact for ARPEGE as for ALADIN !!! By applying RICUT=1. one will get in ARPEGE a silly forecast of the 1998 storm (980 hPa), while in the ALADIN the forecasted cyclone is less deep than for smaller RICUT.

I can hardly explain this strange dualism of the ARPEGE and ALADIN model for the same scheme. Probably, the boundary conditions used for ALADIN were very important for the correct forecast ... but I don't think, that it is normal to get such different tendencies only because of coupling from the reference ARPEGE files. Anyway, this will be the topic for further study during next year. The positive feature of the ARPEGE run with RICUT=0.25 is the much better DDH statistics for temperature and moisture budget against RICUT=0. To make it even better, I enabled to switch off the scheme at surface (LDRYSIS=LMOISIS=.F.)

4.1 Conclusion:

Summarizing all the experiments done during my latest ALATNET period, I would tell, that the experiments done on the mixing length profile seem to me as the most promising for the future and having the most stable theoretical basis. The next step will be the creation of a barotropic atmosphere and academical tests (probably outside of the ARPEGE/ALADIN model). This should verify the concept of turbulence - cyclogenesis relationship, that I stated in the project of my thesis.

The experiments with CSI show, that we should be at least not afraid from the use of the "normal" wind instead of the geostrophic one. However, the missing CSI parameterisation doesn't seem as the reason of our ARPEGEAD/ALADINAD problems, although some cases were improved by introduction of the shear-linked convection scheme (mostly due to environments with higher wind-shears).

The possible reason of our problem with false cyclogenesis can be related with the character of the K-theory, that generally fits very well for stable conditions (turbulence produced by eddies of small diameter in stable stratified layer with significant wind shear). Hear we can nicely approximate, that the departures from horizontal wind (u' v') are nearly of the same order as the departures from the vertical velocities (w') and this is dependent on the horizontal wind shear (du / dz). However, as stated in the textbook of Garratt (1999), it was shown, that in convective layers (CBL) and in similar environments as exist in our false cyclones (where the horizontal wind shears are small and the vertical velocities high), the w' departures exceed the horizontal ones even several times (experimentally proved) and the K-theory is unable to describe correctly the fluxes in those environments. An adjustment is possible using the TKE formulation.

Although, I don't think, that the adjustments of the parameterisation of turbulent fluxes will give a single solution ideal for all kind of situations, I plan to continue with the CSI experiments in the future to find better set ups (or formulas) for the modified Richardson number. This work will further concentrate more on the Christmas storm cases, while it seems, that the false cyclogenesis is a problem of different kind and could be already significantly suppressed using the scheme of Semi-Lagrangian horizontal diffusion of Dr. Vana.

Appendix:

The executable, that can be used for all of the above mentioned experiments can be found on delage under:

/cnrm2_a/mrpe/mrpe685/ALATNET/bin/ald/al_on_cy25t1/op4v01

the name of the executable is CSIG

One can find an example of the script and namelist for the recent experiments on tora:

/u/gp/mrpe/mrpe685/public/csig

Description of attached figures:

Fig1.ps, Fig2.ps : profiles of the mixing length with the new shaping function for the USU8 experiment (the new profile is blue, dotted, compare with the profile of cycora-ter and cy-25t1op401)

Fig3.ps, Fig4.ps : Profiles of the exchange coefficients if the wind shear is constant for momentum and heat, respectively by Ri = 4.5

Fig5.ps: The Richardson flux number by above mentioned conditions

Fig6.ps: reference forecast of ALADIN (cy25t1op401)

Fig7.ps : forecast with modified shape of mixing length using the setup USU8

Fig8.ps : forecast with BBB=0.1 BCC=4. BDD=2.03 and with modification of the mixing length only for the heat

Fig9.ps : basic forecasts of ALADIN for cycle 25t1op401 for the 1998 storm

Fig10.ps : forecast of ALADIN with modified shape of the mixing length (USU8 setup)

Fig.11.ps : forecast of ALADIN with CSI modification of the Richardson number by Ricut=0 (neutral mixing conditions if CSI appears)

Fig12.ps : forecast of ALADIN with CSI modification of the Richardson number by Ricut=0.25

