Response to the Reviewers

H. Asmuth et al.

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Dear Reviewers,
First of all, we'd like to thank you for the many detailed and constructive comments on the manuscript. They clearly helped to improve the quality of the paper. Following your suggestions some parts of the paper have been largely restructured. Firstly, parts of the theory on the LBM have been rewritten and extended in order to give a better introduction to people not familiar with the method. Secondly, we extended the code-to-code comparison by an initial study of a turbulent inflow case. As mentioned by Reviewer 1, such a comparison comes with additional challenges in terms of the inflow but we agree that it generally states an improvement to the paper. Lastly, based on the comments of Reviewer 2 as well as remarks of Reviewer 1, we have shortened the investigation of the impact of the third-order limiter, clarified the underlying motivation and corrected our wording in terms of implicit LES. Consequently, also parts of the abstract, introduction and conclusion were adapted with regards to the changes.
Please find below our detailed answers (black) to the comments (blue). Note that line and page numbers referred to in our answers correspond to the new corrected manuscript.

1 Reviewer 1

1.1 General Comments

- The level of detail given is sufficient most of the times, however is lacking in some areas. The introduction to the LBM method needs improvement. Whilst trying to be basic it still assumes a lot of in-depth background knowledge and the notation is inconsistent at times, leading to confusion.
  We have incorporated the more detailed changes suggested in the supplement and rephrased selected parts in all subsections on the LBM. For further details, see our answers to the comments from the supplement.

- Choices of the numerical setup remain unclear and need to be discussed in more detail, especially the sensitivity of their results to those choices.
  Again, we refer to our answers to the supplement comments.

- The authors also should try to avoid subjective statements and rely on quantitative evidence.
  We have added a direct comparison of the two numerical approaches in terms of the L^2-relative error norm providing a more quantitative measure of the observed differences. *See, Figure 6 as well as the in-text discussion on page 14*. The same measures are incorporated in the discussion of the new turbulent inflow case. *See, page 20.*

- Even though the authors mention in the conclusion that "..., the presented work underlines the great potential of wind turbine simulations using the LBM", there is no code-to-code comparison for turbulent inflow cases. They are right to argue that it is difficult to have similar inflow turbulence for both codes (this is also the case when comparing vortex methods with FV solutions), however this should not stop them from conducting at least an initial investigation. Predicting the transition characteristics in uniform inflow, though of academic relevance, might not be the best showcase for the applicability of LBM in engineering wind energy flows. Whilst not adding a turbulent flow case to this paper should not prevent its publication, it diminishes its scientific potential.
  A code-to-code comparison in turbulent inflow is now provided in *Section 5*. As in other studies, the main challenge here related to different downstream evolutions of the imposed turbulence. We have discussed the occurring differences (in an empty domain) without going into too much detail, see *Section 5.1*. After all, a more thorough investigation thereof should be addressed by a more fundamental investigation elsewhere. Still, we agree that the comparison of the wakes in turbulent inflow states a good addition to the paper.

- Additionally, the conclusion is missing some detail on the future flow cases to be investigated and the exact challenges in modelling ABL flows.

We added some elaborations on the upcoming challenges, mostly related to the simulation of wall-modelled boundary layers. *See, page 25.*

1.2 Comments in Supplement

- P. 1, l. 1: The abstract should also reinforce as to why the LB method has such great potential in WE, which ultimately lead to this research. The motivation for conducting this research should be stated at the start of the abstract, ie answer the question: why should readers care?
We agree. The introductory sentences in the abstract were changed accordingly:
"The high computational demand of large-eddy simulations (LES) remains the biggest obstacle for a wider applicability of the method in the field of wind energy. Recent progresses of GPU-based (Graphics Processing Unit) lattice Boltzmann frameworks, though, provide significant performance gains alleviating such constraints..."

