General Comments:

The manuscript entitled "How do convective cold pools influence the boundary-layer atmosphere near two wind turbines in northern Germany?" by Jeffrey D. Thayer and co-authors studies the statistical footprint of convectively induced cold-air outflows on thermodynamic and dynamical properties of the atmospheric boundary layer at a research wind park in northern Germany. By applying a custom detection algorithm and analyzing meteorological in-situ and remote-sensing observations, the authors characterize the temperature, humidity, and wind signals of 120 cold-pool passages within the rotor layer of wind turbines. Their findings suggest that cold pools can temporarily increase wind energy output by up to 50 %, whereas the associated wind fluctuations vary asymmetrically across the rotor layer and the increased near-surface static stability could impact the turbulence in the wakes of a wind turbine.

Overall, the manuscript is well-written, clearly structured, and guides the reader well through the different parts of the study. In the introduction, the current state of knowledge on this relevant and timely topic as well as the new contribution of the study are clearly outlined. The used experimental setup and analysis methods are described in a mostly understandable and transparent way. Although the results part covers several aspects of this research topic, it is presented in a balanced way and does not become lengthy. Moreover, the results are always discussed in the context of the current literature. The only criticism I could make is that I feel that the study could have gone a step further and be clearer on possible implications of the results for wind energy applications, which would support the significance of this work even further. Nevertheless, my overall assessment of this study is very positive and after addressing the minor comments listed below, I am happy to recommend the manuscript for publication in WES.

Specific Comments:

- Title: I suggest to use the more common term "atmospheric boundary layer" instead of "boundary-layer atmosphere".
  We think this is a good suggestion and have changed the title accordingly.

- Line 13: Please introduce Theta_v.
  Thank you for catching this. This has been changed in the revised manuscript.

- L14: I would go with the term "static stability".
  This is a good suggestion and has been changed accordingly in the revised manuscript.

- L19: Please clarify that the -2 K cooling refers to the near surface conditions.
  This is a good suggestion and has been changed accordingly in the revised manuscript.

- L46: "Limited work" by itself does not define a scientific gap in literature. What exactly do the existing studies miss?
  This is a valid critique, though we do see this sort of language used often in other studies. Nevertheless, we have clarified this sentence in the revised manuscript to highlight that the scientific gap related to wind ramps lies in a lack of detailed examination of convectively-induced wind ramps: '*Beyond this 'convective systems' wind ramp category broadly outlined in Steiner et al. (2017), we are not aware of additional work concerning wind power fluctuations induced by convective wind ramps over Europe*.'

- L49: "Rarely": See previous comment.
  Yes, we should have provided more clarity when using this word. To the best of our knowledge, Canepa et al. (2020) may be one of the closest studies to our work, in terms of analyzing thunderstorm outflows with meteorological mast and lidar data in a coastal location. They also highlight changes associated with wind-energy-relevant variables (e.g., wind speed, turbulence intensity), and verify the

presence of thunderstorms (using satellite and lightning data) as we do. However, they focus only on 10 high-end thunderstorm outflow events (i.e., downbursts), while there are less intense thunderstorm outflow types that still have important impacts for wind turbine power output and structural loads. They detect cases only through wind speed time series and do not place much emphasis on whether the thunderstorms produce surface rainfall (they include both wet and dry downbursts), while we focus on the surface temperature changes to identify thunderstorm cold pools and prescribe that measurable rainfall must occur at WiValdi. We also have a distinct difference in terms of focusing on wind energy and providing turbine power observations, while they focused generally on how the thunderstorm outflows could impact engineering structures.

We have amended this sentence in the revised manuscript as follows:

'*For example, Lombardo et al. (2014), Gunter and Schroeder (2015), and Canepa et al. (2020) each analyzed strong thunderstorm outflow events (i.e., downbursts) using wind energy-relevant variables with a limited sample size (10 or fewer), but did not investigate a larger sample of cases or the broader range of thunderstorm outflow intensities that can occur.*'

- L103: Name the measurement heights of the mast since these are visualized e.g. in Fig. 7.

This is a good suggestion. We have now included the sensor heights in the revised text as follows:

"*One 150-m meteorological mast located 2 D in front of the westernmost turbine provides inflow conditions throughout the turbine rotor layer. On this 'inflow mast', ultrasonic anemometers (hereafter, sonic) lie at heights of 34 m, 62 m, 85 m, 120 m, and 149 m, while temperature and relative humidity sensors are located at 10 m, 34 m, 62 m, 85 m, 120 m, and 143 m. Barometers are only located at 10 m and 85 m, so pressure values at other heights are obtained via the hypsometric equation starting at 10 m.*"

- L104: How high are the three meteorological masts?

