

# REBUTTAL

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## Introduction

Here follows the rebuttal to the review for publication of <https://wes.copernicus.org/preprints/wes-2025-276>. We much appreciate the critical and constructive feedback of all reviewers. We shall first broadly address some major points of feedback before elaborating on individual comments.

5 The main critique on this work addressed the validity of the problem statement on simulation using the actuator line method (ALM), which was addressed by 2 out of the 3 reviewers. Both these reviewers request a more elaborate explanation of the implementation and an intermediate analyses of the 2D situation to effectively bridge the gap between our (simple) conceptual explanation and our (much more complex) demonstration of the effect on a wind turbine. We shall address these suggestions in detail in the revised manuscript.

10 Regarding the implementation, we have opted in our current version for a conceptual description, portrayed by the left panels of figure 1, which we considered easy to understand and explain. This explanation uses strictly "explicit" principles, where all derivatives are based on the current state, to then advance (both CFD and turbine model) to the next time step. This leads to an apparent inconsistency in the implementation, where the sample is not centred on the projection anymore. Besides, it is not how most ALMs are implemented. We understand now that this raises concern and that it distracts from the main point;  
15 namely that the bound vortex shifts and shrinks depending on the set-up (depending on  $\Lambda$ , the parameter defined in this work). It is not due to a poorly chosen sampling location that our described problem (i.e. the shifting/shrinking of the bound vortex) occurs; this is due to the discrete nature of the force projections. We shall rectify this by using a more standard implementation and provide a more elaborate description of the implementation, focusing attention to those elements that cause the problem to emerge (the shifting/shrinking bound vortex), and adjusting reference to the implementation throughout the manuscript,  
20 including remaking some of the figures.

Regarding the 2D analyses, our preliminary simulations provide more clarity on the mechanism at play, which clearly advocates for their inclusion in the revised manuscript. Namely, it is mostly the movement of the blade relative to the grid, not (or much less) the movement relative to the flow, that causes the shifting and shrinking to occur. This is explained by considering the integration scheme governing advection, which generally distributes advective forces in time different from how the ALM  
25 distributes its forces in time. Since in wind turbine simulations the movement of the blade, relative to the grid, makes up most of its movement relative to the flow, we do not expect this difference to affect our further results and conclusions. We shall use

these 2D analyses, together with a more elaborate explanation of the implementation, to paint a much clearer picture of what exactly is happening to the bound vortex and why/when we expect it to occur.

We shall now summarize the feedback of each reviewer and our intended action, and address their individual comments.

## 30 **1 Reviewer 1**

This reviewer comments on an insufficient literature review, the role of the implementation on our description of the bound vortex and on the results, and request an additional analysis. In the revised version we shall address these points. The literature review will specify in more detail what motivated previous work and what they achieved. We shall address concerns regarding the implementation in an additional section that gives a schematic description of the type of implementation to which our description of the bound vortex applies, and explain exactly what causes the presently described problem (the shifting and shrinking vortex for increasing values of  $V*dt/epsilon$ ) to arise. We shall simulate the 2D situation as requested, which more directly relate to the (1D) principles we use to describe the bound vortex.

### 1.1

#### **Manuscript**

40 “Despite its wide and frequent use, there is no thorough consensus on how the ALM is best operated, nor is it well understood how it affects the outcome of a simulation. In this work we aim to address the topics of convergence of simulations and velocity sampling using a quantitative description of the bound vortex, and discuss best practices regarding computational efficiency and accuracy. We first introduce how the ALM operates and how it is treated in literature before further introducing its problems.”

#### **Reviewer Comment**

45 This statement is overstated and does not reflect the breadth of existing literature on ALM usage and parameter sensitivity. There is a wealth of literature on how to use the ALM and the influence of parameters. The authors need to do a deeper dive into the literature to better understand the challenges that are there and the ones that have been resolved.

#### **Author reply**

The authors agree that the current literature review does not do appropriate justice to what has been done and why. We shall improve this by elaborating more on the motivations and conclusions of individual works. At the same time, the variety of methods and settings being discussed in recent literature, particularly those concerning velocity sampling, does not signal a thorough consensus on the topic. The overstatement was intentional, to direct the mind of the reader early on. However, we understand the comment of the reviewer and will adapt accordingly.

## 1.2

### 55 **Manuscript**

“Various corrections can be found in literature to make these 2D polars more applicable to the 3D situation, most commonly addressing tip behaviour”

### **Reviewer Comment**

60 The corrections that are referenced in this are not to correct the 2D polars. They are meant to include the missing induction from an unresolved ALM. The authors should study the referenced papers to better understand the challenges that are being addressed in the literature.

