Advancing closures for stably stratified turbulence in global atmospheric models

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ABSTRACT

Atmospheric turbulent motion under stable stratification continues to fascinate with its richness of structure, variability and phenomena, while seriously challenging our understanding of fluid mechanics and ability to predict weather and climate change. In this proceeding I will present nothing new - most of it is based on my doctoral thesis and work by others. Rather, I will take this opportunity to express my views on how I believe we can best pursue the challenge of representing stably stratified turbulence in large-scale atmospheric models in the short- and longer term.

1 Introduction

In the late 1990's a gap in our understanding was identified by scientists at the European Centre for Medium Range Weather Forecasts (ECMWF): If the diffusivity of the turbulence closure scheme in their global weather forecast model was increased, well beyond what can be justified with micro-meteorological observations, then weather forecasts improve significantly. The findings partly sparked the formation of the GEWEX Atmospheric Boundary Layer Study (GABLS, Holtslag and Randall, 2001), and have largely set the agenda for a significant part of research in the field since then. I can think of at least three possible reasons for the gap between what we understand and what seems to work in the ECMWF model[1]:

1. Micro-meteorological observations may be biased towards low diffusivity, for example due to limitations in the instrumentation, how they are deployed, or problems with statistical self-correlation.
2. Models have issues related to different processes, such as clear-sky radiation, clouds, convection, gravity wave drag or surface representation, that are partly amended by introducing excessive turbulent diffusion.
3. Processes that occur in reality are not represented in the models, for example slope-flows, unresolved surface heterogeneity or certain sources of gravity waves.

The forecast issues that are helped by increasing the diffusivity are: i) A night-time surface air temperature cold bias is reduced (Viterbo et al., 1999), and ii) the life-cycle of synoptic low-pressure systems is shortened to become more realistic[2].

The two identified model-issues benefitting from the increased turbulent diffusivity under stably stratified conditions first deserve some separate consideration (Sections 1.1 and 1.2). In what follows, I

[1] I talk mainly about the ECMWF model, because it is around that the discussion revolves. Other models may have similar, or different issues.

[2] Anton Beljaars, personal communication

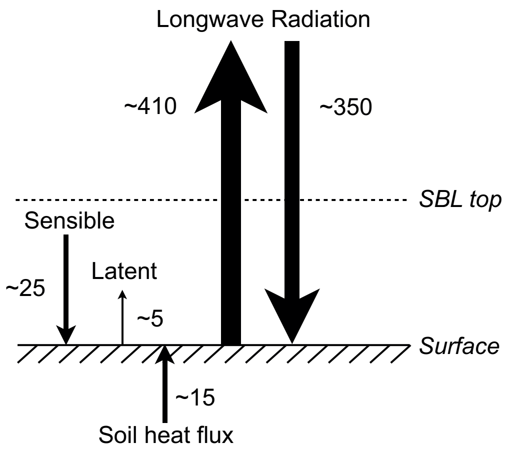

Figure 1: Nighttime mid-latitude continental surface energy balance. Approximate fluxes are in units of Wm^{-2} and are averaged 'by eye' from observations of the GABLS3 case.

will approach the problem of modeling stably stratified atmospheric turbulence in large-scale models from a fairly practical position. I will very briefly explain what turbulence closures must do, and give an overview of the major different approaches, while discussing their issues and how they relate to the identified problems in large-scale models.

1.1 Nighttime cold bias

First, let us consider the surface energy budget of the mid-latitude continental nighttime boundary layer (Figure 1). Numbers are observations from the GABLS3 case. The surface energy balance is dominated by cooling induced from the difference in the up- and downwelling longwave radiation fluxes, which is partly compensated by turbulent sensible- and soil heat flux towards the surface. To appreciate how delicate the balance is, one only needs to estimate that the infrared radiation changes by 4-5 Wm^{-2} for a black body temperature change of 1 K. In this perspective, even small relative errors in the longwave radiation calculation can have large impacts on the surface temperature. Such errors could arise from biases in the atmospheric temperature, water vapor, aerosol, clouds, surface emissivity or the radiative transfer calculation itself. For example, Zygmuntowska et al. (2011) found that biases in Arctic clear-sky longwave fluxes during summer can be explained by a strong dry bias in the ERA-Interim reanalysis.

