We appreciate the referee's careful review of this manuscript and constructive comments that we have used to improve the quality of the work. We provide the referee comments in italics and response in standard font. Proposed manuscript changes (if substantive) are in color and describe changes made in response to each comment or groups of comments.

Referee Comment: *The manuscript presents an analytical model of the forces acting on an airfoil as it undergoes simultaneous pitching and plunging. This model is a linear combination of the Theodorsen function and the Sears function. The model results are validated against numerical simulations of a NACA 0012 airfoil.*

Unfortunately, I do not believe that the manuscript warrants publication in Wind Energy Science, as it lacks novelty and scientific insight. The model is a linear combination of two decades-old analytical models, the Theodorsen function for a pitching airfoil and the Sears function for a plunging airfoil. In the introduction, the authors nicely list the work that has been done in this field over the past decades. It is rather trivial that such a linear combination yields a reasonable description of combined pitching and plunging in cases where non-linear effects are small.

We generally agree with the referee's summary of the scope of the work. As we hope to emphasize in the remainder of the response, studying the superposition of transverse-gust and airfoil-oscillation disturbances, even with thin airfoils, is relevant and important to the wind energy community. It may not be surprising that the combination of the Sears and Theodorsen models yields accurate predictions for small-amplitude combined disturbances, but given the wide range of reduced frequencies and amplitudes we examine in this study, we believe it is still useful for establishing the utility of such a modeling framework and coupling it to extant models.

Referee Comment: *As the authors point out, the model fails when non-linearities become important, which is stated to be outside the scope of this work. However, this is exactly the regime that would have been interesting to model. In addition, the manuscript does not provide insight into the flow physics to explain the observations provided herein. Instead, the authors vaguely allude to viscous effects and flow separation, but many of the explanations are postulative and unconvincing. While it may be true that empirical models like Leishman-Beddoes provide less physical insight than analytical ones, the model presented herein breaks down for more complex flow behavior, whereas the parameter space well described by the model is also already well understood, so that little novel insight is provided.*

Furthermore, the authors state that simplifying assumptions used for the numerical simulation limit its applicability to the small-amplitude perturbation regime. This however *limits the ability of the numerical simulations to serve as validation for the analytical model, since a validation should reveal when these assumptions break down. Currently, both the analytical and numerical approaches in this manuscript rely on major assumptions that do not hold true for real wind turbines, but no reliable validation is provided to evaluate these assumptions.*

While we agree with the referee that the model and numerical setup both make simplifying assumptions that may limit their immediate applicability, we disagree that such assumptions render this study irrelevant for wind-energy applications. Despite existing bodies of literature on gusts and airfoil oscillations, the parameter space of superposed disturbances of both gusts and airfoil oscillations remains very much unexplored. For example, a newly published experimental study by Feng and Wang (2024) examines pitching motions of a NACA 0012 airfoil in a sinusoidal transverse gusts and finds good agreement between measurements and potential-flow models, so it is not a settled fact that the combined effects of two linear phenomena are well predicted when flow is governed by formally nonlinear equations.

Furthermore, many experimental studies in unsteady aerodynamics, including the aforementioned work, are done with relatively low Reynolds numbers (often on the order of 10,000 for water-channel experiments) and reduced frequencies (limited by actuators). By contrast, our simulations allow us to reach higher Reynolds numbers and reduced frequencies difficult or impossible to achieve experimentally. These are closer to the regime of real wind-turbine blade sections than many other fundamental aerodynamics studies, which are often geared towards applications in biological propulsion or light aerial vehicles.

We considered it most appropriate to first focus on the small-amplitude limit where classical analytical models might be useful, so as to confirm that the theoretical framework provides accurate physical insights, before moving on to viscous and nonlinear effects. Simplifying both the theoretical and numerical approaches allows us to disambiguate these dominant underlying physics. Therefore, we believe the physical insights presented in the current work are quite relevant to the wind-energy community, even if the parameter space and assumptions involved do not exactly match those of real wind-turbine blade sections.

