Future developments at ECMWF

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1. Introduction

This lecture is mainly designed to set the stage for the group discussions of this workshop. I shall take a quick look at the future and describe which developments we expect to take place at ECMWF in the field of the planetary boundary layer. These developments are planned in response to the known weaknesses of our present parameterisation scheme, weaknesses which I shall briefly mention.

Meteorologists are well aware that predicting the future is tricky. This case is no exception and some of the ideas which I shall mention are still very tentative. It is my hope that, during this workshop, we shall analyse these ideas critically, and possibly come up with other suggestions.

2. Short term prospects

An important development in our parameterisation scheme, which we hope to implement soon, is the introduction of the diurnal cycle. Up to now the model has run operationally with the solar radiative input averaged over 24 hours.

There were two reasons for avoiding the diurnal cycle in our first operational model. First it was felt that relatively small errors on the large heating during the day and cooling during the night could result in a relatively large error on the small net effect of the daily variations. Hence we felt that it would be better to compute directly the net effect by using the average solar radiation. In this way we could better control the total energy budget and gain experience with the behaviour of the model without the further complication of the daily cycle.

In addition there was a more practical problem. Our radiation scheme is relatively complex and uses a high amount of computing time. With the operational time requirements we can afford to do the radiation calculation only twice per forecast day at the present model resolution. When the operational model was designed we did not have an interpolation scheme which could simulate the daily variations accurately enough, and without excessive increase in computing time or drastic simplification of the model.

The practical problem has now been overcome. The full radiation computation will be done several times a day, but not at every grid point. For example we can do it every 3.5 hours and every 4 points. We believe that it is better not to divide the day by an integer number in order to avoid exciting a harmonic of the daily period; hence the choice of 3.5 hours. Then a linear space interpolation of the transmission, reflexion and emission of each layer will be done and the radiative fluxes computed at every point with the correct cloudiness. However, instead of storing the fluxes themselves, we shall store an equivalent short wave transmissivity ε, and an equivalent long wave emissivity τ for each level:

$$\tau = F_{sw}/\mu_o\ I_o \tag{1}$$

$$\varepsilon = F_{lw}/\sigma\ T^4 \tag{2}$$

where F_{sw} and F_{lw} are the short wave and long wave net flux, I_o is the intensity of the incident solar radiation at the top of the atmosphere, μ_o the cosine of the solar angle, σ the Stefan-Boltzmann constant and T the temperature of the layer. At each time step we can now compute the net radiative flux simply from

$$F = \mu_o I_o \tau + \varepsilon \sigma T^4 \qquad (3)$$

where the current solar angle and temperature are used. The space interpolation is very accurate. The only important inconsistancy is to interpolate the effect of water vapour linearly, whereas ideally it should be associated with the variation of cloudiness from one point to the next. The extrapolation in time is less accurate since the variation in cloudiness cannot be taken into account at all, not even for the grey processes. Hence a shorter interval between full radiation computations will always be an improvement.

Together with an interpolation scheme for the radiation, new surface parameters need to be chosen. Following Deardorff (1978), a two-parameter model of the soil was chosen, with a heat capacity corresponding to a period of 1 day and a soil water capacity of 2 cm for the quick response, and a heat capacity corresponding to 1 year and a water capacity of 20 cm for the slow response.

Recent tests have shown that this scheme appears to behave quite well. First we compare integrations with and without the daily cycle. Figure 1 shows the evolution of the surface temperature in the two cases at three points taken from a global integration. The first point, in the Sahara desert, is typical of a large daily variation. It shows that, contrary to some of our fears, the two runs do not drift apart from each other. The second case, a point in North America, suggests that the daily variation is mainly a modulation of longer term changes due to synoptic perturbations. The third point, in South America, shows the largest differences that we have observed between the two runs. These differences seem mainly associated with the varying cloudiness through convective processes.