The approach taken by Viterbo et al. (1999) was to increase the turbulent sensible heat fluxes towards the surface, essentially extending the ideas of Louis (1979). At the time, this was probably a reasonable assumption given the weak understanding and poor constraints on stably stratified atmospheric turbulence. Today, we know that the closure overestimates the stable boundary layer height, and underestimates the near-surface a-geostrophic wind turning. Still, recent attempts to reduce the diffusivity in the ECMWF model inevitably leads to increasing cold-biases.

1.2 Synoptic eddy activity

Mid-latitude cyclones are unstable baroclinic eddies that gain energy from atmospheric mean flow. The eddy activity is governed by a balance between the production of eddy energy and the rate at which it is dissipated. Although latent heat release certainly plays a role in real mid-latitude cyclones, one can understand the growth of baroclinic eddies from a purely dry dynamical framework. Making a series of assumptions, one can derive the Eady growth rate of baroclinic eddies:

$$\sigma \propto \frac{f}{N}\left|\frac{d\mathbf{V}}{dz}\right|,$$

where f is the Coriolis parameter, N the Brunt-Väisälä frequency, and $\mathbf{V}$ the wind vector. At mid-latitudes, one can to first order neglect variations in f and N. Then the growth rate is proportional to the vertical wind-shear, that is, effectively the strength of the tropospheric jets. The jet strength is, in turn, controlled by the tropospheric Equator-to-pole temperature gradient that sets the upper-level pressure gradient, the rate of conversion to eddy energy, and the drag exerted on the mean-flow jet itself. In models, this drag mainly pertains to turbulent stress and various kinds of orographic drag.

In this somewhat simplistic view of the problem, it appears that synoptic activity can be controlled by three sets of processes: Differential diabatic heating, mean flow drag and consumption by baroclinic eddies. And so, errors in one could be compensated by the other, although, I am convinced that sorting this out in a model is going to be much more challenging than I give the impression of here.

1.3 Why is it so difficult to model stable boundary layers?

It is relatively easy, at this point to understand why modeling the stably stratified boundary layer is a challenging task: As stratification increases, for an arbitrary reason, turbulent eddies are suppressed in their vertical extent, and therefore get less effective at transporting heat and momentum. The reduced heat transport will tend to increase the stable stratification further, which constitutes a positive feedback between the mean flow and turbulence. This behavior is the opposite to the dry convective boundary layer, where the feedback between stratification and turbulence is negative.

If a model of stably stratified turbulence has a slightly too strong reduction in turbulent diffusivity at increasing flow stability this positive feedback will amplify the stability of the flow resulting in too shallow boundary layers. On the other hand, a too diffusive model will tend to remain closer to neutral where mixing is effective. Luckily, even this dynamical system contains a negative feedback: As the stable boundary layer gets shallower the amount of shear across the layer increases, which will tend to increase turbulence. This is a consequence of the free-flow windspeed being set by the large-scale geostrophic balance combined with the no-slip surface boundary condition.

2 Turbulence closures

The task of a turbulence closure in an atmospheric model is very simple: 'Given a mean flow, provide the vertical fluxes of heat, momentum and tracers'. The mean flow and turbulence parts are separated using Reynolds decomposition of the atmospheric variables, $u = \overline{u} + u'$. Already here is the first challenge of a closure; to define what is meant by the mean flow in the very wide spectrum of atmospheric motion (Figure 2). This spectrum is traditionally thought to have two peaks, one in the synoptic scales driven by baroclinic instability and another in the micro-scales driven by convection and shear instabilities.

Current supercomputers are capable of running atmospheric models that span scales across about three to four orders of magnitude, that is thousands of grid points in each horizontal direction and on the order of hundred vertical levels. For global models this is equivalent to a smallest well-resolved[3] scale of about 50 km to more than 1000 km, depending on the application. Effectively our filter-scale for the Reynolds decomposition must therefore be somewhere in the mesoscales.

