Review of WES-2025-147: "Under-resolved gradients: slow wake recovery and fast turbulence decay with mesoscale Wind Farm Parameterizations"

This was a difficult review. There are many good contributions in this work: the manuscript is exceptionally well written, the approach is logical, and the figures are really effective. Many simulations had to be conducted, stored, post-processed, and analyzed for this work and I am painfully aware of what that means. But at the end I cannot help but concluding that there is very little new information in it and, actually, quite a few attempts to "sell" new concepts that are really not new and, if anything, misrepresent the real problems.

1. The title is misleading: by using the plural ("parameterizations") one is led to believe that multiple WFPs are going to be evaluated in the study, whereas only one, that by Fitch et al. (2012), has been assessed. There is no explanation as to why only Fitch's was chosen or why the other three WFPs included in WRF (MAV parameterizations) were not considered. Only the sensitivity to the correction factor α for TKE was evaluated, but that does not change the WFP, still Fitch's. Based on this and the other issues below, I have concluded that this paper is a sensitivity study of the Fitch parameterization to one tuning parameter and cannot be generalized to any other WFP.

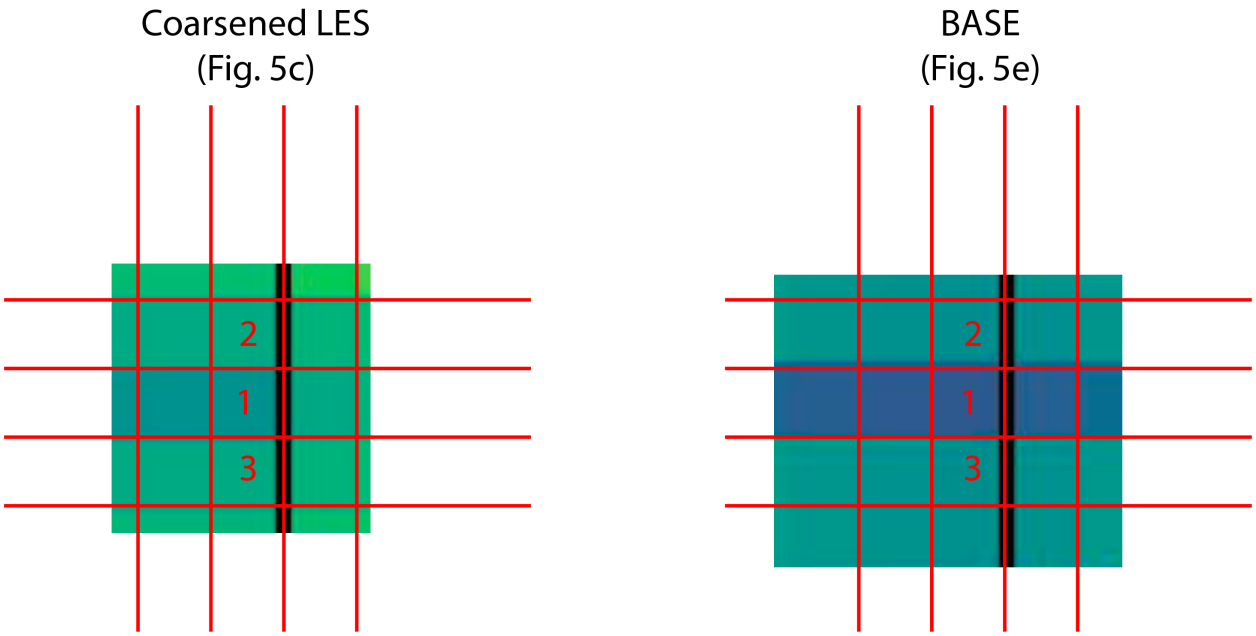
2. The paper proposes some sort of a new interpretation of the malfunctioning of the Fitch parameterization: the authors name it "under-resolved gradients". They basically claim that these under-resolved gradients are the fundamental reason why WFPs (but in reality it's just Fitch's) do not resolve the wind speed deficit and added TKE patterns accurately. This is not correct. One could simply replace the term "under-resolved gradients" with "sub-grid wakes" and none of the proposed findings would be new anymore. It is not the local gradients per se that cause differences in the wake recovery, it is the missing wakes; the local gradients are the obvious and inevitable consequence of having a wake with a localized wind speed deficit. There would be no gradient if there was no wake, obviously. By shifting the attention to the gradients, the true issue disappears, which is that the wake effects need to be parameterized better than how it's done in Fitch's. The wakes are missing, therefore (obviously) the gradients are missing, so let's focus on the missing wakes, not on the missing gradients.

