# Review of acm-2015-1015

## Overall Comments:

This review concerns the manuscript acm-2015-1015, which I reviewed previously. I have read through the authors' replies to both previous reviews. I still have some substantial concerns related to the overall methodology of the paper and some of the technical details. I tend to agree with the other reviewer's comments that the organization and presentation of the paper could be significantly improved. After reading through the paper quite a few times, I believe I may have a handle on the overall methods of the study. Based on my own (probably biased) opinion, I feel that the authors have made things much more complicated and confusing than they need to be. I would summarize the study as follows: A mesoscale CFD model (Meso-NH) is used to simulate flow in the atmospheric boundary layer at the kilometer scale. Since it is not computationally feasible to resolve the smallest eddies, the velocity provided by Meso-NH is essentially a 'filtered' velocity that is missing energy due to subgrid effects. The overall goal of this paper, is to develop a methodology that can give a statistical representation of the *total* velocity (resolved plus subgrid) on a grid with arbitrary resolution. For this task, they use a Lagrangian stochastic dispersion model to give the total Lagrangian velocity, which is aggregated over an Eulerian grid cell using a local averaging operator. The Lagrangian model is based on that of Pope, but it is essentially the same as that of Thomson to within a constant and minus the 'drift correction' terms. The unusual thing here is that it would appear that although there is some resolved turbulence in the Meso-NH solution, they apply a RANS Lagrangian model, which seems that it is inconsistent (more on this below). The authors also throw in some arbitrary buoyancy and truncation error correction terms, which in my opinion are inconsistent as they are ultimately just arbitrary Gaussian noise.

To test the model, the authors use Meso-NH to simulate a 15-hour portion of the BLAAST experiment on a grid of $256^3$ points. But since the authors argue that it is too computationally expensive to consider all this data, and instead they only consider a 15-minute period on an $8 \times 8 \times 4$ sub-set of the grid. The Lagrangian model is driven by filtering the $256^3$ "fine" grid simulation, to give a "coarse" grid simulation that is half the resolution. The Lagrangian model (driven by the "coarse" data) is validated by comparing back to the "fine" grid data. It is my impression that the authors feel that the velocity statistics from the Lagrangian downscaling model should match the Eulerian statistics on the "fine" grid. They of course find that the downscaling model adds in back in too much energy, but overall they conclude that "The particle wind seems in good agreement with the high resolution wind".

Overall, the problem seems to me to be that 1) the model is inconsistently formulated, and 2) the "validation" methodology is ill-posed. Unless I am misunderstanding something, the solution to all of this is relatively straightforward, as much of this has been done in the past in different ways, the pieces just need to be put together. As detailed below, this involves 1) using a form of the Lagrangian model that is theoretically consistent, and 2) validation using a consistently-posed study. The comments below are not meant to be overly onerous or critical, but rather helpful. For transparency I would also note that stochastic downscaling of mesoscale data is not my research area, and thus I am not trying to "protect" my own work in any way (i.e., whether or not this work is published has no effect on my own work).

## Previous Review Major Comments:

1. Sub-grid turbulence model: My previous comment was essentially that it appears that the model is inconsistent with behavior that is expected in a subgrid turbulence model. My claim was that the particles should be driven by the resolved Eulerian velocity field, and the Lagrangian model should be predicting the unresolved velocity.

Based on my understanding, the behavior of the model presented by the authors appears to be inconsistent with that of a subgrid-scale model. Perhaps this is because my understanding of the model formulation is still incomplete, in which case the authors may be able to explain its consistency through the following simple thought experiment. Simply consider how the model should respond as grid resolution is varied. As the grid scale approaches the Kolmogorov scale, the subgrid effects should tend toward zero (this is the most fundamental quality of a subgrid model). And in the context of the particle model, the particle velocity should match the Eulerian velocity: $V \to v$ and $W \to w$. Forgive me, but looking at the equation following Line 419 I don't see how this will happen. In the opinion of this reviewer, the authors either need to demonstrate that the model is at least consistent in this regard, or use a model formulation that is consistent.

I am still not entirely sure why the authors have set up their Lagrangian equations the way they have, rather than using a standard approach that is known to be consistent (it is no more difficult or costly). The authors are of course free to use any approach they like as long as it's consistent, I am just curious.