Fig13.ps : The original ARPEGE forecast of the 1998 storm for cy25t1op

Fig14.ps : The forecast of ARPEGE with application of conditional symmetric instability for turbulence while RICUT=0

Fig15.ps : The forecast of ARPEGE with application of conditional symmetric instability for turbulence while RICUT=0.25

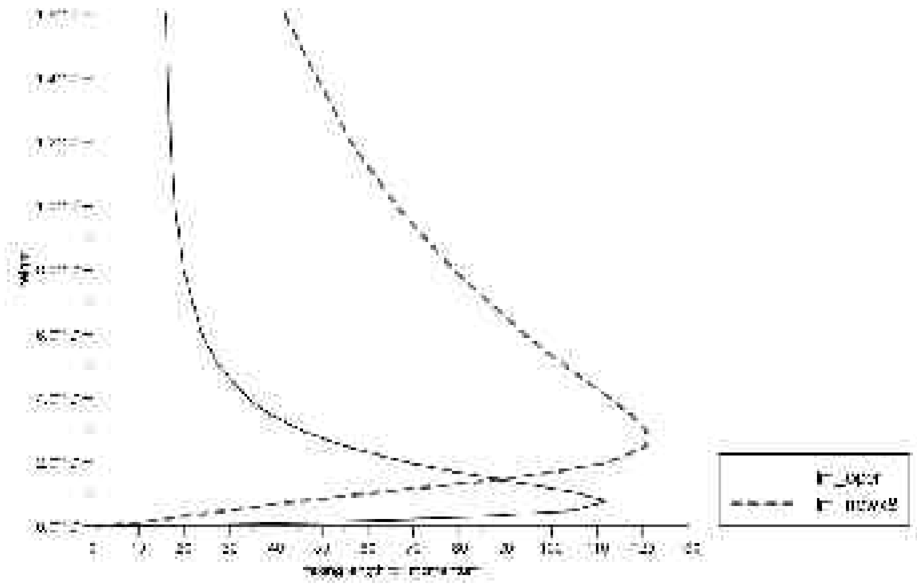

Fig1: profiles of the mixing length with the new shaping function for the USU8 experiment (the new profile is blue, dotted, compare with the profile of cycora-ter and cy-25t1op401)

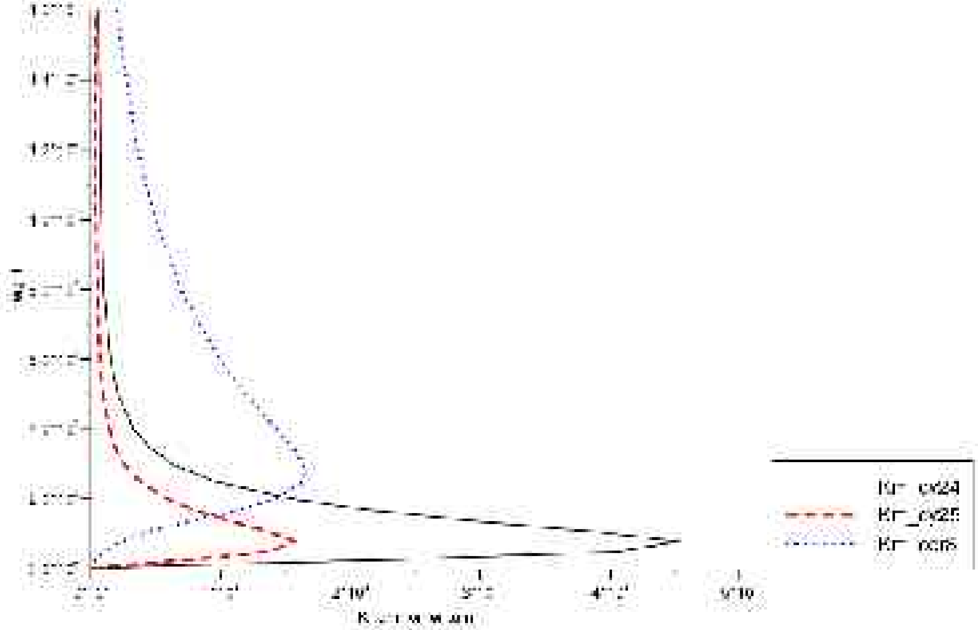

Fig2: profiles of the mixing length with the new shaping function for the USU8 experiment (the new profile is blue, dotted, compare with the profile of cycora-ter and cy-25t1op401)