### **Author reply**

While tip behaviour (which is the common denominator of the mentioned works) is a 3D phenomenon, the corrections indeed address an ALM phenomenon not related to the polars itself. We shall rectify this.

## 65 1.3

### **Manuscript**

“Practically, convergence is often associated with how well the relevant physical phenomena are represented, since adding more detail (thereby changing an input) to a simulation that already appropriately represents all relevant physical phenomena will not lead to a better representation (it was already appropriate).”

### 70 **Reviewer Comment**

This sentence is not clear. Convergence has to do with the solution approaching the limit as the resolution is increased. It is not about how well the relevant phenomena are represented, that should be accuracy.

75 The authors are talking about different things in this section. There is the convergence of grid spacing (dx, dy, dz). And there is also convergence in time (dt). Apart from that, there is the sensitivity to ALM parameters such as epsilon. The authors are mixing the many elements of the problem, but they should expand this section and clarify what convergence is. The literature has a good grasp of “convergence” related to epsilon/dx. However, the authors should make the point here that the convergence of  $\epsilon / dt * U$  has not been studied thoroughly.

### **Author reply**

Thank you for pointing out our lack of clarity, we shall expand this section as suggested.

**Manuscript**

“These three steps (velocity sampling, aerodynamic routine, force projection) are executed at each node at each time step before advancing to the next, such that the velocity is sampled a distance  $\Delta s$  away from its previous location relative to the flow, where the force projection was centred.”

85 **Reviewer Comment**

This is not necessarily true and a properly implemented ALM algorithm should use a numerical method where there is consistency between the sampling and application of the force. For example, I recommend that the authors look at the work of Kuhn et al (WE 2025, Sec 2.5.2, eq 36).

In the case of sampling and applying the force at different locations, depending on the numerical algorithm used in the solver, the implementation of the ALM might not be consistent with the numerical method of the code used.

**Author reply**

We shall clarify the general principles of the implementation from which the problem emerges that we describe. This will be an additional section among the methods.

To comment on consistency between sample and projection; a key insight from our description of the bound vortex is that it strays from the location where it was last projected by a distance dependent on the parameter  $V*dt/epsilon$  (figure 1c). Does the reviewer mean with consistency that the sampling is done exactly in the centre of the vortex? Then this consistency depends not only on the implementation, but also on its set-up (i.e. on  $V*dt/epsilon$ ).

To add a small note on the work of Kuhn et al., they state that as they sample (at step  $n$ ), they centre their projection half a distance travelled by the node ahead of their last sample (at  $n+1/2$ ). This implies that the next sample is also half a distance-travelled ahead of the force projection (at  $n+1$ ). Consider our figure 1, panel (c), the  $x$ -axis shows the distance-travelled, with on the  $y$ -axis the location of the bound vortex; it has a slope of  $1/2$  in the region  $\Lambda \lesssim 1$ . Thus our description gives a clear explanation of *why* their method gives the correct result. We do not intend to repeat this argument in our manuscript, as we consider addressing each individual implementation too much of a widening of scope. We instead aim to clarify exactly what elements of the implementation cause the described problem (shifting/shrinking bound vortex) to emerge. We shall also shortly comment on the effect of the Runge-Kutta 3 scheme (the time integration scheme used in our simulations) on the projection, which we use specifically to explain features of our 2D analysis. This elaboration on the RK3 scheme also serves as an example of how the principles of our description (i.e. acquiring vortex properties from the cumulative kernel) may be extrapolated to other implementations/schemes.

## 1.5

### 110 **Manuscript**

“We can therefore deduce the vortex properties of the flow induced by the AL by considering this graph of cumulative velocity increments. The cumulative velocity increment at any location  $x$  and time step  $n$  is as follows;”

### **Reviewer Comment**

This is making an approximation regarding the solution of the moving problem as a collection of Lamb-Oseen vortices that appear and disappear in a discrete way. However, this is not the right formulation. This is assuming that at every time, there is a collection of discrete vortices in the flow. However, that is not the way that the solution will look like.

In continuous forcing, the induced vorticity/velocity field evolves continuously and the discrete superposition interpretation must be justified against the governing equations/time integration.

