

# Responses to Reviewers Comments

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## The wind farm pressure field

Both Reviewers RC1 and RC2 provide careful and thoughtful comments on the manuscript. I will be able to incorporate their suggestion in the revised manuscript.

### Common criticisms of RC1 and RC2:

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. However, the RL case is needed to fully understand the origin of the pressure field.

In addition, both reviewers point out that the RL assumption is being used in some industry applications. Thus, my paper can also be used to understand and evaluate those applications. I will follow their advice and add such a discussion to the revised manuscript. This approach adds motivation to the RL analysis.

Both reviewers RC1 and RC2 request a more complete discussion of the role of non-hydrostatic pressure fields near wind farms. I neglected these fields and did not fully explain why I did so. I agree with their criticism. I will add a complete explanation on non-hydrostatic dynamics in the revised manuscript.

Neither reviewer discussed the two methods (Green's Function and Rankine Body) that I used to compute the RL pressure dipole and to illustrate the role of wind farm drag. This omission is slightly discouraging as these complementary methods underly one of the main insights in the paper. I will try to clarify this point in the revisions.

Neither reviewer mentioned finding technical, logical or mathematical errors in the manuscript.

Both reviewers wish I had structured the paper differently by starting with a more complete description of the model. I will attempt to improve this in the revised version. Still, I think that both reviewers had a clear understanding of the formulation I used.

### 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 will add a short outline to the manuscript.**

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.

**I was not aware of previous RL treatments. I will add such a discussion.**

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.

**I will clarify how the hydrostatic assumption is used. I will be much more explicit concerning the role of non-hydrostatic pressure fields.**

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 1 already shows how the GW and RL cases differ quantitatively. I will expand 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 will try, but an analysis of farm shape is not the subject of the paper.

## 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 had missed this point and will add such a discussion.

#### 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.

#### 3) *More description*

Yes. I will add 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.