Response to Reviewer Comments

We sincerely thank the reviewer for the thorough and thoughtful review of our manuscript. The detailed comments and suggestions have been extremely valuable and have led to significant improvements in the paper. We have carefully considered all comments and revised the manuscript accordingly.

The revisions include clarifications of the mathematical formulation, improvements in terminology and notation, additional explanations and figures, corrections of ambiguities identified by the reviewer, simplification of several sections and appendices, and updates to the references. We believe that these changes have substantially improved the clarity, consistency, and overall quality of the manuscript.

A detailed point-by-point response to all comments is provided below.

1. Top of page 2: Change "... their height derivatives (the atmospheric parameters are assumed to be horizontally uniform but vertically varying)." to "... their height derivatives. (The atmospheric parameters are assumed to be horizontally uniform but vertically varying.)" *Response: We have corrected the text as suggested.*

2. Page 2: While it is good that you now use the terms physical and numerical, there would be no harm in also mentioning that Hines (1973) used the terms "standard" and "nonstandard" for these two respective types of multilayer methods. *Response: We have revised the text as suggested and now also refer to the terminology of Hines (1973), namely the "standard" and "nonstandard" multilayer methods.*

3. Page 3, last paragraph: In the sentence that begins "This arises by transforming ...", "directions" should be changed to "direction" and "direction" should be changed to "directions". *Response: The text has been revised accordingly.*

4. Page 4, last paragraph of Section 1: The term "amplitude" used here and elsewhere seems ambiguous, since amplitude is commonly thought of as a real value, while the amplitudes referred to here are complex. Perhaps it should say "complex amplitudes". I would call them mode values. See Knight et al. (2022, eq. 2.3), for instance. *Response: We have revised the text as suggested and now refer to these quantities as "complex amplitudes (hereafter referred to simply as amplitudes)."*

5. Page 5: Make h lowercase in "density scale Height". *Response: We have corrected the text as suggested.*

6. Page 6 following eq. (6): Where it says that the propagation matrix has altitude-independent elements, it should be mentioned that this applies to a thin layer. *Response: We have revised the text and now use the formulation: "...and* A *is the propagation matrix with altitude-independent elements within a thin atmospheric layer."*

7. Top of page 8: Please provide a reference for the terminology "viscosity-wave mode" and "thermal- conduction wave mode". You designate the first dissipative root (closest to the gravity-wave root) "viscosity" and the second one "thermal conduction". Why should it not be the reverse, with the first one "thermal conduction" and the second one "viscosity"?

8. Page (8), eq. (20): Less-than-or-equal ($\leq$) symbols should be used instead of $<$ here. The only situation in which $<$ can be guaranteed is when the imaginary frequency shift is known to be sufficient, and that only applies to the two gravity-wave roots. *Response: The strict inequality signs have been replaced by non-strict inequalities ($\leq$) throughout.*

9. Page 9, first paragraph of Section 3: Change "denote" to "denoted" in "... are denote by ...". See also the note for page 4 on amplitudes. *Response: The text has been corrected accordingly.*

10. Page 10, eq. (33): It says $z_{l+1} \leq z \leq z_l$. Should l be changed to $l+2$? *Response: We have corrected Eq. (33) by replacing l with $l+2$.*

11. Page 10, last line: "Amplitude" occurs again here. *Response: The term "complex amplitude" is now used at its first occurrence, and the present occurrence follows the convention introduced there.*

12. Page 11: Normal modes generally have zero phase speed. Perhaps it would be better just to say "mode". *Response: We have revised the text accordingly and now use the formulation: "In this way, a pure mode is injected into the system."*

13. Page 11, eq. (44): The variable z occurs in the exponent. Perhaps something should be subtracted from it, like z_l , so the variation near z_l can be described. *Response: We have corrected Eq. (44) by replacing z with $z - z_L$ in the exponent.*

14. Page 12 after eq. (47): This would be clearer if it said "for one particular q value" at the end of the sentence. *Response: We have revised the text as suggested and now use the formulation: "...and selecting the qth component as that used in the normalization condition..."*

15. Page 12: The alternative type of lower boundary condition becomes problematic when the kinematic viscosity is very small, i.e., below altitude ˜100 km. In this altitude range, conditions are essentially inviscid, and the large imaginary parts of the dissipative dispersion-relation roots (or, equivalently, the real parts of the eigenvalues), lead to roundoff error in the computation of the three upgoing mode values at the lower boundary. This problem is explained in the last paragraph of Section 4.3 in Knight et al. (2021). The solution is to let the dissipative mode values be zero at the lower boundary and to lower the dimensionality of the lower boundary condition so that only the value of a state variable is specified and not its first two derivatives. Please mention this issue here and refer to Section