### **Author reply**

120 We believe there is a misunderstanding here. We do not describe the bound vortex as a collection of Lamb-Oseen vortices, but as a collection of forcings by the ALM. The equation that follows (eq 7, line 170) depicts the thick lines in figure 1 (bottom left); the sum of the little Gaussians, not the sum of multiple Lamb-Oseen vortices. These Gaussians add up to create a region of vorticity somewhere, in which we model the velocity as a single Lamb-Oseen vortex. We are not sure this misunderstanding warrants adjustments to our manuscript. Our description mentions only a single Lamb-Oseen vortex (equation 5, which contains

125 no summation signs) and a cumulative kernel (equation 9, which does contain a summation sign), so not a cumulative vortex. We understood this may be difficult to follow on first read-through, therefore we summarized the main principles of our description in the last paragraph of section 2.2. This is already part of our manuscript. However, since the description of the bound vortex is central to our work, we want to avoid confusion here. We will keep this ambiguity in mind as we further restructure the text.

130 The discrete superpositions are thus not so much an interpretation, rather something that is directly forced upon the flow. The interpretation comes when describing velocities in this region of vorticity as a Lamb-Oseen vortex, which does not directly follow from the forcing of the ALM, but requires solution of the CFD solver. We justify this shortcut against literature, lines 148-149.

## 1.6

### 135 **Manuscript**

“The velocity sample  $V_s$  that we use for validation is taken at the location of where the future force projection will be.”

## **Reviewer Comment**

This raises concerns regarding the consistency of the force-velocity coupling and requires further clarification. The authors should explicitly document how velocity sampling and force application are synchronized in time and space.

### 140 **Author reply**

We shall clarify this in an additional section among methods (the same we mentioned in 1.4).

## **1.7**

### **Manuscript**

“2.4 Software”

### 145 **Reviewer Comment**

This section should be revisited to talk about the numerical details of the ALM implementation in regard to the time-stepping scheme and the location of the actuator point.

### **Author reply**

We shall clarify this in an additional section among methods (the same we mentioned in 1.4 and 1.6). While the specifics of each  
150 implementation may add some variation on our description of the bound vortex, we wish to emphasize the common principles  
in a separate section. This signifies that the description of the bound vortex does not only apply to our specific implementation,  
but to all that share a common principle; namely that the fluid receives finite (non-infinitesimal) velocity increments at locations  
that are spaced a finite (non-infinitesimal) distance. Whether this discretization is due to a finite time step, a finite iteration step  
(i.e. one that is paired with displacement of the node, and thus suits the description of how the fluid is forced in the previous  
155 sentence), or something else, does not matter in principle.

## **1.8**

### **Manuscript**

“We observe that most points lie close to the  $y = x$  line, thereby confirming that flow near AL nodes generally behaves as  
described by Eq. (13) and by extension, Eq. (5).”

### 160 **Reviewer Comment**

The results are quite off from the line, I would not consider this to be good agreement, it is at best a first-order approximation.

## Author reply

We want to clarify that we intentionally avoided strong statements such as "good agreement", and also that this statement is followed by a section dedicated to applying nuance to this statement. We are not trying to make our results anything more than what we are showing. We are simply signalling that there is some predictive value in our description, that applies to a wide variety of set-ups, and that we may allow ourselves to see what we can learn from this description. We shall soften this statement further to reflect this.

Regarding the order of approximation; Taylor expanding equation 13 gives a first order term of  $|x-x_0|/rv$ , which scales with  $v*dt/epsilon$  for low values of  $v*dt/eps$ , following figure 1e. We have previously performed analyses similar to figure 3 using only this first order term (or  $v*dt/eps$ ), which shows the data points to gradually curve away from the  $y=x$  line at larger values. This shows that the higher order terms are needed to appropriately describe the observed behaviour. We shall not mention this lower order dis/agreement in our manuscript to keep the story simple; we just use the Lamb-Oseen model as-is.

## 1.9

### Manuscript

“Lastly, it is observed that simulations using the smallest kernel sizes ( $\epsilon = \Delta x$ , and  $\epsilon = 1.5\Delta x$  to a lesser extent) deviate downwards for larger values on the  $x$ -axis. . . .”

### Reviewer Comment

These values of epsilon ( $epsilon/dx < 2$ ) are not recommended in the literature. The authors later justify including these; however, including those doesn't seem to add any value to the discussion, other than adding more error and uncertainty. I recommend that the authors remove this discussion and revisit their choice for range of epsilon to be  $epsilon/dx > 2$ .

