Review of *"Observations of wind farm wake recovery at an operating wind farm"* by Krishnamurthy, R., Newsom, R., Kaul, C., Letizia, S., Pekour, M., Hamilton, N., Chand, D., Flynn, D. M., Bodini, N., and Moriarty, P.

The provided manuscript thoroughly analyses the vertical profiles of the vertical momentum flux and vertical wind speed within a wake induced by a large wind farm in the US Great Plains. In their paper, the authors distinguish between several meteorological parameters, including atmospheric stability, boundary layer height, presence of LLJ events and extreme veer and shear occurrences. Further, the authors provide an exemplary extreme case with a very high downward flux in the wake induced by the presence of a gravity wave. The results show a clear dependence of vertical momentum flux and wind speed deficit on the prevailing atmospheric stability regime, as well as on the presence of extreme events, such as LLJs and in one particular case a gravity wave. Further, observations suggest, that the wind farm's effects are present throughout the entire atmospheric boundary layer, even far above the rotor plane. Thus, the manuscript addresses internationally relevant questions of importance for the scientific community within the scope of the journal.

From my point of view, the language used in the presented manuscript is very nice and the writing style is easy to follow. The chosen title is concise and represents the content of the paper quite well. The authors provide a very thorough and informative literature overview and separate their work from previous research. However, the reference list needs to be checked again as some of the references from the text are missing in the bibliography (e.g. Stevens, 2016 and Parson et al. 2019, Rottman and Simpson, 1989, Draxl et al. 2019).

Within the introduction of the paper, the objective statement is formulated very vague. Instead, I would suggest that the analysis of the wake properties is directly included (cf. comment #7).

The paper's general structure, as well as the presentation of the results, are not reader-friendly. I would suggest reorganizing the paper and first presenting the measurements carried out and elaborating on the data post-processing methodology afterwards. Also, the used measurement devices including the used time frames should be presented more concisely. Further, within the results section, objective description of the results and subjective interpretation a not always distinguishable, which can lead to confusion. Further, some of the Sections provided in the manuscript don't add to the main part of the story and may be moved to an appendix. Further, the main story of the paper could be presented more concisely by adding some of the Sections into an appendix (cf. comment #2).

Also, I think adding some further analysis about the impact of the ABL depth and LLJ characteristics on the observed wake properties would greatly benefit this paper. However, as the results are very original (i.e. observations of momentum flux in the wake of a wind farm and their distinction between the different meteorological circumstances) and interesting for the scientific community, I would like to see an improved version of this manuscript published in the future.

Considering this and the major comments presented in the following, I would recommend the manuscript for a major review.

General comments:

1. The structure of the paper makes it hard to grasp many of the underlying principles easily. To gain a thorough understanding of the topic and the results, multiple reads were necessary, with a

lot of jumping in between Sections, to fully understand the whole picture. I think, following the IMRaD structure (Introduction, Methods, Results and Discussion) would benefit the overall presentation of this paper.

1.1. In my opinion, the presentation of the measurement campaign (Section 4) should be positioned earlier (before Section 2). I think – as this part is really well written and can easily be followed - it would benefit the understanding of the paper and prevent some of the doublings occurring within the paper. All in all, a more concise and concentrated introduction of the measurement locations, devices, scan parameters etc. would be very helpful.

1.2. Then in Section 3, the flux estimations would follow, as these are the post-processing methods carried out on the collected data.

2. Some of the presented sections are – although very interesting to read – not contributing to the main storyline of the presented paper and should thus be either cut entirely or moved to the appendix.

   2.1. Section 2 (Mathematical preliminaries), may be moved to the appendix as it is beneficial information, but not strictly necessary to follow and understand the general story of the paper.

   2.2. The same is true for Section 6 (Internal Boundary Layer height). The analysis carried out here does not contribute to the objective statement in the introduction of the paper and thus may only be considered additional information and moved to the appendix. Also, some of the information is doubled in Section 2 and Section 5.4 so it may also be integrated into one of these sections to make the paper more concise.