4.3 of Knight et al. (2021). *Response: We have revised the discussion of the alternative lower boundary condition and now explicitly address the numerical difficulties that arise when the kinematic viscosity is very small (corresponding approximately to altitudes below 100 km). Following the discussion in Section 4.3 of Knight et al. (2021), we explain that the large dissipative eigenvalues may lead to severe roundoff errors in the computation of the dissipative-mode amplitudes from the Vandermonde system. We further note that, in this regime, a more robust approach is to set the dissipative-mode amplitudes to zero at the lower boundary and retain only the ascending gravity-wave mode, as recommended in Knight et al. (2021).*

16. Page 12, eq. (52): For multiplication of a column vector, **a**, from the left, B should be indexed by k, m, not m, k. The same applies to eq. (53) and maybe elsewhere. *Response: We have corrected Eqs. (52) and (53).*

17. Page 13, just after eq. (55): It may be problematic to set $\mathbf{b}_1 = [1, 0, 0]^{\mathrm{T}}$ when kinematic viscosity is very small at the lower boundary. In this case, the dissipative mode values will have to be small under realistic conditions, but they will have to be set to large nonzero values in order to realize the three-dimensional lower-boundary condition. It might be preferable to use the approach described above, where only the state variable value is specified and only the gravity-wave mode is used. *Response: We thank the reviewer for this observation. This point is essentially the same as that raised in Comment 15. The revised manuscript now discusses the numerical difficulties associated with the boundary condition $\mathbf{b}_1 = [1, 0, 0]^T$ in the nearly inviscid regime and refers to Section 4.3 of Knight et al. (2021). We also state that a more robust approach in this situation is to retain only the ascending gravity-wave mode and set the dissipative-mode amplitudes to zero at the lower boundary.*

18. Page 13, item 3: A reference for LAPACK should be included. *Response: We have included a reference to LAPACK.*

19. Page 13, item 3: It would be better just to cite Knight et al. (2022) here. Although the approach in Knight et al. (2019) can be understood as a special case, eigenvectors are not explicitly mentioned and only implicitly occur via the Vandermonde matrix. There is a related issue, which is that here there is normalization by the absolute value, while in Knight et al. papers the eigenvector is divided by the value for one state variable, meaning that the eigenvector element is one, i.e., unity, for that state variable. It is not clear why you normalize by the absolute value. *Response: We have revised the text as suggested. We now cite Knight et al. (2022) and normalize the eigenvectors by the value of a selected state variable, implying that the corresponding eigenvector component is equal to one.*

20. Page 14 after eq. (58): It says that the matrix has "$3M - 1$ sub- and superdiagonals". This is ambiguous. I asked about this previously, but the issue has not been resolved. Does this mean $3M - 1$ of each, i.e., $3M$

-1 subdiagonals and $3M - 1$ superdiagonals? If so, it should say this. The way it is written, it can be interpreted as meaning $3M - 1$ sub- and superdiagonals combined. One factor that contributes to the confusion is that the matrices $\mathbb{A}^0_{l,l+1}$ are diagonal, meaning that there are fewer superdiagonals than subdiagonals in eq. (57), technically. *Response: We have re-examined the band structure of the matrix and revised the text to explicitly distinguish between the numbers of subdiagonals and superdiagonals. The revised text now reads: "The matrix $\mathbb{A}$ has $KL = 3M-1$ subdiagonals and $KU = 3M - 1$ superdiagonals (excluding the main diagonal) and can therefore be stored in banded form and treated using standard band-matrix techniques. To solve the resulting banded system of linear equations, we employed the LAPACK routines ZGBTRF and ZGBTRS [Anderson et al., 1999]."*

21. Page 16, first paragraph of Section 4: Correct "it what follows". *Response: We have corrected the text as suggested.*

22. Page 17 before eq. (78): It says it applies the Fourier transform to eqs. (145-147), but this is not exactly true, since ρ' appears in those equations. Perhaps it would be better to say that you apply eq. (77) as in Appendix A without referring to specific equations in the appendix. *Response: We agree with the reviewer and have revised the text accordingly. The reference to Eqs. (145)–(147) has been removed, and the sentence now reads: "Applying the Fourier transform and introducing the transformed variables according to ..."*

23. Page 17 eq. (81): This condition is problematic at inviscid altitudes for the reason given above. *Response: As discussed in Comments 15 and 17, the applicability of this boundary condition is limited in the nearly inviscid regime. We have therefore revised the text and now write: "Assuming that the lower boundary conditions can be expressed in terms of state variables, we consider at the lower boundary z_1..."*

24. Page 17 eq. (82): This A looks similar to the A used on page 6. *Response: We have revised the notation accordingly and replaced A by the calligraphic symbol $\mathcal{A}$.*

25. Page 17 following eq. (82): Correct "s s". ´*Response: We have corrected the text as suggested.*

26. Page 18 eq. (85): Why do you normalize based on the absolute value of the real part? Why not just set f'_{sq_0} itself to the value on the right? *Response: Our intention is for f_{bq_0} to represent a prescribed physical amplitude, which is positive by definition, rather than a particular value of the perturbation field at the reference point. Since the perturbations are represented in complex form, the physical quantity is obtained from the real part of the solution. The absolute value is therefore introduced so*

that the normalization condition is imposed on the magnitude of the physical perturbation. We have clarified this point in the revised manuscript and now explain that f_{bq_0} is a prescribed positive amplitude and that the normalization condition specifies the magnitude of the perturbation at the reference point.

