

Response to Reviewers and Community Comments

Experimental wind tunnel dataset of wake and turbine measurements for wind farm control

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We thank the two anonymous referees for their careful reading, and for the thoughtful, constructive feedback. Their remarks have helped us sharpen the paper. Below, we reproduce the original comments in blue italics, then provide our point-by-point replies in black. Wherever relevant, we also indicate what has been changed in the manuscript.

A revised version of the manuscript is attached to the present reply, with additions highlighted in blue and deletions marked in red.

Response to Anonymous Referee #1 (RC1)

We appreciate the referee's very positive evaluation and the helpful suggestions. We respond to each point in the following.

General comments

Comment 1: *The data set is unique and has the potential to be a reference case for wind farm control in the future.*

Response: Thank you for this assessment. We are grateful that you recognize the potential value of the dataset as a reference for the wind-farm control community.

Comment 2: *The experimental data are also accompanied by a series of numerical models, from CAD to ALM-LES model of the turbines. This is also nice to have, as potential users of the dataset can also make use of one of the available numerical models, should they see this fit. At the same time, I expect this will create a bias, with most future users employing the same inflow/ALM etc model. Since no model is perfect, this is not ideal, but this is no reason not to include the numerical models.*

Response: This is a valuable point, and Referee #2 also raised a closely related concern (major comment 5). We agree that giving users ready-made models can nudge them toward adopting the same inflow/ALM choices, even when independent implementations would be preferable.

Still, we think the upside is substantial: the models reduce the barrier to entry, provide a fully documented and reproducible reference setup, and let users cross-check their own configurations against a known baseline. To make the intended use unmistakable, we added text in Sect. 5 and in the conclusions stating that the models are optional companions and reference implementations, not default prescriptions, and that users are encouraged to develop and validate independent models against the measurements.

Comment 3: *One aspect the authors could consider is the continuous update/modification of the data description. A data description article review can obviously not check whether the description is entirely complete, or if something can be improved, in the same way an actual user of the data set could. The authors could consider making this explicit and ask for feedback in the paper.*

Response: We agree, and thank you for the suggestion. Since the dataset is hosted on Zenodo, we can rely on its native versioning support. That enables us to revise documentation (for instance README files and metadata) in response to user input while preserving citability through versioned DOIs.

Following your recommendation, we added a statement in Sect. 4.3 and in the conclusions explicitly inviting users to report issues, unclear points, or suggestions, and we commit to maintaining the repository documentation accordingly.

Minor comments (annotated PDF supplement)

Minor comment 1: *(Abstract, "individual pitch control") Is this the same as helix or is it for load reduction? If the latter, please clarify.*

Response: Here, individual pitch control (IPC) is used for load mitigation, following the classical Bossanyi-type approach described in Sect. 2.2.6. This is different from the Helix strategy, which also uses individual pitching but aims at wake mixing. We clarified this in the abstract and introduction by explicitly writing "individual pitch control for load mitigation."

Minor comment 2: *(Fig. 1) For clarity, please specify that Φ is negative in Fig. 1b.*

Response: Done. The Fig. 1 caption now states that the shown angle Φ is negative.

Minor comment 3: *("at the apex of the G1 rotor") Apex or hub-height? Would it not be better to quote hub-height values, as usually done in the community?*

Response: In line with prior usage in the literature (Jonkman et al., 2009; Fontanella et al., 2021; Panda et al., 2025), we use "rotor apex" to refer to the rotor's central point where the blades pitch axis intersects at the turbine front. For an untilted and not coned rotor (as the G1's), the distance from the ground of the rotor apex coincides with the hub height. We added this clarification to the manuscript.

Minor comment 4: *(Inflow description) Here only the inflow TI is described, however mean profiles are also important. For completeness please include this information (profile exponent for example) and direct the reader to Sect. 4.1.1 for more info.*

Response: Agreed. In Sect. 2.1.1 we expanded the inflow descriptions to include the mean velocity profile characteristics (power-law shear exponents), and we added a clear forward reference to Sect. 4.1.1, where the full inflow mapping is documented.