- P. 1, l. 13: Be more specific and mention actual values for the difference between the two codes in terms of forces, CPU time etc. instead of "good agreement", "difference" "significant speed-up". These statements are subjective.
We have added an exemplary quantitative statement in the abstract (*P. 1, l 8f.: "The near-wake characteristics in laminar inflow are shown to match closely with, for instance, differences of less than 3% in the wake deficit"*). However, we think that of some of the statements referred to by the reviewer can not be avoided in the abstract. Particularly, as they also have a summarising purpose.

- P. 1, l. 10: What does "generally" mean in this context
The whole sentence was removed in the rewriting of the abstract.

- P. 4, l. 3: Could be worth mentioning that index notation is used here in three dimensions for describing each node. The grid directions could also be added in some form to Figure 1, without showing necessarily all indices for all nodes.
Both changed accordingly.

- P. 4, l. 4: For people not familiar with LB methods these lattice specific set of velocities need a little more explanation, as they will otherwise will not understand the difference between lattice velocity c mentioned in Eq(2) and particle velocities. In the end the idea that each particle should end up at another node ie $dx = dt = 1$ actually leads to the statement that all components of e_{ijk} are integers, which is not necessarily obvious, but very important in discretizing the Boltzmann equation. That this also means that multiple speeds exist in D3Q19 and D3Q27 lattice types should be mentioned as this is quite different to classic FV formulations. In this context it could be mentioned that each lattice comes with its own set of weights and velocities, and that weights need to fulfill certain criteria to be suitable to solve NS equations using LB. Showing how the weights matter could be done by showing the actual formulation for f^{eq}_{ijk} or simply stating that they go in there.
We agree. First of all, we have corrected some inconsistencies in our original manuscript, i.e. a mix of dimensional and non-dimensional units in the same section. The non-dimensionalisation is now solely dealt with in *Section 2.3*. This clearly led to some confusion when it comes to the advection of PDFs. Furthermore, we have added details on the equilibrium (including the weights) as well as the general concept of the two-step collide-and-stream process. *See P. 4-5.*

- P. 4, l. 8: It would be good to add delta x to Figure 1 and explain it. "The cube has side length 2 delta x ...."
Changed accordingly.

- P. 4, l. 13: Is the summation range important? Is it from -1 to 1?
Yes, this is arguably often not made very clear in the notation in the LBM literature. We have now expanded the sum explicitly for each coordinate direction (*see Eq. (6)*). Also, we now explicitly mention the space of i, j and k (*see P. 4, l.4*)

- P. 4, l. 14: The order of the moment is the sum of the indices alpha, beta, gamma
Clearly an important addition. Addition accordingly, *see P. 5, l.1.*

- P. 5, l. 1: Maybe equation?
Yes, as mentioned earlier, the equation for f^{eq} as well as further explanations were added.

- P. 5, l. 2: Expand here. By only stating the equation its significance remains probably unclear to most people. This is really showing a direct relationship between NS equations and LBE.
Given the length of a comprehensible elaboration of the Chapman-Enskog expansion (or other similar

approaches), unfortunately, we can not expand on this in great detail and keep on referring to the literature, similarly to the vast majority of applied studies using the LBM. Yet, we have added the crucial aspect that the starting point of the the Chapman-Enskog expansion is a moment expansion of the LBE itself (*P.5, l. 5* which provides a first idea of the general concept and a link to the raw velocity moments introduced before.

- P. 5, l. 4: kinematic
  Changed accordingly.

- P. 5, l. 7: As mentioned further down it might be worthwhile to mention the different steps used in LB. Otherwise pre-/post-collision might be unknown terminology.
  As mentioned above, details thereupon are now provided. *See, Eq. (8) and (9).*

- P. 5, l. 22: Without knowing that LB has a collision and streaming step, this reference might lead to confusion. Maybe another term could be used or the two steps will have to be explained.
  We agree. This should be clarified now due to the aforementioned changes.

- P. 5, l. 25: First time mentioned here in this section, maybe mention already in line 15. Geier ... PDFs, ie.,cumulants. This method is referred to as CLBM. or something like that.
  The abbreviation is now being introduced in the introduction. Also the suggested change to line 15 was incorporated.