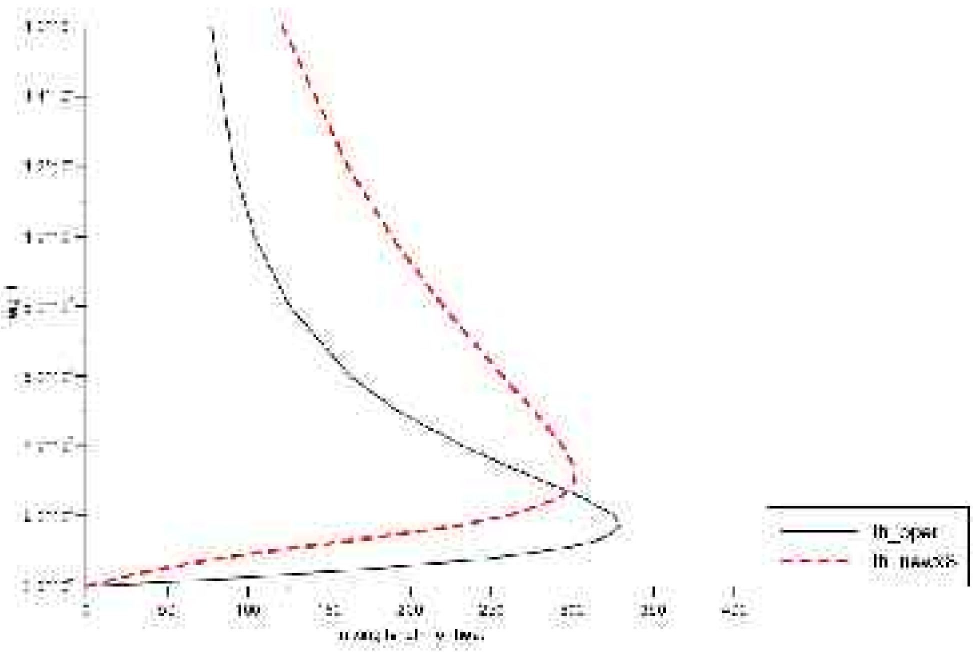

Fig3.: Profiles of the exchange coefficients if the wind shear is constant for momentum and heat, respectively by Ri = 4.5

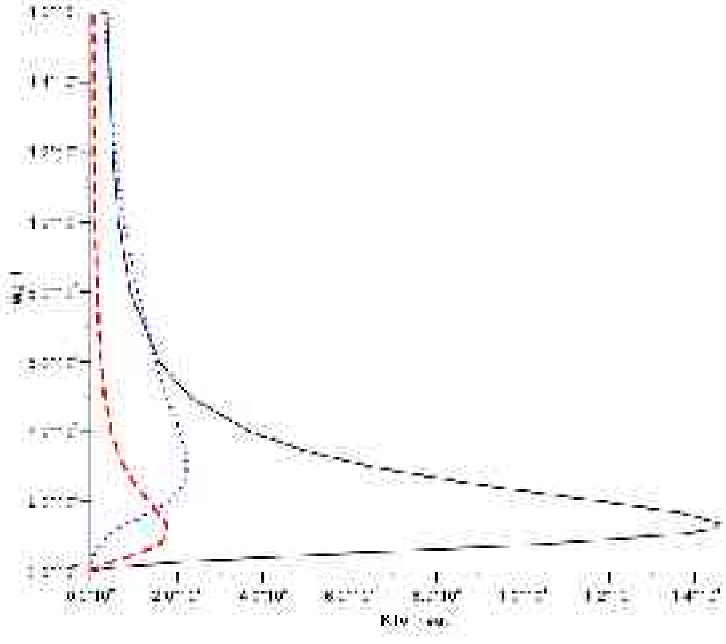

Fig4: Profiles of the exchange coefficients if the wind shear is constant for momentum and heat, respectively by Ri = 4.5