[3] Here I count scales to be well resolved when 4-5 times the grid-point spacing, while the most expensive NWP models run at around 10 km grid-spacing, and the typical climate models used for centennial simulations run on 100-300 km grids.

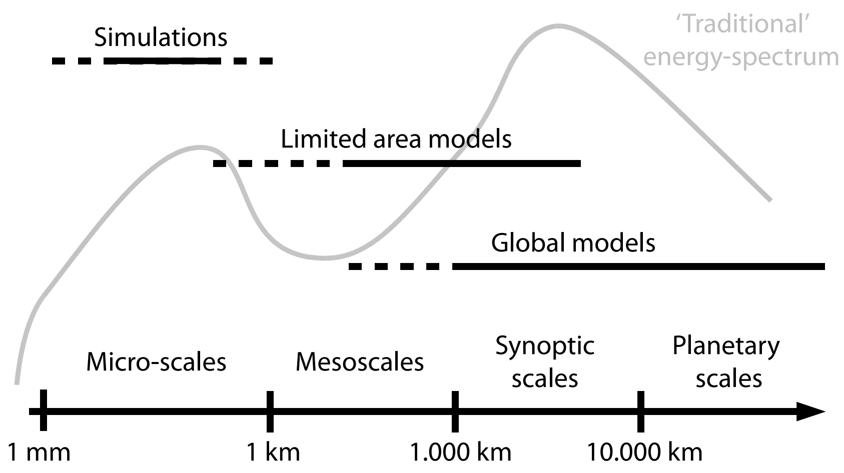

Figure 2: Illustration of our understanding of the motion scales in the atmosphere. Modified from Mauritsen (2007).

In the past, we have been content with this situation by the argument that there is not much motion energy in the mesoscales; the notion of a so-called mesoscale gap. By this argument one can place the filter-scale anywhere within the mesoscales as dictated by the model resolution, and the exact position would not matter much because the bulk of unresolved energy is anyway in the micro-scales. There is, however, evidence that the mesoscale gap is frequently insignificant in stably stratified flows, as well as in free atmospheric flows (**?**). Unfortunately, theories of how to parameterize the influence of this mesoscale motion on unresolved fluxes are lacking, we only have a theoretical framework for the micro-scales. Attempts have been made, but so far I have not been too impressed with the results. Further, the instrumentation we use to collect turbulence observations is often not very suitable to quantify the influence of mesoscale motion. So, at least in the near-future we will continue to apply 'turbulence closure' only to the micro-scales until a useful theoretical framework emerges.

2.1 K-closures

Most closures predict the vertical turbulent conductivity (K_h), viscosity (K_m) and diffusivity (K_χ), and it is frequently assumed that $K_\chi = K_h$. In first-order closures these K's have the following form:

Shear is given from mean flow

$$K = f l^2 S$$

Empirical or *ad-hoc* non-dimensional stability function

'Magic' dimensional length-scale

where f is a non-dimensional stability-dependent function - not to be confused with the Coriolis parameter, l is the turbulent mixing-length, and S is the mean-flow vertical wind shear, $S = |d\mathbf{V}/dz|$, which is provided to the closure. The formulation of the K's depend on the type of closure that is being solved, first-, 1.5-, second-order and so on, but in essence it still boils down to understanding nearly the same problem. I will elaborate on the ingredients of a closure below.

2.2 Turbulent length-scales

The formulation of the turbulent mixing length-scale in the K-closure is the most important part. It is often being related to the peak of the micro-scale energy spectrum, or the beginning of the inertial range, but that is not very useful for model parameterizations because no information is *a priori* available on these properties of the flow. Essentially the length-scale formulation should describe the size of the problem. Near the surface eddies are limited by the distance to the surface (the law of the wall), in the core of a typical stable boundary layer eddies are on the order of 10 m, in dry convective and very windy boundary layers more on the order of 100-1000 m, while near the top of the inversion capped stratocumulus-topped marine boundary layer the dominant mixing eddies can be as small as a few meters or less.

