I commend the authors for the extent of their revisions and the detail and rigor of this manuscript. It is clear that the authors conducted a significant amount of analysis (and all of the figures are very sharp and easy to read), and this work will be an excellent contribution to the analytical wake modeling literature. I recommend this manuscript for publication with some minor revisions, as noted below, concerning some clarifying remarks and the summary of the contributions in Sections 4 and 5.

Line 249: Is this ratio L/R = 0.9 similar to the other references (DiDonato and Jarnagin, Ali, Cheung) that use this circle-rectangle analogy? It would be helpful for the reader to know if the authors' reasoning is consistent with the other similar methods in the literature.

Line 277: I believe the empirical expression for the wake expansion rate k* = 0.3837*TI + 0.003678 comes from this Niayifar and Porté-Agel paper (https://www.mdpi.com/1996-1073/9/9/741) rather than the 2014 Bastankhah and Porté-Agel one referenced by the authors.

Figure 2, Line 293: Is there any hypothesis for why the analytical predictions for the circular disk break down for rho=0, and why the rectangular integral performs better? Also, this trend is visible for x/D = 6-10, not just at 10D downstream.

Figures 2-3: Would the authors consider combining Figures 2 and 3, as they have done with the data in Figure 4? I'm wondering if it would be easier to compare the rectangular and circular disks if they are plotted on the same figure. If the authors find that this presentation would be too cluttered, I think it is fine as is.

Section 3: There are a few points throughout this section where the authors refer to the accuracy of the analytical predictions relative to the numerical predictions (for example, "high accuracy" at line 322), and I think these claims could be supported by some quantitative information to clarify to the reader what the authors consider to be "accurate" versus "inaccurate": perhaps the maximum relative error of the rotor-averaged deficit predictions? I know that the authors do look at RMSE in Section 3.7 but it is specifically for different resolutions and distributions of averaging points.

Section 4: Overall, I find this section to be a bit repetitive considering the amount of detail and discussion throughout Section 3. There are some new ideas presented here: the modified definition of the coordinate frame in Eq. 37, the physical meaning of different averaging orders in line 453, the superposition of multiple wakes discussed around line 493, the partial waking discussion at line 516 (also mentioned in my next comment), and the nacelle wind speed deficit at line 531. However, much of the text that I haven't highlighted here is a summary of the results presented immediately before in Section 3. My suggestion is to expand Section 5 to include much of this summarization, which would streamline the ideas presented in Section 4 and make the paper feel less dense towards the end.

Line 514: I think two ideas are being combined here. First, there is the definition of the wake velocity deficit and the wind speed used for normalization: zero velocity deficit means that the local wind speed is assumed to be equal to the wind speed at the location of the upstream turbine. The authors point out that this assumption—which applies to both averaging techniques—could be invalid for wind farms with heterogenous flow fields. Second, there is the "partial waking" effect, which traditionally refers to how the wake region only partially overlaps with the rotor swept area (https://wes.copernicus.org/articles/7/433/2022/), leading to a non-uniform distribution of velocity across the rotor. The averaging process across the rotor swept area is very important in these cases of partial wake overlap. I don't fully understand the connection the authors are trying to make in this paragraph—how exactly does the wake deficit normalization factor relate to the partial waking issue?