

# Reviewer 1 response

We wish to thank the referee for their helpful and constructive comments, and their suggestions have helped improve the quality of the manuscript. Below please find our responses to the reviewer’s questions and comments.

## Main comments

1. **Introduction, Line 45:** The authors write: **In the WakeBlaster model, a single streamwise momentum equation is solved by advancing 2D planes of the velocity field, and the introduction of wakes is accomplished through direct manipulation of the velocity profiles. However this limits the ability of the model to handle veered or stratified inflow conditions.. I agree regarding the wind veer, but a stratified inflow following MOST could still be handled by a single streamwise momentum equation, as the Wake Blaster model employs. I recommend the authors to rewrite this statement.**

The updated manuscript has revised the introduction so that the sentence now reads: “However this limits the ability of the model to handle veered inflow conditions.”

2. **Section 2.1:** The authors write: **Previous studies have shown that the k epsilon model can accurately simulate stratified ABL conditions (Alinot and Masson, 2005) and was also successfully used in prior simplified models for wake dynamics (Cheung et al., 2025). However, Alinot and Masson (2005) applied MOST to the k-epsilon model, which represents the atmospheric surface layer, not the atmospheric boundary layer.**

In sections 2.1 and 2.2 of the revised text, we have clarified that the the work of Alinot and Masson (2005) applies to the atmospheric surface layer.

3. **Section 2.1:** The performance of the Durbin time-scale limiter for the application of wind turbine wakes is quite sensitive to the ambient turbulence intensity, as shown in van der Laan and Andersen [2018]. Hence, a general calibration of the model with LES for a wide range of cases may be difficult.

The current implementation of the parabolized RANS formulation includes some features which have been previously considered in other  $k - \epsilon$  RANS wake models [2], but can be modified depending on the desired range of conditions. We consider this study as a proof-of-concept for the parabolized formulation with an actuator disk, and the RANS closure model itself can be substituted with alternate, more robust models in future studies. The underlying calibration process, turbine models, and solution process should remain unchanged. In section 2.1 of the updated manuscript, we have included additional discussion emphasizing this point, and included references to van der Laan and Andersen [6] for additional details.

4. **How is the TI defined in Table 1? Is based on the TKE? Please clarify as different TI definitions are used in the wind energy community.**

Table 1: Input parameters for the low and high WS stable atmospheric cases.

Parameter	Low WS	Med WS
Surface roughness $z_0$ [m]	0.0001	0.00004
Surface heat flux $q_w$ [W/m <sup>2</sup> ]	-1.5	-6
Wall temperature $T_w$ [K]	284.15	283.3
Obukhov length $L$ [m]	500	275
Friction velocity $u_*$ [m/s]	0.175	0.2125
Lapse rate $\Gamma$ [K/m]	0.0	0.002

In the updated manuscript, we have clarified that this calculation of TI in the table is based on the standard deviation of the hub-height horizontal velocity, non-dimensionalized by the mean of the hub-height horizontal velocity. Additional details on the atmospheric conditions can be found in the work of Brown et al [1], and Frederik et al [4].

**5. What is the domain height in the RANS model? Is it 300 m?**

At the end of section 3 of the updated manuscript, we have included the vertical domain height used in the RANS calculations, which is 400 m.

**6. There are several undefined parameters. What are the values of the roughness length, Obukhov length, friction velocity, wall temperature, heat flux, and specified lapse rate for the RANS model, for both inflow cases? What is the chosen value of the von Kármán constant?**

A table with the details on the input parameters used in the RANS inflow cases is included in the updated manuscript, and duplicated here as table 1. In all RANS simulations considered here,  $\kappa=0.42$ .

**7. Section 2.2: What are the lateral ( $y = 0$  and  $y = Ly$ ) and outflow boundary conditions ( $x = Lx$ )?**

In section 2 of the updated manuscript, we have provided the lateral boundary conditions used in the RANS model, which are:

$$u(z) = u_0(z), \quad v(z) = v_0(z), \quad w(z) = 0, \quad \Theta(z) = \Theta_0(z), \quad (1a)$$

$$\frac{\partial k}{\partial y} = 0, \quad \frac{\partial \epsilon}{\partial y} = 0, \quad \frac{\partial p}{\partial y} = 0. \quad (1b)$$

at the  $y = y_{min}, y_{max}$  locations. Because the RANS model uses a parabolized version of the governing equations, no outflow boundary should be provided, as the streamwise  $x$  direction becomes time-like in the marching process.

