Summary

The authors present a method to forecast the maximal guaranteed inertia provision by a wind farm. For that they combine:

1. The results of an optimization problem to derive the maximum achievable inertia constant of a single WT for given combinations of curtailment level and wind speed under various restrictions
2. A forecast of the ambient wind conditions in the wind farm with a high spatial resolution
3. A wind farm of 12 WTs modelled as cP / cT LUTs, which are iteratively used in
4. A wake model of the wind farm.

The question of how much inertia a wind farm can support and how the support will affect the flow of the wind farm is highly relevant and new to the reviewer's knowledge. Insights into the constraints for inertia provision of single WTs in the different operating conditions are also highly relevant but have been discussed (less detailed) before. Different strategies for the curtailment of single WTs have been analysed before. The integration of such strategies and VSM into WT control have been shown before but is most likely still relevant and can still be optimized.

Overall it is not 100 % clear to me which parts of the paper are actually new and which parts have been published before. This is especially true for section 3.4 and reference Thommessen and Hackl 2024. If all of section 3.4 is new, it should be considered to break up the paper into two papers to allow a more detailed discussion: one on the optimization for individual WTs and a second for the specific effects in a WF. Just by looking at the number of pages and the level of discussion on WT effects compared to those of effects in a WF, the latter part might need some extra work. If it is decided to keep the paper as one, that should be reflected in the title.

However, despite the issues mentioned above, I can still recommend to publish the work as I consider it being relevant for the future discussion of the addressed issues.

Style

Please include a table with the most relevant parameters, e.g. by combining it with the list of abbreviations

While the general level of figures is good, figure 14 seems to be overloaded. Limiting the depicted WTs to exemplary ones or grouping them (e.g. first row, heavily influenced by wake effect, etc) would help to understand the differences between the similarly affected WTs in the WF.

Some important definitions of terms are missing, which the authors use heavily in the argumentation, e.g. initial ROCOF → what time is meant? First second, first 10 ms?

Fast frequency response should be defined and differentiated from inertial response in section 2, it is assumed that the authors follow the definition by Eriksson et al.

Please consider choosing a different title. To me it suggests that the work is about control strategies and operational constraints of wind farms, these are (mainly) discussed on WT level in the paper. While this is of course also important for the wind farm, I had other expectations after reading the title (e.g. how to best distribute frequency support among the WTs in a WF).

The abstract should read more like a summary of the paper at hand and less like an introduction. Furthermore, it is unclear why abbreviations are introduced there that are then reintroduced in section 1.1.

The conclusion should be reworked and be more focused on the findings in this paper, e.g the section on the background can be left out in my opinion.

Citations

Reference list must be reworked, harmonized and enhanced. For instance, NREL has an excellent documentation hence a reference like NREL: FORIS should not be used in this way

A lot of earlier important work is missing, e.g. but not limited to work on frequency support with WTs by G. Ramtharan, J.B. Ekanayake and N. Jenkins (2007) and NREL (J. Aho, L.Y. Pao, P. Fleming et al.) roughly 2012 to 2016.

It is unclear how the authors chose the citations (especially in section 1 and 2) given the tendency to self-citation while fundamental work of other authors is often missing here. One but not the only example: in line 20 the original source and not a self-citation must be used. It is good practice in science to give credit to the original authors of research.

There are numerous important statements without a (sufficient) reference given, e.g. on market design for inertia.

Methodology and results

Assumption of constant wind in the wind farm is arguable, especially due to the high non-linearity of the inertia constant to the wind speed

It is unclear whether the resolution in time of the wind speed forecast in the wind farm is sufficient for the intended forecast given the high non-linearity between wind speed and achievable inertia constant. Especially, how the minimum wind speed of the probability distribution is defined, whether it may further be reduced by turbulence and what effect that would have on the achievable inertia constant for single wind turbines. If turbulence is not included in the minima, the spatial distribution has a lesser smoothing effect on the sum of inertia constants than argued on page 7.

Has the influence of the MRS-based derating on WT loads been analysed yet?

Equation (15): what wind speed is used for that equation (rotor-effective?). Could it be a problem to measure / calculate that value in reality while operating at a reduced power setpoint?

Good to see that the authors use very challenging frequency events for their analysis. However,

1. Given that the authors use a control system, which increases the rated speed compared to the reference design, it should be shown, that the WTs do not have a risk of overspeed when a power reduction is needed.
2. It would be interesting to see how the proposed controller performs when H_V is higher (e.g. e.g. at 6 or 7 m/s) → assumption would be that the effects on the aerodynamic performance are more relevant and thus more interesting for the discussion

Shouldn’t the maximum rotor speed be added as a constraint for the optimisation problem when a power reduction is needed?

For the first test case, H_V is rather small. In fact, it is so small, that it may not be sufficient to allow a stable operation of the grid when all units had such an inertia constant. This is even more important when getting closer to full load operation. It would be great if the implications of the derived achievable inertia constants for the grid operation were (briefly) discussed.

It is unclear, why the inertial response is prioritized over fast frequency response even during the frequency recovery, i.e. IR is hampering the frequency recovery.

Might be a problem with the missing definition, however, it is unclear how the proposed control is able to reduce the initial ROCOF (in my understanding at the moment of the frequency disturbance) when it is stated in line 330 that measurements of the voltage (and most likely the calculation of the frequency is needed). Please clarify it.

Figure 4 shows that the optimal rotor speed is maximum 20 % above the minimal rotor speed during the transition between region I and II. Why is the minimum rotor speed not the constraint for the lowest wind speeds (up to appr. 4 m/s) in figure 11 b, which show a reduction of 20 % compared to MPPT for these wind conditions?

Figure 18: in what cases is the sum of H_V in a WF underestimated when wake effects are neglected?

It is discussed whether or not a power reduction should be allowed during the speed recovery phase. While this is certainly an important issue, it is very important to compare similar cases. At least for some time, the Quebecois grid code (which usually the relevant case for Godin et al.) allowed very long recovery periods (in the range of 30 s) for moderate power increases. Thus only very small power reductions are needed during the recovery. This is very different from the simulated case study with higher power increases and with a much shorter recovery period.