## Author reply

We find, in this work, that we can describe the bound vortex as function of the single parameter  $V*dt/epsilon$ . Common recommendations in literature restrict the value of this parameter to  $< 0.5$  (i.e. using  $dt=dx/V_{tip}$  and  $epsilon=2dx$ ). Since we expect our description to be valid beyond this value, we would like to validate there (although we agree that the data from simulations using  $eps < 2dx$  do not make a particularly convincing validation), which we can only achieve by using large time steps or small epsilon. We note that some authors also study the behaviour beyond these recommendations, e.g. Zormpa et al. (2025).

## 1.10

### Manuscript

“Figure 5 shows the thrust coefficient  $C_t$  of the wind turbine for various time steps ( $\Delta t$ ) and kernel sizes ( $\epsilon$ ).”

## Reviewer Comment

This figure result is unexpected. This strong dependency on time-step suggests that there could be an inconsistency in the ALM implementation. I also recommend removing results of  $\epsilon/dx < 2$ . Also, it is not clear, what is the value of epsilon in these simulations? Is it changing with the grid?

### 195 Author reply

We would like to point out that the range of values are similar to those found in e.g. Zormpa et al. (2025). We later comment, addressing our third reviewer in section 3, that we are not intending to demonstrate a state-of-the-art wind turbine simulation, but rather an easily interpretable set-up. Perhaps that applies here as well. The value of epsilon scales with the grid, as we indicate in the legend ( $\epsilon[\Delta x] = \dots$  we read as "epsilon, in units dx, equals ..."), although in this figure all results are from  
200 simulations of the same  $dx=4$ . We use this notation to be consistent with table 1.

## 1.11

### Manuscript

“there is an erroneous process (i.e. sampling within the bound vortex yielding  $V_d \neq 0$ ) affecting the outcome of the simulation.”

## Reviewer Comment

205 This is the key element of this manuscript. As written, it appears more consistent with an inconsistency in the numerical force-velocity coupling than with a fundamental limitation of the ALM formulation.

### Author reply

We agree this is a key element of this manuscript and that we must clarify this further. The reviewer correctly identifies there is some nuance related to the implementation that we glossed over. As we explained in the present introduction of our reply to  
210 this reviewer, we consider this a mistake that we shall rectify. We refer to our reply of comment 1.7 on how we intend to do this. We lastly want to stress that our main point is not invalidated by a poor choice of implementation; as long as the fluid receives finite (non-infinitesimal) velocity increments at locations that are spaced a finite (non-infinitesimal) distance, the bound vortex shifts (fig 1c) and shrinks (fig 1d).

## 2 Reviewer 2

215 This reviewer addresses the problem statement, the role of the implementation, a lack of reference to lifting line theory whose principles underlay the ALM, request a 2D analysis, and express their concern on errors piling up from the various unresolved vortices.

We shall clarify our implementation in an additional section. This description of the implementation should also make it clearer what problem we are addressing, as it will focus on those parts that are responsible for the described flow phenomenon and its behaviour (i.e. the bound vortex, and its shifting and shrinking for increasing  $v*dt/eps$ ). An analysis of the 2D situation will be in another added section.

Regarding the reference to LL theory, we notice a difference in point of view from our reviewer. We consider the ALM purely from its output; how it forces the flow, while LL theory describes the intended behaviour of the ALM. The key point of this work is that the ALM does not necessarily what it is intended to do (i.e. it does not exactly mimic a LL), because the position and size of the bound vortex changes, depending on the set-up (dt and epsilon). At the same time, we recognise that by leaving it unmentioned we may not communicate in the most effective way. We shall adjust our text accordingly with this goal in mind.

The concern of missing induction of the unresolved vortices affects two outcomes; the velocities at the blade, and the thrust/behaviour of the simulated turbine. We discuss local velocities in section 3.1; figure 3 shows trajectories of points that mostly lie near the  $y=x$  line, except where there appear spanwise gradients in the forcing which we refer to as the finite cylinder and tip effects (whose effect we explain using figure 4). We believe that it is the tip effect that the reviewer attributes to missing vorticity, whose effect on the data (of figure 3) we have explained in detail in lines 285-289 and figure 4. In our analysis (figure 3), the tip effect shows up as a difference between the sampled velocity and the undisturbed velocity. We refer to section 2.3 in which we explain how we determine the undisturbed velocity and to appendix A in which we state that indeed, the undisturbed velocity does not fully account for disturbances of the tip/trailing/etc vortices.