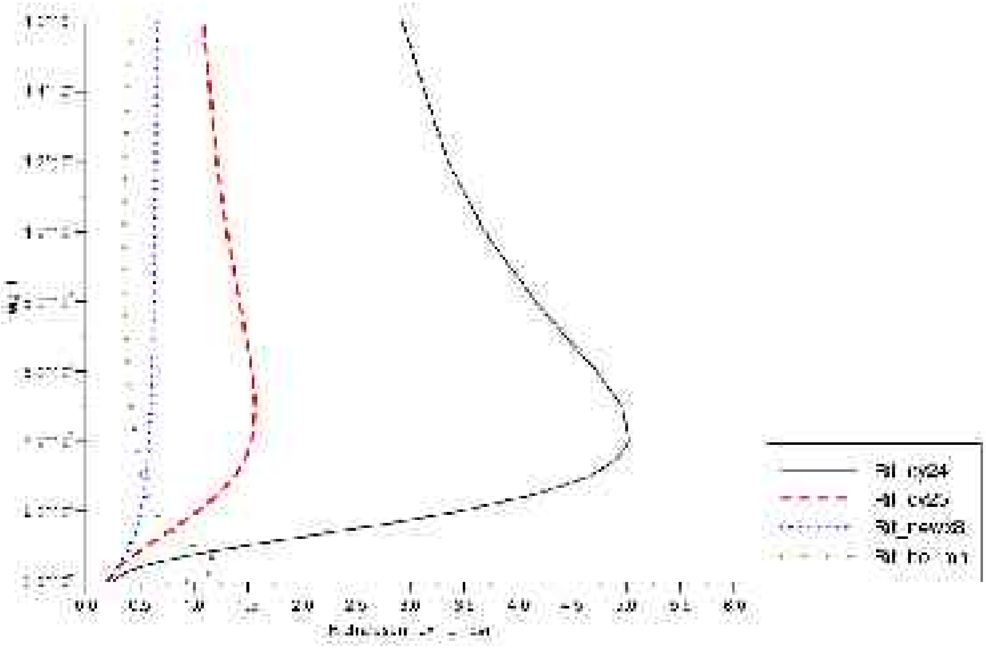

Fig5: The Richardson flux number by above mentioned conditions

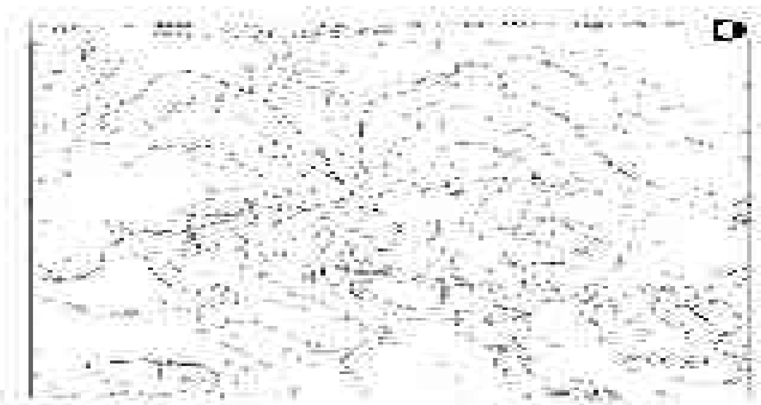

Fig6: reference forecast of ALADIN (cy25t1op401)

