Review of acm-2015-1015

Overall Comments and Recommendation:

In this paper, the authors have developed what they feel is a model for subgrid turbulence in a mesoscale atmospheric model. According to my understanding of the model, a coarse-grained turbulence model is first averaged, then used to drive a Lagrangian stochastic dispersion model which is said to represent the effects of subgrid scale turbulence. The authors note that this is a first step, and thus there is only one-way coupling, or in other words the Lagrangian particles are not used to send energy back to the larger scales. As viewed by the authors, the novel aspect of the paper is that, in contrast to previous work, no assumption is made about the shape of the probability distribution function for the subgrid scales.

Based on the description given of the modeling methods, I have some major concerns with the theory behind the work, which are addressed below. As such, I recommend major revisions before considering the manuscript for publication. I have also given some minor suggestions mostly regarding formatting that would be helpful if there is a future review. I have not provided an exhaustive list of minor revisions given the degree of revision suggested below.

Major Comments:

1. The methods described in this paper are not a "sub-grid turbulence model". This is simply a RANS Lagrangian particle dispersion model. Essentially, the authors average out the grid-scale turbulent motions, and instead use the mean fields to drive the Lagrangian dispersion model (at least, this seems to be how it is described in the text). Thus, the mean field is the 'resolved' component and the Lagrangian evolution equation is used to add in the 'fluctuating' component, or in other words the authors are using a Reynolds decomposition:

$$u_i = \langle u_i \rangle + u_i'. \tag{1}$$

where $\langle u_i \rangle$ is an ensemble average, and u_i' is the fluctuation from that average. If I am understanding the paper correctly, the authors then prescribe $\langle u_i \rangle$ using the Eulerian simulations, and u_i' is (indirectly) determined using the Lagrangian stochastic evolution equation.

In order for this to be considered a "subgrid scale" model, the following decomposition would be used

$$u_i = \tilde{u}_i + u_i'', \tag{2}$$

where now $\tilde{u}_i$ is the filtered component (filtered at the grid scale), and u_i'' is the unresolved or subfilter component, which would be determined by solving the evolution equation in terms of the subfilter component:

$$du_i = a_0 dt - a_1 \left(u_i - \tilde{u}_i\right) dt + b\, dW, \tag{3}$$

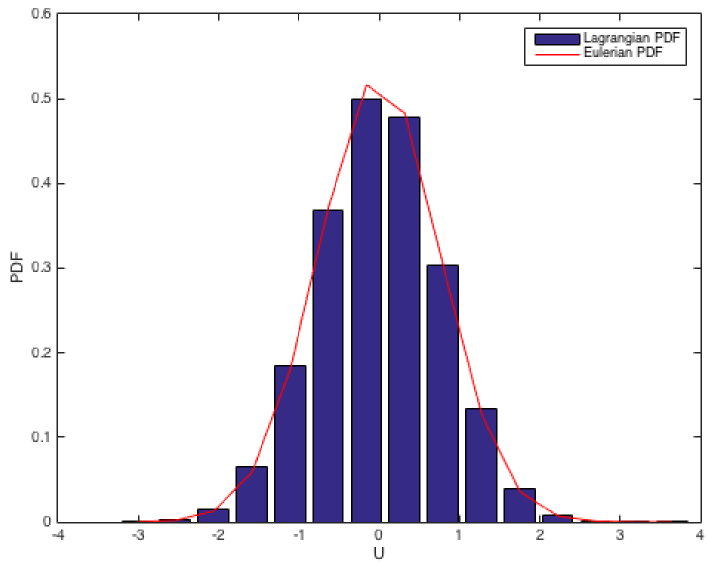
where dW is an increment in a Weiner process, and coefficients a_0, a_1, and b are determined such that Eq. 3 satisfies the Navier-Stokes Equations. For examples of determining the coefficients see Weil et al. (2004), Shotorban and Mashayek (2006), or Vinkovic et al. (2006). These examples are applied to traditional LES applications, but the principles are the same as in the present manuscript. Thus, the turbulence calculated at the grid-scale is retained, and modeling is reduced to specifying only the subgrid scale turbulence. Why filter out valuable resolved turbulence by averaging? The point of subgrid scale modeling is to retain as much information as possible, such that modeling is simplified in that we only have to model the smaller, more 'universal' scales.

