Response to reviewer 1:

We thank the reviewer for his additional comments and we highly appreciate his support for further improving the manuscript. We marked the reviewers comments blue and our answers black.

Main concern #2:

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:

o line 6: “the results show that very large wind farms cause flow effects that small wind farms do not”;

o 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”;

o line 711: “Overall, the results show that very large wind farms trigger much more complex flow effects than small wind farms do.”

We think that the abstract and introduction clearly state, that the main focus of the paper is on very large wind farms and that hence the expectations of the reader are directed in the right direction. E.g. in the abstract we write:

> "The objective of this large-eddy-simulation study is to investigate the wake properties and the power output of very large potential wind farms in the German Bight [...]"

In the introduction we write:

> *"We provide new insights into the wake properties and power output of very large wind farms and how these depend on the varied parameters. Specifically we want to answer these questions:*
> *1. How is the flow inside and above the boundary layer affected by very large wind farms?*
> *[...]*
> *4. How much power output or power density can be expected for very large wind farms?*
> *[...]"*

However, it is true that we make several comparisons to small wind farms, also in our final and main conclusions. We do that to highlight the characteristics of very large wind farms, which would be difficult without a comparison to small wind farms. We do not investigate the small wind farms in the same detail as we do for the large wind farms, because many other papers already have investigated small wind farms (as we have shown in the introduction). We find that it is sufficient to refer to and cite the findings of other authors about small wind farms.

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.

We agree that two more idealized and/or smaller setups, one for small and one for large wind farms would enable us to make better comparisons between small and large wind farms. We have chosen this special setup including large and small wind farms, because of two reasons:

1. We want to show the wake effects and power output for the specific case of the German 2040 expansion target in the German Bight for the most typical weather situations. We think that the investigation of this special case is of great interest for researchers and industry involved in the energy transition and for the society in general.

2. It was a constraint by the project funder to include all wind farms in all priority areas in the German Exclusive Economic Zone.

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.

We decided to leave the focus of this article on very large wind farms and to not make a systematic comparison between small and large wind farms. Hence we choose approach 2. As written above, we think that introduction and abstract make already clear, that the focus lies on very large wind farms. However, we would like to follow your suggestion and tried to add statements that clarify that we do not present a systematic comparison between small and large wind farms. We found that such a statement only makes sense right at the beginning of the results section (line 300). There we added:

> To highlight the characteristics of very large wind farms some comparisons to small wind farms are made. However, the focus of this work lies on very large wind farms, so that a systematic comparison between large and small wind farms is not conducted here 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:

o 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.

o 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.

We agree that the advection of kinetic energy is an important term in the energy budget analysis of a wind farm. However, the intention of this analysis was rather to examine those processes that drive the wake recovery and limit the power output further inside very large wind farms.
We also agree that we have not clearly defined what we mean by "energy input", so we have rephrased the beginning of section 3.2.2:

> To examine the dependency of the wind farm efficiency on the turbine spacing and the BL height in more detail, an energy source analysis is made in this section. Here, an energy source is defined as an energy input to the flow, i.e. a process that drives the wake recovery. This can be one of the following:
>
> 1. Vertical turbulent flux of kinetic energy at rotor top level, W_{vkef}
> 2. Work done by the geostrophic pressure gradient on the flow below rotor top level (bottom of the BL), $W_{gpg,wt}$
> 3. Work done by the perturbation pressure gradient on the flow below rotor top level, $W_{ppg,wt}$
>
> The analysis is a simplified version of the analyses made by \citet{Abkar2014} and \citet{Allaerts2017a} and does not claim to be a complete energy budget analysis. The intention of this analysis is to show which processes dominate the wake recovery and thus limit the achievable power density of very large wind farms. Thus the advection of upstream kinetic energy is not considered here. The above named sources are calculated as follows:

We also considered to include the advection term into Figure 11. Unfortunately this would require at least a doubling of the range of the vertical axis so that the entire figure becomes unreadable. So, finally we decided not to include the advection of kinetic energy in the Figure and to only mention in the text that it is a dominant energy source for the wind turbines (line 605):

The dominant energy source for the first turbine rows is the advection of kinetic energy. The advection is not included in Fig.~\ref{fig.xz_2x3_power_densities} because it is larger than the other terms and would make the quantification of the smaller terms difficult.

We renamed the section 3.2.2 from "Energy flux analysis" to the more suitable name "Energy source analysis".

We have also made some other small corrections in this section, e.g. renaming "kinetic energy flux" to "vertical kinetic energy flux". The changes can be seen in the attached "diff"-document with highlighted changes.

o 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.

We have now done this in line 612.

o 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?

We have now specified where this 70 % hold:

> The work done by the geostrophic pressure gradient on the flow below the rotor top level achieves a power density of approximately 0.6 W m^{-2}. It is thus not the dominating energy source inside the wind farms but it still contributes approximately 20 % to the sum of all sources $W_{total}=W_{vkef}+W_{gpg,wt}+W_{ppg,wt}$. In the downstream half of the wind farms the ratio between the wind turbine power and W_{total} is approximately 70 %

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?

We deleted these sentences, because a comparison between our non-idalized and the named idealized infinite wind farm setup does not make sense.

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).

We have rephrased this part, see further above.

• 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.

We have added a note on that in line 521:

> However, currently running investigations with much higher Rayleigh damping heights show the same behaviour.

Caption of figure 11: Specify what is indicated by the yellow regions.

Done.

Section 2.4 consists of one very long paragraph which makes it difficult to read. Split up into several paragraphs for readability.

Done.

Line 348: You are mixing up subcritical and supercritical. Supercritical means Fr>1, subcritical means Fr<1.

We found a mistake in line 342, where Fr<1 (smaller than 1) was accidently stated as supercritical. We now corrected to Fr>1 (greater than 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?

Yes, we have removed it.

Response to reviewer 2:

We thank the reviewer for his additional comment. We marked the reviewers comment blue and our answer black.

Thank you for your re-submission. You have addressed essentially all the comments to my first review by making adjustments to the manuscript.

I point now only to one technical detail:
Please only note that regarding the previous comment "L505, how is the power computed for each turbine" you have included a reference, Wu and Porté-Agel. Please check if the year is correct as the work you refer seem to be from 2011 instead of 2010. However, if that was the case (it does when comparing the DOI you provide), the power calculation seem to be missing in that article.

The correct year is 2011. We corrected that. We also added further information about the thrust and power calculation in line 97 because it is not included in the citet references, as the reviewer has stated.

tum sink and an angular momentum source (inducing wake rotation). The ADM-R is described in detail by Steinfeld et al. (2015) and Wu and Porté-Agel (2011). The actuator disc is divided into several segments along the radial and tangential direction to allow for a non-uniform thrust distribution over the disc. The lift and thrust force of each segment f_l and f_d is projected

on the axial (f_a) and tangential (f_t) direction:

$$f_a = -f_l \cos\Phi - f_d \sin\Phi\,, \qquad f_t = -f_l \sin\Phi - f_d \cos\Phi\,, \tag{4}$$

where Φ is the angle between the local wind vector and the disc. The rotor thrust F and torque M are then calculated as the sum over all N segments at radius r_i:

$$F = \sum_{i=1}^{N} f_{a,i}\,, \qquad M = \sum_{i=1}^{N} f_{t,i} r_i\,. \tag{5}$$

The wind turbine power is calculated out of the rotational speed of the rotor n_{rotor} and the torque:

$$P = 2\pi\, n_{rotor}\, Q \tag{6}$$