- P. 6, l. 6ff.: Until here the connection between lattice and method has not been discussed and should be mentioned earlier.
  We have added the crucial fact that the CLBM is only defined on the D3Q27 lattice as opposed to, e.g. SRT and MRT collision models.

- P. 6, l. 14.: Inflow velocity to the cell? In the end the scaling does depend on each cell otherwise c unequal 1.
  No, "*u_0 is the inflow velocity at the inlet" (see P. 6, l. 29).* More generally, it just refers to a global reference velocity used for the non-dimensionalisation.

- P. 6, l. 29ff.: Is Eq.5 also valid for CLBM and MRT, if so this is not mentioned in previous sections.
  Yes it is. A brief explanation was added in Section 2.2: *"Each moment is then relaxed individually towards a referring equilibrium moment m_{eq}. The individual relaxation rates of the hydrodynamic moments (up to second order) remain physically motivated with the second-order relaxation rate given by Eq. (4)" (See P. 5, l. 19ff).*

- P. 7, l. 23.: Maybe this sentence is sufficient in explaining the adaption by Geier. The following equation does not necessarily add anything to the description if it is not fully explained in detail as Geier did. Currently it is pretty confusing, especially when comparing to Eq8.
  We prefer to keep to explicit formulation of the limiter in the paper. After all, we think that it helps to understand the impact of values chosen later in the comparison in Section 6.

- P. 7, l. 24.: How does the index m translate to those in Eq8? This notation taken from Geier 2017b does not match with the one in Eq8 where omega_{alpha,beta,gamma}
  We have added a clarification that ω_m refers to the relaxation rates of the third-order cumulants and modified our explanation.

- P. 7, l. 25.: What is lambda?
  The limiter λ_m is now explicitly introduced before Eq. (15).

- P. 7, l. 26.: Capital C? In Eq7 c was used. A consistent choice should be made. In section 2.2 capital C represented the scaling coefficients.
  We agree and changed the notation in Eq. (15).

- P. 8, l. 9.: Describe in text
  Description added accordingly.

- P. 8, l. 10.: c is a classic variable for chord, but c is also the lattice speed in this paper. Consider using a different variable name
  The former c (chord) was renamed to c_a to avoid confusion.

- P. 8, l. 14.: distance between the centre and what
  We clarified: *"...and d is the distance from the centre of the blade element to the point in space where the force is applied." (See P.9, l. 3f)*

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- P. 9, l. 27.: Though it is not crucial to the code-to-code comparison, it would be important to comment on the size of the domain. The boundaries are relatively close for a turbine operating at max Ct. Could a simulation be performed for 4D in the cross-directions to check the influence? NS and LB might be differently impacted by this.
  We initially compared our results to a domain with $5\,D$ cross-section. On the other hand, in our previous study (Asmuth et al., 2019) we used $10\,D$ (however, including grid refinement). Similarly to other studies on blockage (e.g. Sarlak et al., 2016) we did find a small impact on the wake. Yet, notably different behaviour in the two codes could not be observed. Following the Reviewer's suggestion we now added a brief comment on this: *"The resulting blockage ratio amounts to $\beta = 0.022$ and was found to have negligible impact on the code-to-code comparison" (P.8, l.18f.)*

- P. 9, l. 30.: Reference missing or add a figure.
  We added an exemplary figure of the temporal convergence of the TKE in both codes for the laminar and turbulent inflow case with highest resolution, see Fig. 3.

- P. 10, l. 7.: Though implicitly stated, mentioning the grid size ni,nj,nk and total DOF would clearly show that these are not small computations.
  We agree. The numbers are now mentioned in Section 4 (*See P. 11, l.12*).