As we do not use data from the meteorological mast array in between the turbines, we decided to remove this part of the sentence for succinctness and to reduce any confusion around what mast data we are using.

- Fig. 1a: I suggest adding the locations of some major cities and a distance scale for reference.

This is a helpful suggestion. We have added the nearby major cities of Hamburg, Bremen, and Kiel. The inclusion of Hamburg is especially useful now for understanding the relative proximity of the Kirsch et al. (2021) observations to our wind park. We decided to not include a distance scale since the latitude and longitude labels are already provided, and "distance" is kilometers would somewhat change throughout the map given the change in latitude.

- Fig. 1b: I would also mention in the text that the position of the MWR and lidar has changed over time.

Thank you for catching this. We have now mentioned this in the revised manuscript as follows:

"*We note that the lidar and MWR were slightly moved within the wind park every couple of years (Fig. 1b, blue squares), but were moved on the same days, kept adjacent to each other, and remained east of the easternmost turbine by approximately the same distance.*"

- L129: For clarity, I suggest to mention that the microwave radiometer is a passive instrument.

This is a good suggestion. We have incorporated this in the revised manuscript as follows:

"*Adjacent to the lidar, there is a passive HATPRO G5 microwave radiometer (MWR; Rose et al., 2005) manufactured by Radiometer Physics GmbH that was installed on 26 November 2020 and which provides vertical profiles of temperature and humidity. With an associated ground weather station for obtaining surface environmental conditions, an MWR passively retrieves vertical profiles through usage of multiple brightness temperature measurements within the oxygen and water vapor absorption bands.*"

- L151: Please introduce Theta_v.

Thank you for catching this. This has been changed in the revised manuscript.

- L161: What does "positive daily wind anomaly" mean? Does this refer to the maximum wind speed of the day, a positive anomaly relative to the daily mean wind speed, or something else?

This phrasing could be misleading, we agree. This phrase means a positive wind speed anomaly relative to the daily mean wind speed. In other words, this means that the wind speed around the time period of a cold pool gust front is larger than the average for that calendar day. We applied this criterion so that we detect gust fronts that stand out from the background wind conditions on a given day. We have re-phrased this in the revised manuscript as follows:

"*Continuous-θ_v-decrease time periods include at least one time step of measurable rainfall exceeding 1 mm hr^{-1} and a positive wind speed anomaly relative to the daily mean wind speed within +/- 10 minutes of T_0. This rainfall threshold is used to remove instances of very weak convection or possible rainfall measurement error, while the wind speed anomaly is inspired by Kruse et al. (2022) and verifies a more significant cold pool gust front strength compared with the background flow conditions (given our interest in quantifying cold pool impacts on wind turbines).*"

- L172: Both Kruse et al. (2022) and Kirsch et al. (2021) use a 20-minute period to detect temperature drops related to cold pool passages.

This is a good catch. We were somewhat confused by the phrasing in Kirsch et al. (2021). This has been changed in the revised manuscript as follows:

"*The 30-minute time constraint is similar to that of past work (20 minutes for Kirsch et al., 2021 and Kruse et al., 2022).*"

- L175: Can you specify "at least somewhat"?

We could have explained this better, and we agree that we should describe this more quantitatively for reproducibility purposes. We have re-worded this sentence as follows:

"*Finally, we prescribe that θ_v must increase after reaching its minimum value and that this increase occurs within 60 minutes of T_0.*"

- Section 3: As this section is rather long, I suggest to introduce sub-sections for a clearer structure.

We agree that Section 3 is quite long relative to the other sections. Therefore, we have now separated Section 3 into two subsections constituting 'near-surface' and 'hub-height' environmental changes.

- L209: Please specify the exact time period instead of only the years.

This is a good suggestion and has been changed accordingly in the revised manuscript.

- Table 1: Why does the table only show the pre-event data? I think that listing the actual cold pool signals would be more instructive for the reader.

We did not originally include the pre-event data since the median time series of the relevant near-surface variables were being shown in Figure 3. However, we agree that comparisons with other work is easier with the monthly climatological values in Table 1. Therefore, we have now added the relative changes of virtual potential temperature, maximum rainrate, and wind speed as additional columns in Table 1. We keep the original columns since they provide necessary context for the 3 new columns and they allow for comparisons with relationships described in Kirsch et al. (2021) and (2024).