We discuss behaviour of the turbine in section 3.2 and figure 5, and particularly in appendix B, where we explain to what extent we expect the bound vortex (and our description of it) to be the sole explainer of the thrust variation. Term (4) of equation B3 represents the dependence of flow through the rotor as a function of epsilon, which we may interpret here as the degree by which any of the ALM vortices (tip/root/trailing) are resolved. Since we are using rather egregious values of epsilon, we expect it to affect  $C_t$  (left hand side of equation B3, plugged into equation B1). This also means, which is the point we make in appendix B, that we are not able to isolate the effect of the bound vortex (term (6) of equation B3) in our analysis of figure 5. Thus, while the point raised by our reviewer is valid, we consider it appropriately discussed.

## 2.1

### Manuscript

The actuator line method (ALM) is a popular method for representing wind turbine blades in computational fluid simulations. It utilizes a model to compute aerodynamic forces that requires an undisturbed flow velocity relative to the blade. However, the input velocity and output forces are sampled and applied locally; within the bound vortex that forms around the blade, and is thus disturbed.

### **Reviewer Comment**

250 The AL is a lifting line representation for NS solvers. It does not require "undisturbed" inflow, it should include disturbed inflow (from the forces of the turbine itself). Whilst this might align with the wording used within the paper one needs to be very precise here, you are looking into the effect of the bound vortex and how it interacts with velocity sampling + time stepping.

### **Author reply**

255 We see that our statement is too general and thereby not correct. Time stepping is indeed crucial for the problem to occur. We shall adjust accordingly.

### **2.2**

### **Manuscript**

260 Consequently, this attribution to an erroneous process implies that convergence of the ALM is only partly related to the degree of representation of the relevant physical phenomena, which convergence is often assumed to imply.

### **Reviewer Comment**

Convoluted, I don't understand

### **Author reply**

Understandable, we shall split this/rewrite. To clarify, we make multiple statements in this single sentence:

- 265
- sampling within bound vortex is an erroneous process
  - the degree of convergence should be related to the degree of representation of the relevant physics
  - however, the erroneous process is partly responsible for spread in outcomes (non-convergence)
  - and, the erroneous process is not recognized to be responsible for this non-convergence

### **2.3**

### **270 Manuscript**

In such simulations blade forces are modelled, allowing for a flow solution that includes the blades without needing the detail to fully resolve flow around their intricate geometry.

### **Reviewer Comment**

275 It is a lifting line for CFD, ie it uses 2D aerofoil polars to compute the local forcing along the lines, but has been shown to represent the vortex system around rotors well (see early AL work by Mikkelsen, Troldborg, Ivanell etc)

### **Author reply**

We prefer a more general description at this point in the introduction.

### **2.4**

### **Manuscript**

280 Despite its wide and frequent use, there is no thorough consensus on how the ALM is best operated, nor is it well understood how it affects the outcome of a simulation.

### **Reviewer Comment**

285 I think there is pretty strong consensus actually. 3D Gaussian kernel, eps at least  $2 \cdot dx$ , tip smearing correction and  $dx/(\Delta \cdot \omega \cdot R) < 1$ . Please review existing literature more thoroughly. However this does not mean that your investigation is not relevant, but one needs to be careful with such bold statements.

### **Author reply**

This is indeed somewhat overstated. Operation of the ALM here refers specifically to the velocity sampling method. We shall be more specific.

### **2.5**

### **290 Manuscript**

The original description of the ALM by Sørensen and Shen (2002) uses a velocity that is sampled at the location of the node; a single point.

### **Reviewer Comment**

295 In the context of velocity sampling, it is very important to note right here, as to why this was done by the AL inventors. It mimicks a LL, and that sampling location self-induction from a bound vortex = 0, so one does not need (at least theoretically if there are no numerical issues, see for instance Forsting & Troldborg 2020) no correction.

### **Author reply**

Thank you for this addition, we shall incorporate it.

### **2.6**

#### 300 **Manuscript**

such as sampling within a volume around the node (Churchfield et al., 2017)

### **Reviewer Comment**

Be more specific. Churchfield looked into using an integral velocity sampling, which was actually first proposed by Forsythe et al. in 2015. Their reasoning for doing so is actually very compelling, however as also shown by more recent work by Taschner et al. it of course introduces some time filtering.

### **Author reply**

We shall provide some more detail on what motivated the various cited works.

### **2.7**

#### **Manuscript**

310 strictly only valid for the steady state situation

### **Reviewer Comment**

I actually disagree with this statement, unless I am misunderstanding it. This needs some background discussion. It would be good to discuss this in terms of the AL algorithm and its implementation (there are plenty of papers that detail at what time step velocities are extracted and when and where they applied). If the AL simulation is converged (for instance via sub-iterations) at each time step, why would we not get the same as in the steady-state?