27. Page 18 after eq. (86): It says "as in Section 3, we define $\widehat{F}_q(z,\omega)$ ...". There was no $\widehat{F}_q$ defined in Section 3, so this needs clarification. *Response: We agree that $\widehat{F}_q(z,\omega)$ was not explicitly defined in Section 3. We have therefore revised the text and now write: "Following the approach used in Section 3, we introduce the quantities $\widehat{F}_q(z,\omega) = [\mathbf{e}(z,\omega)]_q$, $q = 1, 2, 3$, ... "*

28. Page 19 eq. (92): Again, why do you normalize by the absolute value of the real part? *Response: This normalization is identical to that used in Eq. (85). As explained in the revised manuscript, the absolute value is used because the prescribed quantity represents a positive physical amplitude.*

29. Page 19 eq. (97): This conflicts with the use of δ for imaginary frequency shifting. You could use Δ here and define it to make sure its meaning is understood. *Response: We have revised the notation accordingly and now use δ_D to denote the Dirac delta distribution, thereby avoiding a conflict with the imaginary frequency shift δ.*

30. Page 19 eqs. (97-98): The combination of (86) and (94) gives $\widehat{F}_{\mathrm{s}q_0}(z_1,\omega_0) = 2\pi A\delta(\omega-\omega_0)$, which is inconsistent with (97). The first part of (98), does not make sense if $\widehat{F}_{\mathrm{s}q_0}$ is a scalar multiple of a delta function, since a delta function is defined in terms of integration and does not take on any value. *Response: We agree that Eqs. (97)–(98) were not formulated correctly in the distributional setting. We have therefore removed these equations and reformulated the discussion. The revised text now reads: "Applying the Fourier transform with respect to time yields a forcing term proportional to $\delta_D(\omega-\omega_0)$, showing that the excitation is concentrated at the single frequency ω_0. Consequently, the frequency-domain problem is solved only at the excitation frequency ω_0. The corresponding harmonic amplitude at the lower boundary is prescribed to be $\mathcal{A}$, while the first and second vertical derivatives vanish, i.e.,..."*

31. Page 19-20 eqs. (99-102): I did not try to figure these out given the problems with eqs. (97-98), so I recommend double-checking them. Also, the reason for normalizing by the absolute value of the real part is again unclear. *Response: We have carefully rechecked the derivation of Eqs. (99)–(102) and confirmed that the results are correct. Specifically, $F'_{sq}(x,z,\omega) = \mathcal{A}S(\omega)e^{-jk_x x}C_q(z)\widehat{F}_q(z,\omega)$, and for $S(\omega) = 2\pi\delta_D(\omega-\omega_0)$, the inverse Fourier transform yields Eqs. (99)-(101). Concerning the normalization, the same reasoning applies as in Eqs. (85) and (92): the normalization is*

imposed on the magnitude of the physical perturbation rather than on its signed value. We have clarified this point in the revised manuscript.

32. Page 20 eq. (106) and surrounding text: This paragraph says that you impose the GC condition, but you actually use the LC condition. It is better not to use the GC condition. I recommend deleting this equation and paragraph. *Response: We have removed Eq. (106) and the associated paragraph.*

33. Page 21 eq. (108): This is not equivalent to (107), but I do not see a problem with it. The condition of (107) was introduced in Knight et al. (2024) because of its compatibility with the imaginary frequency shift selection method described in Appendix A of Knight et al. (2021). Your (108) is a more relaxed condition, but it should still suffice to ensure that upgoing and downgoing roots can be defined. Here is an example that will demonstrate why they are not equivalent....*Response: We agree that Eq. (108) is not equivalent to Eq. (107) and that it represents a less restrictive condition. We have therefore revised the text accordingly and now state: "A less restrictive condition is obtained by requiring that, at each layer l,"*

34. Page 22 eq. (115): Why is t_0 subtracted from t here but not in (112)? This seems inconsistent. *Response: The subtraction of t_0 in Eq. (115) is not an additional time shift, but follows from the factor* $\exp(-j\omega t_0)$ *in the Gaussian source spectrum. After the imaginary frequency shift, this factor gives* $\exp(-\delta t_0)\exp(-j\omega t_0)$*. Combining* $\exp(-j\omega t_0)$ *with the inverse-transform factor* $\exp(j\omega t)$ *yields* $\exp[j\omega(t-t_0)]$*, and combining* $\exp(-\delta t_0)$ *(with the recovery factor* $\exp(\delta t)$ *yields* $\exp[\delta(t-t_0)]$*.*