- Table 1: Does the measurement accuracy of the instrument allow to show the data with a two-digit accuracy?

This is a good point that we did not originally consider. The ground weather station sensors are quoted as having an accuracy of 0.2-0.3 K or m/s. Therefore, we feel that rounding to 1 decimal place is reasonable.

- L218: What is the median temperature decrease over the cases? Here, showing the corresponding data in Table 1 would help (see earlier comment).

The median decrease in θ_v at 2 meters over all detected cases from the MWR ground weather station is 2.7 K. As the cold pool event signals are now also included in Table 1, this value should be

more obvious and understandable for the reader. This is now mentioned throughout the manuscript, including in reference to this comment as follows:
"*With T_0-30 minutes being a proxy for the pre-event environment, we find a median relative decrease in θ_v of 2.7 K (red) during the cold pool passages that occurs in the span of ~20 minutes, starting at T_0-5 minutes and reaching a minimum around T_0+15 to T_0+20 minutes*."

- Figure 2: The differently colored dots and outlines are very hard to see in the plots.

  This is a valid critique. We have now removed the polygon centroid dots (as they are not consequential for our analyses), have edited the Figure 2 caption to remove the magenta dot description since that was a holdover from a previous version of the plots, and have increased the linewidth of the outline for polygons that overlap with WiValdi to enhance the contrast with those polygons that do not.

- L267-269: Are these measurements taken at the inflow mast? This is relevant for the interpretation of the results and could also be clarified elsewhere.

  This is a valid critique. We only used in-situ mast measurements from the inflow mast. We did not use data from the 3 meteorological masts in between the turbines, but we show all masts in Figure 1 to show an accurate representation of the measurement structures present within the WiValdi wind park. Nevertheless, we have indicated in additional places throughout Sections 3-5 that the in-situ mast observations within the turbine rotor layer being used are from the inflow mast.

- Fig. 4: I assume that the central lines show the respective median but this is not indicated in the figure caption.

  Thank for you catching this omission. We have amended the Figure 4 caption to include this description.

- L290-291: The median temperature evolution at 85 m (Fig. 4d) shows a slightly weaker signal than near the surface (Fig. 3). It might be worth mentioning that this is consistent with previous studies (e.g. Kirsch et al., 2021).

  This is a great suggestion! We have incorporated this content into the revised manuscript as follows:

"*This decrease in median mast θ_v at 85 m (Fig. 4d) shows a slightly weaker signal than near the surface (Fig. 3), which is consistent with previous studies (e.g., Kirsch et al., 2021)*."

- L294-305: This paragraph feels a bit disconnected from the rest of the section. I am not entirely sure that the purpose of the case study is, apart from demonstrating that the composite properties of a cold pool also materialize in a single case. If the authors decide to keep the case study, I suggest to move it to an earlier location in the section, maybe in connection with Fig. 2.

  This is a good suggestion, especially given that we are showing a case study for the convection snapshot shown in Figure 2c. We have moved the case study to now be Figure 3. This then allows the reader to understand how different variables fluctuate with time during cold pool passages before we show composite statistics in Figures 4 and 5.