- P. 10, l. 15.: Explain this choice. From your previous study it seems that there is a large Ma sensitivity at least of the blade forces. This somewhat contradicts the statement in section 2.2 that Ma is a free variable.
  Thanks for pointing this out. The statement in section 2.2 refers to the common pratice in applied studies of the LBM. After all, body forces of such a high magnitude as applied by the ALM seem to be rather special case. Depending on Mach number *and* smearing width the forces can change notably. We now added the folowing comment to clarify this: *"A preceding study has shown that the forces determined by the ALM can be significantly more sensitive to the Mach number than the bulk flow depending on the smearing width (Asmuth et al., 2019). Under consideration of this issue we chose Ma = 0.1 referring to CFL = 0.058 for the CLBM cases. (See P.11, l. 20ff)*

- P. 11, Figure 3.: Make use of markers instead of lines. As the results lie on top of each other as it seems.
  Yes, large parts of the plotted lines overlap. We added markers and some opacity to increase the visibility of the points.

- P. 11, l. 10.: The only reason for choosing a small epsilon is to make the AL behave more like a lifting line. A small epsilon therefore might get you closer to the wake of a full-rotor simulation, if we believe that a lifting line solution is the truth. Choosing a small epsilon only because the spatial resolution allows it, is not really a good motivation.
  In this point we partially disagree. As mentioned by the Reviewer, a smaller epsilon will get the solution closer to the lifting-line solution. Also, more distinct tip-vortices can be resolved if the overall resolution allows for this. As for typical grid resolutions, we therefore think that epsilon should always be chosen as small as the numerical framework allows for (considering numerical stability etc.). In our opinion, not doing so would refer to an introduction of an avoidable inaccuracy. In order to clarify this, we added the following comment: *"Mind, that unnecessarily large smearing widths would imply larger deviations from the underlying lifting line theory and are therefore undesirable" (See P.13, l.3)*

- P. 11, l. 11ff.: Why not use the forces from either method and then prescribing them. Then all differences would originate from the methods and not differences in the forcing.
  This is an interesting idea. Indeed, we employed this approach in a different study in a different context (that is currently under review) by simply prescribing the velocity in the ALM as opposed to sampling it from the flow field. However, for this comparison we deem it important to compare the two set-ups without any modifications. After all, differences in the wake originating from different forces would also be found in later applications of the model and therefore state an inherent characteristic of each numerical set-up.

- P. 12, Figure 4.: It would be nice to show the convergence history of the statistics for both methods. Are they similar? The wiggles in u in the 12D plane for the NS are they hinting at limited averaging? Are these azimuthally averaged profiles?
  The exemplary temporal convergence of the TKE now shown in Fig. 3 gives some insights into this.
  As for the 'wiggles' these are indeed statistically converged characteristics. It seems that this additional inflection typically occurs upstream of the beginning of a larger wake meandering. An instantaneous

impression thereof can also be seen in Fig. 7 in the contour plots. And, no, these plots are not azimuthally averaged.

- P. 14, Figure 6.: What is happening here?
  A brief explanation was given in the caption of the figure. The Ti at this part simply becomes very low. This again seems to be a consequence of the wake being laminar and the low resolution of the referring case.

- P. 15, l.3.: This is appreciated, however adding a few lines to Figure 7 for this case would reinforce your argument, without adding any more words.
  We appreciate this suggestions. Dashed lines of the corresponding cases with the AllOne CLBM were added in the plot along with some modifications of the paragraph discussing it (*See, P. 16, l. 13ff*).

- P. 15, l.3.: Reformulation could help here to ensure the reader understands what is being compared, as it might be unclear. The "Smagorinsky case" also employs the limiter function however lambda is 10^6 and thus the limiter is not active. For the other cases the Smagorinsky model is switched off and instead the implicit nature of the limiter used for damping. In fact what is being compared is LES with ILES. Stating this would make it very obvious. Also what is the motivation for comparing them in the first place? Neither in the introduction nor here is the motivation for this exercise stated, only in the conclusion is the potential of these methods finally mentioned. Are considerable reductions in computational time expected?
  Mostly based on the corrections suggested by Reviewer 2 we have shortened and restructured large parts of this entire case study. The explicit questions raised here by Reviewer 1 should now also be clarified in the new manuscript. Without going into further details here, we therefore refer to the new Section 6.