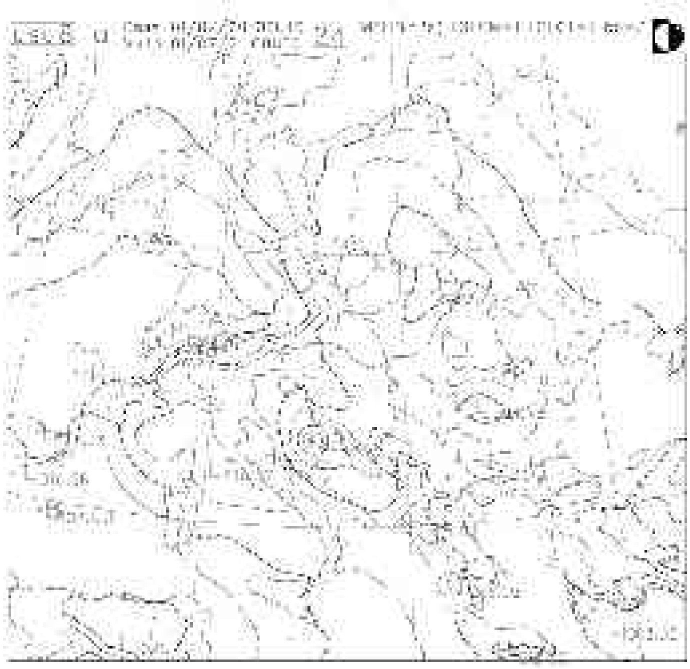

Fig7: forecast with modified shape of mixing length using the setup USU8

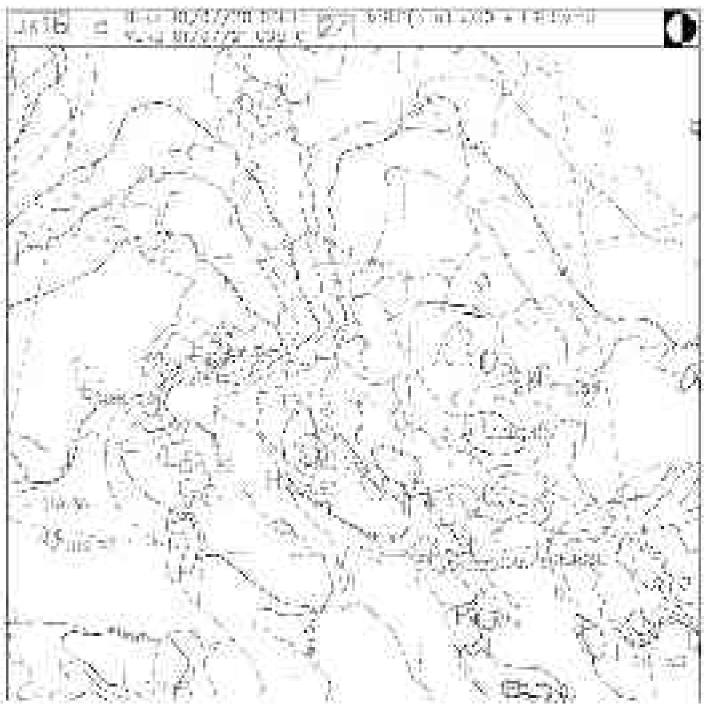

Fig8: forecast with BBB=0.1 BCC=4. BDD=2.03 and with modification of the mixing length only for the heat

