Responses to reviewers

WES-2023-56 | Research article

Revision Date: August 16, 2023

General responses to reviewers RC1 and RC2:

Both reviewers wish I had structured the paper differently by starting with a more complete description of the model.

I revised the description of the model by moving the governing equations to the front. (Line 66-75)

Both reviewers RC1 and RC2 worry about the accuracy and utility of the Rigid Lid (RL) case that is described in my paper. They want to know how the RL case can be applied to the real world. This is a fair criticism as I was mostly concerned with this RL case as a theoretical construct.

I have improved the theoretical discussion of the RL model and added a section on the application of the RL model to industrial models (Lines 245-258)

Both reviewers RC1 and RC2 request a more complete discussion of the role of non-hydrostatic pressure fields near wind farms.

I have added a complete explanation of non-hydrostatic dynamics in the revised manuscript (Lines 85-89). I also added a paragraph to the introduction about the cause of the pressure field (Lines 52-55).

Major Manuscript Changes

1. **(lines 52-55) Added a short paragraph about the causes of the pressure field.**
2. **(lines 59-63) Added a brief outline of the paper at the end of the introduction.**
3. **(lines 66-75) Moved the governing equations forward to make the presentation more logical.**
4. **(lines 85-89) Added short paragraph about the non-hydrostatic part of the pressure field.**
5. **(lines 104-107) Added a better explanation of Table 2.**
6. **(lines 145-174) Interchanged the paragraphs on pressure cause and pressure impact to make the presentation more logical.**
7. **(lines 249-258) Added a short paragraph about the use of the RL assumption in industrial models.**
8. **(lines 278-299) Shortened and simplified the conclusions.**

Specific Responses to RC1

Main comments

1. At the end of the introduction I miss an outline of the paper. As no outline

is presented, it is currently not clear how the paper is structured. This makes it harder to understand the relevance and significance of the paper.

I have added add a short outline to the manuscript. (Lines 59-63)

2. The comparison of the case with atmospheric gravity waves versus a rigid lid scenario is also interesting in light of the two main approaches for large-eddy simulations of wind farms that can be found in the literature, i.e., either resolving the atmospheric boundary layer and part of the free atmosphere (which supports the formation of gravity waves) or using a pressure-driven boundary layer with a rigid lid condition. A short literature survey and a quantitative discussion on how different the two approaches are would therefore be useful.

Thanks. I have added a brief new section on how my RL results could be used in "industrial" RL models.(Lines 249-258)

3. The description of the model in section 2 is too limited. Part of the governing equations are in fact shown in section 3, so why not present them when introducing the model? Furthermore, on line 109 it is stated that "the pressure field p(x,y) is derived using the hydrostatic assumption," but it is not clear to me what is meant by this.

Yes, I agree. I have moved the governing equation forward (Lines 66-75). I have also added discussion of the non-hydrostatic effects (Lines 85-89).

4. Sections 3-10 all seem to focus on the rigid lid case, but the link back to the realistic case is missing a bit. What do the conclusions for the rigid lid case mean for the more realistic case? How different are the results when the inversion is not a perfect rigid lid?

Table 2 already shows how the GW and RL cases differ quantitatively. I expanded the discussion of this Table.

5. I have the impression that some of the theoretical results are in line with previous findings in the literature. For example, equation 25 shows that upstream flow blockage increases with farm width, and this has been found before (e.g. Allaerts & Meyers (2019). Same for the decay of the perturbation

away from the farm (see eq 19 and 22), has this behavior been observed before (not sure myself)? It would be worthwhile to tie obtained results with what has been found before.

I take your point, but an analysis of farm shape is not the subject of the paper. I would rather not get into this.

Specific comments

1. Line 19: "... the wind slowing by these farms ... . This issue has an extensive literature, ..." I find this statement about the wind slowing a bit vague. Do you refer to the upstream flow deceleration, the wind deficit behind individual turbines, slowing down of the atmospheric boundary layer above the wind farm? All of these? Please make this more clear.