3. The connection between the momentum flux and wake recovery could be worked out in a little more detail.
4. Often (e.g. L.7, L. 70, L.335, L.343) the authors talk about the momentum flux within a wind farm. However, as this is not really what was measured, I suggest aligning with the rest of the formulations saying “downstream”, “surrounding” or “within the wind farm wake” …
5. Some variables are introduced in a slightly confusing way. For the atmospheric boundary layer, δ is introduced, whereas δ_{IBL} refers to the actual height of the internal boundary layer. Here, the naming of the variables should be consistent to avoid confusion.
6. In section 3.2 a correlation between the different flux estimates is presented. However, very little discussion on the non-negligible scatter between the two estimates is provided in later sections and no explanation on how the observed differences are accounted for in the following analysis is given.
7. In the results section as well as section 3.2, (objective) results and the (subjective) interpretation and discussion of these are very mixed up. I suggest at least introducing a new paragraph when starting the discussion. However, the best case would be to introduce a new section, where a separate discussion of the observations is carried out.
8. In general, abbreviations should be rechecked, as some are either introduced very late or introduced and then not used consistently (e.g. LLJ)
9. Introducing more paragraphs or line breaks would significantly increase the readability of the paper
10. Figures are sometimes labelled (a) and (b),… and sometimes top, left, etc. Here, consistency would be nice. The same is true with the choice of the used lines and markers between all the different profiles.
11. Also, a clear description of how the shown vertical profiles (either via mean or median,…) is missing. It would also be very interesting to see horizontal error bars showing the e.g. standard error of the mean of the profiles to assess the significance of the presented results

12. Mathematical operators should not be written in italic (e.g. sin and cos in L. 156 or log in L. 424) and variables should be in italic (e.g. β in L. 447)
13. The units in the Figure labels are sometimes in round and sometimes in square brackets, please align.
14. Degrees are sometimes represented as °, "deg" or written completely as degrees. Please align.
15. Please check the reference list again. Some of the cited literature is missing. Exemplary are . Stevens, 2016 and Parson et al. 2019, Rottman and Simpson, 1989, Draxl et al. 2019…

Specific comments:

1. L. 13: Why are you not mentioning your observations regarding the atmospheric stability here? I think the results are really interesting and worth mentioning in the abstract.
2. L. 20: The provided abstract is more of a teaser of what is to come in the paper. It only provides very limited insight into quantitative results and no qualitative statements.
3. L. 31: Maybe – as you are also talking about offshore wind farms – it makes sense to consider, that during stable stratification offshore wind farms in the German Bight induce wakes are observed from in-situ measurements more than 50km downstream of the wind farm (Platis et al., 2018) and may even cause a detectable decrease in power production for downstream wind farms (Schneemann et al., 2020)
4. L. 35: At first, I was a little confused by the term "rotor layer", maybe a half-sentence explaining what you mean here would be nice
5. L.36f: I do not think, the introduction of the variables u' is necessary here. However, if you choose to do that, please include a quick explanation of the indices and dashes and what they indicate.
6. L. 60: What is meant here by the wake "grows"? Does it grow in space or does the wind speed deficit increase? A little more explanation would be nice.
7. Very vague description of the paper's objective. Instead, I would suggest directly mentioning wake recovery, e.g.: "*In this paper, we investigate the wake recovery of a wind farm, by investigating the momentum balance […]*" (L.74).
8. L. 78-81: I would rather place this part in the conclusion part of the paper.
9. L. 86: As stated before, I would move this part into an appendix, to make the paper more concise. No information necessary for understanding your paper gets lost here.
10. L. 95: Instead of ": The turbulent entrainment of mean kinetic energy", I would rather directly talk about the momentum flux here, or instead provide the prognostic equation for the kinetic energy to highlight the connection between the two.
11. L. 118: I don't fully understand, what is meant by: "$\delta_{IBL}(0)$ is the internal boundary layer height of the wind turbine rotor top". Is it that $\delta_{IBL}(0)$ is equal to the upper tip height? Maybe you could explain this in the text or with a quick equation.
12. L. 144: You could directly introduce the term "Eddy-covariance method" here. This would save you from having to reintroduce how you obtain fluxes from anemometers in L. 180
13. L:170: Here, the measurement technique is introduced. However, if all the different scans along with their locations, devices and time frames would be introduced in one central section (move Section 4 forward) I think this would save a) a lot of space and b) increase the understanding of your measurements.
14. L. 184: Amount of digits used between (a) and (b) are not the same
15. L.186: L as the Monin-Obukhov length has not been introduced up to this point.
16. L.187: Here * is used as a multiplicator, in the figures it is the "convolve" sign. Here, consistency should be achieved.