**8. Section 2.4: The use of a uniform thrust load distribution typically leads to an overestimated velocity deficit compared to a more realistic force distribution (as for example the model from Sørensen et al. [2020]). This is because a realistic force distribution has a low force near the blade root**

**leading an additional wake recovery, which is not captured by a uniform load distribution, see for example Simisiroglou et al. [2017].**

We agree with the reviewer that the uniformly loaded turbine model described in section 2.4 is not a realistic depiction of actual turbine loads when compared with more sophisticated models, and does not adequately handle situations where more complex turbine control strategies may be required, e.g., during peak shaving or in conditions past rated. The differences due to the uniform blade loading are clearly visible in the near wake profiles for  $x/D \leq 2$ , where the centerline wake deficit is overestimated, and there is no expectation that the uniformly loaded turbine model would accurately capture this region.

While this current model is overly simplistic compared to the LES turbine representation, the formulation presented in this work is sufficiently general to allow for future implementations of more capable actuator disk models, including those based on BEM theory and those connected to more realistic turbine control systems, e.g., ServoDyn. However, within the scope of the current work, the downstream wakes from the uniformly loaded disk model is able to demonstrate reasonable agreement with higher fidelity approaches, and is an improvement over previous studies who used an assumed wake shape or directly superimposed a velocity deficit on the wake profiles, which is harder to generalize. In future work, we hope to expand the fidelity of available actuator disk models in SANDWake3D, and capture additional features missing in the current study.

- 9. Section 2.5: The calibration involves two single wake cases from LES that differ in wind speed (6.52 and 9.05 m/s). However, these inflow wind speeds correspond to nearly the same thrust coefficient, namely, 0.83 and 0.8. Furthermore, a similar ambient inflow TI of 3.1-3.6% is used. Therefore, the calibration is covering a very limited space. I recommend to perform a calibration with LES single wakes for a range of thrust coefficients and ambient TI levels; e.g.  $CT=0.5-0.8$ ,  $TI=0.04-0.15$  (corresponding to offshore and on-shore conditions). The author could also consider to use different stability conditions/ wind veer in the calibration as the focus of the present work is the inclusion of more complex atmospheric physics.**

For the purposes of an initial demonstration of the parabolized RANS approach, its calibration process, and the included actuator disk model, the range and variety of atmospheric conditions were intentionally limited, and used on previous LES cases where the wake behaviors were well-studied. The selected inflow conditions did not exceed the rated wind speed of the IEA 15MW turbine, as the blade tip loads in region 3 differ significantly from those in region 2. This blade loading would not be accurately captured by the uniformly loaded actuator disk model (see discussion in question 8 above), and also affect the initial wake profiles, so including these cases in the calibration procedure would unfairly skew the process as changes in the RANS coefficients may attempt to compensate for limits in the turbine model representation. Once additional actuator disk models are implemented in this formulation, we hope to widen the range of applicable wind speeds and thrust coefficients.

Despite these constraints, recent studies using the SANDWake3D model have shown promise in terms of its applicability to additional atmospheric conditions. In section 4.2 of the revised manuscript, we demonstrate how the model can be applied to an onshore wind turbine in a different inflow condition. Without changing the calibrations of  $C_\mu$ ,  $C_{1\epsilon}$ ,  $C_{2\epsilon}$  in section 2.5, and only adapting  $C_k$  for the ambient turbulence levels, the wake for a NREL5MW turbine in a 11.4 m/s slightly convective atmospheric boundary layer with no appreciable veer was reasonable well captured (see figure 1).

10. **The title includes atmospheric boundary layer, but MOST is used as inflow which represent the atmospheric surface layer. Therefore, I recommend to use the latter instead.**

In the revised manuscript, the title has been modified to “SANDWake3D: A 3D parabolic RANS solver for atmospheric surface layers and turbine wakes”.