### **Author reply**

This statement needs the context of this work. We show in this work that when you have a time dependent simulation, something happens to the bound vortex (it shifts and shrinks) and thus with the velocity sample. This context is not given here yet, so we shall omit this statement here.

**Manuscript**

The main assumption of the ALM is thus that local 3D effects at the blades can be described using polars that map from an undisturbed velocity to the force vector on a 2D airfoil.

**Reviewer Comment**

325 No. This is a fundamental principle underlying LL theory. Please read papers discussing the inner and outer vorticity system and LL). The outer system is the 3D wake vortex system and the inner the 2D bound problem for which it is valid to use 2D polars (and only 2D polars to avoid double counting)

**Author reply**

I think there is a misunderstanding here. The main point of this sentence is to contrast local<>undisturbed, i.e. the fact that  
330 polars are used in the ALM to map local velocity to local force (local>local), while upon construction (in controlled environments, windtunnel/CFD/...) these polars map "far upstream" velocity to local force (nonlocal>local). The 3D<>2D contrast is mentioned to give the reader a sense of the complexity of the fluid (described by 3D velocity vectors and acted upon by 3D force vectors) and the simplified interaction it has with the structure (which uses 2D principles, but more importantly, undisturbed "far upstream" flow). Regardless, we realize this paragraph confuses the reader and will reconsider it.

**Manuscript**

In reality, a blade section accelerates fluid as it passes over a (2D) surface, while a CFD model uses a 3D grid. This requires a choice to be made about what volume of fluid the blade section is to accelerate.

**Reviewer Comment**

340 I don't understand. The AL and LL both accelerate flow in the same way, the AL just has a viscous core due to the forcing. This has nothing to do with having a 2D or 3D grid. Try running a 3D CFD simulation with an AL and symmetry BCs and make a cut through the middle (done by many authors you are citing) and compare to a 2D simulations (watch out regarding the kernel normalisaiton). You should get the same velocity p'rofile.

**Author reply**

345 I think there is a misunderstanding here. We want to emphasize the fundamental difference between what happens in the outside world and in a (3D) CFD simulation. We are not considering 2D simulations. The force in the real world is due to a pressure

difference, integrated on the 2D surface of the lifting structure. The force in the 3D simulation must be applied to a 3D grid. This translation requires a choice to be made about what volume of fluid the blade section is to accelerate since there is no theoretical equivalent "correct" value.

350 This is a more fundamental explanation of why force needs to be smeared than is often found in literature, which often involves arguments of numerical stability (note that forcing a single CFD node is just another choice of volume that is accelerated; the volume or mass/density of fluid that the node represents). We shall elaborate this explanation along the lines of what is noted above, or use a more common explanation, since the current one is not essential to our story.

## 2.10

355 **Manuscript**

numerical stability, rather than accuracy or convergence.

### **Reviewer Comment**

Forsting & Troldborg expanded on this also looking into its dependence with chord

### **Author reply**

360 Interesting, thanks for the suggestion.

## 2.11

**Manuscript**

allows for better subsequent velocity sampling.

### **Reviewer Comment**

365 symmetry? or why

### **Author reply**

Their conclusion addresses spherically symmetric vs oval/cigar shaped kernels.

## 2.12

**Manuscript**

370 general consensus on a size of  $\epsilon \geq 2\Delta x$

## **Reviewer Comment**

Not in LBM solvers actually, there one can go lower and be more precise it depends on the circulation distribution, it is not arbitrary.

## **Author reply**

375 Noted.

## **2.13**

## **Manuscript**

A problem inherent to this three-step operation is that the force projection of step 3 disturbs the velocity sample of step 1 of the next iteration or time step.

380 **Reviewer Comment**

This is argument is hard to follow without explaining what AL algorithm you are using. If you do it correctly this should not be an issue.

## **Author reply**

385 We shall include an additional section covering the general principles of the implementation from which the problem (i.e. the shifting and shrinking bound vortex) emerges. We still think it is appropriate to make this statement in the introduction (before the explanation in the methods), but will expand this using a similar statement in literature. We found that your earlier suggestion of Forsting and Troldborg (2020) nicely explains this. Quoting from their introduction: "the self-induction at the very centre of the bound vortex should, following vortex theory, be nil. However, Shives and Crawford [7] found that in CFD simulations it is in fact always non-zero"

390 **2.14**

## **Manuscript**

At the same time, the analytical study of Martínez-Tossas et al. (2017) advocates for sampling at a single point. They describe the flow field near the actuator line (AL) as a superposition of analytical steady state solutions of the flow subject to the lift and drag forces separately.