Minor comment 5: *(Figure 2) In Figure 1, the indices (a) and (b) appear before the relevant descriptive text, here they appear after. Please select one type and be consistent throughout the document.*

Response: Thanks for catching this. We corrected the inconsistency by updating the caption of Fig. 1.

Minor comment 6: *(Temperature transducer, " $\pm 0.1\%$ of Full Scale") Please provide the full scale for the sensor or directly a quantitative measure.*

Response: The Mescon TH 12-U full-scale range is 100°C, which corresponds to $\pm 0.1^\circ\text{C}$ uncertainty. We updated the text accordingly.

Minor comment 7: *(Pitot tube "in front of the cluster") At what distance?*

Response: The Pitot tube sits about 3–4 diameters upstream of the first turbine, far enough that rotor induction (blockage influence on the probe) is negligible. The exact probe position is included for each test in the corresponding metadata. We added this to Sect. 2.1.2, along with a short justification. This also answers Referee #2 minor comment 2.

Minor comment 8: *(Figure 3) Minor comment, can be ignored. The paper would look much better if the font in the figures was the same as in the main text.*

Response: Understood. Where feasible, we regenerated figures using a font consistent with the main text.

Minor comment 9: *(Torque controller integral gain changed in January 2019) Please state if and how that affects the user of the dataset.*

Response: We clarified this in Sect. 2.2.6. The update of the integral gain $K_{Q,I}$ affects only the closed-loop transient response during fast wind-speed changes; steady operating points (tip-speed ratio and power coefficient) do not change.

Minor comment 10: *(Probe attitude "precisely measured") Within $\pm 0.1^\circ$? How precisely?*

Response: The dual-axis inclinometer SPECTRON L-212T has $\pm 0.3^\circ$ accuracy. We replaced the qualitative wording with this quantitative specification.

Minor comment 11: *("with the turbines not installed") $\rightarrow$ "without the turbines".*

Response: Corrected.

Minor comment 12: *(Torque-drift correction) Are runs where this correction is applied labeled in the dataset? If not please add this, or somehow list them, so that the users are aware if they are using a corrected dataset or not.*

Response: Only a limited fraction of tests showed torque-signal drift requiring the correction described in Sect. 3.4.2. Those tests are explicitly flagged in the metadata (test matrices). We added a statement about this in Sect. 3.4.2.

Minor comment 13: *(Torque-drift correction) Also, does this only apply to the most upstream turbine? Is the correction applicable to the downstream turbine(s) operating in the controlled or uncontrolled wake of the upstream one?*

Response: The correction depends only on the torque drift estimate (from the calibration phase before and after each test) and the rotational speed. If the machine operates in Region II, the correction applies whether or not G1 is in a wake. We added this clarification to Sect. 3.4.2.

Minor comment 14: *(Symbol δ) This is missing from the nomenclature. Please double check for all symbols.*

Response: Thank you. We added δ (encoder quantization interval) to the nomenclature and cross-checked the symbol list against the full text. This also addresses Referee #2 minor comment 3.

Minor comment 15: *(Gearbox ratio deviation of 0.2%) Is this deviation from the nominal reduction ratio? How does this translate to rotor angular velocity?*

Response: Yes, it is the deviation of the real gearbox reduction ratio from the nominal value due to manufacturing tolerances. Because rotor speed is inferred from the torque-actuator encoder via the nominal ratio, this yields up to 0.2% relative uncertainty in rotor speed (for example about ± 1.7 rpm at 850 rpm). Sect. 3.6 has been updated to make this explicit.

Minor comment 16: *(Overall uncertainty as the sum of the individual contributions) Is this to be more conservative than selecting the square root of the sum of the squares of each contribution?*

Response: Yes. Since independence between contributions cannot always be guaranteed, we chose the arithmetic sum as a conservative upper bound. We now state this rationale directly in Sect. 3.6.