17. L. 195: Instead of saying “amount of stratification”, I would suggest using “strength” or “degree” of stratification.
18. L.203: Here, a cross-correlation between the two different flux estimations could be used to eliminate the difference in measurements due to the spatial displacement (if the devices are oriented in wind directions)
19. L. 209: The discussion regarding the difference between the two flux estimations is rather short. Further, the results are not picked up again in the results or conclusion Sections and thus feel a little lost in the paper.
20. L. 210: In my opinion, this chapter should be moved to the front, as it would support the flux estimation section.
21. L. 211-214: This part is more of an introduction and thus may be placed there.
22. L. 236: Here a reference to the corresponding Equation would be nice.
23. L. 249: The description of the measurement site is very well written and good to follow. However, I would very much appreciate a more in-depth description of the orientation of the lidars and sonic anemometers, to further understand the discrepancies in flux estimations highlighted in Section 3.2. Also, I would suggest to provide a table containing all the different measurement devices, the quantities they measure and the time frame in which they were available. You could also already introduce the fact that only southerly wind sectors were used. All this would then save a lot of space in the following sections and especially the Figure captions.
24. L. 250: The map showing all the different measurement stations is very well done. However, for conciseness, I would suggest zooming in to the relevant part of the area, containing the measurement locations that were actually used. Further, I am missing the location of the ARM SGP central facility to better follow the flux estimation procedure and the Section with the gravity wave measurements.
25. L. 261: A quick table presenting the different stability regimes and borders of *L* would be helpful
26. L. 262: In my opinion, this chapter could be merged with Section 2, as both deal with the estimation of the (internal) boundary layer height. This would make the paper more concise and the authors would avoid providing similar information at two different stages of the paper.
27. L. 279: How is the statement regarding the momentum flux difference threshold of 1% backed up? Is there literature available?
28. L. 289: In Figure 4, the legend is missing. Also in the title, θ is used as wind direction instead of Φ. Also, as this chapter primarily deals with the IBL height detection, maybe you could provide vertical lines showing the “rotor layer”, as well as your mean IBL height.
29. L.308-313: This sounds more like an introduction to me. Or it would also fit in the section describing the methodology to estimate fluxes. However, here I think it distracts a lot from the results.
30. L. 316: The authors refer to a difference in the results due to the diurnal cycle here. I think it would be really interesting if these results were shown in the paper, as they are not present in the referenced Figure.
31. L. 319-320: The authors use expect here a lot. Maybe the sentence could be rephrased to avoid this subsequent use of the word. Further, an explanation on why this is expected is missing.
32. L. 347: In Figure 5 it would be very nice, to visualize the rotor layer (which is stated in the caption, but not present in the Figure). Also, to really emphasize the difference between the situations, which are worked out quite well by the authors, I would suggest that the scaling of the x-axes is kept constant throughout all three subfigures.