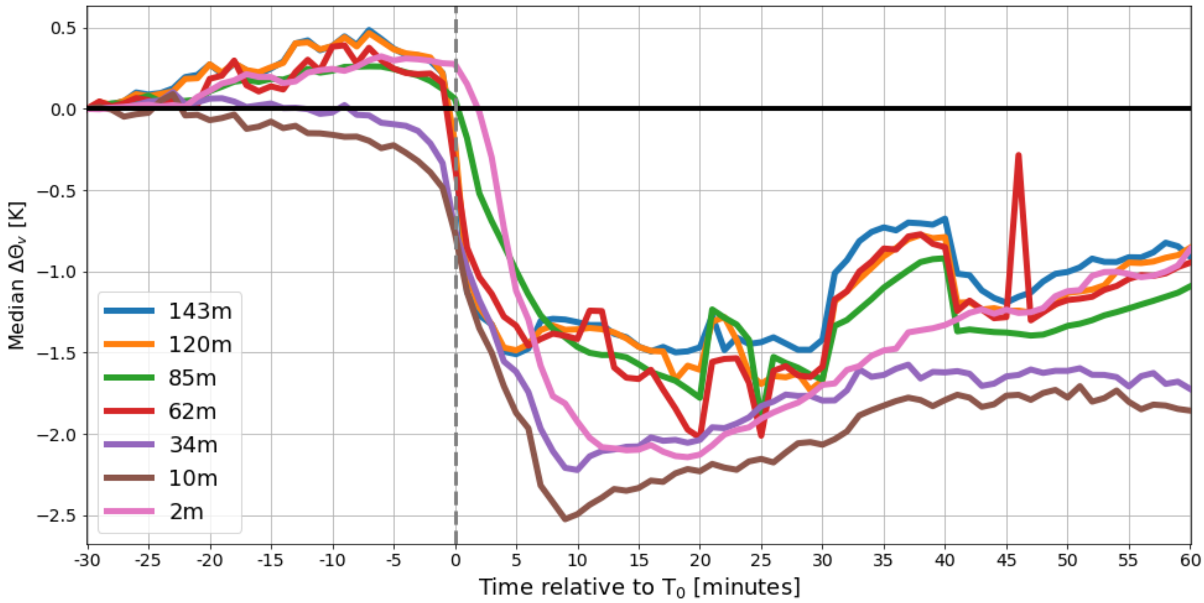
- L312: I would say that the wind speed perturbation reaches up to 800 m rather than 650 m.

  Yes, you are correct. We were trying to be too conservative with our description. The median relative wind speed increase has a zero-crossing point close to 800m, and the median relative wind direction change first crosses the zero point at roughly 700m. Therefore, we have edited the manuscript to reflect an estimated cold pool depth from these 2 metrics of 700-800m.

- L336: I am not convinced by this argument. I am inclined to think that the "nose" in the profile at hub height points to an impact of the rotor itself, but I can only guess what process causes this profile. This would be very interesting to know but probably involves some speculation.

  Yes, we agree that this statement is somewhat speculative. However, as the measurements in Figure 7c come from the inflow measurement mast, which is 2D upstream (to the west) of the western turbine (and given that rotor layer winds associated with these cold pool events predominantly come from a ~westerly direction), these measurements emanate from the boundary-layer flow that has not yet impacted or interacted with the WiValdi turbines. If you are referring more to potential blockage effects upstream of the western turbine that could cause such a 'nose' feature in θ_v, we believe that the inflow mast is sufficiently upstream enough that blockage effects are quite negligible and would not cause this 0.5-1 K difference between the hub-height and the rotor bottom/top heights. As such, we do not think that the turbine rotor would be the cause of this 'nose' in θ_v.

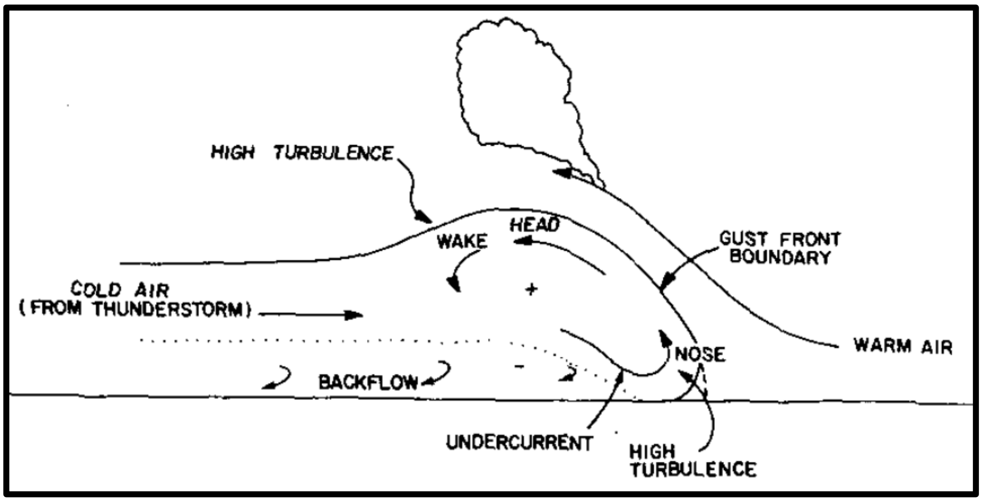
We think it is more likely that given the nose in wind speeds shown in Figure 7a, this part of the cold pool gust front protrudes more into the warmer air out ahead of the propagating cold pool. And so, with the expected enhancement of mixing in the turbulent nose of the gust front, we believe that the hub-height air could be mixing more with the warmer air to produce less of a relative decrease in θ_v than above and below hub-height. The median θ_v time series for all cold pool cases (below) shows all Inflow Mast temperature sensor heights (10, 34, 62, 85, 120, 143 m) with the 2-m Ground Weather Station θ_v, and confirms that the hub-height θ_v (green line) from T_0 to T_0+5 mins does not decrease as quickly as the temperatures at other heights within the rotor layer (i.e., 34-150 m). Before and after the gust front passage, the 85-m θ_v more closely lies in the middle of all heights. Therefore, we must conclude that something different occurs for the thermal vertical profile due to the gust front.