Ideally, a successful length-scale should capture all the possible regimes of turbulence in the atmosphere, and beyond, in one unified formulation. There exists certain regimes where physically-based formulations apply, but it remains an open question how these should be combined, and if additional limits must be considered. The inclusion of moist processes, for instance, makes the formulation significantly more complicated. Needless to say, there is a certain amount of *magic* to finding a well-working turbulent length-scale formulation for atmospheric models.

Because no information is available on the turbulence field, with exception of the friction velocity, most first-order closures use a prescribed mixing length for neutrally and stably stratified flows (Blackadar, 1962):

$$\frac{1}{l} = \frac{1}{\kappa z} + \frac{1}{l_0}, \tag{1}$$

where $\kappa \approx 0.4$ is von Karman's constant, z is height, and l_0 is an asymptotic mixing length. The ECMWF forecast model currently uses $l_0 = 150$ m.

There can be no physical justification for a length-scale formulation such as Equation (1) for atmospheric turbulence with a fixed dimensional l_0. A physically based formulation cannot contain dimensional parameters because of the turbulent flow self-similarity across vast scales of motion. Some formulations do use the surface friction velocity to estimate the length-scale (e.g. Steneveld et al., 2006), however, this choice makes them inherently non-local because all turbulence in the entire model column is scaled with the surface based turbulence, which is not necessarily realistic. A good example is turbulence in conjunction with an elevated jet, which has nothing to do with the surface friction velocity.

Early on, Rossby and Montgomery (1935) used physical arguments to come up with formulations for the depth of neutral and stable boundary layers. Qualitatively, the ideas of the boundary layer depth-scaling carry over to the modern concepts of turbulent mixing lengths and turbulent dissipation lengths. First, Rossby and Montgomery considered the neutral boundary layer in a rotating framework, that is the neutral Ekman layer (Ekman, 1905). In modern terminology, the neutral boundary layer is characterized by:

$$\frac{1}{l} = \frac{1}{\kappa z} + \frac{f}{C_f \cdot \sqrt{\tau}}, \tag{2}$$

where τ is the momentum flux or turbulent stress, and C_f is a non-dimensional parameter. One can also find formulations with $\sqrt{\tau}$ replaced with $\sqrt{E_k}$, in which case the value of the parameter changes (e.g. Angevine et al., 2010). One may simply think of it as a typical turbulent velocity. Mauritsen et al. (2007) inversely found $C_f \approx 0.2$ by tuning their model to fit large-eddy simulations; it may prove very difficult to determine C_f by anything but computational flow simulations. The magnitude of $l_f \equiv C_f \cdot \sqrt{\tau}/f$ increases with increasing turbulence, and decreases with increasing rotation. For typical mid-latitude atmospheric conditions l_f is on the order of 1000 m near the surface, and therefore not important relative to κz, while it decreases with height, dominating the flow in the upper parts of the neutral boundary layer. Note that the full magnitude of the rotation - not just the vertical part - should be used in the formulation to properly account for Equatorial conditions. Rossby and Montgomery (1935) then extended their

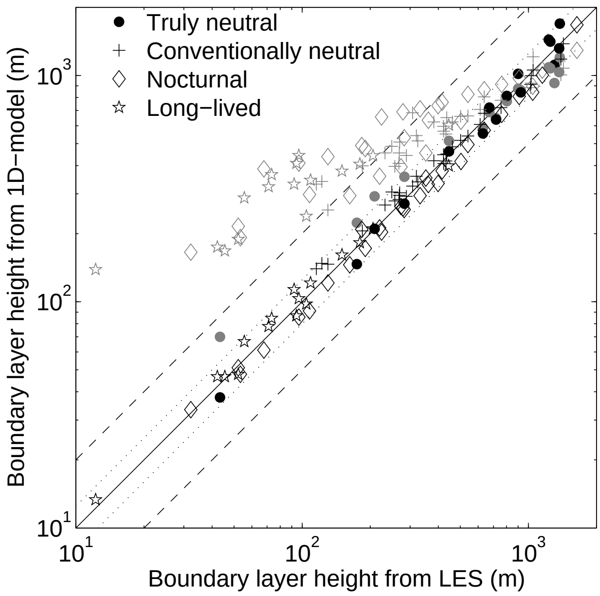

Figure 3: Comparison of boundary layer height from nearly hundred large-eddy simulations with two turbulence closure models. Black symbols is a model based on Equation (3) by Mauritsen et al. (2007), while grey symbols are based on Viterbo et al. (1999) applying Equation (1). Note that also the other parts of the closures are different. Reproduced from Mauritsen et al. (2007).

analysis to stably stratified conditions. After reading their paper again, however, it seems to me that their analysis is not equivalent to the below. Interestingly, they apply the concept of turbulent potential energy - a concept which is only slowly gaining ground in the atmospheric turbulence modeling community.