11. **There are several model choices that are inconsistent with the chosen RANS equations. This means that the inflow is expected to develop downstream due to an imbalance with the inflow, which is based on my experience with elliptic RANS models:**

**It is unclear how the wind veer is modeled in the proposed RANS formulation, as the inflow model is based on MOST, which does not consider wind veer. A Coriolis force is neither considered in the momentum equation. Do the authors directly use the wind veer from the LES model to rotate the MOST profiles? If this is the case, then the applied RANS equations are not balance with the inflow model. For elliptic 3D RANS models, it is common to perform a 1D precursor simulation (height is the only spatial variable) to determine the inflow for the wind farm successor simulation.**

In the updated manuscript, the discussion regarding the modeling of the wind veer in the ABL inflow and the RANS equations has been clarified. As opposed to AMR-Wind or other similar LES approaches, it is not possible to use the same parabolized RANS code to generate a precursor and also solve for the turbine wake field. Hence, a separate source for the ABL inflow profiles must be considered – for instance, a 1D precursor simulation as the reviewer suggested. For the purposes of the current study, we use the Monin-Obukhov solutions supplemented to a veer profile derived from the precursor LES solutions, as this allows us to ensure that the wake-veer interactions can be compared as consistently as possible.

The revised manuscript now also includes a discussion of the Coriolis force in the formulation of the parabolized RANS equations. The Coriolis forcing term can be included in a straightforward fashion as body terms to equations (1b) - (1d), and included in the solutions without any numerical difficulty. However, for the cases considered here, the Coriolis body forces are not expected to cause a large deflection of the wake over the distances of interest, as the Rossby numbers are relatively large ( $Ro = U_\infty/(f_c D) \sim 290 - 400$ ). This is consistent with the observations of Keck and Howland [5]). Note that any wake deflections due to the Coriolis are also in addition

