

Reply to Associate editor decision: “Publish subject to minor revision”

The authors would like to thank the associate editor and the reviewer for their time and for the useful feedback. The inputs that they provided have contributed to the improvement of the paper.

A list of point-by-point replies to the reviewer’s suggestions is reported in the following. The reviewer’s comments are in black, and our replies in blue.

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

The authors

Suggestions for revision (from anonymous referee #2, see Report #1)

Thank you to the authors for carefully considering my comments and updating the paper. I feel that my comments have, for the most part, been suitably addressed. I have one remaining concern about the use of time discounting, and a smaller comment/interest that may not be within scope here regarding the dependency of price on ambient conditions.

1. Regarding the choice to omit time discounting from the optimization problem: I do not understand the argument presented in lines 558--566 of the revised manuscript. As I understand it, the point of time discounting is that the power generated in year 1 *is* more valuable than the power generated in year 10. The power generated in year 1 generates profits for the wind farm owner that can be reinvested, generating extra income over the next ten years. If that means that the turbines should be operated more aggressively in the short term (incurring more damage, but also generating a higher profit) and then less aggressively in later years, then this is the optimal solution. The authors state "But dispatching the less performing actions [in later years] would clearly be a suboptimal choice, as better ones had been computed and used in earlier years". I think this statement is not correct; those "less performing" actions *are* the optimal actions for the later years, when damage has already been incurred to generate profits in earlier years. I will again admit to not having any formal training in economics, but I believe this is exactly the point of considering time discounting in long-term planning: it highlights that actions should be taken in the short term, even at the expense of being able to take those same actions in the long term, because the profits generated now are more valuable than future profits.

Thank you for this thoughtful comment. We agree with the reviewer’s general interpretation of time discounting: when using a net present value (NPV) formulation as shown in Eq. (19) of the revised manuscript, it is indeed optimal in a classical economic sense to prioritize higher profits in earlier years, even if this leads to more conservative operation later due to increased damage.

To clarify our understanding on this point and the suitability of the proposed optimization problem in considering time-discounting during control synthesis, we have deeply revised the discussions in Sect. 3.4.3.

Additionally, we have carried out a preliminary analysis that includes NPV-driven approach using the proposed WFC formulation and compared it with the other

approaches for a simplified case study. Section 4.1.6 discusses the preliminary findings and demonstrates that the proposed formulation does allow accounting of effects of time discounting.

2. Regarding the revenue model presented in Section 3.4.1: I agree with the approach taken, to fit a model to the general trend in market price (blackboard R) as a function of the inflow condition W_a . This seems to me most straightforward (and so probably best!) way to capture the trend in prices while gridding over only inflow conditions. However, I wonder if the authors can comment on how much of the variability in market price this approach might capture (and miss)? Figure 3 shows that while there is a dependency of price on wind speed, there are a wide range of prices occurring at each wind speed evaluated, suggesting that while it is generally a good idea to apply more aggressive offsets at low wind speeds (from a revenue perspective, in any case), there may be times when operating that the price of electricity is low at low wind speeds, in which case the value of wake steering is much less. Can the authors provide any comment on this?

As correctly noted, Figure 3 indicates that while a clear trend exists between market price and inflow wind speed, there is substantial variability in instantaneous prices at any given wind speed. The regression model used in Section 3.4.1 is designed to capture this average dependency, and therefore primarily represents the expected market price conditioned on inflow conditions, rather than the full spread of possible price realizations. As a result, the approach captures the mean component of the price signal but does not explicitly represent higher-order variability (e.g., variance or extreme price events).

However, because the control strategy is optimized over long-term conditions, it is expected to reflect the average economic impact of the control policy rather than its performance under specific short-term market realizations. A more precise quantification of the impact of price variability would require a time-resolved co-simulation of wind conditions and market prices, in which the control policy is evaluated against a full time series. This is an important direction for future work but lies beyond the scope of the present study.

We have expanded the discussion in Section 3.4.1 to include this information. Thank you for this insightful comment.

Suggestions for revision (from anonymous referee #1, see Report #2)

The authors revised the manuscript deeply and addressed all my previous comments. In my opinion, the manuscript can be accepted for publication.

References are missing in the author's tracked changes file and obviously need to be included in the final version.

Thanks for the observation. We have ensured that the new author's tracked changes file has no missing references.

I have two minor additional comments the authors might want to address:

1. Figure 9: I missed a bit of clarification on why the greedy case fails to reach the nominal lifetime. Where is that behaviour coming from, and does it have implications for the interpretation of the other cases?

Thank you for this comment. We have expanded the discussion in Section 4.1.3 of the revised manuscript to offer clarifications on this point.

The reduced lifetime in the greedy case originates from the assumption, introduced in Section 4.1.1, that one turbine has already accumulated more damage than expected during the first 10 years of operation. This is intended to represent deviations from design assumptions, for example, due to unmodelled operating conditions or fault-related loading.

Because the greedy strategy does not explicitly account for long-term damage redistribution, it continues to operate the turbines to operate aiming at short-term power production. As a result, the already overutilized turbine continues to experience high loading, leading to accelerated damage accumulation and ultimately preventing it from reaching its nominal design lifetime.

This assumption of elevated initial damage is applied consistently across all strategies. Therefore, while it shifts the absolute lifetime values, it does not affect the relative comparison between strategies, which remains the focus of the analysis.

2. The authors assume a fixed correlation between wind prices in their economic model. However, Figure 3 shows that the correlation is weak and the spread of prices per wind speed is large. Some discussion of the implication from that would be a nice addition. Please refer to our response to the 2nd comment from anonymous referee #2.