Brost and Wyngaard (1978) and Nieuwstadt (1984) suggested to parameterize the turbulent length-scale in the limit of stably stratified conditions away from the surface as the ratio of the inertial to the buoyancy forces. For the turbulent velocity representing inertial forces they used the vertical velocity variance, and the buoyancy forces were represented by the Brunt-Väisälä frequency. Using instead $\sqrt{\tau}$ as velocity scale, and interpolating this limit with the neutral length-scale (2) we obtain the length-scale used by Mauritsen et al. (2007):

$$\frac{1}{l} = \frac{1}{\kappa z} + \frac{f}{C_f \cdot \sqrt{\tau}} + \frac{N}{C_N \cdot \sqrt{\tau}}, \tag{3}$$

where C_N is another non-dimensional parameter. Mauritsen et al. (2007) found $C_N \approx 2.0$. They used this formulation in their turbulence closure scheme to model neutral and stably stratified boundary layers of depths across two orders of magnitude (Figure 3). Note how the closure by Viterbo et al. (1999) which is based on the rigid Equation (1) is unable to reproduce the variation of the boundary layer height because of the fixed l_0, and is really only matching the large-eddy simulations in the 500-1000 m range.

Dimensional arguments yields yet another limit involving vertical shear, $l_S = C_S \cdot \sqrt{\tau}/S$. Note the analogy with l_f and l_N. This length-scale behaves as κz close to the surface. Therefore, bluntly including it as another term in the length-scale violates the law of the wall (Mauritsen and Enger, 2008). Interestingly, it can be shown that l_S can replace κz if $C_S = 1.0$, which might mean that the law of the wall is nothing but a special case of the more general shear length-scale. I haven't seen this length-scale implemented in an atmospheric model, yet.

2.3 Flow similarity and stability functions

Stability functions are non-dimensional and ideally derived from observations on the basis of a similarity-theory. There are at least two very distinct ways this can be done; one is based on Monin and Obukhov (1954) similarity, the other on the Richardson number (e.g. Klipp and Mahrt, 2004). The approaches are fundamentally different in that Monin-Obukhov scaling defines the flow stability in terms of the turbulent fluxes:

$$\frac{z}{L} = \kappa z \cdot \frac{g}{\overline{\theta}} \cdot \frac{-\overline{w'\theta'}}{u_*^3}, \tag{4}$$

whereas the Richardson number approach is defining flow stability in terms of the mean flow gradients of buoyancy and wind:

$$Ri = \frac{N^2}{S^2}, \tag{5}$$

where z is height, L the Obukhov length, g gravity, $\overline{\theta}$ is the mean flow potential temperature, $\overline{w'\theta'}$ the vertical potential temperature flux, u_* is the friction velocity, and N the Brunt-Väisälä frequency.

2.3.1 Monin-Obukhov similarity

Monin-Obukhov scaling has been unbelievably successful for more than half a Century, it is built in to practically every atmospheric model in existence today in some form. In Monin-Obukhov scaling one would organize turbulence data into non-dimensional forms, plot it as functions of z/L and fit empirical stability functions. For example, one can plot the non-dimensional shear:

$$\phi_m(z/L) = \frac{\kappa z}{u_*} \cdot \frac{\partial u}{\partial z}, \tag{6}$$

and obtain a remarkable agreement that ϕ_m increases dramatically in stably stratified conditions from its neutral limit. This indicates that the momentum flux, represented by u_*, gets smaller for a given wind shear as z/L increases, which makes intuitive sense.