2. The authors state that the novelty of the proposed modeling methodology is that (Lines 77-78) "The method we suggest differs from these previous works: no assumption is made on the pdf shape." This is not true. By using a Langevin-based equation for the particle motions that is forced by a Weiner process, *the authors are effectively specifying the shape of the pdf.* Although Sects. 2.2 and 2.4 of Pope (1994) (for example) states that these methods do not assume a pdf, the form used by the author does assume a pdf. Particularly, it is assumed that the pdf is Gaussian with variance of $\sigma^2 = \frac{C_0 K}{2C_1}$ (where of course K is the turbulent kinetic energy). The authors can verify this for themselves. Take the Lagrangian evolution equation, simulate some particles, and calculate the pdf. I have attached some sample MATLAB code to do this in an appendix of this review. For simplicity I have assumed that ε and K are constant, and that there is no mean drift (i.e., $\nabla p = \langle U \rangle = 0$). Means could be included, but they should only shift the pdf and not affect its shape. We should find that an ensemble of particles should have a velocity pdf of

$$P = \left(2\pi\sigma^2\right)^{-1/2} \exp\left(-\frac{u^2}{2\sigma^2}\right), \tag{4}$$

where again $\sigma^2 = \frac{C_0 K}{2C_1}$ is the prescribed velocity variance. As a side note, this is not exactly true for the authors' equation for W given the arbitrary way that they have included buoyancy (i.e., it is random and thus will generate erroneous additional variance). If it was correctly modeled, it should create a skewed velocity distribution and thus none of the analysis above or in the manuscript is valid. See Cassiani et al. (2015) and the references therein for examples.

The output of the MATLAB code verifies this (shown below). In essence, it seems that it's not necessary to simulate all the Lagrangian trajectories. Simply use the above equation to get the pdf. If the velocity is of interest, substitute σ^2 at any point (interpolated) and use the pdf along with a uniform random number generator to draw a random u.



3. In the (unnumbered) equations following line 348 (note that line numbers are messed up here, there are more than 5 lines between labels 335 and 340), why is an additional noise term added to the position evolution equation (X)? This is unconventional. Typically, the stochastic noise comes in through the velocity. By adding additional noise to the position statistics will not agree with the velocity statistics. By definition, a particle moves according to its velocity (see for example: Pope, 1994, Eq. 17).

4. Did the numerical solutions produce the so-called "rogue trajectories" (e.g., Yee and Wilson, 2007; Postma et al., 2012; Wilson, 2013), and if so, what was done to deal with them? And how might this affect results, particularly the energy spectra?

5. One problem with validating these subgrid models is that we often don't know what the correct answer is, and many times using no model is better than using a bad model. My opinion is that a more careful validation should be performed before making claims that the model is performing well. Based on the exercises presented, I do not feel such claims could be made. If the authors wish to test the model more closely, perhaps an *a priori* test, or even a toy problem may be a better means of testing the model.

Overall: With all of the above said, I'm not sure exactly what I would do to resolve all of these issues. Here is one suggestion. First, the authors should resolve the issues with the modeling and make it a true subgrid scale model. Then this could turn into a more applied study that is less about modeling methods and focuses more on the BLLAST experiment. I don't know all the measurements available from the BLLAST experiment (it is not well described in the paper), but perhaps you could force the LES with some larger-scale data and compare to some local measurements. Compare the two and discuss the successes/challenges.

Minor Comments:

1. Please number the equations.

2. Anytime an equation is added, it seems to mess up the line number count that follows.

Appendix: Example MATLAB Code

```
clear

C0=4; %Kolmogorov's "universal" constant
C1=0.5+0.75*C0; %Pope's modified constant
eps=0.1; %turbulence dissipation rate
K=1; %turbulent kinetic energy


N=100000; %number of particles to be simulated
T=10; %length of simulation
dt=0.1; %time step


%initialize particle velocities using a normal distribution with unit
%variance.  NOTE: the initial values shouldn't matter provided T is
%large enough (particles should 'forget').
u=randn(1,N);

%march the velocity in time
for t=1:ceil(T/dt)
  u=u-C1*eps/K*u*dt+sqrt(C0*eps*dt)*randn(1,N);
end

%calculate the PDF from Lagrangian velocities
[P,U]=hist(u,15);
dx=U(2)-U(1);
P=P/sum(dx*P);%normalize so PDF integrates to unity


%"exact" Eulerian PDF
sigma2=K*C0/C1/2;
Gauss=1/sqrt(2*pi*sigma2)*exp(-U.^2/(2*sigma2));

%plot it
figure;hold on;
bar(U,P)
plot(U,Gauss,'-r')
xlabel('U')
ylabel('PDF')
legend('Lagrangian PDF','Eulerian PDF')
```

References

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