So why not calculate the total velocity $V$ as the sum of the resolved Eulerian grid velocity (known, call it $v$) and model the unresolved component using the SLM (unknown, call it $\tilde{v}$); i.e., $V = v + \tilde{v}$? Note that in the current paper, $v = -\nabla_x \overline{p}$ which is wrapped into the total equation for $V$, whereas here $v$ is interpolated from the Eulerian simulation to the particle location and thus is 'known'. Then the evolution equation for $\tilde{v}$ is left to model, which is

$$d\tilde{v} = -C_1 \frac{\varepsilon}{e} \tilde{v} dt + \sqrt{C_0 \varepsilon} dB, \tag{1}$$

where $\varepsilon$ and $e$ are turbulent dissipation rate and subgrid TKE, respectively, which are interpolated from the Eulerian grid to the particle position. Now, our model will at least be consistent as the grid is refined: $e$ tends toward zero as the grid scale tends toward zero (as a result this dissipation term becomes very large and damps out all fluctuations), and thus $\tilde{v} \to 0$ and $V \to v$. This formulation is also consistent if you go the other way and tend toward RANS. As the grid scale becomes very large, $e \to K$, $v \to \overline{v}$, $\tilde{v} \to v'$, and thus we converge to a standard RANS downscaling model. This seems important for Meso-NH, which can be 'switched' between RANS and LES modes. Only in the case of RANS where no turbulence is resolved is the resolved velocity equal to $-\nabla_x \overline{p}$ and the unresolved TKE equal to $K$.

2. Gaussian assumption: Unfortunately, I am still in disagreement with the authors' position, as well as their newly added statements, e.g., "It leads that in this study, in a given grid cell, particles are samples of different Gaussian pdf. " It doesn't matter if the turbulence statistics vary in space, the particle velocities will be locally Gaussian with variance, dissipation, etc. equal to the Eulerian value specified at that point. This is the idea behind the Pope/Thomson Lagrangian stochastic dispersion models. We specify a PDF at every point, and we seek to generate an ensemble of Lagrangian particles that has that PDF and has a local dissipation rate of $\varepsilon$. This is how the models are derived, and how they work out in practice. Any deviation from this is caused by numerical error (or in the authors' case it could be due to the arbitrary stochastic terms that were added). The advantage of using an SLM rather than just sampling a PDF is that the particle velocities are correlated in time (this is a result of the fact that they must have dissipation rate of $\varepsilon$, or more directly that they follow Kolmogorov's second similarity hypothesis). Also as a side note regarding why the histograms presented by the authors (Fig. 1 of reply) do not appear Gaussian: the sample size is too small to make such an assessment. With so few samples (i.e., 75), the histograms are unlikely to look Gaussian unless we get lucky. The authors can test this using the MATLAB code I provided in the previous review, and setting N=75. In this case the PDF will usually look quite non-Gaussian (depending on 'luck'), and as N is increased the PDF converges toward Gaussian.

With all that said, I am not particularly concerned with this issue as it pertains to the manuscript. The original motivation for this comment was related to the novelty of the paper. Let's say for the sake of argument that the authors are correct that for some reason the particle velocities are highly non-Gaussian when aggregated over a grid cell. Even if that is so, what is the novelty of the downscaling methodology when considering the work of, e.g., Bernardin et al. (2009)? Looking at their Eq. 19b, the only difference I see between the authors' velocity equation is the arbitrary "buoyancy" term that was added. Based on my own viewpoint, I would say that the novelty lies in the fact that the authors have used the Meso-NH model, which based on my limited understanding, is somewhat of a hybrid between a RANS and LES code. In that case, it seems that the downscaling method should be consistent in that regard, and thus it might make more sense to use the approach described in the previous comment.

3. Stochastic numerical error 'correction' term in particle position equation: Can the authors provide any references that 'correct' for the numerical errors in this way? I can't see how adding additional dispersion somehow corrects for the numerical dispersion – in this case I don't see how two wrongs make a right, as both the error and correction appear to be additive and dispersive. Typically, numerical truncation errors are reduced by reducing the timestep, or there are refinement methods that use a systematic approach to (usually iteratively) improve the solution. Maybe the authors are using some method that I am unfamiliar with, in which case I am curious to read more (perhaps from a reference).

Also, I would note that the 'correction' is being added to the position evolution equation, yet the explanation given by the authors seems to pertain to errors related to the velocity evolution equation. How are the two related?

4. Rogue trajectories: Overall, the authors' response was sufficient. I would recommend one thing the authors might try. What happens to the power spectra when you decrease the particle integration timestep $\delta t$ by, say, an order of magnitude? Does it get rid of some of that extra energy at small-scales? If so, there may be some issues with numerical stability although it might not be manifesting as 'rogue' trajectories in the traditional sense.

5. Validation: Even on the "fine" Meso-NH grid, the velocity field is presumably still missing unresolved (subgrid) energy, which I'm guessing is not negligible (looking at the spectra in Fig. 9, it appears to be substantial). This is why the Mesh-NH velocity is much "smoother" than the particle velocity. So the When you filter (i.e., average) to get the "coarse" grid, some additional energy is removed, let's call that $\Delta e$. So the total TKE for the coarse grid is $K_{res} + \Delta e + e$. Here is something to try: why not use $\Delta e$ instead of $e$ in Eq. 1 (above), and compare to the resolved TKE from the "fine" solution? In this case, the goal of the downscaling model is to recover $\Delta e$ rather than $\Delta e + e$, which means you can directly compare to $K_{res}$ for the fine grid. My opinion is that simply saying that for future work "higher resolution simulations should be performed" is not acceptable.