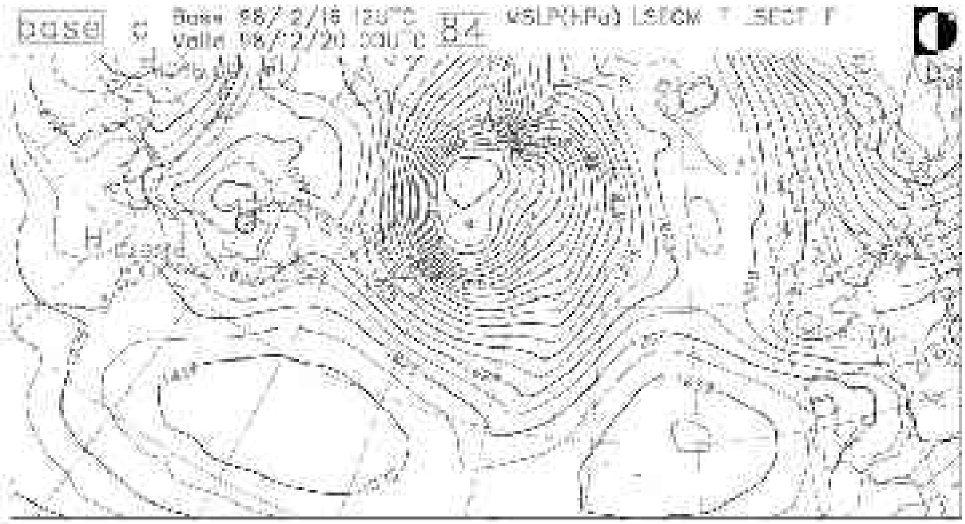

Fig9: basic forecasts of ALADIN for cycle 25t1op401 for the 1998 storm

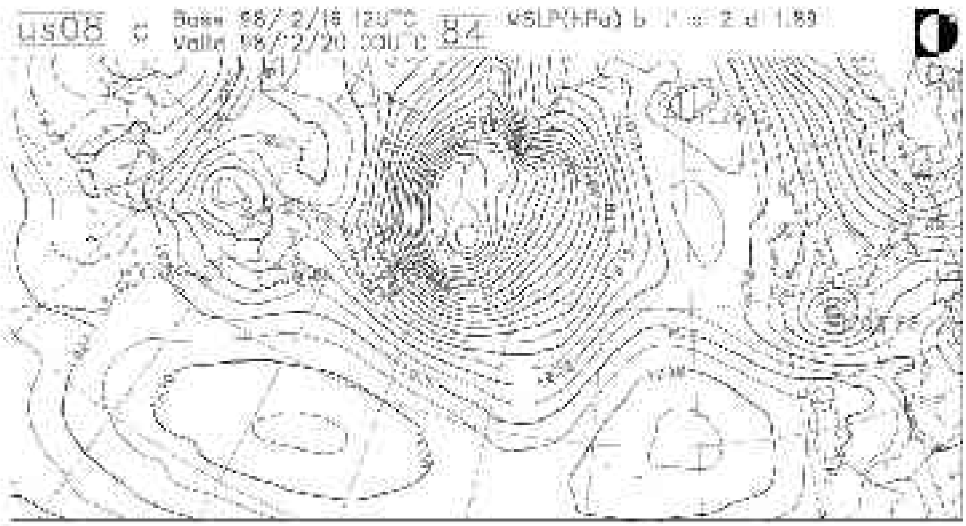

Fig10: forecast of ALADIN with modified shape of the mixing length (USU8 setup)

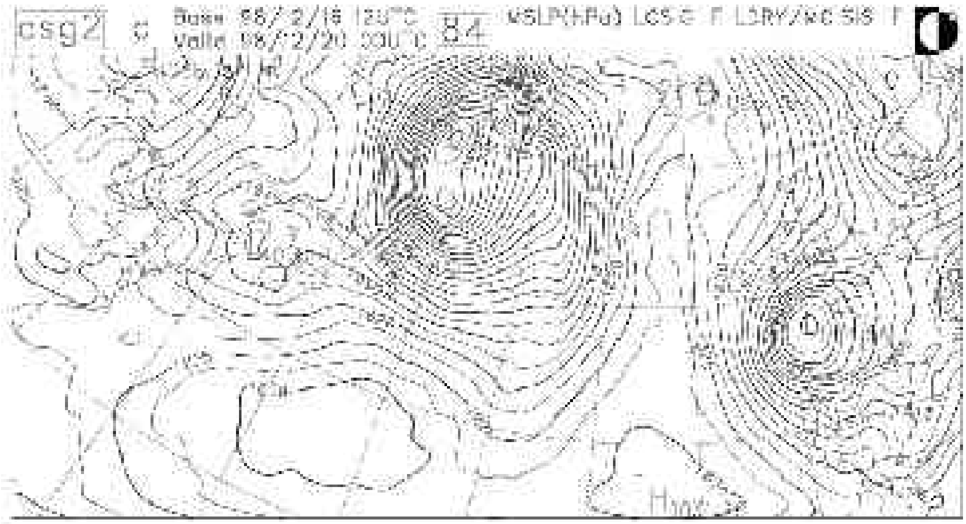

Fig.11: forecast of ALADIN with CSI modification of the Richardson number by Ricut=0 (neutral mixing conditions if CSI appears)