Minor comment 17: *(Sect. 4) Please avoid one-sentence paragraphs.*

Response: Agreed. We revised the text throughout the manuscript to merge one-sentence paragraphs into surrounding material.

Response to Anonymous Referee #2 (RC2)

We appreciate the positive evaluation and the detailed, constructive feedback. Below we answer each point.

Major comments

Major comment 1: *Some details of the measurement equipment are probably too much for the article. For instance, from my perspective, adding the model number for each component in section 2.2 is not necessary. Similarly, sections 2.2.3, 2.2.4 and some details in the beginning of section 2.2.6 can be omitted from the main manuscript. These details could be provided as supplementary material. The intended users of the dataset are numerical modelers – burdening them with too much technical details of the experimental setup is probably not necessary.*

Response: We see the point, and agree that not all users will need the same level of hardware detail. At the same time, we expect the dataset to be valuable to experimentalists as well, and in a data description article we consider long-term traceability and reproducibility a central goal, which is why we prefer to keep a fully detailed description of the apparatus, including component identifiers. For that reason, we would like to maintain the original version of the text.

Major comment 2: *On a similar note, why is there a need for section 3.1 in the article? Could this be better suited in supplementary notes?*

Response: Sect. 3.1 describes the assembly procedure, which directly feeds into the positioning uncertainty reported in Sect. 3.6 and therefore matters for interpreting wake measurements. Improper assembly, for example failing to rigidly fix the turbine at the base, can also affect the aero-elastic response. On that basis, we believe this material should remain in the manuscript.

Major comment 3: *Given the length that the authors go to in order to explain the experimental setup, it seems rather superficial that they simply assume that the yaw and tilt biases are stationary in section 3.4.1. There should be some verification/validation for this assumption.*

Response: We agree that the assumption needs explicit backing. We expanded Sect. 3.4.1 to report the verification: we quantify how yaw and tilt biases vary across wind speeds in the tested range and show that the variability is small compared with the quoted 0.2° uncertainty on the hot-wire angles.

Major comment 4: *In comparison to the details provided on the experimental setup and execution, the description of the datasets seems rather brief. For a potential user of the data, this section might be of the most interest. I would have expected the authors to provide details of the experimental conditions for each dataset. For instance, lines 595-597 mention that turbines are operated under a range of control conditions. However, no further details on control strategies are presented. Same can be said of other subsections in this section. I understand that the user can consult the metadata on it, however, given the detail given to the earlier sections, I would have expected a nice and clean overview of experimental conditions for each campaign.*

Response: We agree, and thank you for the suggestion. Section 4 has been expanded. For each dataset, we now provide a compact overview of the tested conditions, covering inflow, wind-farm layout, and control strategies. These summaries complement the machine-readable metadata in the repository, so readers can understand the experimental scope without downloading the data first.

Major comment 5: *Section 5 does not have a proper motivation. If the intention of the article is to provide an open access wind tunnel dataset for independent development/validation of numerical codes, providing numerical implementation in some existing codes adds a bias factor, and may unintentionally redirect the potential user towards using these codes rather than independent development/testing/validation.*

Response: We appreciate the comment, and Referee #1 flagged the same risk. We added a clear motivation at the start of Sect. 5. The numerical models are shared to (i) encode the turbine geometry, structure, aerodynamics, and control in machine-readable form, (ii) lower the entry barrier, (iii) provide a verified reference implementation that users can use as a baseline when checking independent setups, and (iv) sharing the models is also a fundamental and indispensable step towards ensuring the reproducibility of research, which is one of the central goals of data description papers.

We also inserted an explicit caveat that these models are optional and not prescriptive, and that independent development and validation against the experimental data are encouraged.