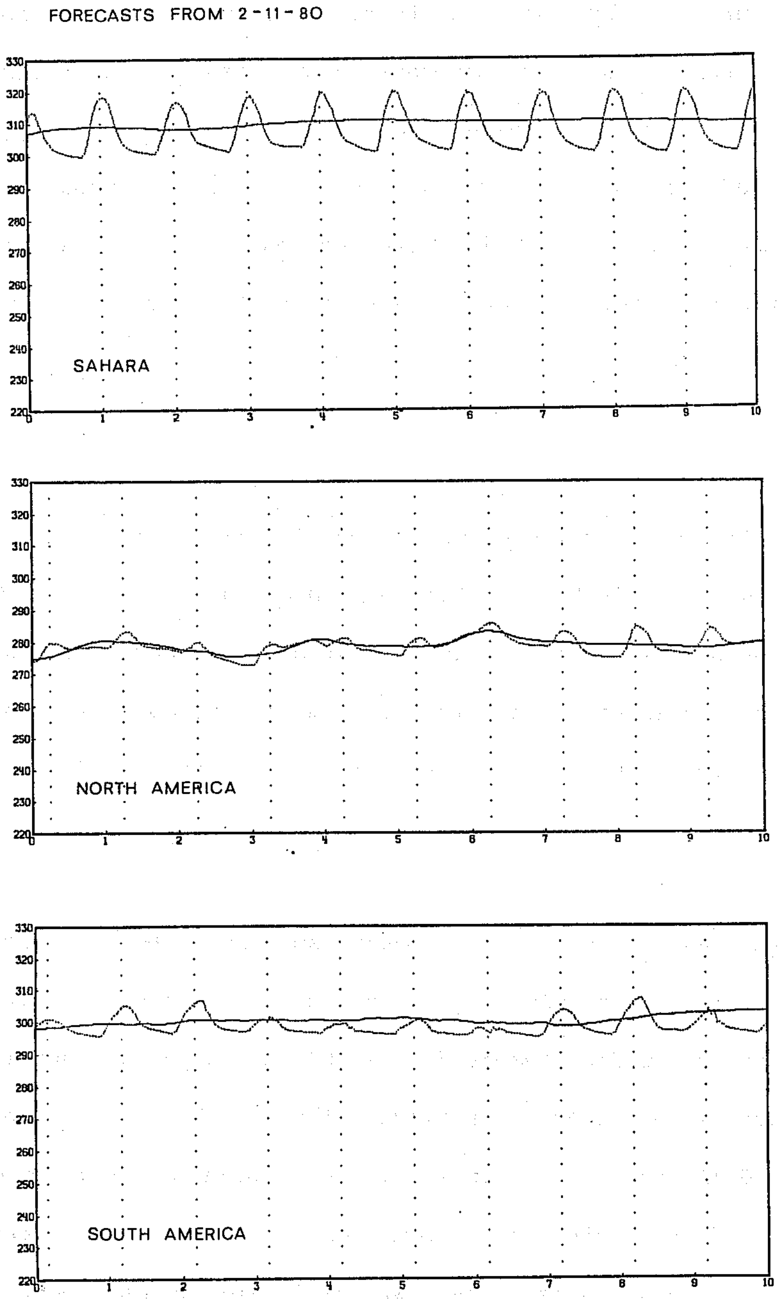

Fig. 1 Time variation of the surface temperature at 3 points of a global, 10-day forecast with and without the diurnal cycle.

If we look at the effect on the surface fluxes (Figure 2), we see that the introduction of the diurnal cycle mainly increases the sensible heat flux and decreases the latent heat flux. These changes are not very large, the maximum difference being about 10 w/m^2, but in the right direction since it increases the Bowen ratio which tends to be too low in our operational model.