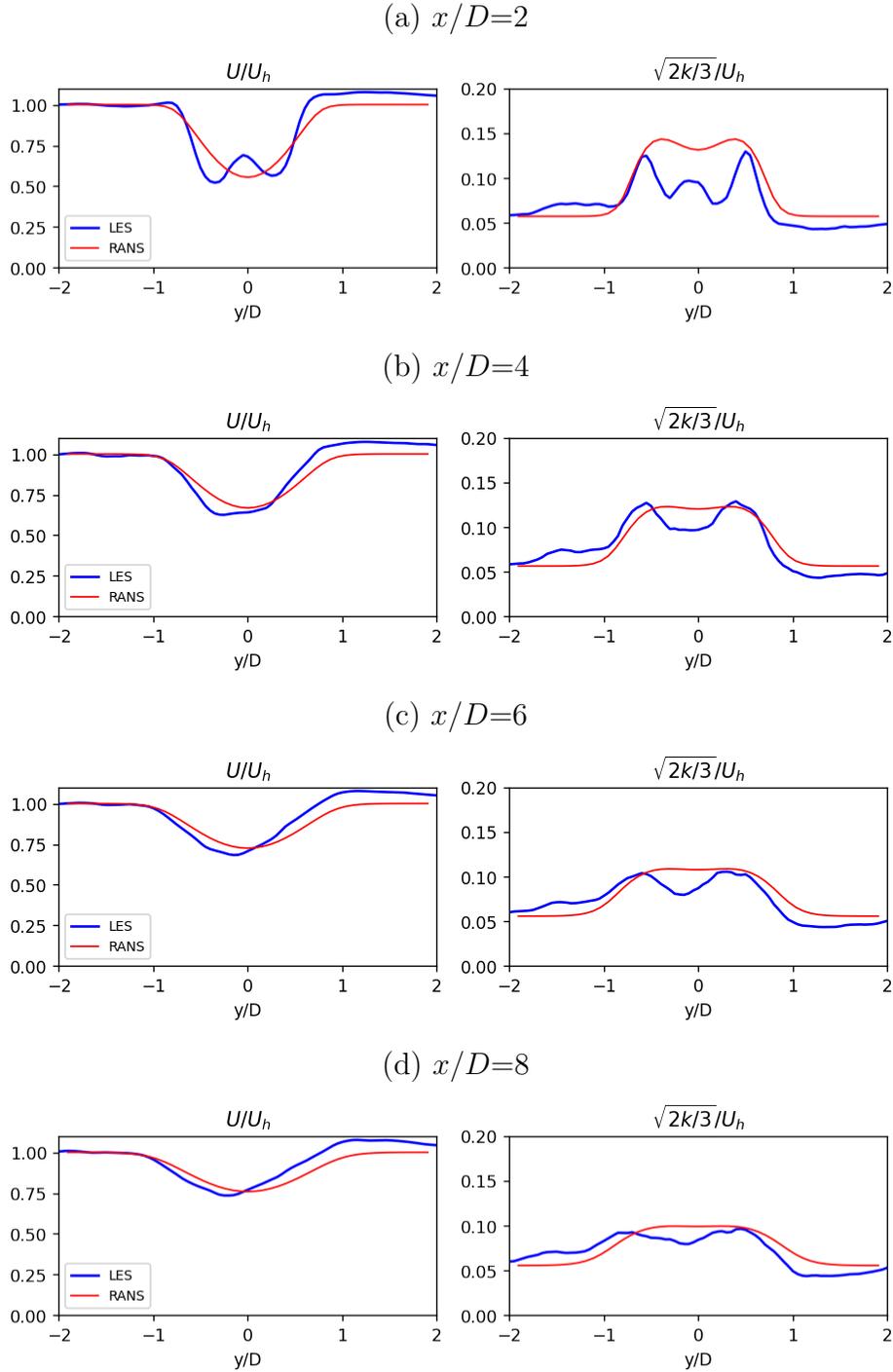


Figure 1: Comparisons of the hub-height velocity and normalized TKE profiles between RANS and LES for the NREL5MW turbine in a slightly convective 11.4 m/s inflow condition.

to the wake-veer interactions, which is naturally due to the presence of wind veer, of course.

**Equation 13: Why is  $C_k$  used here?** The MOST  $k$  profile is normally defined as:  $(u_*^2/C_\mu)\sqrt{\phi_m/\phi_\epsilon}$ . Hence  $C_k$  is related to  $C_\mu$ :  $5.48C_k = 1/C_\mu$  and is not an independent parameter. If one changes  $C_k$ , then one sets effectively a different  $C_\mu$ . In addition, one other turbulence model constant needs to be adjusted to make the RANS+ $k$ -epsilon model in balance with a MOST, see for example Richards and Hoxey [1993]. Following the calibration from Sect. 2.5, the effective eddy viscosity coefficient is  $C_\mu = (1/(5.48C_k))^2 = (1/(5.48 \times 0.72))^2 = 0.0642$ , which is not equal to the reported value of 0.076, and means that the RANS model is not in balance with the inflow.

In the revised manuscript, we have clarified that the presence of the  $C_k$  parameter is to adjust the RANS TKE to be roughly in line with the LES TKE magnitude at the turbine hub-height. Because the behavior of the  $k-\epsilon$  RANS model may be sensitive to the magnitude of TKE, and direct comparisons of the TKE profiles with the LES are desired, the  $C_k$  parameter was used to allow the two to be judged consistently. As the reviewer notes, this leads to a possible imbalance between the inflow profiles and the RANS solutions, but any development of inflow RANS without turbines is relatively minor (see below).

**The MOST model of Alinot and Masson (2005) is not in full balance with the RANS equations as the parametrized turbulence model constant (Eq. 5) is only valid up to a certain height, see van der Laan et al. [2017]. Hence, if one has a large vertical domain, then this model is no longer valid.**

The authors agree with the reviewer that the Monin-Obukhov similarity profiles are only applicable in the surface layer, and do not apply in cases with large vertical domains with temperature inversions and other phenomena. In the cases considered here, the profiles derived from Monin-Obukhov similarity theory provide reasonably accurate results up to the upper rotor tips of the IEA15MW turbine (at  $z=270$  m), and thus may be applicable to a large variety of cases for onshore or offshore turbines. In other situations with low-level jets, these inflow profiles would not be valid, however, the parabolized RANS formulation could still be used assuming suitable inflow conditions were derived.

**The use of an active temperature equation in combination with a nonlinear temperature profile will not result in a steady-state solution. Hence, the inflow will develop downstream.**

**If the authors decide to keep their model choices, then I recommend to include a result without turbines to show the downstream development of the inflow, preferable for a large horizontal domain in the x-direction, e.g. 50 km.**

In tests where the inflow was evolved without the presence of any turbine (Figure 2), the velocity profiles for the Low WS and Med WS cases were shown to have minimal changes over the span of 20D (4800 m).

