Legend:

**###: “…”** – Line numbers + relevant text from paper.

Abcd: Domingo’s Comments

Abcd: My comments/questions to JKL.

Comments:

**254: “for the convective case, the mesoscale simulations contain convective circulations with low levels of turbulence, despite their relatively coarse resolution (Figure 4 b). These structures are explained by the effects of the use of terra incognita (TI) \citep{Wyngaard} resolutions. The length-scale of realistic mesoscale convective structures is on the order of the mesoscale resolutions selected for this study. Because of this, certain structures are partially resolved, resulting in the presence of unrealistic heterogeneities, along with other unrealistic characteristics \citep{Honnert1,Beare,Zhou2,Ching,Shin}. These unrealistic flows have been studied in the context of mesoscale-to-LES grid-nesting \citep{Mazzaro}: although their effect can influence the evolution of turbulence, they do not produce a significant bias in the turbulence observed past the development portion of the domain.”**

IS this an optimum case to analyze in the context of inflow generation techniques? You are combining the effects of under-resolved to resolved convection and small scale generation in one problem, which makes difficult to have a clean analysis and understanding of what the force perturbations do.

My hope is that my previous work helps to justify the use of this case, by stating that the mesoscale heterogeneities don’t affect the solution beyond the fetch. I’ve thought about this comment a lot, since this case selection such a huge element of the paper, and have a few thoughts.

I’d say that in the contexts in which these perturbations would be used, under similar convective conditions, you would almost always see these structures present. The only two ways I can see for suppressing them are:

* Using significantly larger grid-refinement ratios. I’m talking about ratios on the order of hundreds based on tests that Domingo and I did in the past. I don’t think this would produce results that would be a relevant to the research being performed.
* Increasing the Smagorinsky coefficient significantly. Again, this is something that Domingo and I tried to do in the past. We had no success in producing smooth, idealized flows. I know that he’s successfully done this in recent work, with real simulations. However, I would argue that this is an alternative method that won’t become a standard in the near future. So, again, I don’t think that results under these conditions would be as relevant to the general public.

Based on this, I think that the setup that we are using is optimum for analyzing the inflow turbulence generation techniques, in the context of the convective simulations that people would commonly use them on.

I’ve looked at Domingo’s previous papers, to recall how he’s done his simulations:

* BLM 2014: His parent domain has no heat-flux. So, he’s modeling a spatial transition between a neutral mesoscale domain and a convective microscale domain, rather than a purely convective case. Therefore, his turbulence generation fetch is influenced by both the perturbations, but also the stability transition. His justification was to avoid the “undesired effect” of having these structures in the parent domains. I think that by pointing out my previous study, in which the focus was to see whether the terra incognita structures affect the state of the turbulence after the fetch, I’m showing that these structures don’t necessarily produce any negative effects on the solution, especially when we add perturbations.
* MWR 2018: He states that the convective case is forced by a “smooth” mesoscale flow, but I haven’t been able to find an explanation of how he achieved this. Based on the information that he gives about his setup, there definitely should be structures in the mesoscale flows. The only other comment I was able to find is where he says that his setup is similar to the BLM 2014 setup. So I’m assuming that he also used the neutral to convective transition? I wanted to check whether he used a very very large grid refinement ratio, but I couldn’t find the parent resolution that he used. Am I missing if from somewhere obvious? Are you able to see what he used?

**339: “we see that horizontal force-perturbations show the quickest development of the final levels of turbulence spectra for theta, u and w, even when compared with the optimized theta perturbation method”.**

This is irrelevant in a neutral ABL!!!

I’m not sure what this comment means. I can’t really figure out what is “irrelevant” in the analysis on this paragraph. I suppose that if I had to guess, I’d say it’s because I’m looking at the spectra of θ for a neutrally stratified case, which I address more in depth for a later comment. Do you see something I don’t? The comment makes it appear as if something is obviously irrelevant (with the “!!!” and all), but I really can’t see it.

**384: “The application of theta perturbations produces very high levels of turbulent energy in the theta field, while having no influence in the initial velocity fields”.**

This is not true. It is in fact physically impossible to have that situation. Thermal and velocity fields are "coupled" and a unique entity, so you cannot develop thermal variance without influencing the velocity field accordingly...