The crucial test is of course the comparison with observations. I show here only one example which is typical of all our results. Figure 3 is a meteogram for Bordeaux, for the forecast starting on 11 June 1979. The forecast results, sampled every 12 hours, are joined by a solid line, while the stars are observations. The 2 meter temperature is deduced from the forecast output by an interpolation between the surface temperature and the temperature of the lowest model level. We see that at the beginning of the forecast the predicted amplitude of the temperature variation compares very well with the observed one. However it can be seen that the predicted cloud cover is much lower than the observed. This implies that, at equal cloudiness the computed temperature variation would have too small an amplitude. This is confirmed by the end of the forecast period where computed and observed cloudiness are similar but the amplitude of the predicted temperature variation is too small.

This underestimation of the temperature variations could be adjusted by a change in the soil parameters. However we have observed in our tests that the daily variation of the pressure in the tropics is already overestimated. An increase in the temperature changes would enhance this error. Hence the present choice of parameters is a compromise between these two errors.

These modifications will now be tested by our standard benchmarking procedure, and if everything goes well they will be introduced into the operational model.

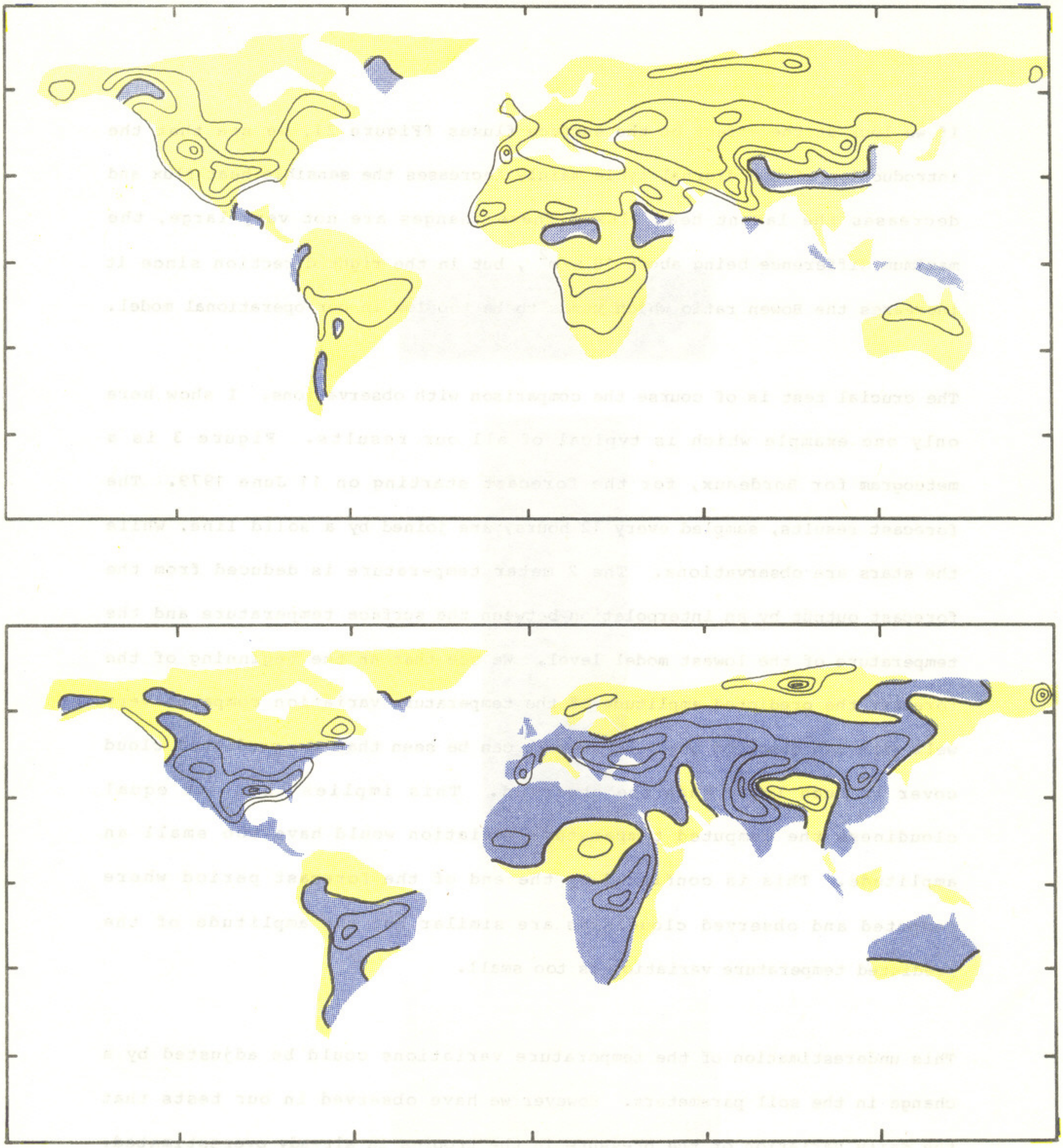

Fig.2 Difference between the surface fluxes computed with and without diurnal cycle, averaged during a 10-day forecast.
Top: sensible heat flux difference
Bottom: latent heat flux difference
The figure has been smoothed. The contouring interval is 2 w/m^2. Blue is negative (flux smaller with the diurnal cycle) and yellow is positive.