35. Page 22 eq. (116): There is normalization by the absolute value of the real part again here. Is there a particular reason why you want to normalize in such a way that the sign of the real part is preserved? *Response: This normalization is identical to that used in Eqs. (85) and (92), and the same explanation given in our responses to Comments 26 and 28 applies here.*

36. Bottom of page 22: It says "From these date, we read". This is unclear. Do you mean "data"? *Response: We have corrected "date" to "data".*

37. Page 23: Background ion velocity is not included in the list of outputs from IRI. Why is this? Does IRI not provide background ion velocity? Is background ion velocity included in your model? It appears in the derivations in Appendix B. If it is included in your model, then the source of this information should be given. Otherwise, it should be stated that it is not included. It was seen in Knight et al. (2025, Section 4.3) that background ion velocity is important for describing the effect of gravity waves on the ionosphere. See the paragraph that begins with "Hooke (1970, Equation 6) . . . ". The gravity wave disturbs the flow of ions, which in turn disturbs the electron densities. *Response: The background ion velocity is included in our model. However, it is not an output quantity read directly from the*

IRI data file. Instead, the background field-aligned ion diffusion velocity u_{D0} is computed internally using the ambipolar-diffusion approximation described in Appendix B.

38. Page 23: Please give a reference for cubic spline interpolation with regularization. *Response: We have added appropriate references for cubic spline interpolation with regularization.*

39. Top of page 24: The statement "(b) a simplified model ..." should be clarified. I believe that you are talking about the case considered by Midgley and Liemohn (1966), Volland (1969), Francis (1973), and others from around the same time period. This case is discussed on page 2. It is not really homogeneous. The background temperature and kinematic viscosity vary with altitude, but their gradients are dropped from the governing equations, which allows the eigenvalues to be grouped into opposite pairs. It would be better to say locally constant temperature and kinematic viscosity. *Response: We have revised the terminology throughout the manuscript. The text now reads: "a simplified model for a windless atmosphere without ion drag, in which the gradients of the background temperature and kinematic viscosity are neglected."*

40. Page 24: When the terms GMMA, SMMA, and GMMN are introduced, refer to the sections in which the corresponding methods are described. *Response: We have revised the text accordingly and now refer to the sections in which the GMMA, SMMA, and GMMN methods are introduced.*

41. Page 24, item 6: This seems like a minor detail which can be omitted from the paper. I say more about this in comments on the appendix. *Response: We have removed Item 6 from the manuscript.*

42. Page 24, start of Section 6: It says 10:00. Is this local or universal time? A few lines down, it says "magnetic index is 7.0". What is the specific name of the magnetic index? *Response: We We have clarified that the specified time is 10:00 UT and now explicitly identify the geomagnetic activity parameter as the Ap index, set to 7.0, corresponding to slightly disturbed geomagnetic conditions.*

43. Page 25: Where it says "The code provides altitude-dependent profiles of input parameters ..." this seems unnecessary and a little confusing. It does not make sense for inputs to also be outputs. This sort of detail that is best left for a user manual. *Response: We have revised the text to avoid the input/output ambiguity. The revised text now reads: "The numerical model uses altitude-dependent profiles of atmospheric and ionospheric parameters, including temperature, mass density, pressure, ..."*

44. Page 26. "Diffusion velocity" is mentioned. What does this mean? Ion velocity? *Response: The quantity refers to the background field-aligned*

ambipolar diffusion velocity of the ions. To make this clear, we have reformulated the text as: "the background field-aligned ambipolar diffusion velocity (see Appendix B)."

45. Page 27 towards the top: Can you explain the large phase shifts between general and simple results in Figure 3? It should be possible to explain this in terms of differences between the vertical wavenumbers. You could add a plot of the gravity-wave roots to help with this. The phase difference should be determined by the real parts of the vertical wavenumbers, i.e., the imaginary parts of the eigenvalues. *Response: We have added the following explanation to the manuscript: "In addition to the amplitude differences, Fig. 4 exhibits substantial phase shifts between the solutions of the general and simplified models. These phase shifts can be attributed to differences in the real part of the gravity-wave eigenvalue, or equivalently, in the vertical wavenumber of the ascending gravity-wave mode. Since the wave phase accumulates with altitude in proportion to the real part of the vertical wavenumber, even moderate differences between the gravity-wave roots of the two models lead to noticeable displacements of the extrema of the temperature and velocity perturbations."*

46. Middle of page 27: It says "When $\mathbf{E} \times \mathbf{B}$ drifts are not included …". The word "drift" does not occur in the paper before this line, which means that the reader will not know whether $\mathbf{E} \times \mathbf{B}$ drifts are included or not. This should be clarified. Also, please give a reference for auroral convection and ion drag as a forcing mechanism in the auroral region. *Response: To clarify the role of $\mathbf{E} \times \mathbf{B}$ drifts in the present model, we have added the following text: "In the present model, $\mathbf{E} \times \mathbf{B}$ drift velocities are neglected, and the ion velocity is approximated by the field-aligned neutral velocity plus the field-aligned ambipolar diffusion velocity (see Appendix B). Consequently, the classical auroral-convection regime, in which $\mathbf{E} \times \mathbf{B}$-driven ion motion transfers momentum and energy to the neutral atmosphere through ion drag, is not represented here [e.g., Richmond and Matsushita, 1975; Fuller-Rowell and Rees, 1984]. "*