In our revised manuscript, we have better clarified the implementation of and relevance of our work to the wind-energy community and offered more commentary on limitations and advantages of our chosen problem setup.

Referee Comment: *Could you comment on the importance of the center of rotation, specifically pitching around the quarter chord vs around another point? For real wind turbines, what would be the best approximation of the rotation point?*

The Theodorsen model includes the center of rotation as a free parameter. In real wind turbines, the rotation point for a blade section will depend on the aeroelastic characteristics (e.g. bending and twisting) of the turbine blade from the blade root up to the section. As our focus in this study is to investigate aerodynamic loads and not structural deformations, we chose the quarter-chord point as a convenient reference for the airfoil-oscillation kinematics. Since the Sears problem represents the effects of a convective gust, it should be unaffected by the choice of center of rotation.

Referee Comment: *Could you comment to what extent it is possible or appropriate to correct your model for effects like airfoil thickness, camber, non-zero mean angle of attack and finite span? All of these are crucial in moving away from the idealized case to real application. In particular, could you comment on 3D effects and the extent to which this model holds for real wind turbines given that their blades have finite length and radially varying chord and inflow velocity vector?*

This work considers a thin NACA 0012 airfoil consistent with a potential flow setup and investigation of pre-separation behavior. Corrections already used in the literature can generalize thin-airfoil findings to a broader class of shapes: this strategy mirrors the development of the Beddoes-Leishman model for dynamic stall, which is widely used in the wind-energy community and which was originally derived for thin airfoils. We cite the work of Lysak *et al.* (2013, 2016) on thickness corrections in the manuscript (*cf.* lines 48, 378-379, and 422-423 in the original manuscript). The extension to the Sears function by Goldstein and Atassi (1976) and Atassi (1984) has been shown to account for the effects of camber and non-zero mean angle of attack (*cf.* Cordes *et al.*, 2017). Corrections for 3D effects have also been explored by Massaro and Graham (2015). Such literature demonstrates that it is possible to adapt the basic linear framework used in our study to scenarios more representative of real wind turbines and our future work may examine these corrections as a path towards a complete, all-inclusive model for blade design.

Specifically regarding 3D effects, our approach conforms to the assumptions of blade-element analysis that are commonly used for wind-turbine design and analysis. In these formulations, corrections can be added for tip losses, but generally the aerodynamics are parameterized using independent 2D blade sections, and radial variations (*e.g.* in chord length or inflow velocity) are accounted for separately for each section. Therefore, the extent to which our proposed unsteady modeling framework can account for 3D effects matches that of traditional BEM approaches.

Referee Comment: *Could you elaborate on what you mean when you say the Reynolds number is "low enough so that the nonlinear effects of high Reynolds-number turbulence are limited"? What are these Reynolds number effects you expect to not be* *present, and to what extent are the simulations applicable to wind turbine blades, given that real blades operate at Re_c about an order of magnitude higher than your study?*

We agree with the referee that the wording of this sentence was unclear, and we recognize that our simulation Reynolds numbers are lower than those in utility-scale turbines. Most experimental measurements of unsteady aerodynamics, including the data used to validate our simulations, can only access $R \approx O(10^5)$. There are indeed differences in airfoil stall characteristics (both static and dynamic) that appear at higher Reynolds numbers, as shown by recent experiments done in a high-pressure wind tunnel (Brunner *et al.*, 2021; Kiefer *et al.*, 2022). However, for disturbances below the airfoil-stall limit, increasing the Reynolds numbers approaches the inviscid-flow limit.

We will clarify our wording and justification for our examined parameter range.

