The paper compares the flow behaviour and energy household of "small" and "large", infinitely wide, wind farms based on two large-eddy simulations. The size of the "large" wind farm and the corresponding large-eddy simulation is considerably larger than most LES studies of wind farms that have been performed to date. As a result of this, the author is able to show for the first time (to my knowledge) that very large wind farms trigger inertial waves in the wake of the farm. In addition to this new physical flow mechanism, the paper highlights a number of interesting differences in the energy household of "small" and "large" wind farms. The paper is also well written, the methodology is neatly described, and the results are analysed in great detail. I therefore believe the paper could be of great value to the wind energy community.

However, I also have some serious concerns about the manuscript. Most importantly, I believe the introduction fails to achieve one of it's primary purposes, namely, to put the presented research into perspective. Apart from two references to depict the size of modern wind farm projects (Herzig, 2022, and BSH, 2021), and a reference to the grand challenges paper of Veers et al. (2019) to indicate the importance of understanding wind farm flow physics, the introduction contains only one reference to relevant past work. To make matters worse, that reference is to the author's own work. This is simply unheard of. The introduction as it is now gives the impression that wind-farm flow physics and LES thereof is new and has only been explored by the author himself, while there is in fact a large volume of published studies available which this work inherently builds upon. I'm fully aware that later sections of the manuscript do include more references to relevant work, but already in the introduction the context of this work needs to be described. What other studies have looked at wind farm flow physics? What is the size of wind farms in typical LES studies? How does that compare to this work? Furthermore, the author often discusses several flow mechanisms like wake deflection, inversion layer displacement, wind-farm blockage, gravity waves, etc. (see, e.g., line 34 and line 94 for first mentioning of some of these effects), but these concepts have not been introduced properly in the manuscript. The author seems to assume that the reader is already familiar with these concepts and gives no description or proper reference to the literature. I don't think wind-farm flow physics already reached the point where it needs no introduction. In fact, some of these flow mechanisms have been discovered fairly recently and are still topic of active research. In conclusion, I believe the manuscript requires a proper introduction that describes the state-of-the-art in wind-farm LES research, puts the presented research into perspective, and explains the flow mechanisms relevant to this work.

Apart from my main comment about the introduction, I have a few more scientific and technical comments as listed below:

Scientific comments

1. The manuscript repeatedly talks about the size of wind farms (in terms

of the rated power) considered in the simulations. The size of the wind farm is perhaps relevant for computational resources (it shows how much computational resources are available to the author), but from a physical point of view the size is irrelevant since the farms are infinitely wide in the spanwise direction. For the flow physics of infinitely wide wind farms, what matters is the streamwise length of the farm or the number of turbine rows. In this respect, I have the following specific comments:

(a) Lines 7-8, 42-43, 77-78: The rated power of infinitely wide wind farms is meaningless as it is physically irrelevant and depends on the choice of turbine spacing and spanwise extent of the numerical domain, as these two parameters determine the number of turbine columns resolved within the simulation. Please use a more relevant parameter to distinguish the different cases.

(b) Line 79: Why is the large wind farm simulation (16 turbine rows) twice as wide as the small one (8 turbine rows) if you are using periodic BC anyway? What is the reasoning behind this?

(c) Line 92: "Note that the small wind farm is already as big as the largest wind farms of most other LES studies, e.g. Wu and Porté-Agel (2017) (length 19.6 km, rated power 0.36 GW) ..." Comparison of power for infinitely wide wind farms is meaningless because it depends on how many columns are resolved (see also previous comment). Your study has 8 and 16, Allaerts&Meyers had 9, Wu and Porté-Agel had 5...

(d) Line 48-49: "To my knowledge this is the second largest wind farm LES study in terms of domain size and wind farm power after the study of Maas and Raasch (2022)." Why is it relevant that this is the second largest wind farm LES study? Instead of just mentioning the ranking, a quantitative comparison with wind farm size in other LES studies would be more interesting.

2. The inertial wave developing in the wake of the farm is quite an interesting finding, and it leads to some strong statements related to wind speed ups and impact on downstream located wind farms (see, e.g., line 12-13 and line 173-174). If these statements hold in general, they could have strong implications for wind energy deployment. Therefore, I'm surprised to see that the study is based on only two simulations, and I share past reviewers concern about verification/validation of results. I do agree with the author that it is not so easy to turn off gravity or the Coriolis force, but there are other options to increase confidence in the presented results. For example, I assume that the amplitude of the inertial wave depends on how much the flow decelerates inside the farm, so you should be able to see different wave amplitudes with different wind farm lengths or even different wind farm layouts. Maybe it is worthwhile to consider a wind farm of intermediate length or a different layout. Alternatively, if the flow behaviour you are seeing is indeed an inertial oscillation triggered by the wind farm, the

wave length should be independent of wind farm length and depend on the Coriolis parameter. You could consider a different latitude (boundary-layer height is governed by the temperature structure and subsidence so no issue there) and see whether the wave length changes accordingly.