47. Page 27 towards the bottom: These explanations for the effects of horizontal wavelength and wave period are flawed, since they imply that the change in amplitude with altitude is controlled by wavelength and period, which is generally incorrect in the inviscid region. If the background parameters (aside from density) are constant, then the change in amplitude with altitude is only affected by the $i/2H$ term in the vertical wavenumber in the inviscid region. This is illustrated by the cases considered in Knight et al. (2019, Section 6.1). As viscosity and diffusion start to take effect (with increasing altitude), the effects of wavelength and period can be understood in terms of Broutman et al. (2019, eq. (16)), which gives an asymptotic approximation for the imaginary part of the vertical wavenumber for small kinematic viscosity. You could also explain the differences in terms of the imaginary parts of the exact wavenumbers, which

are computed by your model. The statement "for a given forcing" is too ambiguous. How do you quantify the forcing in such a way that waves with different wavelengths and periods can be compared? It would make more sense to talk about wave amplitudes for the lower boundary condition. *Response: We agree that the original explanation was misleading because it could be interpreted as implying that the altitude variation of the amplitude in the inviscid region is controlled by the horizontal wavelength and wave period. We have therefore reformulated the discussion as follows: "In the inviscid region, the altitude variation of the wave amplitude is controlled primarily by the density stratification through the $j/(2H_a)$ contribution to the vertical wavenumber (cf. Eq. (18)) and is therefore largely independent of the horizontal wavelength and wave period. At higher altitudes, however, viscosity and thermal conduction become increasingly important, and the attenuation rate is determined by the imaginary part of the vertical wavenumber of the ascending gravity-wave mode. Since the horizontal wavelength and wave period modify the gravity-wave dispersion relation, they also modify the corresponding vertical wavenumber and its attenuation rate. Consequently, waves with different values of λ_x and λ_t experience different amounts of dissipation during upward propagation, leading to the amplitude differences observed in Fig. 6. "*

48. Bottom of page 27: It says "The total solution mode ..." This seems like a misuse of the term "mode". Mode refers to a summand associated with one of the eigenvalues. If you sum the modes, it is not a mode anymore. *Response: We have revised the terminology and now avoid using "mode" for the summed solution. Specifically, we use the terms total solution $\mathbf{e}_l$, ascending-wave contribution $\mathbf{e}_l^+$, and descending-wave contribution $\mathbf{e}_l^-$. The term mode is now reserved only for an individual contribution associated with a particular eigenvalue.*

49. Page 28: The purpose of the oval in Figure 2 should be given in the caption or the text. *Response: We thank the reviewer for this suggestion. We have clarified the purpose of the oval in the figure caption. The caption now states: "The oval indicates the altitude region where ion drag produces a small deviation in the vertical wavenumbers."*

50. Page 28: The caption for Figure 2 says "vertical wavenumbers ... for ... gravity waves ...". This is ambiguous. Is this talking about gravity-wave modes? If so, then say gravity-wave modes. *Response: We have revised the figure caption to explicitly refer to the gravity-wave modes. The caption now reads: "Comparison of the imaginary parts of the vertical wavenumbers associated with the ascending (k_{z1}) and descending (k_{z4}) gravity-wave modes."*

51. Page 28: A new figure or panel is needed in order to make it clear that the imaginary parts of the ascending and descending gravity-wave roots are separated over the entire altitude range in Figure 2. I recommend plotting the difference $\mathrm{Im}(kz4\ ) - \mathrm{Im}(kz1\ )$ in logarithmic scale. *Response:*

We have added two new panels showing the quantity $Im(k_{z4}) - Im(k_{z1})$ on a logarithmic scale. These panels clearly demonstrate that the ascending and descending gravity-wave modes remain separated throughout the entire altitude range. We have also added the following explanation to the main text: "To demonstrate that the ascending and descending gravity-wave modes remain distinct over the entire altitude range, the third and fourth panels show the quantity $Im(k_{z4}) - Im(k_{z1})$ on a logarithmic scale for the general and simplified models, respectively."