   Why are the gradients missing or under-resolved? Because, with the Fitch WFP, the wind speed deficit added by the turbines is smeared in the volume of the grid cell and therefore it is not possible to resolve or create the proper y- or z-gradients. This is an implicit limitation that is obvious and is not the cause, but rather the direct consequence, of the WFP.

   Furthermore about the gradients: the paper describes them as some sort of an LES feature that is almost undesirable ("the LES … wind speed profiles feature sharp gradients that resemble spikes" or "Once the spikiness disappears, the differences between the [LES and WRF] models stabilizes"). The LES do not simulate just the

gradients, they simulate the full wake and therefore gradients appear. The gradients are the effect, not the cause.

Lastly about the gradients: as described in the manuscript, these gradients are a function of Δy (and Δz), thus a direct comparison between LES and WRF is not correct because the two models have different resolutions and different Δys. But one can look at the magnitude of gradients in the Coarsened LES and compare them to those in WRF. In the figure below, which is a zoomed version of Fig. 5, one can notice that the gradients in WRF on the right (U@1 – U@3)/Δy or (U@2 – U@1)/Δy are actually larger than those in the LES (on the left). The whole discussion in Section 4.1.1 is therefore moot.



3. The second focus of the paper is the so-called "rapid decay" of TKE in the resolved wake in WRF with Fitch's. This terms is improper because it implicitly assumes that it is sufficient to add TKE in the grid cells of the wind turbines and then the resolved processes will just advect and redistribute this TKE downwind and, as such, the issue is just that this happens too quickly. Basically, the (wrong) assumption here is that the source of TKE is at the turbine. While this is absolutely true for the wind speed deficit and partially true for a single turbine's TKE in the near-wake via tip vortices, it is absolutely not true in the far wake. The authors are in fact aligned with the literature when they recognize the well-known fact that (l. 438-439) "the shear production of TKE depends on spatial gradients, which are under-resolved" but they are incorrect in stating that "TKE fails to persist" there. Instead, it is not FORMED there because of the under-resolved shear. It's not a decay problem, it's a missing addition problem.

4. Ultimately the true issue is: what are the authors proposing to resolve these issues? How can we resolve these gradients better? After all these computationally expensive

and time consuming simulations to, in my opinion, demonstrate the obvious, what is the solution to the issue? None is proposed. I do not see the ultimate value of this paper to the scientific community, it fails to show novelty except semantically, and it does not provide guidance, let alone a solution, on how to mitigate the issue.

5. As a final note, I find it extremely inaccurate to state that: “Beyond improving the subgrid parameterizations of momentum sinks and TKE sources, it is equally important to develop new strategies that account for the effects of under-resolved gradients in NWP-WFP frameworks.” Again, the gradients are under-resolved precisely because the wakes are not well parameterized. These are not two separate issues, they are the same issue. By focusing the attention on one of the effects of the problem, i.e., the gradients, rather than the root cause, i.e., the misrepresented wakes, this paper ultimately has no use and therefore should not be published.

Minor issue

Figure 5: there might be some errors here. First of all, why is TKE zero in the black column right upstream of the wind farm in panel (h)? For TKE to be zero, added TKE would need to be negative, is this even possible? In panel (l), I would expect a very small error in the last column of the wind farm in the center row where the BASE case shows a grid cell with the highest TKE with the same yellow shade as the Coarsened LES in (d). Why is it in the darkest blue instead, indicating the largest error? The errors should also be smallest along the center row, but there is no such feature.