3. I have several questions about the wavelength analysis from line 325 onward. First of all, I believe equation 14 is incorrect. Based on equation 12 and 13 and assuming that you define the "absolute wavelength λ" as the wave length in the direction of phase propagation, i.e. $\lambda = \lambda_x \cos\alpha$ (note that this is nowhere defined!), I find that the absolute wavelength should be given by

$$\lambda = \frac{1}{\sqrt{1 + \frac{f^2 \sin^2\alpha}{N^2 \cos^2\alpha}}} \frac{2\pi U}{N} \tag{1}$$

Second, looking at the first two entries in table 1, how is it possible that λ for wave type 1 and 2 (small wind farm) is the same for two different inclination angles α? According to eq. 14, there should be a unique relation between α and λ for U, N and f constant.
Thirdly, eq. 14 has two unknowns: the inclination angle and the wave length. The manuscript does not mention how you come to the results in table 1? Did you measure the wavelength in the simulation and then calculated the inclination angle? Or did you approach it the other way around, estimating the inclination angle from the figures and then calculating the wave length based on eq. 14?

Minor/technical comments

1. Eq.3: In the last term on the right-hand side, the subscript of u should be j instead of i.

2. Line 105: "... which is enough for resolving the gravity waves with a wave length of approximately 5 km." How did you calculate that wave length? This is coming out of nowhere. Please explain, or refer to later section.

3. Can you briefly describe the radiation boundary conditions of Miller and Thorpe (1981) and Orlanski (1976) in the paper? As the inertial oscillation triggered by the wind farms is a wave phenomenon itself, with a very large wave length (see figure 2 and 3), how do you know that the flow results are not affected by the outflow boundary? Clearly, in figure 3a, the wave extends all the way down to the outflow boundary. If you would put that boundary closer or further away, do you still obtain the same wave properties?

4. Line 119: What values are used to come to the advection distance of the convective time scale? Also, please explicitly mention the definition of the convective velocity scale w^* and the values used to calculate the quantity.

5. Section 2.3: Please explain the particular choice of surface heating and boundary-layer height (for instance, a boundary-layer height of 600m in convective conditions seems quite low).

6. Line 130-132: "The initial horizontal velocity is set to the geostrophic wind (Ug,Vg) = (9.011,-1.527) ms-1, resulting in a steady-state hub height wind speed of 9.0 ms-1 that is aligned with the x-axis." How did you find this particular geostrophic wind speed and direction? Did you find this by trial and error or did you use a particular method (e.g., a wind speed/angle controller)?

7. Line 144: "... so that the inertial oscillation has decayed ..." I assume you are referring to an inertial oscillation in time that occurs after the simulation is initialized? Please explain why an inertial oscillation is triggered. Please also clearly mention that you are talking about an inertial oscillation in time to avoid confusion with the inertial oscillation observed downstream of large wind farms.

8. Line 182: "Further downstream the wind turn clockwise ..." → "Further downstream, the wind turn*s* clockwise ..."

9. Line 209: "Because the wind farms are infinite in the y-direction, the perturbation pressure gradient force is parallel to the x-axis and has thus no effect on the wind direction at first." Is this because the perturbation pressure is due to gravity waves, which are uniform in y direction because the wind farm is infinitely wide? This statement needs more explanation.

10. Line 220: "If the Rossby number [based on the wind farm length] is smaller or close to 1, Coriolis effects become dominant ...". It is not necessarily true that Coriorlis forces become dominant at Rossby number close to 1. I agree that Coriolis forces become dominant as the Rossby number decreases, but you cannot claim (at least not based on two simulations) that the tipping point is for Rossby equal to 1.

11. Figure 6: It is surprising to see that neither TKE nor TI shows an oscillation. I would expect at least one of the two to be affected by the oscillatory behaviour in wind speed, as TI is related to TKE normalized by that same wind speed. How do you explain this? Further, on lines 235-236 you say that "TKE is greater in the small wind farm, because the wind speed is greater." How is TKE related to the wind speed magnitude? TKE production is related to wind speed gradients, so I don't see how TKE is directly related to wind speed magnitude.

12. In lines 248-250 you say that "TI does not show the oscillatory behavior that the wind speed and direction show because turbulence has time scales that are orders of magnitude smaller than that of the mean flow and therefore hardly affected by the Coriolis force." Are you saying that the turbulence time scales are orders of magnitude smaller than the inertial oscillation period, and that therefore TKE rapidly adapts to changes

in wind speed such that TI (or TKE) is constant? Please clarify this statement.

13. Line 252: "The last two sections ..." → "The previous two sections ..."

14. Line 349: "...which approximately corresponds the the length of the small wind farm ..." should be "...which approximately corresponds *to* the length of the small wind farm ..."

15. Line 356: "Due to the large ratio of L_{wf}/L_s in the large wind farm case, wave type one and two can be clearly distinguished in this study." Why would a larger ratio of wind farm length to Scorer parameter make the wave more distinguishable?

16. Section 3.5.1: How did you scale the budget terms from $\mathrm{W}\rho^{-1}$ to MW per turbine? Did you assume $\rho = 1.17\,\mathrm{kg\,m^{-3}}$ like in eq. 11?

17. Caption of figure A1: apprxomation → approximation