33. L. 354: LLJ could be introduced earlier and is not used consistently hereafter.
34. L. 358: I think it would be very interesting, to compare different LLJ definitions, as they are leading to very large differences in analysis. Also, in a recent paper, [Hallgren et al. (2023)](#) provide a new concept of LLJ detection using the wind speed shear instead of a fall-off, which seems to be less sensitive to the used measurement device and available height window. I think adding this new definition to your work could make your results more interesting.
35. L.361: Is this stability distribution only for LLJ events or does it include also non-LLJ events? This description is not very clear, also not in the caption of Figure 6.
36. L.368: This result is very interesting and should be shown in a Figure somewhere. From Figure 6, this statement cannot really be verified
37. L.375: Is there any reasoning behind the separation of LLJ events into these height intervals? Some insight into your analysis would be very nice.
38. L. 392: Figure 6 is missing a) and b), there is no legend given. Also, I would like to ask for an explanation of the error bars. Further, I think it would be very interesting – also considering the claims from L. 368 – if instead of the bottom picture, two plots with Z_{LLJ} on the y-axis and U_{LLJ} and U_{hub} on the x-axes respectively were shown. The top picture looks really nice and is very informative. Only a statement about whether all events or only LLJ events are considered in the stability distribution would be nice.
39. L. 400: I think it is more common to use up- and downstream instead of up- and downwind. Also, the authors state, that the LLJ height is modulated when passing through the farm. Here it would be really interesting to see how many LLJ events are recorded in the different core height intervals and maybe also provide another plot showing the LLJ height up- vs downstream of the wind farm.
40. L. 408: I do not think, that Figure 8 provides any significant benefit to the story of the paper. Also, as it does not show the measurements of the analysis by the authors. Instead, I would be really keen to see a quantitative analysis of the indicated LLJ shift upwards during the passing of the farm. Maybe showing the difference in Core height also as distributed over the different stability regimes would be interesting here. However, a similar Figure showing more schematic view of the measurement devices and their location with respect to the wind farm would be very helpful in Section 4.
41. L. 438: Is there a specific reason that the median is used throughout the paper instead of the mean? I think it is valid, if there are large outliers present, but a quick hint on why that is done would be very nice.
42. L. 441-446: Here, you are introducing a new research question, which is usually done in the objective statement within the introduction. Also, I think, this question should have been considered in previous sections, as the presented paper deals with wake recovery throughout all result sections. However, as the authors do not plan to answer this it would be in my opinion more fitting in the Conclusion/Outlook part of the paper. Also, there is a source missing for your presented claim.
43. L. 449: Here, the authors partition their results to events with high and low shear. However, a clear definition of what that means is missing. I would suggest including a description of the distribution of both, α and β, to be able to categorize the division into different veer and shear classes.
44. L. 465-467: I suggest moving this to the introduction or the mathematical preliminaries Section
45. L. 483-488: This sounds more like a conclusion in my opinion.
46. L. 488: Section 5.4, deals not really with the impact of ABL height on wake recovery, but instead concentrates on the extent of the wake within the boundary layer. However, I think that what the title promises is actually a very interesting and important part of what the

entire paper promises. Here, I think it would be very interesting, to different momentum flux profiles for different boundary layer heights and perform the analysis based on that. The way it stands now, I think this section does not provide useful information for the story of the paper.

47. L. 491: This figure is the same as Figure 4 no? What is the added value of providing this plot? As per my previous comment, I really like the idea of this specific analysis and would like to see different momentum flux profiles for different boundary layer heights here. Or, another interesting aspect would be a scatter plot providing the maximum vertical momentum flux vs. the boundary layer height.
48. L. 492: What is the benefit of using the ceilometer measurements over the boundary layer height estimates from the lidar profiles measured at A2 and H? Interesting analysis would also be to have a look at the difference between boundary layer height estimates from the ceilometer and lidars.
49. L. 495: The title suggests an analysis based off of multiple detected gravity waves, when instead only a single event is used for the analysis, thus I would suggest altering the title of this section. This could also be part of the LLJ section, as the following analysis is also based heavily on the LLJ characteristics observed during the event.
50. L. 496-507: This part reads like an Introduction and thus I would suggest moving it there.
51. L. 508-516: This is a nice description of the results, but I would ask to align the date and time representations format with the x-label of Figure 12 and the rest of the paper. Maybe, you could also back up your claim about the observed oscillations being a gravity wave by comparing the observed frequency to the theoretically expected frequency, using e.g. the Brunt-Väisälä frequency.
52. L.526-528: I would consider this comment rather speculative. Can the authors somehow provide a backup for this hypothesis?
53. L. 541: Finally, I think a table analysing the effects of the different effects in comparison with one another would be very helpful to categorise your results (e.g. Comparing average momentum flux deficit at hub height or something similar). This would also add to the discussion in this chapter about what other factors might come into play during this "extreme event" altering the momentum flux and vertical wind profiles.
54. L. 543: The title of Section 6 is very vague and does not represent the content
55. L. 555: Is there any specific reason why only LLJ situations are chosen for this analysis? If yes, I would kindly ask you to provide the reasons to better understand the conclusions followed from that analysis.
56. L. 561: To what standards are the results considered satisfactory?
57. L. 563: In my opinion, this chapter does not add to the main story of the paper. It matches however quite nicely with Section 2 and provides additional material to your paper (esp. Section 5.4). Thus, I would suggest moving it to the appendix. Further, I think it would be very interesting to dig more into the cases when the model is over- and underestimating and to see whether certain patterns can be observed here.
58. L.574: "can" seems a little too strong, as this is not always the case. Instead, I would suggest a "may" here.
59. L. 577-578: This is a very interesting finding. However, it is not really shown in the paper beforehand. As per my previous Comment, I would really like to see this analysis being carried out more thoroughly.
60. L. 583: The first conclusion is not really novel and has been observed before.
61. L. 588: For this conclusion a categorization on whether this is rather long or short is missing.