Unfortunately, Monin-Obukhov similarity scaling suffers from a number of problems. The most significant problem, in my opinion, is due to statistical self-correlation and has been noted for more than three decades (Hicks, 1978). When plotting ϕ_m versus z/L, it is clear that u_* appears in the denominators of the non-dimensional expressions on both axes. Now, suppose there is a small negative error in the measurement of u_*. Then z/L will become larger, but so will ϕ_m, which will tend to make data line up and make ϕ_m increase rapidly by completely artificial means. Bruce Hicks wrote in his note:

> *'The suggestion of an artificial correlation imposed by analysis methods is by no means new, and may well fall into the category of common knowledge ... After all, the purpose of any analysis is certainly not to create a mere semblance of order where only randomness exists.'*

The issue of self-correlation should be known to anyone working on statistical analysis of observations in science; I was taught that errors must be independent when making statistical relations during my first year of undergraduate studies. On the other hand, I do appreciate the seductive nature of self-correlation, much like the Sirens of Greek mythology luring sailors with their beautiful songs to wreck at the coast of their island, self-correlated plots at first appear convincing because of the collapse of data towards simple power laws. Unfortunately these power-laws are dictated by the error covariance, not physics.

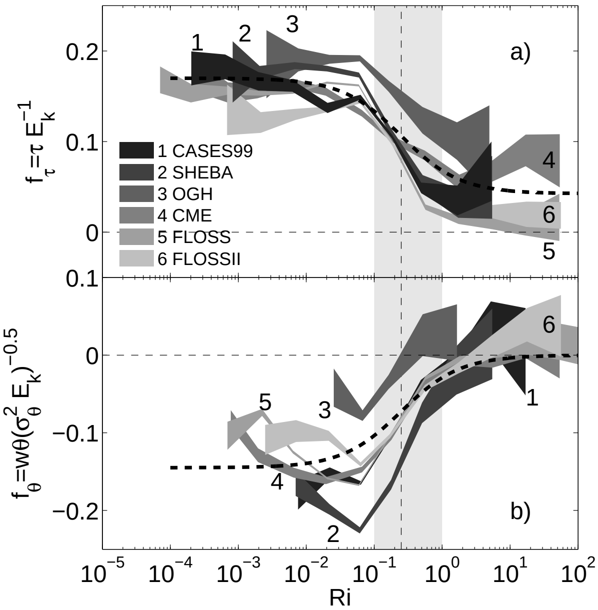

Figure 4: Observed non-dimensional a) momentum and b) heat flux as a function of Richardson number from six different field experiments. Grey shaded areas are 95 percent confidence intervals on the bin mean value. Bins with few observations are therefore wider. Modified from Mauritsen and Svensson (2007).

2.3.2 Gradient-based similarity

The alternative to MO similarity is to instead relate the mean flow stability to the Richardson number (Equation 5), which is defined purely in terms of the mean flow itself. This has a number of advantages, but is also challenging in certain respects. While the theoretical, observational and modeling development surrounding MO is very far advanced, of course, the Ri-based alternatives are few and often somewhat incomplete as theories (e.g. Klipp and Mahrt, 2004; Sorbjan, 2006; Mauritsen and Svensson, 2007; Mauritsen et al., 2007).

An example of Ri-based similarity is shown in Figure 4. The two panels show normalized momentum flux and heat flux from a compilation of data obtained from six field campaigns. All the datasets exhibit a distinct regime change from weak stability at $Ri < 0.1$ to strong stability at $Ri > 1.0$. Particularly interesting is that the normalized momentum flux tends to stay finite at even very large Ri, which indicates that active turbulence occurs. This finding is contrary to MO, which indicates rapidly decreasing momentum flux with stability because of the above mentioned self-correlation. As we shall see below this has strong implications for how we understand turbulence under strong stratification.

The main advantages of using Ri as a basis for similarity is that it is always well-defined in terms of the mean-flow, and if using only turbulence quantities on the y-axis it yields self-correlation-free plots. There are, however, also unresolved issues with observations presented in Figure 4. Unlike MO which is all based on a single instrument (typically a sonic anemometer), Ri-based scaling requires data to be obtained from three different levels and from 3-5 instruments: A sonic anemometer measures turbulent fluxes and variances, while instrumentation (either additional sonic anemometers, or slower sensors) situated above and below measure the mean flow gradients. Only a limited number of field experiments applied such configuration of the instrumentation, and it does place large demands on instrument calibration.