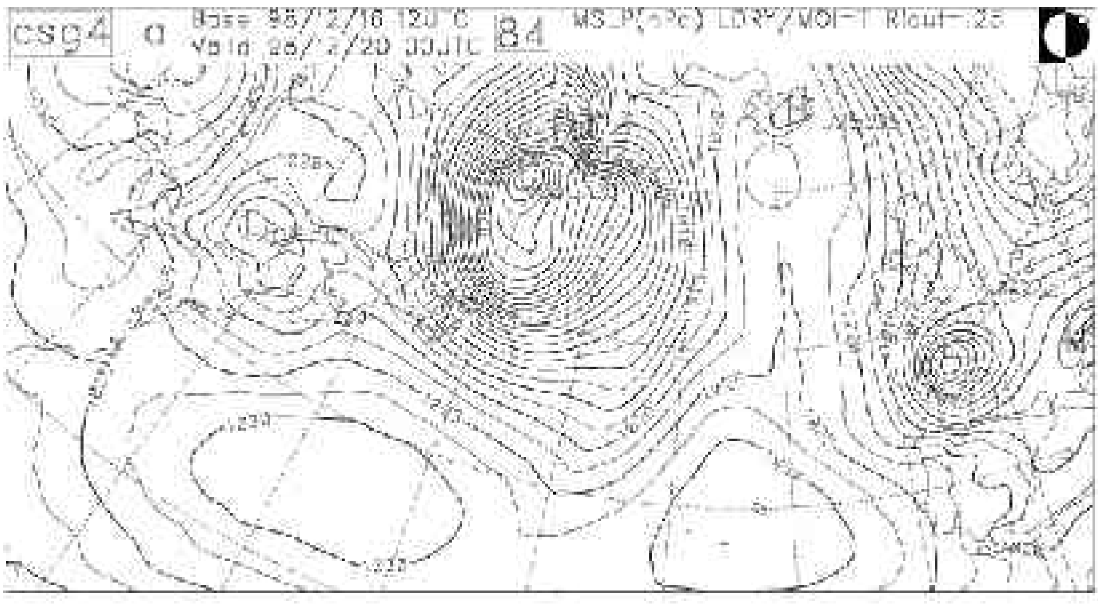

Fig12 : forecast of ALADIN with CSI modification of the Richardson number by Ricut=0.25