52. Page 28. The x axes of the three panels of Figure 2 should be made consistent. It is confusing for the lowest panel to have a different x axis. If it is not possible to use the same x axis for the lowest panel, then it should be moved into a separate figure. *Response: To avoid any confusion arising from the use of different x-axis scales, we have removed the third panel from Figure 2 and presented it as a separate figure.*

53. Page 31: It is puzzling that the 300 km and 500 km results are more similar to each other than to the 400 km results. Why are the 400 km results not between the 300 and 500 km results? This needs to be explained. *Response: We have added the following explanation to the manuscript: "The dependence on λ_x and λ_t can therefore be understood in terms of the corresponding gravity-wave roots computed by the model, whose imaginary parts determine the attenuation rates and whose real parts determine the phase propagation characteristics. Because the plotted quantities are signed perturbations, differences in the real part of the vertical wavenumber may also produce noticeable phase shifts between the profiles. Consequently, the 400 km profiles do not necessarily lie between the 300 km and 500 km profiles, even though all solutions are normalized to the same prescribed lower-boundary amplitude f_{bq_0} of the selected perturbation component."*

54. Top of page 32: "… should satisfy the relation $\mathbf{e}_l = \mathbf{e}_l^+ + \mathbf{e}_l^-$. In all our simulations, this identity is satisfied." Why is it necessary to state this here? Is it not obvious from the earlier discussion? Page 32 towards the top: It says "the ascending mode is dominant". This seems like a misuse of the term "mode", if a mode is defined to mean the part of the solution corresponding to one of the eigenvalues. Knight et al. (2022, eq. (2.3)) defines modes, for example. If you have a different definition in mind, please state it. *Response: The statement that $\mathbf{e}_l = \mathbf{e}_l^+ + \mathbf{e}_l^-$ was retained because it serves not only as a theoretical identity but also as a practical verification of the numerical implementation. We have clarified this point in the manuscript by adding the sentence: " This identity was verified in all computations and was used as an internal consistency check of the code. " We also agree that the term mode should be reserved for a contribution associated with a particular eigenvalue, consistent with the terminology of Knight et al. (2022). Accordingly, throughout the revised manuscript we have replaced expressions such as "ascending mode" by "ascending-wave contribution" or "ascending-wave solution", as appropriate.*

55. Page 32: The discussion of imaginary frequency shifts in the main text should be simplified. Currently, it discusses the results of a sequence of steps, where the steps are given in Appendix D. This discussion will only make sense to readers who have read the appendix, so it belongs in the appendix. Instead of giving the results of the individual steps, I recommend just saying that the process described in Appendix D leads to an interval of δ values and then stating the interval. You can also say that you select the one that is closest to the center of mass, etc., and give the selected δ. Beyond that, it is problematic if you get different solutions for different δ values. If all of the δ values are valid, meaning that the layerwise causality (LC) condition holds for all of them, then the results should be identical. This is stated in Knight et al. (2022) following eq. (2.58), which refers to results from Knight et al. (2019). Please state this δ-independence result here. If LC holds for all of the δ values, then the only possible explanation for obtaining different results is that the numerical blowup problem described in Knight et al. (2021, Appendix B) is occurring for some δ values. If this is the case, then it should be obvious from the time-domain plots, since the solutions will grow exponentially towards the end of the time interval. Without a resolution of this issue, I recommend saying in the paper that further study is needed to resolve this issue. *Response: We have substantially simplified the discussion in the main text and moved the details of the admissible-δ determination procedure to Appendix D. The revised text now focuses on the resulting admissible interval, the selection criterion, and the final choice of δ. We have formulated the following paragraph: "The determination of the admissible interval of imaginary frequency shifts follows the procedure described in Appendix D. For the present case* $\lambda_x = 500$ *km and* $\lambda_t = 40$ *min, this procedure yields the interval* $[\delta_{\min} = 10^{-6}\ s^{-1}, \delta_{\max} = 32.24 \times 10^{-6}\ s^{-1}]$*, within which the search for admissible solutions is performed. Specifically, candidate solutions corresponding to values of δ in this interval are examined, and only those satisfying the layerwise causality condition throughout the entire altitude range are retained. Five equidistant values of the imaginary frequency shift δ within this interval were considered. For each value, the wave parameters were computed and the layerwise causality condition was verified throughout the entire altitude range. All five solutions satisfied this condition and produced nearly identical maximum values of the perturbed horizontal velocity, vertical velocity, and temperature (Table 2). To select a representative solution, we considered the three-dimensional space formed by the maximum amplitudes (*$\overline{u}_{\max}$*,*$\overline{w}_{\max}$*,*$\overline{T}_{\max}$*). The final solution was chosen as the one whose amplitude vector is closest to the center of mass of the five candidate solutions. This criterion yields the third solution, corresponding to* $\delta = 16.62 \times 10^{-6}\ s^{-1}$*. For this solution, the maxima occur at 11.06 hr and 248.00 km for the horizontal velocity, 10.98 hr and 303.64 km for the vertical velocity, and 10.90 hr and 257.45 km for the temperature. "*