Determining Ri is limited at the low end by the sensors ability to determine very weak buoyancy gradients, and at the high-Ri end by their ability to determine weak wind-gradients. It may for example be difficult to distinguish slightly unstable from slightly stable conditions, which may contaminate particularly the normalized heat flux at small Ri. Further, the sonic anemometers may be noisy to different extents which influences the estimates of variances, less so the fluxes. Consequently, the level of the normalized fluxes may be instrument- or post-processing dependent; compare for example the dataset 2 and 3 normalized heat flux: The other datasets happen to all use the same post-processing and they do agree well.

Empirical fits to data, such as the dashed lines shown in Figure 4 can be used as a basis to form a turbulence closure model. Such a closure model needs to predict or diagnose the turbulent kinetic energy and turbulent temperature variance. Then, because Ri is given by the mean flow state, it is straightforward to obtain the fluxes. Mauritsen et al. (2007) present one way to do this, whereby total turbulent energy is predicted, and then the contributions from turbulent kinetic and potential energy is diagnosed. This can also be done in a hierarchy of ways, and I would encourage a systematic study equivalent of Mellor and Yamada (1974).

2.4 Does a critical Richardson number exist?

Long-standing theories suggest that if the stratification dominates over the wind shear, then turbulence will decay and the flow will tend to become laminar. Understanding whether this hypothesis is correct or not is central to modeling stable boundary layers. Richardson (1920) investigated the evolution of the atmospheric motion energy of small scale disturbances, the turbulent kinetic energy (E_k). For this, he first derived the budget equation for E_k:

$$\underbrace{\frac{DE_k}{Dt}}_{\text{Storage}} = \underbrace{\tau \cdot \mathbf{S}}_{\text{Shear}} \underbrace{+\frac{g}{\theta}\cdot\overline{w'\theta'}}_{\text{Buoyancy}} \underbrace{-\varepsilon}_{\text{Dissipation}} \underbrace{-\frac{\partial F_k}{\partial z}}_{\text{Transport}}. \tag{7}$$

If we ignore the vertical transport of E_k, then Equation (7) states that the rate of change of E_k is a balance between the shear production, buoyancy conversion and viscous dissipation. The shear production is positive, while the buoyancy term is negative in stable stratification. At the time, Richardson did not know how to parameterize the turbulent kinetic energy dissipation, but he figured that it should somehow be proportional to the amount of turbulence. Later, Kolmogorov (1941) showed that for isotropic turbulence the dissipation is proportional to $E_k^{3/2}$.

Richardson then argued that if we consider the case when E_k is small, but finite, such that the turbulent shear production is not zero, but sufficiently small that dissipation can be neglected. Then turbulence will grow ($DE_k/Dt > 0$) if the shear production exceeds the buoyancy term:

$$\tau \cdot \mathbf{S} > -\frac{g}{\theta}\cdot\overline{w'\theta'}, \tag{8}$$

which can be rewritten to:

$$\frac{K_m}{K_h} > \frac{N^2}{S^2}. \tag{9}$$

The entity on the left hand side is known as the turbulent Prandtl number, while on the right hand side we recognize the Richardson number, such that the inequality reads:

$$Pr_T > Ri. \tag{10}$$

Richardson then assumed that $K_m = K_h$, in the lack of better, such that the necessary condition for turbulence to grow can be stated $Ri < Ri_c = 1$, where Ri_c is the critical Richardson number. This means that if the flow stability is weak, $Ri < 1$, then turbulence will grow to a level where the production is balanced by dissipation. Contrary, if $Ri > 1$ the flow is too stable to support turbulence growth according to Richardson's results. Later theoretical studies have supported Richardson's notion by other means (Chandrasekar, 1961; Miles, 1961; Howard, 1961).

