I thank the authors for their careful consideration of my comments and suggestions. Many of my comments have been addressed very effectively, and I believe this has improved the quality manuscript. However, I feel that two of my main concerns have not been fully resolved yet, and I believe that these aspects need more attention. I clarified below why I think my main concerns are not sufficiently dealt with, and I also listed some other minor comments. I hope the authors will take the time to resolve these remaining concerns.

Remaining main concerns:

- In response to my original comment on including analyses of the smaller wind farms (main concern #2), the authors stated that additional discussions would lead to a too long paper, and as a compromise they added one additional figure. The authors also stated that more detailed investigations of the differences between small and large wind farms is subject to future work. I agree with the authors that the paper is already quite long and adding 4 more figures would probably make the paper too long. However, I am not satisfied with the current solution of adding one figure and a paragraph somewhere in the middle of the paper, and the reason is twofold. First, the narrative of the paper and hence the expectations of the reader have not changed in this revised manuscript, so one of the main messages of the paper is still focused on the difference in flow behavior of small versus very large wind farm clusters. For instance, the revised manuscript still contains several statements introducing or summarizing these differences as one of the key findings:
    - line 6: “the results show that very large wind farms cause flow effects that small wind farms do not”;
    - line 681: “These results show that the power output and the wake of very large wind farms behave very differently compared to small wind farms”;
    - line 711: “Overall, the results show that very large wind farms trigger much more complex flow effects than small wind farms do.”

  Second, if you are not analyzing the LES data in detail to show the differences between small and large wind farm clusters, then why did you run such a large domain? You could also just run an LES of zones 2 and 3 (as a matter of fact, for the current wind direction there seems to be no interaction between zones 1 and 2+3, so you could also run separate LES of zone 1 and zone 2+3. Is there any benefit of running one big simulation?). Knowing that you ran these massive large-eddy simulations including smaller wind farm clusters (so the data is there), I still wonder while reading the paper why the comparison between small and large wind farms is not conducted consistently for every aspect analyzed throughout the paper. I think you can take two approaches here. One approach would be to do every analysis consistently for small and large cluster, and try to reduce the length of the paper by condensing the figures and make more optimal use of space (for example, figures 7 and 8 take up two entire pages, but do you really need a color figure per case? Do you fully discuss the shown velocity contours of all cases, or could you replace this with a figure showing the IBL growth for various cases?). This would be the preferred approach from a scientific point of view, but it might require redesigning some of the figures. The second approach is what you intended to do, i.e., refer more detailed investigations to future work. However, in this case I think you need to manage the reader’s expectations better and say up front what the main focus of the paper is (flow behavior in very large wind farms). Moreover, you should indicate to what extent you will address the

differences with smaller wind farms, and mention in the paper when certain investigations are out-of-scope but will be part of a follow up study.

- Considering my original main concern #3, I appreciate that the authors included the perturbation pressure gradient in the energy analysis. However, I still have a couple of issues with the energy budget analysis:
    - I feel that the divergence of horizontal advection of kinetic energy is an equally important term of the energy budget equation: it is effectively a source of energy (i.e., a positive term in the budget equation) which will be significant near the wind farm leading edge, and it also shows how the kinetic energy below rotor top level is depleted by the wind farm (for example, it explains why you see a wind farm wake). I think that this term is essential for energy budget analysis of finite wind farms, and I believe that it should therefore be included in the analysis and in the figures. Now it is only mentioned as a side note to explain a difference with literature, but I don't think that is sufficient.
    - I still have an issue with the term "total energy input". I appreciate that the authors now include the perturbation pressure, but as I said before the divergence of horizontal advection is also an energy source (a positive term in the budget equation). You could argue that vertical flux and pressure gradients are external sources adding energy to the region below rotor top level and are therefore called the total energy input, but then you should specify that that is how you define an energy input. In that case, however, depletion of the kinetic energy flux is another mechanism that needs to be considered. Saying that wind turbines extract x% of the total energy input without accounting for how much they deplete the energy flux below the rotor top level is only telling half of the story.
    - Note that line 617-618 still mentions the total energy input as W_vkef+W_gpg,wt. Maybe add an equation that defines the total energy input W_total,wt.
    - In your reply to my main concern 3 subquestion b, you say that the power input by the perturbation pressure averaged over the entire farm length is approximately zero, and this justifies the 70% statement. This makes the paragraph from line 618 to 625 starting with "The wind turbines extract approximately 70 % of the total energy input …" even more confusing to me. Where does this 70% hold? Is this averaged over the entire farm length, or is it only in the bulk of the farm, or does it hold everywhere? Further, I don't understand the note you added on the divergence of the horizontal advection. You report a higher percentage of the total energy input extracted by the wind turbines than the reference values for infinite wind farms, so how can additional energy from divergence explain that you already extract more energy?

Minor comments:

- Line 581: The revised manuscript still says "… can be compensated for by two sources of energy:". This should be "three sources" (or four if you decide to include the divergence as a possible source of energy).
- Related to my concern about the clockwise flow deflection above the boundary layer: It is good to hear that new investigations show that the clockwise flow deflection

remains with the damping layer farther away. However, nothing is changed in the paper, so other readers might still be faced with the same doubts. Please add the fact that new investigations support the validity of the results to the paper where appropriate.

- Caption of figure 11: Specify what is indicated by the yellow regions.
- Section 2.4 consists of one very long paragraph which makes it difficult to read. Split up into several paragraphs for readability.
- Line 348: You are mixing up subcritical and supercritical. Supercritical means Fr>1, subcritical means Fr<1.
- Line 678-679: “This redistribution is done by a favorable perturbation pressure gradient … (not shown in Fig. 11).” You added the perturbation pressure gradient work in the revised figure, so I guess the statement between brackets can be removed?