Major comment 6: *Finally, it needs to be pointed out that as detailed in section 4, the dataset presented in this work is not 'new' but is a collection of many experiments (which are very much appreciated) going back as far as 2016. Still, the conclusion starts with 'This paper presents a new open-access experimental dataset ...', which seems a bit contradictory and incorrect to me. I think the article should be explicit about which experiments are new or unique to it, and which are old. This also brings in the question that does making a collection of existing datasets open access constitute the basis for a data description article, or should it be a new dataset that has not been published/accessible before? Perhaps the handling editor can be a better judge on that than we, but it is a question that needs to be addressed.*

Response: We agree the wording needed tightening. While some of the campaigns have been analyzed in earlier papers, the underlying data had not been released publicly. What is new here is the open-access release of the complete, harmonized collection, along with uniform post-processing, uncertainty quantification, metadata, and companion numerical models.

We revised the abstract, introduction, and conclusions accordingly, for instance by writing “a newly released open-access dataset” instead of “a new dataset.” On whether an open release of previously non-public data is appropriate for a data description article, we believe this fits WES's scope for this manuscript type, which aims to document and make valuable datasets citable.

Minor comments

Minor comment 1: *G1 rotor is first mentioned in section 2.1.1 without any prior/proper introduction. For a reader unfamiliar with this rotor, it is difficult to relate/contextualize the boundary layer characteristics with the rotor. For instance, on first read, I do not know what the hub/top/bottom height of G1 is, so the turbulence intensity number with respect to it is not very intuitive.*

Response: Agreed. At the start of Sect. 2 (before the boundary-layer inflows), we added a short introduction to the G1 turbine, including rotor diameter $D = 1.1$ m and hub height $z_H = 0.825$ m, with a forward reference to Sect. 2.2 for the full turbine description.

Minor comment 2: *Section 2.1.2, line 124: what is the distance between the pitot tube and the wind farm cluster? Is the tube sufficiently upstream such that it is not influenced by the blockage of the rotor?*

Response: As stated in our reply to Referee #1 minor comment 7, the Pitot tube is placed about 3–4 diameters upstream of the first turbine, where induction effects are negligible. We added the distance and a brief justification to Sect. 2.1.2.

Minor comment 3: *I think some of the variables are missing from the nomenclature section. For example, I could not find P_R in the nomenclature.*

Response: Thank you. P_R has been added, and we cross-checked the nomenclature against the manuscript (also covering Referee #1 minor comment 14).

Minor comment 4: *In fig 7 it may be beneficial to add a 1:1 line as well.*

Response: Agreed. We added a 1:1 reference line to every panel in Fig. 7 and updated the caption.

Minor comment 5: *I would have appreciated a few figures visualizing some key data from the dataset as well.*

Response: We understand the request. We believe the appendix figures comparing simulations and measurements already provide that kind of overview: they report mean values and the temporal variability ranges for key quantities (power, rotor speed, pitch angles, structural loads, wake velocities). For that reason, we did not add further figures beyond those already included.

Response to Community Comment by J. Gordon Leishman (CC1)

Comment 1: *The manuscript presents an open-access wind-tunnel dataset for wind-farm-control studies using scaled G1 turbine models. While the dataset itself may have some practical value, the manuscript is not sufficiently strong as a journal article. It reads mainly like a laboratory data manual, with lengthy descriptions of hardware, sensors, actuator specifications, calibration procedures, data channels, acquisition rates, post-processing steps, and numerical model files. Much of this information may be useful for repository documentation, but it does not constitute a scientific contribution.*

Response: The submission is a data description article, a format explicitly supported by Wind Energy Science. For this article type, the aim is to document the dataset, including apparatus, calibration, post-processing, uncertainties, and data organization, at a level that allows correct use and makes the dataset citable. In that context, the detailed material cited in the comment is not peripheral at all; quite the opposite, it is the core of what the manuscript type asks for. Very clear explanations and abundant guidelines are provided on this type of manuscript at various places on the journal web pages, including https://www.wind-energy-science.net/about/manuscript_types.html, https://www.wind-energy-science.net/peer_review/review_criteria.html, <https://www.wind-energy-science.net/submission.html>.

Both anonymous referees reviewed the paper with these criteria in mind and identified the dataset as valuable, with Referee #1 calling it “unique” and potentially a reference case.