The schematic given below from Goff (1976) provides a helpful visualization of the gust front features we are trying to describe above. Do you perhaps have additional ideas about what could cause this 'nose' feature in temperature?

In any case, these sentences have been rewritten in the manuscript as follows to make it clearer that this finding is speculative:

"*The 'nose' in θ_v (Fig. 7c), with a lesser relative change at hub height compared to above and below, corresponds to the nose-like shape found in the wind speeds. Kirsch et al. (2021) find a similar 'nose' in θ_e at 110 m using mast observations at specific heights near Hamburg, however, this thermodynamic feature is not observed by Kruse et al. (2022) for cold pools over the Netherlands. We speculate that a θ_v 'nose' could perhaps be a manifestation of increased mixing in the gust front nose with the warmer background air ahead of the advancing cold pool, but we can not definitively identify the cause of this thermodynamic 'nose' feature, and so we leave this determination to future work.*"



Goff (1976)

- L344-345: Could this be caused by different adjustment times of the temperature sensors between surface and mast (if the sensors are different)?

The Met Mast temperature and humidity sensors (Hygro-Thermogeber-Compact 1.1005.54.441 sensor manufactured by Thies Clima) have a quoted response time of <20s, and the ground weather station attached to the MWR (Lufft WS600-UMB) is quoted as having a response time of <18s for 95% of the time. Therefore, we do not think the θ_v difference between the ground weather station and mast sensors is due to the adjustment time periods of the sensors.

The median θ_v time series (above) shows the 2-m ground weather station decrease starting slightly after that of the mast heights. This is most likely due to the ground weather station being located approximately 1-km east of the Inflow Mast. Since the cold pool would predominantly impact the mast before the ground weather station, averaging from T_0 to T_0+5 mins yields a smaller relative decrease in θ_v at the ground weather station as shown in Figure 7c.

We also must mention that there is warming which precedes the gust front passage (as the background flow ahead of the cold pool is lifted above the gust front nose), with this interaction complicating our "relative change" calculation. Nevertheless, it appears from the plot above that the near-surface temperature just decreases at a slower rate than the heights above, and that the near-surface temperature does eventually reach a lower minimum temperature than the mast heights within the rotor layer as would be expected. Therefore, we do not think that Figure 7c shows any feature out of the ordinary. It just comes down to the separation distance between the sensors.

- L400: Since section 6 mostly summarizes the methods and findings of the study, I suggest to call it "Summary & Conclusions".

This is a good suggestion and has been changed accordingly in the revised manuscript.

- 470-472: I find the closing statement of the study rather weak. I would hope for more concrete implications for the wind energy applications to increase the significance of this timely and relevant study. Moreover, the authors could clarify what is exactly the benefit of an larger experimental setup compared to the current one.

This is a very valid critique. We have added additional content in the last few paragraphs outlining more implications of this work, along with future suggestions for observational networks and analysis.

<u>Technical Corrections</u>:

- L102: Add space in "4.3 D".

This has been changed accordingly in the revised manuscript.

- L190: What does "WN" mean?

WN is the name of the DWD radar reflectivity dataset that we used. It is not an acronym, but rather is simply a name given to this particular radar reflectivity composite dataset by DWD that encompasses the time period of our study.

- L267: Add space in "subrange of".

Thank you for catching this. This has been changed in the revised manuscript.

- Figs. 4, 5, 7, 8, 9: I suggest moving the labels a) to d) to the top left corner of the subplots for consistency.

This is a valid critique. We have decided to place the figure subplot labels at the bottom right of each panel for consistency.

- L373: Add space in "cut-out hub-height".

Thank you for catching this. This has been changed in the revised manuscript.