This comment is in the space-limited abstract. There is no room there for more detail in the abstract.

2. Line 25: "In a stably stratified atmosphere, ..." I think this statement can be misleading for the reader. Gravity waves only form in regions where the flow is stably stratified, but you can get gravity waves also above neutral and unstable boundary layers. The statement could be misinterpreted as if gravity waves only occur when the atmospheric boundary layer is stably stratified (i.e. when the surface heat flux is negative).

I have improved the description of the model formulation..

3. Line 28: I believe the term used by Bleeg (and many others) is blockage rather than blocking.

OK. I changed it to Blockage.

4. Line 29: "... over the farm, it can fight back against the turbine drag, ... Finally, it alters the recovery of the wake." Have these effects been observed in the literature? Please cite relevant studies.

This was discussed in my first paper (Smith 2010). This paper is cited.

5. Line 33: "According to Gribben and Hawkes (2019), the local non-hydrostatic pressure disturbances decay inversely ..." Please clarify whether this result is for a single turbine or for a farm.

I improved my discussion of non-hydrostic effects and the Gribben/Hawkes paper (Lines 85-92)

6. Line 34: "The farm-generated hydrostatic pressure disturbance is more far-reaching." What do you base this statement on? Evidence from literature (if so, add references), or is this based on your own findings (then say something like "as will be shown in this study, ...").

OK. I modified this language.

7. Line 43: Is there a specific reason why the model is limited to hydrostatic gravity waves?

The linearized model could be extended to non-hydrostatic cases, but those effects are small for farms larger than a few kilometers. I have added a discussion of this point. (Lines 85-92)

8. Line 53: Specify what you mean by "wrapping"

OK. I added a mention of the FFT method giving periodic wrapping.

9. Line 54: Please define "DAR" more clearly (I assume rotor disk area to covered surface area, where the latter is assumed to be $s_x s_y D^2$).

OK. I rewrote this sentence to be clear (Line 98).

10. Line 86: "When N=0, the displacement approaches zero as 1/g' and when g'=0 it approaches zero as 1/N." This relationship would be more clear when plotted in a figure.

I prefer the table as I could quantify more variables, each with different units.

11. Improve caption of table 2. Currently, it is not clear what is listed and how some of the parameters are defined (e.g. for the definition of Gamma one needs to refer to the main text).

OK. I improved the caption for Table 2 and the text. (Line 104)

12. Line 109: "When this equation is solved for the perturbation wind, the scalar wind deficit is computed from …" This transition is too fast, I needed to look up the definition in Smith (2022) to understand that the definition results from a linearization. Please explain more clearly how the scalar wind deficit is obtained.

I already cited a reference for this. I now added a reminder about the linearization. (Line 74)

13. Line 116: "Because the pressure field p(x,y) decays at infinity, it does not influence TD …" Again, going a bit fast. The pressure term vanishes because of the divergence theorem and the fact that p decays at infinity, but for this you also need the continuity equation to go from $U \cdot \nabla p$ to $\nabla \cdot Up$.

This proof is just the Fundamental Theorem of Calculus because U is a constant. When you integrate dp/dx you get the difference in p at infinity. This difference is zero if the disturbance decays.

14. Line 122: "In 2-D non-divergent flow …" The assumption of non-divergent flow can be made because of the rigid lid case, or does this also hold in the GW case? Please clarify.

OK. I clarified the text to say that the rigid is required.

15. Figure 3: Why do you apply a low pass filter to (b)? What high-frequency content are you filtering out? Noise due to the low resolution?

The Laplacian involved two derivatives that amplify noise. The RL case has sharp gradients because the farm has sharp boundaries.

16. Line 146: "The linearized Bernouilli equation … is approximately valid upwind, …" Upon what is this statement based? Did you check whether this approximation is valid?

This result follows from the governing equations (1) at F=0 upwind and the Rayleigh friction is small.

