Comment on amt-2022-219
Anonymous Referee #2


Review of “Inferring surface energy fluxes using drone data assimilation in large eddy simulations” by N. Pick and coauthors.

My background is mostly in data assimilation but I am little familiar with surface energy fluxes.

The manuscript by Pirk and co-authors introduces existing data assimilation methodology (plus a new assimilation method hybridising two previous methods) into a new application area of surface energy fluxes observations by new autonomous technology (drones). The topic is interesting and has practical outcomes for the best use of drones and the further exploitation of a promising technology.

The manuscript is very nicely written and is at a very mature stage already, making an enjoyable read. The methods are well presented, evaluated rigorously and the results make a convincing case to take the methodology forward. I only have a few minor questions.

On the data assimilation side I appreciate the introduction of the PIES scheme, which is original to my knowledge. The PIES scheme does not seem to bring much improvement and the authors are open on the shortcomings of the method. What I am missing is a sentence explaining the reasoning behind the PIES scheme: why replace the penultimate iteration of the ES-MDA method and not other ones? Otherwise the comparison of the assimilation methods is done in a correct way. An indication of their respective computational costs would be useful as a perspective.

The synthetic experiments results seem to argue against the random exploration flight strategy, although for a reason related to the data assimilation technique (their effective observation errors are smaller). The authors should insist that their experiments do not disqualify the random flight strategy but may want to devise their observation representation errors more carefully.
There is only one difference between the synthetic case and the real observations case and that is the independent versus correlated H and LE parameters. The authors do not come back to this difference in the discussions: does the correlation of parameters work well or should it be done differently?

The authors also use several statistical metrics to evaluate the methods from the classical RMSE and bias to the CPRS and KLD. It would be interesting to have the authors recommendation on how useful or redundant these metrics are in practice.

Detailed comments and typos:
- l.15: “variance” is missing “minimum variance”.
- l.239 “in a cyclic manner”: do you mean the model domain is cyclic?
- l.245: I miss an argument that the temporal representativity is somehow related to the spatial representativity of the observations, the discrepancy between the size of the instrument on the drone and the LES cell dimension.
- l.255: Can you explain why 2 m/s errors on wind speed is "conservative"?
- l.283: “the” is missing before EnKF.
- l.420: “mean local differences”: this notion is maybe familiar to the surface flux community but I would needed a little definition (horizontal gradient? Positive in which direction?)
- l.616 and 617: “an d x N matrix” should sound better as “a d x N matrix”.

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