- P. 25: Not sure Appendix B is needed. Referring to Geier 2017b in the body (as is done) might be sufficient.
  We agree and removed Appendix B.

2 Reviewer 1

2.1 General Comments

- P. 5, l.6.: ν is the kinematic viscosity
  Changed accordingly.

- P. 5, l.6.: Section 2.2: The CLBM is described in Section 1 in physical quantities. The reader unfamiliar with LBM might wonder why in addition a rescaling / normalization would be necessary as sketched in Section 2.2. The answer to this question is that LBMs are generally always implemented in non-dimensional units, meaning $\Delta x^{LB} = \Delta t^{LB} = 1$. This is also the case here as $c_s^{LB} = 1/\sqrt{3}$, which is the non-dimensional lattice speed of sound in LBM. This section should be re-written, the normalization and it purpose expressed more clearly.
  Thanks for pointing out these inconsistencies. After a general revision of Section 2, based on the comments of both reviewers, we have made sure that all formulations in Section 2.1 are consistently given in dimensional form. Furthermore, we have provided extra motivation and clarifications of the non-dimensionalisation in Section 2.3.

- Section 4.2, Table 1: A proper convergence analysis would not evaluate the error norm versus the next finer resolution (which will invariably lead to superconvergence), but employ a reference computation at least 4x finer than the highest resolved computation in order to obtain accurate order of accuracy estimates. However, attempting a convergence analysis for a test case in transition is a somewhat futile effort, as evidenced by Table 1. The CLBM as well as the QUICK scheme should lead to 2nd order accurate results and not 1st order as shown in Table 1. My suggestion is to remove in particular this table.
  We agree that our initial approach might have been misleading. The table was removed in the new manuscript. Our initial motivation though, was to provide a quantitative measure comparing the two numerical approaches. Instead of comparing the convergence of the two approaches, we therefore now provide a direct comparison of the two solutions at each referring grid resolution by means of the L^2-relative-error norm. See, *Figure 6* as well as the in-text discussion on page 14. The same norm is also employed in the new turbulent inflow case to quantify the differences in the resulting velocity profiles. *See, page 20.*

- Section 5 is investigating the influence of a higher-order limiter on the CLBM. The authors apply this parameter instead of the Smagorinsky model for scheme stabilization and imply that this would be implicit large eddy simulation (ILES). It is not. The idea of ILES is to use a tunable parameter such that inherent scheme dissipation plus tuned dissipation agree with the required subgrid scale dissipation of a particular LES model. The approach obviously requires an exact understanding of dissipation behavior of the numerical scheme in the first place plus an exact understanding of the tunable contribution towards the physical meaningful model limit. Just experimenting with a higher-order limiter only demonstrates the availability of such a tunable parameter, but none of the former. I suggest reducing this section considerably and to eliminate the notion of ILES in most places. This section ultimately only underscores that even in the previous section no fully turbulent wake is developed and even the Smagorinsky model is usually only activated to stabilize the computation. As can be inferred from Fig. 8, no fully turbulent spectrum could be establised. In that sence, not even the Smagorinsky model in combination with CLBM is verified at all by the presented computations.

Thank you for your comment, especially regarding the falsely used term *implicit LES*. Apparently, the term ILES is widely used in the literature in connection with the CLBM despite the lacking understanding of its dissipation behaviour. As there appears to be a lacking documentation of the impact of the limiter, our main motivation was to outline the fact that the choice of λ_m is by no means irrelevant as it largely affects the dissipativity of the scheme. Due to latter we now merely conjecture that it could possibly be used as an ILES approach *if* its behaviour was further understood. Also, following your suggestion we have reduced this section considerably. We now only briefly describe the impact of λ_m in laminar inflow using the contour plot of velocity and Ti. The case study in turbulent inflow in this context was removed entirely. Also, we clarified the motivation for this case study.