The velocity energy does not overshoot, but rather develops more progressively.

I’m not sure whether I didn’t explain this point well enough, or if this is indeed not true and I’m just not understanding well enough. Based on the spectra shown in figures 13 (a,b,c)-unperturbed, and (d,e,f)-cell perturbation, you can see that the initial spectra (darkest blue line) for both vertical and horizontal wind speeds are nearly identical between these two cases, indicating that the velocity fields are effectively not being affected by the temperature perturbations until farther downstream. There are some extremely small differences, which you can only barely see when plotting the lines on top of each other in different.

I do agree that u,v,w and theta fields are coupled, but I think that, since the cell perturbation method is directly adding noise to ONLY the temperature fields, there are some physics that must take place for this coupling to be reflected in the simulation. I believe that figure 13 shows this coupling to take long enough that at x=1km, θ fluctuations haven’t yet affected the velocity fields much.

So, maybe I shouldn’t use “no influence”, and instead word it differently to allow for those initial differences that I considered minuscule? If you have any insight about why this may be wrong, or whether my wording needs to change, please let me know. Maybe “no influence” isn’t the best term to use?

**344: “Figure 10. Turbulent spectra of u, w and theta”.**

Theta spectra is irrelevant here!!! It is a neutrally stratified ABL, see what the thermal variance levels are. It is basically noise and not relevant in the context of the analysis you are trying to make. So I recommend removing them.

This I am very confused about and I’ve given it a lot of thought. After all my thinking and turning It around in my I’ve actually ended up feeling that in the context of turbulence generation, especially using the (θ) cell perturbation method, showing the θ spectra is as relevant for the neutral as for the convective case. Here’s my reasoning:

1. In his previous comments Domingo has stated that the “Thermal and velocity fields are "coupled" and a unique entity” (see the last comment).
2. Figure 10 (a,b,c) – Neutral: as early as x = 1km, the effect of the θ perturbations is apparent in all of the spectra components that we are observing: The spectra of the unperturbed case are basically a flat 0 for all components (there was no turbulence). Therefore any positive value shown for u, and w in figures 10 a and b is a consequence of the perturbations, through the conversion from θ variations to kinetic energy.
3. Although in general the magnitude of the variations in the θ perturbations is very small, you can see in figure 10 (a,b,c) that those small variations trigger very large variations in the u and w spectra. So I wouldn’t necessarily call this irrelevant noise.
4. Points 2 and 3 confirm Domingo’s comment from point 1. The components are very closely coupled for the neutral case. θ, u and w changes go hand in hand.
5. Figure 13 (d,e,f) – Convective: at x = 1 km, there is almost no noticeable effect of the θ variations on the u and w fields (from comparing a+b and d+e).
6. From points 4 and 5 I’d say that the thermal and velocity fields are more strongly coupled in the neutral case than in the convective case. This makes sense to me from an intuitive perspective, since there’s a lot more θ variation already in the convective case, so additional θ perturbations may not be as shocking to the system.
7. If the θ perturbations have a more direct effect on the neutral case than on the convective case. How come an observation of the evolution of θ fluctuations is “irrelevant” in this context?

I believe that as long as the θ field is evolving in the neutral case, the u and w field will continue to evolve significantly as well, due to this close coupling. I also believe that this is reflected in figure 10. Domingo’s recommendation is to completely remove θ spectra. I strongly disagree, but don’t want to make it seem like I’m disregarding his comment, and I also want to be sure that my thought process is reasonable, and that there isn’t some major aspect that I’m missing. Any thoughts?

**395: “For the case of vertical force-perturbations, this produces a shorter convergence length than was observed for temperature perturbation or horizontal force-perturbations”.**

How this can be true with the evidence presented in Fig. 13 and what you mention in the next paragraph?

For reference, the next paragraph:

*“An analysis of the turbulence statistics after a fetch of 40 km shows that, unlike in the case of the NBL, under CBL conditions all of the tested perturbation techniques produce nearly identical turbulent statistics after a long enough distance.”*

It seems clear to me from Figure 13 that the fetch is shortest for the vertical force perturbations. Maybe the difference isn’t as obvious when comparing between vertical and horizontal forces, but it certainly appears obvious when comparing forces to θ perturbations. Am I seeing something that isn’t there? Is this not as clearly shown in the figure as it seems to me?

