The authors present a comment on earlier work by Simão Ferreira et al. on a possible theoretical limit for offshore wind energy extraction. The authors express some strong reservations regarding the validity of the claims presented earlier by Simão Ferreira et al., and provide compelling evidence for these. I believe that the manuscript presented is important, in particular given the impact that the earlier work seems to have had (e.g. influencing Dutch policy). I believe the manuscript should be accepted eventually for WES, but have formulated some comments and suggestions below

Major

1. Page 2, line 26: “the proposed limit of the capacity factor… ” This is maybe a bit philosophical, but possibly apart from the field of mathematics, all ‘theoretical’ limits in physics are based somehow on models. The difference is rather the validity/accuracy of the models used to construct the limit, and maybe it makes more sense to directly formulate it in these type of terms. In addition, the term ‘model limit’ is ambiguous and could point to a limit of a model (e.g. in its range of applicability) rather than a ‘theoretical’ limit on a physical process that was obtained from a model.
2. Page 2, line 29: “The plotted limit labelled as theoretical limit…. represents a normalized gross annual energy production (AEP), but this is not clear from the paper”. I’m not sure what your point is here, nor whether this is a criticism or a clarifying comment to the earlier work. Moreover, as far as I see, CF is by definition a normalized AEP?
3. Page 2, line 77: “The equation for $C_{f,0}$ …, is the analytical limit that Simão Ferreira et al (2026) propose, though this is not clearly stated in their work.” Looking at the work of Simão Ferreira et al (2026), I do not believe this is correct. Their “theoretical limit” is obtained using $x = \phi = U_r/(U_0\varepsilon)$, with ε the estimated finite wind-farm losses (see discussion on page 2 and 3 of their paper). A more obvious (possibly well-known/trivial in the resource assessment world) theoretical upper limit would be the evaluation of equation 3 given $x = U_r/U_0$, so using $\varepsilon = 1$. This simply means that we do not expect turbines or farms to be able to speed up the flow while still extracting energy. Given a wind distribution and rated wind speed, the CF naturally follows from the rated power (and C_P). I believe it would be relevant to make this point more explicitly, and possibly add a curve to the figures that corresponds to this more obvious theoretical maximum
4. Page 4, line 87: the assumption of constant thrust coefficient in the Frandsen model is problematic for above rated conditions. Above rated, CT drops with wind speed, so that the infinite wind-farm loss in Eq 4 should gradually approach one. This another major issue in the model that could be pointed out more explicitly

5. The use of the Frandsen model is questionable, and the model is definitely not strongly validated. First of all, follow-up work by Calaf et al (Phys. Fluids 2010) has shown that the Frandsen models significantly underestimates the wind-farm induced surface roughness. More importantly though, both the Frandsen model and Calaf et al.'s extension are based on the assumption that the wind-turbines are in the surface layer of the boundary layer (so $z_h < 0.1H$ with H the BL height). Given z_h in the order of 200m for modern large turbines, this would require $H > 2000$m, which is rarely the case for offshore conditions (because of free atmosphere stratification effects). In practice, wind-farm blockage will start playing an important effect – this could or could not be more detrimental the power reduction predicted by the Frandsen model. However, the main point here is that the use of the Frandsen model is extremely heuristic at the least, and far from it's expected range of validity
6. In my opinion, the choice of $f_{loss} = 0.9$ is very poorly documented (Simão Ferreira et al. point to a technical report by Mortensen et al., 2015, which only provides some very vague estimates, with 10% being the upper range of the losses). This could be better discussed
7. Line 212: "While the model … and infinite wind-farm wake losses". The model can provide an upper limit for no wake losses (this was possibly not pointed out by the original authors), but it cannot provide a limit for the infinite wind-farm cases (cf. point 5 above)

Minor

- Abstract: "wake loss of an infinite wind farm" → "turbine wake losses in" (to avoid ambiguity with farm wakes which is another hot topic of research nowadays)
- Page 2, last line: analytical model cannot account for -> does not account for
- page 3, line 75: define A. At this point, it may be useful to directly also equate x to U_r/U_0
- line 100: wake loss for a finite wind farm -> (cf. minor point 1) + I would expect the wake loss to represent a power loss (rather than a velocity loss as implied by the definition of epsilon)
- line 120 - $M_{rows} = 2.5$. What does this mean?
- Line 141: "and this is related to the fact that a concave polygon is mathematically undefined". Not sure what you mean by this, and if so, how is step 1 and the algorithm on line 125 then well posed?
- Line 185: define A_w