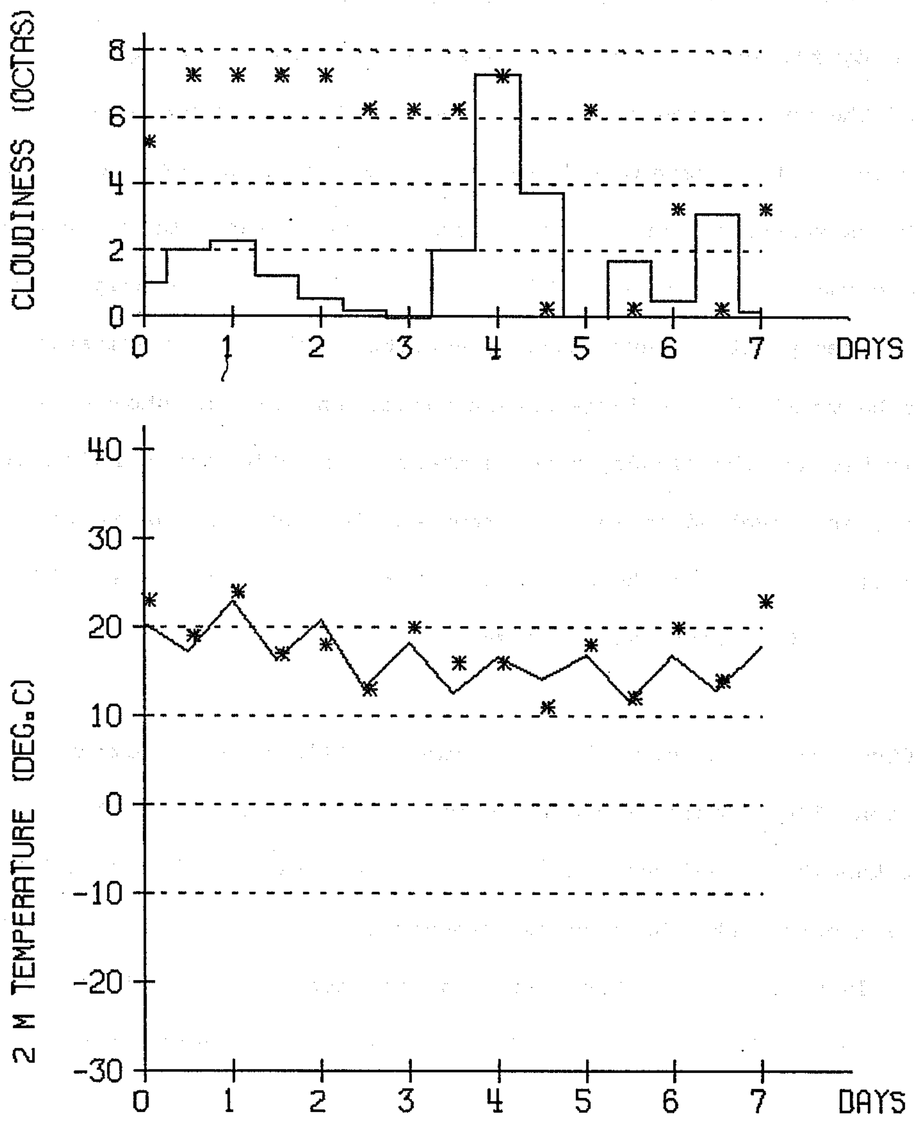

Fig. 3 Comparison between computed and observed temperature at 2 m (top) and cloudiness (bottom) at Bordeaux.

3. Longer range prospects.

When we now consider the longer range prospects for changes in the parameterisation scheme, we cannot talk about the PBL alone, nor even of the vertical eddy fluxes , but we must consider possible changes within the context of the whole parameterisation scheme. These changes should respond to weaknesses in the present scheme, or remove inconsistencies which now exist. These weaknesses are, at the present time, seen to be related mainly to the treatment of clouds. This is partly because clouds, and the associated precipitation, are the only aspects of the parameterisation scheme which can be verified to a large extent, hence the errors show up clearly, and partly because the clouds, through their interaction with radiation and turbulence, are involved in several important feed-back loops which control the vertical structure of the atmosphere, hence the forecasts are likely to be sensitive to the cloud parameterisation.

Let us first consider stratiform clouds. The present criterion for condensation, 100 % saturation within the grid box, is known to be rather poor. We know that condensation can occur in a volume of 200 by 200 km and 100 mb thick before the whole volume reaches saturation. Hence our criterion is physically unrealistic. Furthermore it is inconsistent with the radiation scheme in which the cloud cover is diagnosed as a function of relative humidity.

Sommeria and Deardorff (1977) have proposed a scheme to derive the liquid water content and the cloudiness of a grid box from the knowledge of the variances of moisture and temperature. The extension of their ideas, which were developed in the context of a very fine mesh model, to a large scale prediction model is