395 **Reviewer Comment**

There are multiple other studies as well and also theoretical studies discussing this and all are related to the AL being a LL.

## **Author reply**

We must point out that the referenced work does not explicitly refer to LL. They approach the problem by considering the fluid being forced in a certain way, not why that force is applied or what aerodynamic system it is intended to mimic. Our approach is similar in that sense (we consider the presence of the ALM only as a sequence of Gaussian forcings in the fluid, being less concerned with what determines that force and why), hence we consider this reference most appropriate.

## **2.15**

### **Manuscript**

The works of Zormpa et al. (2025) as well as Muscari et al. (2024) have shown that the variation in loads and performance (i.e. rotor thrust and power) is reduced for some velocity sampling methods with respect to others.

### **Reviewer Comment**

But variations in power or loads are not a proxy for convergence. Are we talking about numerical convergence regarding spacing or time resolution? And is this regarding the bound vortex or the trailed system? As non-convergence of power and loads has been document many times before (see for instance Forting et al 2019) and can be attributed to not correcting for the viscous core introduced by the force smearing.

## **Author reply**

We refer to convergence of multiple simulations reaching the same outcome, as is done in one the referenced works. This is not to be confused with convergence of solution in the mathematical sense, i.e. of the kind that is reached upon iterating the same problem multiple times, such as the process described in the work the reviewer mentions. We shall clarify this distinction further as we understand the confusion.

We do not agree with the reviewers suggestion that convergence in power and loads can be achieved by simply correcting for the the viscous core effect. Such a correction (e.g. the one from the mentioned reference of Forsting et al. 2019) attempts to model what belongs to the domain of the fluid, and thus should be solved by the CFD solver. Besides, it will fail in more complex flows (e.g. turbulent inflows, or flows around moving/floating wind turbines). Additionally, Zormpa et al. (2025) show converge can also be influenced by the velocity sampling method, which we relate to the bound vortex in section 4.1 and figure 6. While we agree that corrections can be an effective and pragmatic solution to bring a simulation closer to convergence (we propose such a correction in this work!), none of them are fool-proof.

## **2.16**

### **Manuscript**

The consensus, or lack thereof, on

### **Reviewer Comment**

There is consensus. I disagree.

### **Author reply**

430 While following LL theory, sampling at the collocation point may be the obvious choice (perhaps this is what the reviewer refers to?), not all authors adhere to it for varying reasons. We interpret this as lack of consensus.

### **2.17**

### **Manuscript**

i.e. those that average multiple points

### **Reviewer Comment**

435 Is that the most prevalent? It is far too expensive and smears sudden AoA changes in turbulent flow

### **Author reply**

Saying this is the most prevalent is indeed a difficult statement to make, we will change this.

### **2.18**

### **Manuscript**

440 provides context for the rest of this work.

### **Reviewer Comment**

please present your coupling between AL and CFD

### **Author reply**

This shall be addressed in a new section.

445 **2.19**

### **Manuscript**

We propose that this disturbance is due to the bound vortex, similar to the vortex found by Martínez-Tossas et al. (2017) which is valid for steady state.

### **Reviewer Comment**

450 Why are you proposing this? This is essentially how a LL works.

### **Author reply**

We use the word "propose" since the bound vortex is not explicitly stated by Martinez-Tossas et al. 2017 and want to be careful not to change their meaning.

### **2.20**

455 **Manuscript**

1. We introduce a future: A future sampling location may move relative to the flow structure caused by the current force projections. 2. We introduce a past: The current flow structure is the result of having been forced at progressively shifting locations in the past.

### **Reviewer Comment**

460 I do not understand what you mean by that and the title of each seem philosophical not technical

### **Author reply**

By introducing time, we introduce a new axis in which the problem can vary. One can go forward or backwards on this axis, or for time, into the future or past. We are using this distinction to signify rigour; we have covered all dimensions of this new axis. It is indeed somewhat philosophical. By adding the new section on implementation, we also expect to be able to better  
465 explain this element.

### **2.21**

### **Manuscript**

We choose to describe the bound vortex using a type of Lamb-Oseen vortex

### **Reviewer Comment**

470 This is not a choice but has been shown by many authors starting by Forsythe to be the case. Maybe you choose it in the time-resolved frame but then I do not understand how your AL coupling works.