Referee Comment: *Why do you investigate reduced frequencies up to k = 4 when you state that the most extreme cases in the real world are k = 1? And why do you not investigate k < 0.2 if that is the range typically observed in the real world? It seems that your parameter space is not directly relevant for wind turbines.*

These higher reduced frequencies can be understood as a representation of higher-order fluctuations that a real turbine blade might encounter. Reduced frequencies of $k > 1$ could be expected for blades encountering atmospheric turbulence and experiencing gust-induced oscillations at similar frequencies. These could also represent higher-order modes from a lower-frequency disturbance or oscillation. They are therefore still relevant for real-world turbine-blade dynamics.

Since unsteady dynamics tend to increase in importance with increasing reduced frequency, we expect that a model that captures unsteady forces at higher values of *k* should perform at least as well at lower values of *k*. This is shown clearly in Figure 4, where even cases with gusts and airfoil oscillations as high as $k = 0.785$ show good agreement with model predictions, allowing us to test the limits of the framework.

We will add additional explanations for our reduced-frequency range.

Referee Comment: *You assume sinusoidal oscillations. Can you comment on how realistic this is and how feasible it is to use this approach for more complex oscillation patterns?*

For the transfer function approach in this work, sinusoidal oscillations represent basis functions for transforming from spectral to physical space. While “real” oscillations and gusts are rarely exact sinusoids, the linear nature of the approach allows for any number of superposed forcings over a range of frequencies. Hence, any disturbance that can be represented by a finite Fourier series should be captured by the model, provided the angle-of-attack amplitudes do not incite nonlinearities.

Referee Comment: *In section 4, you state that the gust is felt by different parts of the airfoil at different times. However, in an incompressible flow, the gust should be felt everywhere in the flow field simultaneously. Thus the explanation is not convincing.*

We agree with the referee that one of the tenets of incompressible flow is that the pressure field is governed by a Poisson equation and responds globally to any disturbance. The gusts we consider here, however, are purely kinematic and do not involve this kind of pressure coupling. A convective gust, as considered in the Sears problem, remains incompressible, but involves velocity fluctuations that travel along at the speed of the inflow. Therefore, the airfoil does not "feel" these effects until the front edge of this disturbance encounters the airfoil, creating a local change in the angle of attack. As the gust travels along the airfoil, different parts of the airfoil will experience different flow velocities based on the local phase of the gust as it passes by each point. Hence, the airfoil does indeed experience the gust in a time-varying manner.

We will clarify our wording in referring to the gust motion boundary condition.

Referee Comment: *In section 5, you state that the dominant source of error of the analytical model is flow separation and stall. However, if I understand correctly, your simulations do not have separation and stall, so how can those effects explain the discrepancy between the model and the simulations? In particular, you do not exceed the static stall angle in any of your numerical simulations, so flow separation should not be the source of discrepancy.*

Our 2D numerical simulations can capture flow separation and stall, though the modeling assumptions inhibit us from making quantitative conclusions about the onset of these eventually 3D phenomena. We note that flow separation and static stall are not the same thing. Even in static contexts, flow can remain attached to the trailing edge while a local laminar separation bubble is formed such that the airfoil does not undergo deep stall despite separation occurring on its surface. These dynamics are present in our simulations; while we may not be able to make quantitative conclusions about their behavior (since we are not doing wall-resolved simulations), it is relevant to hypothesize that these local separation dynamics will impact the accuracy of the model at intermediate angles of attack.

Referee Comment: *In the last paragraph of the manuscript, you discuss dynamic stall. This is an entirely different topic from what is covered in this manuscript. Certainly the model you describe here, if unable to model simple non-linearities in the superposition of pitching and plunging effects, would not be able to describe the non-linear dynamics involved in dynamic stall. Therefore, the connection to this topic here does not make sense to me.*

We agree with the referee's understanding that we do not seek to model dynamic stall in this manuscript. However, dynamic stall and the earlier-discussed flow separation are known difficulties in current models of blade section aerodynamics; our discussion aims to acknowledge that we are aware of the limitations of our approach and discuss prospective future phenomena to examine.

We will expand our flow separation and stall discussion in section 5 to clarify how we could address these phenomena.