not a trivial problem, but is worthwhile investigating. Some of their assumptions will have to be relaxed. For example one cannot assume that the correlation between the deviations of potential temperature and humidity is zero if the model layers are fairly thick. We cannot neglect the normal trend of the potential temperature increasing upwards and the specific humidity decreasing upwards. These trends produce a negative correlation within the grid box. A straightforward way to take care of this would be to assume that deviations around a linear trend are uncorrelated. Thus if we write, in the layer k

$$q(\sigma) = \bar{q}_k + \frac{\Delta\bar{q}}{\Delta\sigma}(\sigma - \sigma_k) + q' \tag{4}$$

$$T(\sigma) = \bar{T}_k + \frac{\Delta\bar{T}}{\Delta\sigma}(\sigma - \sigma_k) + T' \tag{5}$$

then the average over the grid square of the product of temperature and humidity is

$$\begin{aligned} \overline{qT} &= \bar{q}_k\,\bar{T}_k + \frac{\Delta\bar{q}\;\Delta\bar{T}}{\Delta\sigma^2}\,\overline{(\sigma - \sigma_k)^2} + \overline{q'T'} \\ &= \bar{q}_k\,\bar{T}_k + \frac{\Delta\bar{q}\;\Delta\bar{T}}{12\Delta\sigma^2} + \overline{q'T'} \end{aligned} \tag{6}$$

Now $\overline{q'T'}$ can probably be neglected, but the second term normally should not. Similarly the variation of the saturation vapour pressure with height within a thick layer should probably be taken into account. Obviously the scheme must also be extended to precipitating clouds.

Knowledge of the cloud fraction (ratio of cloudy and cloud-free air) is not sufficient, however. What the radiation scheme needs is a measure of the cloud cover. Hence we want to be able to distinguish between an extensive thin stratus sheet and a number of small, thick cumuli. We might be able to relate this to the stability of the layer, the variance of the vertical velocity or the vertical gradient of moisture.