Comment 2: *The central weakness is that the manuscript does not critically establish the value, fidelity, or limits of the dataset. Scaled wind-tunnel wake-control experiments are subject to Reynolds-number mismatch, artificial inflow generation, limited atmospheric realism, actuator-bandwidth scaling, structural-scaling compromises, blockage effects, and simplified farm geometry. These limitations directly affect whether the data can meaningfully validate wind-farm-control models for full-scale turbines. The manuscript describes the apparatus in detail but gives too little critical assessment of what the dataset can and cannot validate.*

Response: We have previously extensively published on these topics (Campagnolo et al., 2020; Canet et al., 2021; Wang et al., 2021; Bottasso and Campagnolo, 2022). However, for completeness, we have now added a new dedicated section, Sect. 6 (“Scope and limitations of the dataset”), covering: (i) Reynolds-number mismatch, mitigation via low-Re airfoil design, and remaining effects; (ii) what the passively generated boundary-layer inflows capture and what they do not (for example no Coriolis-induced veer and no stability effects); (iii) blockage; (iv) actuator-bandwidth and structural-scaling implications; and (v) a plain summary of what the dataset can validate and what it cannot. We believe this makes the manuscript more self-contained: readers can understand the limits of the dataset due to scaling, and look for additional details in the references provided.

Comment 3: *The manuscript also blurs the boundaries among measurement, correction, and modeling. Several quantities depend on post-processing, sensor corrections, load transformations, imbalance removal, derived quantities, and numerical models. A credible benchmark dataset should make these distinctions central, namely, what is directly measured, what is inferred, what is corrected, and what is model-generated. Instead, the paper presents procedural details without a sufficiently rigorous assessment of uncertainty and sensitivity.*

Response: We disagree, as in fact these distinctions are very explicitly discussed one by one with the necessary level of detail throughout the manuscript: Sect. 3.4 lists the corrections that were applied, Sect. 3.5 identifies derived quantities, and Sect. 3.6 provides a quantitative uncertainty budget for all measured channels (Table 1) at 95% confidence.

Also, both raw and corrected signals are stored in the released NetCDF files, so users can -if they wish- use the unprocessed raw measurements. In addition, the metadata flags all runs where specific corrections were applied (as noted in our response to Referee #1 minor comment 12).

Comment 4: *Overall, I do not recommend the publication of this article. The dataset may be appropriate for an institutional repository, a technical report, or a supplementary data release, but the manuscript does not provide sufficient scientific analysis, critical interpretation, or validation to justify publication as a journal article. I recommend that the paper be declined in its present form.*

Response: We strongly disagree. This manuscript is submitted under the data description category, where the primary evaluation criteria are dataset significance, quality, and documentation, not novel scientific analysis. The scientific exploitation of the data is addressed in companion studies in which these measurements have already been used and will continue to be used. This is very clearly explained on the Wind Energy Science web site, which also provides ample and clear guidance to authors: detailed information on data description papers is provided at https://www.wind-energy-science.net/about/manuscript_types.html, and dedicated review criteria are clearly spelled out at https://www.wind-energy-science.net/peer_review/review_criteria.html. We have very carefully designed this paper to match the goals and criteria of this article type. This community comment ignores these facts and the fundamental differences existing between a normal research article and a data-description paper.

Comment 5: *Because the Editor-in-Chief is listed as a co-author and corresponding author, the public record would benefit from explicit clarification that he was fully recused from editorial handling, reviewer selection, discussion moderation, and decision-making. The identification of a handling editor is helpful, but it does not by itself clarify whether the Editor-in-Chief had no role in the editorial process for a manuscript on which he is a co-author.*

Response: All Editors, not only the Editor-in-Chief, are bound to the ethical standards described at https://www.wind-energy-science.net/policies/publication_ethics.html and follow all journal policies, which are described at https://www.wind-energy-science.net/policies/obligations_for_editors.html and linked pages.

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