Regarding a "toy problem": I could think of some tests that could be useful here. What about generating some random 'turbulence' (could be white noise or correlated) on the "fine" grid, then filtering it to get a "coarse" grid? You could calculate the TKE of the fine grid turbulence (this is '$K = K_{res} + e$'), then calculate the TKE of the coarse grid (this is $K_{res}$). Now drive the particle model with $e$ and downscale to the fine grid, where you should find that the TKE of the total particle velocity is $K$. This of course is non-physical and probably wouldn't go into the paper, but could be a good verification check to demonstrate consistency.

## Minor Comments:

1. In my own experience, the term 'downscaling' is usually used to describe a one-way model from large to small scales, whereas 'sub-grid modeling' is typically reserved for two-way coupling where the large-scale model needs to parameterize the small-scales. I would consider this work to address downscaling. I would leave it up to the authors discretion, but they may consider revising the title and certain other instances to make this point clear.

2. Line 25: model → models

3. Lines 41-44: Consider re-wording this sentence. How can AROME airport resolve processes? The authors probably mean that processes at the scale of AROME airport are not resolved.

4. Lines 80-86: The first statement seems to say that an assumption is made that the local PDF is Gaussian. Then it says that locally the PDF samples multiple Gaussian PDFs, and therefore it is not necessarily Gaussian. These seem to contract each other.

5. Sect. 3.2: What is meant by the term 'coupling experience'? In English, this phrase sounds a bit unusual. Is there some particular reason to use the word 'experience', rather than just saying something like 'model coupling', or 'coupling between resolved and unresolved scales'?

6. Sect. 3.3: Consider moving this section until after the model has been introduced (i.e., beginning of Sect. 6). Currently, it feels out of place since this is really just details related to model testing/validation and is not central to the model itself.

7. 400-403: Can the authors explain why they feel that the ideas presented by Kolmogorov are considered "laws"? Typically these ideas are referred to as 'Kolmogorov theory' or 'Kolmogorov's hypothesis', as they are largely based on similarity/scaling arguments.

8. 400-403: How exactly is the model consistent with Kolmogorov theory? Would the authors consider it to be consistent with all of the similarity hypotheses presented in K41, or is it that it is consistent with Kolmogorov's second similarity hypothesis in that the variance of the Lagrangian velocity increments ($\langle du^2 \rangle$) is proportional to the turbulence dissipation rate, i.e., $C_0 \varepsilon dt$?

9. Lines 419.5 (equation): I am not entirely clear on how $K$ is specified. Normally, this would come from the large-scale simulation and be interpolated to the particle position. On Lines 308-310, the authors mention that the TKE is extracted from Meso-NH, which would lead me to believe that is where $K$ comes from. However, Sect. 5.2 would suggest otherwise, that somehow the TKE is calculated afterword, although it is required in the velocity equation itself. Please explain.

10. Lines 423-426: The statement regarding the buoyancy term is vague. There are an infinite number of ways in which this term could be modeled using a random variable. Was this simply an empirical 'knob' that was turned?

11. Lines 504-505: How is the particle velocity initialized? This seems important considering that the particle simulation times ($\Delta t$) are much shorter than the integral time scale, and therefore they are likely to 'remember' the initial condition.

12. Sect 3.6: This title seems inappropriate. When are structure functions ever calculated in the paper? It seems like a more appropriate title might be 'Ensemble averaging' or something of that nature.

13. Lines 635-651 and Figure 7: I don't see the value of this comparison. Firstly, the simulations and the data are not over the same time period. Secondly, am I supposed to look at Fig. 6 and Fig. 7 and say "Yes, the Meso-NH velocity is smoother than the sonic data"? It would be incredibly surprising if that were not the case considering that the simulations don't resolve below the grid scale.

14. Lines 671-674: Should we not expect better agreement if the method is consistent?

15. Lines 855-858 and Fig. 12 caption: It is not clear to me how exactly the 'new' grid was obtained. If the grid resolution is changed, shouldn't the 'Meso-NH' TKE (black line) change as well?

16. Sect. 6.1.3 (velocity spectra): I have quite a few concerns with this section:

    - For the SLM used here, we know that 1. An ensemble of particles at any location should have TKE $K$, and 2. The variance of particle velocity increments at any location should be $\langle du^2 \rangle = C_0 \epsilon dt$, which is consistent with Kolmogorov theory. Given this, should we expect the particle velocity spectra to follow $k^{-5/3}$ scaling?
    - How the information presented in this section considered 'validation'?
    - This section appears to be missing a description of how the velocity spectra are calculated using the Lagrangian particle data.
    - Is the statement on Lines 717-718 meant to imply that the averaging time is insufficient? If so, why present these results?

## References

Bernardin, F., M. Bossy, C. Chauvin, P. Drobinski, A. Rosseau, and T. Salameh (2009). Stochastic downscaling method: application to wind refinement. *Stoch. Environ. Res. Risk Assess. 23*, 851–859.