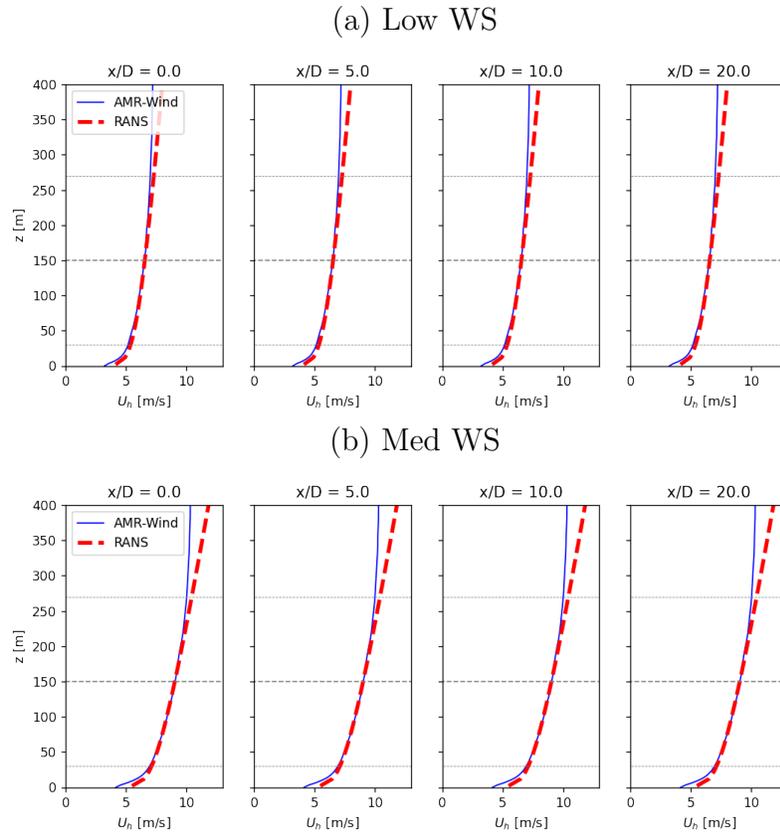


Figure 2: The streamwise evolution of the inflow profiles, without the presence of a turbine wake, for the (a) Low WS and (b) Med WS conditions over a distance of 20 diameters (4800 m).

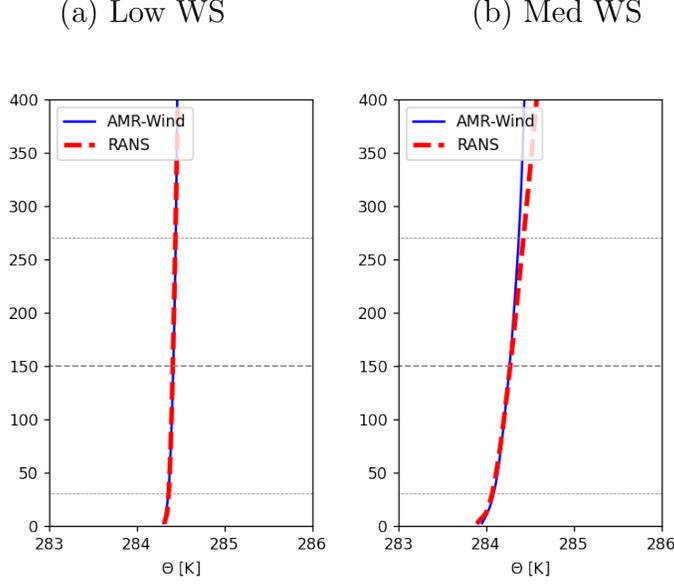


Figure 3: The comparison of the inflow temperature  $\Theta(z)$  profiles between RANS and the AMR-Wind LES computations.

12. **Figure 2: How does the RANS inflow temperature profile compare with the LES results for both inflow cases?**

A comparison of the RANS and LES inflow temperature profiles has been provided in figure 3 of the revised manuscript, and is included here as Figure 3. The agreement between the temperature profiles is consistent with the comparison of the velocity profiles, particularly in the rotor disk region of the domain.