Additionally, Domingo seems to be saying that this can’t be true based on what I say on the next paragraph, but I’m not sure how the next paragraph is related to this evidence at all when for that analysis I’m only looking at the spectra after 40 km.

**445: “The RFPM is able to directly accelerate the flow, thus removing the process of converting potential temperature gradients into kinetic energy, which was necessary for the theta perturbation method”.**

How often is the atmosphere neutrally stratified? There is essentially almost always a contribution from thermal variance, so this statement is not necessarily accurate...

Again, I’m at a loss here when trying to understand his statement. Is Domingo trying to say that the accelerations caused by the forces are also inducing temperature variance, which is in return influencing the velocity fields? Is that the thermal variance contribution that he’s referring to? If so, I don’t see how that’s relevant to this statement. But I get the feeling that I just don’t fully understand what he’s trying to state. Do you see what he may be referring to?

**450: “For the NBL, turbulence is strongly influenced by strong shear in horizontal wind-speeds. Therefore, random horizontal accelerations have a stronger influence on the scales and speed of turbulence generation”.**

Not sure about that. Shear production in the vertical direction is dominant...

Just want to make sure I’m stating this right. What I meant to say is that: “turbulence is strongly influenced by strong VERTICAL shear of horizontal winds”. He’s saying that “Shear production in the vertical direction is dominant”. I think that we are saying the same thing and that my new rewording should clarify that. Is this right?

**454: “which produce strong vertical shear. Therefore, vertical force perturbations, which directly influence this vertical shear forcing, have a greater impact on the generation of turbulence in this regime”.**

Not very accurate. The convective ABL is predominantly "well mixed", only the very near surface is that way. Also, vertical shear forcing sound convoluted. What do you mean?

There is no reference to the paper Branko and I wrote earlier this year explaining the transition mechanisms in convective and stable conditions. Please read through and put your thoughts in the context of the existing literature...

Again, I feel like my sentence wasn’t accurately written, and I want to change the wording to make sure I’m doing it right, and that I’m fully understanding where Domingo’s question came from.

I’d change the text to say:

“For the CBL, strong temperature gradients near the ground surface induce ~~vertical motions~~ horizontal heterogeneities, which produce significant horizontal shear of vertical winds. Therefore, vertical force perturbations, which directly influence this horizontal shear forcing, have a greater impact on the generation of turbulence in this regime.”

Is this wrong? As far as I understand, the convective boundary layer is well-mixed but also contains a lot of horizontal heterogeneities of scales larger than the turbulent motions (convective structures). These structures contain very strong vertical motions that produce a lot of horizontal shear, thus contributing to that mixing. Please, let me know if you don’t think that this statement is reasonable.

**456: “it is expected that horizontal force perturbations would also perform better for stable stability conditions, which are characterized by a dampening of vertical momentum”.**

This is not necessarily accurate. The SBL is much more complex than this statement and this is just a strong speculation.

I agree that the stable boundary layer is very complex, and that this comment is indeed strong speculation. But it Is also what I would expect based on the results that I saw. I’m debating on whether I should change the wording to make it clear that this is just speculation based on the results rather than saying that it is “expected”, or should I completely remove it? I think that if I kept it, I’d make sure to state that “future studies could look at the stable BL to confirm this”. I have no preference either way, but I just felt like including a comment about what could happen with the stable BL would be nice.

**467: “This could, for example, reduce the cost of predicting pollution transport for planning or emergency purposes, and help reduce the time necessary to make time-sensitive wild-fire predictions, among many other applications”**

This is an overstatement. There are many other factors and technical limitations playing a role in these capabilities that are not mentioned here, and that are more relevant that inflow turbulence generation.

I agree that there’s so much more that matters when it comes to these applications, but I guess I wanted to bring it back around to the motivation, even if the motivation feels slightly removed. Is it inappropriate for me to add this statement here? Should I say “help reduce” rather than “reduce”? Would you say that that would address Domingo’s concern appropriately?