Reviewers' comments are colored in black, authors' responses are colored in blue.

Reviewer 1

The revised manuscript is a substantial improvement with respect to the original submission. The description of the methodology is improved, the quality of the figures is better, and the writing is solid now. In its current form, I feel like the manuscript could be accepted into WES. However, I do suggest making a number of more minor changes to further improve the manuscript, listed below:

1. The authors are, in my opinion, still not completely consistent with what the main goal/contribution of this work is. The new title (which I do think is an improvement with respect to the old title) lays the focus on the effect of the controller. The second-to-last sentence of the abstract also quantifies this effect, presenting it as a significant power loss (close to 2%). However, in the conclusions, it is mentioned that this effect is rather small, and that it is as of yet uncertain whether additional tuning is necessary. Furthermore, the last sentence of the abstract seems to indicate a second goal: understanding wind turbine performance in complex terrain in general, which seems to be different contribution that is not covered too much in the rest of the paper. I would suggest unifying goals and findings in the abstract, introduction, and conclusions more clearly.
We carefully proofread the abstract, introduction, and conclusions with your suggestion in mind. The last sentence of the abstract is intended to broaden the picture and reframe the motivation of this article; the idea of this article was to improve the understanding of what drives performance degradation in complex terrain when a streamwise velocity gradient is present. It was unclear before which role the controller plays in the total losses, which complicated the analysis of other studies. Based on this study, we conclude that the controller's influence is likely small relative to the physical limitations imposed by the flow, and we show in detail how the controller operates from an aerodynamic perspective. So yes, we believe our study "[...] sheds light on the interpretation of wind turbine performance results in complex terrain and helps shape efforts to reduce prediction uncertainties for future onshore wind projects in complex terrain", as we wrote at the end of the abstract.

2. Assuming the goal is to assess the effect of suboptimal control on turbine performance (i.e., power), I would have appreciated a table in Section 3 that gives an overview of the effect of optimal versus suboptimal torque control on the power coefficient (in percentages) in each of the topologies. The paper now seems to focus on absolute Cp, but due to the non-uniform flow, this gives a distorted view of the actual effect of mistuned controllers on the power output. In my opinion, this would be the main contribution of this paper if it were to be included. Currently, it only quantifies this number for the most extreme case, leaving the reader unsure about whether this number (close to 2%) is generalizable or only applies to this "least optimal" setup. You admit to using a very specific "extreme" case in Section 2.4, but fail to mention this when presenting the results in the conclusions and abstract.
Thank you very much for this suggestion. The main conclusion from this paper is that a controller tuned for flat terrain does not necessarily track optimal performance in complex terrain, and that, for the investigated case, the flow physics are the limiting factor and not this suboptimal control. However, the exact loss (ca. 2 % in our case) is site dependent, and therefore adding additional comparisons does not necessarily increase the value of the results. In general, for the other conducted simulations, we expect losses to be smaller because the cases are generally less extreme than the case we focus on. Nonetheless, the word 'extreme' is a relative term, and even more extreme cases may be possible. Therefore, we did not mention extreme in the conclusion and abstract, as it might suggest that 2 % is the maximum expected loss. We modified the abstract to make the reader aware that we are only referring to specific cases for the stated numbers.

3. I like the addition of Figure 7, but I'm not sure I love how the flow is plotted here. By using the relative wind speed, the flow looks a little weird, and makes it hard for me to compare different cases to each other as they have different reference wind speeds. I think I would prefer it if you'd have used the absolute wind speed here (but if you have a reason for choosing the relative wind speed, and you can explain to me why the current method is better, that's fine too). Also, I would

appreciate the addition of a flow figure without a turbine, at least in the "main" hill case (B0/C0), to give me an impression of how the hill influences the flow.
It was chosen to normalize the flow by the reference wind speed at the turbine position, because this allows for interpreting the results from a turbine perspective. It is, from our perspective, most consistent with our approach to examine flow physics relative to the turbine. We added a figure of the undisturbed flow for cases C0/C1 and C2.

4. When using very large numbers, such as the value of k or the number of cells used, preferably use scientific notation (1.23*10^6), or otherwise use commas (1,234,567).
We modified the formatting accordingly.

5. In Section 3.2, you changed the reason behind the lower optimal pitch angles found in your paper. I haven't heard this reasoning before, and you don't cite a source for it.
We were not correct in the new reason stated. After an internal discussion and also contacting C. Bak, first-author of the article describing the DTU 10 MW reference wind turbine (Bak et al., 2013), we modified this paragraph and build the corrected argument now on well-known, previously reported reasons.

References

Christian Bak, Frederik Zahle, Robert Bitsche, Taeseong KIm, Anders Yde, Lars Christian Henriksen, Anand Natarajan, and Morten Hartvig Hansen. Description of the DTU 10 MW Reference Wind Turbine. Technical Report DTU Wind Energy Report-I-0092, DTU Wind Energy, Roskilde, Denmark, 2013.