Observations, laboratory experiments and computer simulations do support a Pr_T close to unity in near-neutral conditions, 0.7-0.8 most often being reported. However, Pr_T appears not to be constant, rather observations and computer simulations indicate that it is a function of Ri such that $Pr_T \propto Ri$ for large Ri (A collection of data can be found in Zilitinkevich et al., 2008). In this case the inequality (10) is always satisfied, which means that turbulence will grow to achieve a balance of the shear production with dissipation and buoyancy conversion, in dire contradiction with the before mentioned theoretical studies.

Unfortunately, the analysis of the turbulent Prandtl number as a function of the Richardson number from observations is hampered by the very same statistical self-correlation problems that eclipsed the Monin-Obukhov similarity. In fact, it is possible to show that surrogate random observational data will exhibit the relation that $Pr_T \propto Ri$. This is because S and N are used to calculate Pr_T. Indirect support for an increasing Prandtl number with stability is however found in self-correlation-free plots (Figure 4), showing that the normalized momentum flux tends to a finite value at supercritical Ri, while the normalized heat flux tends to zero.

3 Concluding remarks

It is by now well-established that turbulence, or maybe more correctly micro-scale and mesoscale motion that occur on spatial scales not captured by global atmospheric models, occurs in most parts of the atmosphere, practically all the time (e.g. Nastrom and Gage, 1985; Mauritsen and Svensson, 2007; Balsley et al., 2008). This motion is not confined to the boundary layer, seriously challenging existing atmospheric turbulence closure models which are traditionally thought to mainly be applicable at sub-critical stratification and within the surface-based boundary layer. These schemes are often referred to as 'boundary-layer schemes' – a concept which I find misleading: Turbulence closures must be applicable to the entire atmosphere.

In the short term I believe it is a reasonable strategy by the ECMWF to continue to adjust the existing turbulence closure scheme to improve the weather forecasts. Earlier, the rationale was that the turbulence in the stably stratified part of the atmosphere was the most uncertain part of the model, hence, this is the best part of the model to tune. This paradigm must change; we are now convinced that the model was tuned to be far too diffusive. The excessive mixing impacts for example boundary layer depth, near-surface wind-turning and vertical wind-gradients (important for wind-power forecasts, and tracer- and pollutant transport), and reduces low-level stratiform cloudiness by excessively mixing across the capping inversion[4]. Issues that become increasingly problematic as the demands for derived forecast products increase.

My recommendation is to pursue a more realistic level of mixing in the turbulence closure scheme, but because we can expect that this will have adverse effects on other aspects of the forecast quality, this must be accompanied by adjustments in other parameterizations of the model. For example, the momentum budget can be controlled using orographic drag and the cold-biases may be corrected by altering cloud parameters (Mauritsen et al., 2012). This will, without a doubt, be tedious work and require an altruistic and collective effort in order to succeed.

In the long term, however, model-tuning is no substitute to understanding the basic physics of stably stratified turbulence. I have outlined a few key problems in this proceeding. I have pleaded for a framework where the mean-flow gradients are used to estimate the flow stability through the Richardson

[4]Tests made with the ECHAM6 climate model shows, among other things, profound positive impacts on the sub-tropical stratocumulus when reducing the super-critical turbulent diffusivity of the model.

number, as opposed to using the fluxes: Using empirical stability functions derived from the widely used Monin-Obukhov similarity theory essentially hinder a direct connect between observations and models, because turbulent momentum transport is invariably underestimated due to self-correlation. Rather turning to the Richardson number as the similarity parameter permits exploration of super-critical flows in observations. A key to understanding the physics of the stably stratified turbulence is in the behavior of the turbulent Prandtl number as a function of the Richardson number; if it increases with stability then turbulence can be sustained. Measuring this behavior, however, is going to be challenging.

Finally, I believe that major gains can be achieved by implementing physically based turbulent length-scale formulations such as Equation (3). This will require either predicting or diagnosing second moments of the flow, which means one must abandon the first-order closure for a form of second-order closure. I am not *per se* in favor of increasing the complexity of models, though, as far as I am aware a second-order scheme is the only way to build a closure that is able to contain our current understanding of stably stratified turbulence.

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