13. **Figure 5: At first sight the difference between the low and medium WS cases look significant. However, this is because the dimensional TKE is plotted. I would recommend to normalize the TKE (k) by the ambient inflow velocities to avoid such confusion. For example, one could use  $\sqrt{2k/3}/U_{inflow}$ , to get a TI-representative value while not mixing the result with the velocity solution. I expect that the results will be similar between the two cases. This also applies to all other figures where the TKE in dimensional form is plotted. In general, I recommend the authors to normalize all velocity and TKE plots by their respective inflow values.**

In the updated manuscript, all TKE values have been normalized so that the quantity that appears in the plots is  $\sqrt{2k/3}/U_h$ .

14. **The small wind farm case consisting of 9 turbines took 4 CPU min or 4/3600 CPU hours. Assuming one 1 CPU is used, as it is not mentioned that the code is parallel, then an annual energy production calculation takes  $4/3600 \times 360 \times 22 = 8.8$  hours. This makes the model difficult to use for wind farm optimization purposes.**

Improving the performance of the parabolized RANS method is an ongoing task, and several options are available to increase the computational efficiency. As the reviewer noted, the current code is serial and does not take advantage use of any multi-processor optimizations or any GPU capabilities. In any wind farm setting, we expect some level of domain decomposition to be possible, so that the entire 2D plane does not need to be solved for at once in order to advance the solver. Such implementations would allow for much larger domains or much faster computations for commercial use. However, note that even in its current form, the calculation of wind farm’s performance across a range of wind speeds and directions is an embarrassingly parallel process with low memory overhead, so it is possible to use this code on a cluster or a multiprocessor desktop/laptop without having to wait for sequential runs to finish.

15. **The streamwise pressure gradient is included in the streamwise momentum equation (Eq. 1b). Does this mean that the turbine induction is included? In other words, how does the upstream flow of a single wake look like, for example at 1D upstream?**

In the parabolized RANS formulation, the propagation of information and flow disturbances in the upstream direction is disallowed, meaning that for any streamwise position  $x$ , the presence of any downstream turbines has no impact on the current solution. Consequently, the turbine induction effect is not included in this formulation, and must be calculated separately. One possible approach is to consider the Green’s function formulation for induction in Cheung et al [3], and superimpose the 3D induction field on the RANS solution after each turbine has been encountered. Such a combined approach may be able to capture the elliptic induction effects while retaining the computational efficiency of the current method.

16. **I miss a discussion on some of the model limitations. For example, a parabolic RANS model cannot capture blockage and speed up effects. Effects of an ABL height are neither modeled.**

In the revised manuscript, additional discussion has been included regarding the model limitations, including the blockage effects and ABL considerations. In sections 2.1 and 6, we discuss the turbine induction field:

Note that this RANS formulation eliminates the ability for flow information to travel in the upstream direction, as the parabolization process removes any elliptic behavior of the solution. This means that the effect of the turbine induction field is not included in these calculations, so any flow slowdown or acceleration due to blockage effects will be missing. However, superimposing the turbine induction field onto the RANS solution may be possible, as discussed in section 6.

Future work may also improve on the current parabolized RANS solution by superimposing the turbine induction solution ahead each rotor position. One possibility is to calculate the induction field using a Green’s function approach [3], and correcting the upstream solution for any slowdown or acceleration effects.

In the conclusion section, we also now mention

Additional work may also focus on using alternate inflow profiles, rather than profiles derived from Monin-Obukhov similarity theory. This may allow for more complex atmospheric inflows to be considered, including those with low-level jets, temperature inversion layers, and other such phenomena.

## Minor comments

### 1. Equation 1d: Brackets seems to be missing at the right hand side terms.

In the updated manuscript, equation (1d) has all appropriate brackets on the right hand side.

$$u \frac{\partial w}{\partial x} + v \frac{\partial w}{\partial y} + w \frac{\partial w}{\partial z} = -\frac{1}{\rho} \frac{\partial p}{\partial z} + \frac{\partial}{\partial y} \left( (\nu + \nu_T) \frac{\partial w}{\partial y} \right) + \frac{\partial}{\partial z} \left( (\nu + \nu_T) \frac{\partial w}{\partial z} \right) + g\beta(\Theta - \Theta_0) + f_{T,z} + f_{COR,z} \quad (1d)$$

## References

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