This later point is particularly crucial as far as cloudy layers at the top of the PBL are concerned. The phenomenon which we are trying to simulate is the following. When a growing convective boundary layer reaches the saturation level, a cloud layer forms at its top. The cloud emits radiation and cools sharply at its top, this strong cooling being partly balanced by heating within and below the cloud. The heating and cooling distribution enhances the instability and the turbulence within the cloud layer. The evolution of the cloud is then determined by the moisture flux from below and the mixing with dry air above. The moisture flux at the surface, however, should not be affected very much by the presence of the cloud.

Our model does not behave like that. When the top layer of the PBL is saturated, the radiation scheme is not able to partition cooling and heating within the cloud if it occupies a single layer. The net effect is a cooling of the cloudy layer and a heating below. This destabilizes the whole boundary layer, thus increasing the upward transfer of moisture. Since the mixing with dry air from above is not well simulated either, the whole behaviour of the cloud is quite wrong. In fact we have been forced to suppress this wrong feed-back loop between radiation, vertical diffusion and condensation, by artificially eliminating the cloud cover input to the radiation scheme when clouds occur within a convective boundary layer. This is clearly unsatisfactory.

The remedy to this problem, unfortunately, requires the knowledge of quantities on a vertical scale which our present model cannot give us. We need especially to be able to define a cloud layer which does not necessarily correspond to a model level thickness. Again it is possible that a higher order scheme, providing information on second moments, might help us define these sub-grid scale quantities, although no theory yet exists as to how to go about it.

Another approach is that used in the UCLA model where the height of the PBL is a prognostic variable of the model, and the thickness of a cloud layer at the top of the PBL does not have to be the same as a model layer. This model is described by Randall in this workshop. We have no experience with this kind of model and hope that this workshop will be helpful in shedding light on their possibilities.

Now, what about convective clouds? The Kuo scheme as currently implemented in our operational model has some definite weaknesses. First of all it cannot treat the transport of sensible heat and momentum within the cumulus layer by the eddy fluxes associated with the clouds. Furthermore the connection between the cumulus layer and the PBL is not very satisfactory, the effect on the PBL being treated in a rather arbitrary way. Finally the moisture convergence which actually drives the growth of cumuli in nature is probably on a scale smaller than a model grid square.

The first problem can be solved by chosing a cumulus scheme which computes explicitly the mass flux of the clouds. Various such schemes are investigated at the present time here. I will not dwell on this point in this paper. But the last two points are clearly related to the boundary layer. It seems that again a second order scheme for the turbulence should help since one can imagine cumulus clouds as being triggered by a parcel with an abnormally high temperature or vertical velocity. Hence the mass flux at the base of the clouds could be related to the variance of these quantities.

The question remains, however, whether the moments given by the second order schemes are those representative of the mesoscale, organized motions which trigger cumuli, or whether they are representative only of the small scale turbulence. In the latter case it is a little bit doubtful whether we could actually use these higher moments in connection with the cumulus parameterisation. On the other hand if the mesoscale motions do contribute to the variance described by these schemes, then the cumulus motions themselves should contribute. This would imply a very close connection between the cumulus and turbulence schemes, a connection which has not been developed yet.

Work on a second order closure scheme has already started at ECMWF. As a first step we have decided to test the "level 2" scheme of Mellor and Yamada (1974), in which a prognostic equation is used for the turbulent kinetic energy only. The other second order moments are assumed to be in steady state, with dissipation balancing production exactly. From this assumption some diffusion coefficients can be derived, which are functions of the turbulent kinetic energy and the vertical gradients of the various mean variables. At the time of this workshop we are still having some practical problems with the programming of this scheme, but we are confident that these can be overcome. We also look forward to discussions with the participants to this workshop who have had experience with higher order schemes.

References

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