Reviewer 2.

Authors greatly appreciate the general comments concerning the EGUsphere review critera and all the remarks received from the Reviewer 2, which are very pertinent and stimulating for improving the paper. We will make our best to take all the comments received into consideration, providing adequate and precise answers to all of them. All our answers and comments are reported in red colour and the manuscript will be modified according to them.

Overall/general comments

1. I think the damage survey aspect of the paper is very interesting, but it would be good to have some further details on this, including if there are any notable uncertainties. For example, is it difficult to relate hail damage to near-surface wind velocity?
   In response to the question about the damage survey aspect and its uncertainties, we acknowledge that there are indeed uncertainties in our study. However, at this stage, we are unable to delve deeper into this topic due to a related paper currently under revision by our colleagues. This paper, "Thunderstorm – induced damage to the built environment: A field measurement and post-event survey" by Calotescu et al., submitted to the Journal of Wind Engineering and Industrial Aerodynamics (2023), is expected to address these concerns comprehensively. We have intentionally avoided detailing this aspect in our paper to prevent any conflicting interests with our colleagues' work. However, this work will be properly cited in our paper to allow the reader to easily find more details on this aspect and references to the relevant literature.

2. I have a general concern whether this analytical model of a translating downdraft can represent complex mesoscale circulations that can induce severe winds, such as a rear inflow jet related to the bow echo. It is good to see that the hail damage estimate agrees with the general pattern suggested by the model, but I think it would be good to discuss this further in the manuscript, by noting it as a limitation and/or linking to other studies that have investigated the wind field patterns of bow echoes (and if they look like translating downbursts or not).
   In the revised version of our manuscript, we will more explicitly state that our analytical model is designed to represent the wind field of a downburst at the apex of a bow echo in its mature stage. This model does not encompass the broader, complex mesoscale circulations commonly associated with high winds in bow echoes. We reference key works, notably Fujita (1978) and Weisman (2001), to support our focus. Fujita's study, particularly through its illustrations (referenced in our Figure 4b), shows the formation of translating downbursts at the apex of a bow echo. Similarly, Weisman's 2001 paper (see Figure 4 of his paper) clearly depicts the generation of translating downbursts by the passage of bow echo. These references will be highlighted to acknowledge the scope and limitations of our model in the context of understanding downburst generation in a bow echo.

3. I think this paper lacks a little in discussing the results within the broader literature. How does the model and results fit with previous studies that have done downburstmodelling? Similarly, have previous studies used hail trajectories to evaluate a downburst wind field?
   Regarding the first question, our paper presents an innovative analytical model that distinguishes itself from existing downburst models, as extensively discussed in Xhelaj et al. (2020). Unlike previous studies, our model explicitly differentiates between the translational movement of the thunderstorm cell and the boundary layer wind. This distinction is crucial, as evidenced by studies like Hjelmfelt (1988), which demonstrated

the variability in downburst behavior relative to the ambient ABL flow. Our model assumes the combined wind velocity at a given point as the vector sum of the radial jet velocity from a stationary downburst, the storm cell's translational velocity, and the ABL wind velocity. This approach allows for a more nuanced and accurate representation of downburst phenomena, particularly in their interaction with the surrounding environmental conditions. The behavior of our analytical model and its performance in reconstruction of downburst wind field generated by single convective cells or squall lines is extensively analyzed in two papers Xhelaj et al, (2020) and Xhelaj and Burlando (2022).

Regarding the second question about the use of hail trajectories to evaluate downburst wind fields, to the best of our knowledge, there are currently no studies in the literature that specifically address this approach.

Specific comments

1. Title: The title is a little unclear without having read the article. "Phase space" couldrefer to several things. It would be good to make it clear in the title that this is a study exploring an analytical model representation of a downburst.
   Thank you for your comment regarding the title of our paper, which was also arisen by Reviewer #1. After careful consideration, we have decided to revise the title to reflect more the core focus of our work, as follows:

   "Application of the Teaching Learning Optimization Algorithm to an Analytical Model of Thunderstorm Outflows to analyze the variability of the downburst kinematic and geometric parameters"

   We believe this revised title emphasizes the application of the Teaching Learning Optimization Algorithm within our analytical framework, underscoring its role in downburst outflow modeling. Thanks again for the suggestion!