17. Line 156: "The magnitude of this ratio increases with aspect ratio ..." Not clear to me why this would be the case. Did you try other aspect ratios?

I did a few other aspect ratios. I did not want the reader to over generalize. This paper was not intended to discuss farm shape.

18. Line 170: "... the wake decay length (L=U/C) ..." Is this wake decay length defined in literature? If not, it should be made clear why the wake decay length is defined like this.

I dropped this comment to simplify the sentence. It is true however that the wake decay length is U/C.

19. Line 174: "In incompressible or non-divergent flow, ..." In the GW case, the flow is also incompressible, but flow divergence is allowed and leads to inversion displacement. Does that mean that the role of the pressure as you describe only holds for incompressible and non-divergent flow?

No. In the rigid lid case the flow is horizontally non-divergent so the pressure is determined diagnostically. I had added some text on this point.(Lines 52-55)

20. Line 281: "The stability values need to be an order of magnitude larger before the rigid lid approximation becomes quantitatively accurate." What criteria is this statement based on? When is the rigid lid approximation considered quantitatively accurate? This needs more explanation.

The reader can judge from Table 2.

21. Line 296: "... including the change in ocean surface wind stress caused by turbine induced boundary layer turbulence." Not very clear, and also not sure why this is relevant (not discussed anywhere else in the paper).

The drag from a wind farm comes about 10% from the ocean skin drag associated with the rough ocean surface. This leads to uncertainties. That was my point.

Technical comments

1. Line 28: so call ”Blocking” → so called ”Blockage” (see also earlier comment)

OK. I used “blockage”. In mountain meteorology, the same upstream slowing is called “blocking”.

2. Table 2, entries for parameter A for cases g’=0.05, 0.1, and 0.2 with N=0: The value for A is exactly equal to zero. Is this correct or is this a typo?

Good question. These are supercritical cases and there is no dipole pressure field. I changed the zeros to N/A.

3. Section 4 is missing?

Fixed.

Specific Responses to RC2

Main comments

1) *Reliability and significance of the rigid lid results*

RC2 makes two points here. First, that the RL model is not as accurate as the GW model. This is probably true, but no one has ever offered closed form expressions for the GW model. The point of the paper was to show a qualitative agreement between GW and RL pressure fields and then to derive closed form expressions for the RL case. This approach pushes our understanding forward.

Second, RC2 points out that the industry uses RL models and thus my RL results can be used to evaluate them. I have added such a discussion. (Lines 249-258)

2) Wind-farm-induced pressure field without stratification

I **agree with RC2 that I should be much more explicit about the role of non-hydrostatic pressure contributions. I have added this discussion.**

3) More description

Yes. I added these additional five descriptions to the manuscript.

4) Utility of measuring pressure in the field

I think RC2 may overstate the accuracy of our farm drag estimates. For, example, the ocean skin friction associated with the farm-disturbed ocean wave field is substantial and is difficult to estimate. The pressure field may provide a useful check on the total drag.

Minor comments

Line 28: 'so call "Blocking"'. "Blockage" is the more commonly used term in the wind energy community. It is also the term used in the paper. "so-called" should probably modify "blockage" rather than "so call".

OK. I have called it "blockage". The literature on mountain meteorology calls it "blocking".

Line 80: "the pressure field increased from zero." I think this phrasing can be improved upon. I'm pretty sure I understand what the author means when he writes that the "pressure field increased", but maybe the "pressure disturbances increased" would be better.

Yes. I have reworded this part.

Line 122: It may be worth pointing out that the right-most term in the equation is also zero.

Yes. I rewrote this section.

Lines 172 and 176: The sentence starting “The Rayleigh force in this case is non-divergent…” on line 176 appears to repeat a point just stated on line 172 in the sentence starting “This independence of the pressure field…” Or perhaps I’ve missed a subtlety. If not, perhaps the second sentence should be deleted.

I have rewritten this section.

Line 294: There is a typo at the end of the line between “is” and “proportional”.

Fixed