56. Top of page 35: In order to see how different the solutions are for different δ values, a different set of values should be shown in Table 2. The maximum values do not provide enough information about this. I suggest comparing the solution for each δ to the solution for the selected δ value. *Response: To provide a more informative comparison than the maximum values listed in Table 2, we have added a new figure that directly compares the selected solution with the solutions corresponding to the remaining admissible values of the imaginary frequency shift. We have described this figure in the main text as follows: "Fig. 9 shows the Root-mean-square (RMS) differences in the perturbed horizontal velocity, vertical velocity, and temperature between the selected solution and the remaining solutions. For each altitude, the RMS difference is computed over the entire time interval. The RMS differences remain below 0.003 (0.3%) throughout the altitude range, demonstrating that the computed wave fields are practically insensitive to the selected value of the imaginary frequency shift within the considered interval."*

57. Page 36: It says that the simplified model has a homogeneous atmosphere with constant kinematic viscosity, but this is not literally true. See the comment for the top of page 24. *Response: The wording has been corrected to avoid implying that the simplified model assumes a strictly homogeneous atmosphere or constant kinematic viscosity. The revised text now states: "A simplified model for a windless atmosphere without ion drag is also considered, in which the altitude derivatives of the background temperature and kinematic viscosity are neglected."*

58. Top of page 37: This should refer to Volland 1969 "The upper atmosphere ..." rather than Volland 1969 "Full Wave ...". The former gives governing equations. The latter does not. *Response: The reference has been changed accordingly and now cites Volland (1969).*

59. Page 37: The term $\mathbf{g}$ is not defined. *Response: The gravitational acceleration vector* $\mathbf{g}$ *is now explicitly defined.*

60. Page 37: The term μ_k is first used in eq. (128) but is not defined until after eq. (130). It should be defined earlier. *Response: The quantity μ_k is defined immediately following Eqs. (128)–(130), which are presented as part of a single sentence. Specifically, we state: "Here, $\mu_k = \mu/\rho$ is the kinematic viscosity, and we have assumed that the viscosity depends only on altitude, so that..."*

61. Page 37: It says "we express the hydrodynamic equations as", but it is not all of the hydrodynamic equations, since the continuity equation is not included. *Response: The text has been corrected and now reads: "we express the momentum and heat equations as ..."*

62. Top of page 38: Define g. *Response: We have clarified this point by defining g through the relation: "...where $\mathbf{g} = -g\widehat{\mathbf{z}}$,...".*

63. Top of page 40: H_ρ is used in eq. (137) but is not defined until after eq. (140). *Response: We have defined* H_ρ *after Eq. (137).*

64. Page 41: It says "the linearized equations can be written as follows", but the continuity equation is not included. Maybe say the "linearized momentum and heat equations" rather than "the linearized equations". Also, the z momentum equation (146) is unchanged from (139). *Response: The text has been revised to read: "With these relations, the linearized x-momentum and heat equations can be written as follows: ..." since the continuity equation is not included. We have also removed Eq. (146), as it is identical to the previously presented z-momentum equation (139).*

65. Page 43: It is unclear why both sides of (157), (158), and (159) are divided by the same term. *Response: The division by the same factor on both sides of Eqs. (157)–(159) was introduced to cast the equations into the standard matrix form used throughout the paper,* $\frac{1}{k_x}\frac{d\mathbf{e}}{dz} = \mathsf{A}\mathbf{e}$*, where* $\mathbf{e}$ *is the state vector and* A *is the system matrix. The normalization was therefore performed to make the left-hand-side terms consistent with this formulation and to facilitate the construction of the matrix* A*.*

66. Bottom of page 44: It says "For an isothermal ..." This should be related to a case considered in the main text. It may be the "simplified" case. Again, the atmosphere is not literally homogeneous, and $\mu_{\mathrm{k}0}$ is not literally constant, if I understand correctly. They are locally constant, meaning that background gradient terms are dropped at certain points in the derivation. Please clarify. *Response: We have revised the sentence as follows:: "For a windless atmosphere* ($u_0 = 0$)*, in which the altitude derivatives of the background temperature and kinematic viscosity are neglected,..."*

67. Page 45: It says that eq. (168) is obtained as the determinant of eqs. (160)-(162) with the ion drag terms dropped, but (160)-(162) includes background gradient terms which do not appear in (168). Please clarify. *Response: The original wording was misleading because Eq. (168) is not obtained directly from Eqs. (160)–(162) by simply dropping the ion-drag terms. We have therefore reformulated the discussion as follows: "In Knight et al. [Knight et al., 2024], the dispersion relation reported in Eq. (3.35) was derived by setting the determinant of the corresponding coefficient matrix equal to zero and applying the variable transformation described in Section 2.2. Using the equivalences discussed above, namely the different harmonic convention, the relation* $m = -jk_x\lambda$*, and the different definitions of the state vector, their Eq. (3.35) can be written in the form..."*

68. Page 45: "Aforementioned equivalences" is ambiguous, since it is not clear which "aforementioned equivalences" are meant. It would be clearer if it just said that (168) and (3.35) are equivalent. The sentence mentioning the variable-change method is also unclear. It appears that you have dropped

the background gradient terms in order to obtain (168), while Knight et al. (2024) kept background gradient terms but applied a change of variables to simplify the dispersion relation. That would be a better characterization. *Response: This issue has been addressed by the reformulation described in our response to Comment 67.*

69. Page 45 eq. (169): The use of $C1$, etc., here conflicts with (162). Response: To avoid ambiguity, we have replaced the coefficients C_i in Eq. (169) by the corresponding calligraphic symbols $\mathcal{C}_i$.