2. L8 and elsewhere: Pluralisation issues - should be "measurement challenges". Similarly on line 38, should be "Since downburst events have high frequencies of occurrence…".
   Thank you for pointing out these errors. They have been corrected to "measurement challenges" in line 8 and to "Since downburst events have high frequencies of occurrence…" in line 38 as suggested.

3. L38 and onwards: This paragraph is very long with many ideas – should be separated into multiple paragraphs.
   The paragraph is split to enhance readability, as recommended.

4. L40: "unstable"
   Thanks, corrected.

5. Introduction: The introduction is very technical at times in terms of describing the analytical model and TLBO approach, this could probably be moved to data/methods. Instead (and related to general comment 3), it would be good to havesome background on the "analytical model" – what is it based on, what are the constraints, outputs, etc, and a discussion of similar models – how does this work fit within the broader literature?

   The technical aspects of the analytical model and TLBO approach, initially in the introduction, are moved to the methods section.
   Additional background on the analytical model, including its basis, constraints, and outputs, has been added, along with a comparative discussion of similar models in the broader literature.

6. Figure 4: What do the red/orange/yellow colours indicate? Time of strike?
   The red, orange, and yellow colors in the figure represent the time elapsed since each strike occurred. The color coding is used to illustrate the temporal/spatial distribution of lightning activity during this severe weather event.
   The color gradient from yellow to red shows the progression from more recent to older strikes, providing a clear temporal context for the lightning activity and its movement in space.

   Thank you for the comment. The explanation of the colors indicating the time elapsed since lightning strikes is implemented in the updated manuscript.

7. L188: Repeated sentence.
   The repeated sentence on line 188 has been removed.

8. L207 and elsewhere: grammar issues. "The TLBO algorithm ~~it~~ is an iterative…"
   All grammatical issues, including the one on line 207, have been addressed and corrected throughout the manuscript.

9. Table 2: I wonder if some of the variables in the analytic model could be constrained further by other data sources. For example, the storm speed/direction could potentially be estimated by storm tracking from radar or satellite, while the ABL wind speed and direction could be estimated from reanalysis.

   Thank you for your comment regarding the potential use of additional data sources to constrain the variables in our analytical model. This is similar to a comment by Reviewer #1. We acknowledge that integrating specific parameters like storm speed and direction from radar data, as well as ABL wind speed and direction, could enhance the precision of our model. In the initial phase of our research, our focus was to explore the capabilities of the analytical model coupled with the optimization algorithm without external constraints. This approach aligns with our primary objective of extracting downburst geometrical and kinematic parameters solely from anemometric data, a methodology driven by the availability and abundance of such data compared to the less consistent availability of radar data. Your insight is greatly appreciated, indeed, and we will reflect this perspective in the revised manuscript in section 4.2 and in our conclusions in section 5.4.

10. L269: MDA acronym does not need to be defined anymore.
    Thanks, corrected.

11. L275: I'm not sure whether this is a good reason to exclude storm direction as a variable to analyse. Can a transform be done from degrees to a periodic function?
    E.g. [https://stats.stackexchange.com/questions/148380/use-of-circular-predictors- in-linear-regression](https://stats.stackexchange.com/questions/148380/use-of-circular-predictors-in-linear-regression)