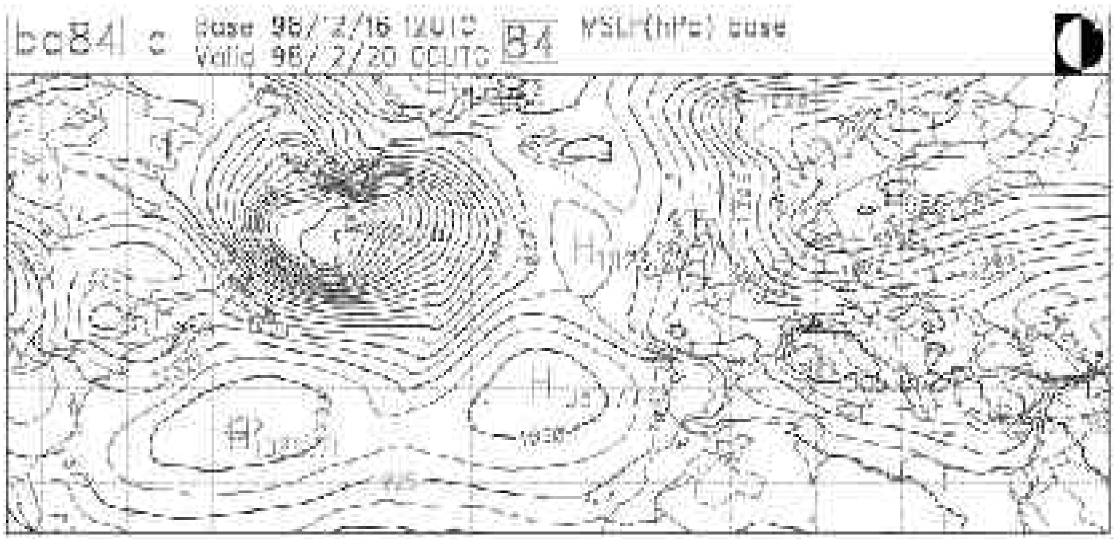

Fig13 : The original ARPEGE forecast of the 1998 storm for cy25t1op

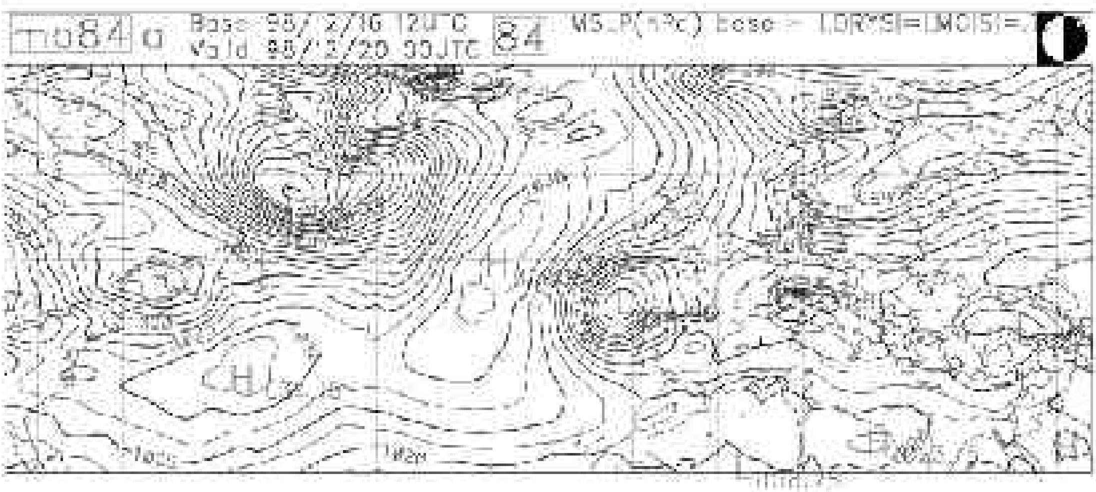

Fig14 : The forecast of ARPEGE with application of conditional symmetric instability for turbulence while RICUT=0

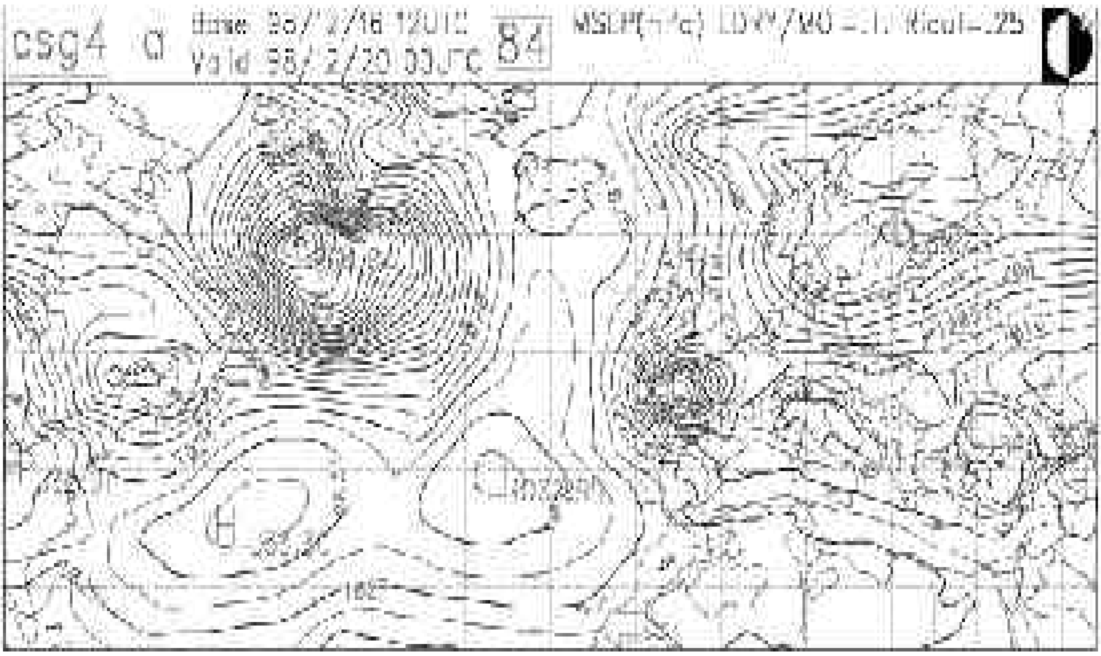

Fig15 : The forecast of ARPEGE with application of conditional symmetric instability for turbulence while RICUT=0.25

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