## Author reply

The choice also includes the last half of the sentence;  $x_0$  and  $r_v$ , we introduce these variables here. The point being that we allow ourselves some freedom in its location and size, as well as its evaluation location (such that it generalizes for different velocity sampling methods, as we demonstrate in figure 6). This freedom is necessary, as is further explained in the same section (2.2) and shown in panels (a) and (b) of figure 1, to accommodate distortion resulting from time discretization. We agree that the presence of Lamb-Oseen vortices has been reported by many authors, of which we give 3 examples in the next sentence.

## 3 Reviewer 3

This reviewer primarily comments on recommendations for setting up simulations following from our work, and how that set-up affects the results.

We would like to refer to section 4.3 where we discuss using our description of the bound vortex as a correction (this our preliminary recommendation). In principle, the sample should become less sensitive to variations in time step size and epsilon similar to how simulations using the LineAve method (Jost et al., 2018; Melani et al., 2024; Zormpa et al., 2025) are less sensitive to these settings. Note that this correction would only account for the bound vortex disturbance, not for those of the tip or trailing vortices. We are not yet confident to do a proper recommendation, since that would require implementing this correction and validating it. We consider the scope of this work to be the description of the bound vortex, and how its primary insight (that the vortex shifts and shrinks as a function of  $v*dt/eps$ ) affects velocity sampling and convergence of simulations. The implementation of the correction and analyses of its performance may be the topic of a future work.

Furthermore, we shall explicitly state that we opt for a very simple implementation of the ALM (i.e. a single point sample, polars with no further correction, projection using a spherical Gaussian kernel). This makes our set-up a sort of minimum working example of the bound vortex in wind turbine simulations, which both eases interpretation and is computationally efficient. This also means that our simulations are not quite state-of-the-art and may be associated with errors larger than found elsewhere in literature, especially since we use settings beyond the recommended range (i.e.  $epsilon < 2dx$  and  $dt > dx/V$ ).

3.1

## Reviewer Comment

line 130: before  $V_\infty$  it should be a colon, not a semi-colon

## Author reply

We shall adjust this.

500 **3.2**

### **Reviewer Comment**

line 150: what is deviation 1? and the subsequent deviation 2 in line 159?

### **Author reply**

This refers to deviations related to the past and future when introducing the time dimension (lines 141-146).

505 Note also that in reply to a comment by another reviewer, we state that we intend to add a more elaborate description of the implementation in which we focus also on those elements from which the problem emerges. This will likely make this passage obsolete.

**3.3**

### **Reviewer Comment**

510 line 213: if you study the problem as a 2D or 2.5D problem there is the possibility to define the undisturbed speed as inlet of the simulation, and use the real  $V_u$ . If the simulation is unsteady or turbulent, there may be the effect of airfoil induction on  $V_u$ . But the axial induction may be limited by the low drag values, while the induction due to lift may be higher.

### **Author reply**

This is true, we shall add analyses of a 2D simulation, or 3D that is homogenous in the 3rd direction.

515 **3.4**

### **Reviewer Comment**

line 297: not sure if you are referring to  $\epsilon$  or to the distance between the sampling locations  $\Delta X$

### **Author reply**

520 We are referring to epsilon. In this section we explain that a node is considered far from the tip if it receives equal contribution from both sides in the direction of the blade (and thus the cumulative kernel looks cylindrical). A node is considered near the tip if the lack of contribution from beyond the tip are felt by that node. We explain this effect in lines 290-294, then relate it to the figure in lines 294-296, to then give a final statement about how this effect specifically applies to varying epsilon.

525 We are hesitant to elaborate our explanation too much since we think all information is in place. Perhaps including this last statement in the initial explanation would be clearer, as well as expanding the explanation to include tips near the blade, not only the (sole) tip at the blade.

### 3.5

#### **Reviewer Comment**

line 393: if the simulation has uniform constant inflow and BEM is deemed to provide correct results, the blade element velocity can be compared with the BEM solution (not only comparing global quantities)

#### 530 **Author reply**

We have considered comparing to the BEM result, but considered our current method of acquiring the "undisturbed" velocity more appropriate. Since we acquire it from the simulation itself, we include all further errors that are made in the simulation. Any further errors will be present both in the sample and undisturbed velocity such that subtracting them should cancel them, effectively isolating the disturbance we are interested in.

535 Furthermore, the statements in this section apply to any simulation using the ALM, as we state in the sentence starting on line 372. We cannot deem BEM to provide correct results in all situations where ALM is used.

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