    Thank you for your comment on incorporating storm direction as an active variable for analysis. The authors possess comprehensive expertise in circular statistics since the objective function or error function estimation between simulated and recorded wind direction data make use of circular statistics when estimating the error.
    We have indeed considered using circular statistics from the beginning of our work. However, including circular variables like storm direction requires the addition of two new "linear" variables (x_dir, y_dir) associated with the circular variable. This in turns increases the number of variables, impacting the clustering algorithm and principal component analysis. Specifically, with storm direction included, the algorithm identifies three clusters explaining about 54% of the total variance, and the behavior of the within-cluster variance remains similar to the bar graph shown in Figure 10 of the manuscript.
    Additionally, incorporating ABL direction as an active variable leads to three clusters explaining only about 46% of the total variance, with a similar pattern in the within-cluster variance as observed in Figure 10.
    To ensure a meaningful explanation of variance by the clustering algorithm and to facilitate the physical interpretation of our results, we opted to consider storm direction and ABL wind speed and direction as secondary variables. The introduction of additional variables for each circular one complicates the interpretation and could potentially dilute the focus of our study.

12. The language can be made simpler in many parts. E.g.: on L285: "The focus of the MDA lies in examining the data matrix from both the solution and variable perspectives, aiming to identify similarities among solutions based on their variables.In essence, the goal is to establish a typology of solutions by identifying groups that exhibit homogeneity in terms of variable similarity" could be simplified to "The focusof the MDA is to apply statistical clustering to identify similar analytic solutions" (please correct me if I have misinterpreted this sentence, but hopefully this example is useful).
    Yes, the interpretation of the sentence is correct.
    We will revise similar complex sentences throughout the manuscript for simplicity and clarity without altering the technical accuracy.

13. Section 4.3: I appreciate the nice explanation of normalisation, but I think it is fairly standard practice in statistical modelling, and can probably be simplified. This is just a suggestion in relation to "presentation quality" (see review criteria above). Similarly on line 446: the expected average contribution calculation could probably be assumed rather than explained fully.
    The detailed explanation of normalization in statistical modeling has been simplified, aligning with the standard practice and focusing on the study's specific application.

14. L327: Damage “survey” rather than “campaign”?
The term “campaign” on line 327 has been changed to 'survey' for better clarity.

15. L345: “where”.
Corrected.

16. L384: “found”.
Corrected.

17. Table 4: Please define the symbols used for the column headings. This will make iteasier to read.
The symbols used for column headings in Table 4 have been defined for easier understanding.

18. I think Section 5.3 could potentially be shortened somewhat. The key point seems tobe that a certain set of variables are more important for explaining the variability in the solution space by PCA, and this result could be presented in a more concise way.
Section 5.3 has been shortened, emphasizing the key point about the significance of certain variables in explaining variability in solution space using PCA.

19. L407: Table 4 also presents…
The suggested text, "Table 4 also presents…", has been added to line 407 for coherence.

20. Table 6: Are these representative cluster solutions based on solutions closest to the mean using all dimensions, or just Xc0 and Yc0 as in Figure 14?
The solutions in Table 6 represent clusters based on all dimensions, not just Xc0 and Yc0.

21. Figure 15: Exactly how is the simulated wind speed on this plot calculated for comparison with observations? Is it the addition of the radial velocity solution and the storm translation speed or ABL wind speed?
In the model developed by Xhelaj et al. (2020), the simulation of horizontal wind velocity is achieved through the vector summation of three separate components: the stationary radial velocity created by a jet impacting a flat surface, the translational velocity of the downdraft corresponding to the movement of the storm, and the ambient wind of the atmospheric boundary layer around the downburst. The simulations are conducted at a height of 50 meters, matching the anemometer's placement. Our domain for simulation spans 20 km x 20 km, with the anemometer centrally located. The simulated wind speed and direction at this central point are then directly compared with the actual data recorded by the anemometer. The difference between the simulated data and the recorded data creates the objective function that is minimized through the optimization algorithm.

22. Figure 5 and 15: Y-axis label should be wind speed rather than velocity?
Thanks, wind speed is more appropriate.

23. L496: “field”.
Corrected.

24. L517: I think this is a key point that nicely sets up some of the goals of the paper, consider mentioning it earlier (unless it was already mentioned, and I missed it)
Thank you, we will mention it earlier in the paper for better context and setup of the paper's goals.