70. Bottom of page 46: Eqs. (177)-(180) and the related discussion could be deleted to reduce length. *Response: We have deleted Eqs. (177)-(180) and the related discussion.*

71. Page 47: Give a reference for (181), (182), and (183). Also, define $g_{\|}$ in (185). Currently, it is defined after eq. (189). *Response: References have been added for Eqs. (181)–(183). Concerning the definition of $g_{\|}$, Eqs. (185) and (186) are presented as part of a single sentence, immediately followed by the definition "where in general $\mathbf{a}_{\|} = (\mathbf{a} \cdot \widehat{\mathbf{b}})\widehat{\mathbf{b}} = a_{\|}\widehat{\mathbf{b}}$ and $\mathbf{a}_{\perp} = \mathbf{a} - \mathbf{a}_{\|}$"*

72. Page 48: Define $\partial/\partial b$ at the first place where it occurs, which is (187). *Response: We have defined $\partial/\partial b$ after Eq. (187).*

73. Page 48: Define $\widehat{\mathbf{b}}$ in statement after (188). *Response: The unit vector $\widehat{\mathbf{b}}$ was defined earlier in the manuscript as the unit vector in the direction of the magnetic field, namely "the direction of the magnetic field $\widehat{\mathbf{b}}$".*

74. Page 48: Can you give a reference for this derivation of the ambipolar diffusion coefficient? *Response: A reference to Schunk and Nagy (2009), where the ambipolar diffusion coefficient and the associated transport equations are derived and discussed in detail, has been added.*

75. Page 51 towards bottom: It says "We have several options", but only two options are given. "Several" usually means three or more. *Response: The text has been corrected by replacing "several options" with "two options."*

76. Page 51 bottom: It says "diffuse velocity". Should this be ion diffusion velocity? *Response: We have replaced "diffuse velocity" with "ambipolar diffusion velocity."*

77. Pages 56-60: Appendix C could be removed to reduce length. If it is included, then a reference should be given for a previous application of this approach. It was mentioned in the responses to my previous comments that it has been applied in radiative transfer. In my view, the approach in Appendix C amounts to a change of basis relative to the approach in the main text and there is no need to mention it except in passing, perhaps. *Response: We have considerably shortened Appendix C. The revised appendix is now restricted to the global matrix method based on the*

matrix exponential representation of the state vector at the grid points, which is the method implemented in our code.

78. Pages 60-61: The description of a method to bound wave periods could be omitted to reduce length. These considerations follow immediately from the dispersion relation, which would be familiar to anyone who has studied gravity waves. Additionally, the derivation has little bearing on the discussion in the main text. This type of information would be more appropriate for a user manual. *Response: We have removed the description of the method to bound wave periods.*

79. Pages 61-62: The description of a method to set frequency and time grids could be simplified to reduce length. It would suffice just to state the grids used in the examples considered in the main text. *Response: We have simplified the discussion considerably and now focus on the discretization parameters used in the simulations. The revised text explicitly specifies all parameters employed in the frequency and time discretization of the Fourier transform.*

80. Pages 62-65: The description of the method for determining imaginary frequency shifts seems overly detailed. It would suffice just to say that an interval of δ values is found for which the LC and SFR conditions hold. The steps involved in arriving at the interval can be omitted to reduce length. In a mathematical sense, the SFR should never fail. If it does fail, this suggests that the time grid does not cover the source adequately or that the aforementioned numerical blowup problem is occurring. I suggest looking at cases where the SFR fails, determining why it fails, and describing this here. It would be a numerical issue, not a mathematical issue. Also, I recommend omitting the application to the global causality condition. *Response: To reduce the length of the manuscript, we have removed the detailed step-by-step description of the algorithm and replaced it with a concise summary: "Starting from prescribed bounds $\delta_{\min}$ and $\delta_{\max}$, the SFR check is first used to identify a numerically reliable interval of frequency shifts. The LC condition at selected altitude levels is then applied within this interval, yielding a refined interval of admissible frequency shifts. To select a representative value, N_δ equidistant frequency shifts are sampled from the admissible interval. For each value, the LC condition is verified over the full altitude range. Whenever this condition is satisfied, the corresponding wave parameters and maximum perturbation amplitudes are computed. The final frequency shift is chosen as the one whose maximum-amplitude vector is closest to the center of mass in the space spanned by the maximum horizontal velocity, maximum vertical velocity, and maximum temperature perturbation." In addition, we have removed the discussion of the global causality condition.*