62. L. 592: This conclusion is a little bit too generalized. What you show in your paper is purely based on LLJ situations and also there is not really the benchmark defined on what "well" is referring to.
63. L. 595: Maybe, you could explain why this point is important a little. Where are the benefits?
64. L. 599: Here, you could specify the connection to your paper a little better.

Technical Corrections:

65. L. 53: Per my understanding it should read "mean winds *within* the ABL"
66. L. 61: I think you are missing a "speed" after wind here.
67. L. 64: I could not find Stevens (2016) in the reference list, please add that reference (also L. 272 and other such as Parson et al., 2019)
68. L. 64: After the citation, some fill word is needed to complete the sentence
69. L. 109 & 110: To make the dimensions work, it should be $<u_{z\_h}>$ or not? As c_{ft} is dimensionless, you can't multiply speed by height, or else the dimensions would be different as for the first terms with u^2 in them.
70. L. 112: Usually, the von Kármán (with accent over the a's) is written as κ not k (also in L. 197)
71. L. 128: The comma between "lidars" and "and" is not necessary
72. L. 138: You are missing the parenthesis around "2020" for the citation
73. L. 150: The "R" is missing on the right side of the equation (u(R), v(R), w(R))
74. L. 155: Using square brackets here is quite confusing, as one line before, they are used to indicate that the variables are arranged as a vector. Maybe you could just use double round brackets here, to avoid this confusion
75. L. 158: <> as the temporal average has already been introduced before
76. L. 165: Φ is missing in Eq 5. Maybe small and capital letters are mixed up here.
77. L. 303: I think you are referring to Figure 3b here.
78. L. 313: Here you are talking about a wind plant, whereas in the rest of the paper you refer to it as wind farm
79. L. 314: I would suggest ending the sentence before "Therefore" and starting a new one to improve readability.
80. L. 329: I think it should read "convectional" not "conventional" updraft
81. L. 400: In the legend, it states that the LLJ core is situated between 100 m and 250 m, I think it is 127 m, no? (cf. L. 374)
82. L. 424: I think there is one log too much everywhere in this equation. It should read log(U(z))=log(U(H)) + α log(z/H). Also as log is an operator it should not be written in italic.
83. L. 575: In my understanding, you are referring to Figure 7 or 8, not 14, correct?

Literature

Hallgren, C., Aird, J. A., Ivanell, S., Körnich, H., Barthelmie, R. J., Pryor, S. C., and Sahlée, E.: Brief communication: On the definition of the low-level jet, Wind Energ. Sci., 8, 1651–1658, https://doi.org/10.5194/wes-8-1651-2023, 2023.

Platis, A., Siedersleben, S. K., Bange, J., Lampert, A., Bärfuss, K., Hankers, R., Cañadillas, B., Foreman, R., Schulz-Stellenfleth, J., Djath, B., Neumann, T., & Emeis, S. (2018). First in situ evidence of wakes in the far field behind offshore wind farms. *ScIentIfIc REPORtS |*, *8*, 2163. https://doi.org/10.1038/s41598-018-20389-y

Schneemann, J., Rott, A., Dörenkämper, M., Steinfeld, G., and Kühn, M.: Cluster wakes impact on a far-distant offshore wind farm's power, Wind Energ. Sci., 5, 29–49, https://doi.org/10.5194/wes-5-29-2020, 2020.