Response to reviewer comments

Based on the comments provided by the reviewers, we undertook a major revision and completely reformulated the manuscript. The major changes to the manuscript are listed below.

1. The revised manuscript is organized as follows. In Section 2, we present the derivation of the matrix exponential solution of the linearized equations, while Section 3 describes stable numerical methods for computing the amplitudes of the characteristic solution in a stratified atmosphere. Here, we focus only on the global matrix method based on matrix exponentials and the scattering matrix method for computing the amplitudes of the characteristic solutions. Section 4 addresses the computation of the perturbed quantities for both harmonic and non-harmonic source functions, that is, for single-frequency waves and time-dependent wave packets. The concepts of causality and the imaginary frequency shift are also briefly discussed. Aspects of the numerical implementation are addressed in Section 5, and representative simulation results are presented in Section 6. Additional theoretical issues are discussed in the appendices. Some of the theoretical aspects discussed, especially in Appendices A and B, may perhaps be unnecessary; however, our intention was to provide a self-consistent and complete description of the models.

2. The linearization model is described in Appendix A. We reformulate the linearized equations for the state vector $\mathbf{e} = [\widehat{u}, \widehat{w}, \widehat{T}, \widehat{\mathcal{U}}, \widehat{\mathcal{W}}, \widehat{\mathcal{T}}]^{\mathrm{T}}$, following the formulations of Vadas and Nicolls (2012) and Knight et al. (2024), instead of using $\mathbf{e} = [\widehat{u}, \widehat{w}, \widehat{p}, \widehat{T}, \widehat{\mathcal{U}}, \widehat{\mathcal{T}}]^T$, as adopted in earlier studies by Midgley and Liemohn (1966), Volland (1969), Francis (1973), and Yeh and Liu (1974). Appendix A provides a general model that accounts for the altitude derivatives of the background velocity u_0, temperature T_0, density scale height H_ρ, and dynamic viscosity μ_0. In addition, it includes a simplified model for an isothermal ($T_0 = \text{constant}$), homogeneous ($\mu_{\mathrm{k}0} = \mu_0/\rho_0 = \text{constant}$), and windless atmosphere without ion drag. Appendix A is organized into the following sections: *Hydrodynamic equations, Linearized equations, Plane wave solution*, and *Dispersion equation.*

3. The computation of the ion-drag force and ion-drag heating is presented in Appendix B. In the revised version, ion-drag effects are incorporated in an approximate manner with the explicit aim of decoupling the hydrodynamic and ion equation systems, while explicitly accounting for the plasma diffusion velocity. This is achieved by neglecting perturbed production and loss terms in the ion continuity equation, reducing the ion momentum equation to ambipolar diffusion by neglecting ion inertia and ion–ion collisions and retaining only field-aligned transport, and assuming fast field-aligned diffusion so that ion perturbations and the plasma diffusion velocity remain nearly constant along magnetic field lines. Appendix

B is organized into the following sections: *Ion equations, Linearized equations, Decoupled system of equations,* and *Plane wave solutions.*

4. The application the global matrix method based on matrix exponentials and the scattering matrix method to compute the grid-point values of the state vector is described in Appendix C. This appendix is organized into the following sections: *Global matrix method with matrix exponential* and *Scattering matrix method.*

5. In Appendix D, we discuss several implementation issues related to the computation of lower and upper bounds for the wave period, the choice of frequency and time discretization for the Fourier transform, and the determination of the imaginary frequency shift using a practical, albeit heuristic, approach.

6. We plan to provide a freely available open-source code for solving the linearized gravity-wave equations on GitHub. Accordingly, only representative simulation results are presented in Section 6. The simulation results are new for two reasons: (i) we employ a new linearization model, and (ii) the previous implementation contained an error in the coefficient of thermal conductivity, which was set to $\lambda_0 = 6.71 \times 10^{-7} T_0^{0.71}$ instead of the correct value $\lambda_0 = 6.71 \times 10^{-4} T_0^{0.71}$, thereby substantially reducing the effect of thermal conduction.

For a better understanding of the revised manuscript, we have included the new version in our response (not in the final form required by the journal). Please find below our detailed replies (in black font) to the reviewer comments (in blue font).

Reviewer 1

We are sincerely grateful to Harold Knight for his helpful and insightful comments. His detailed explanations greatly clarified several results from his work that we had used in this paper without a full understanding. We also warmly acknowledge his recommendation of his two most recent papers, which were previously unknown to us and have now become an important foundation for the present study.

Here are some general issues affecting clarity that occur through the paper:

1. Most citations just give the article reference without a specific section and/or equation number. Throughout the revised manuscript, we have supplemented citations with explicit section and equation numbers wherever necessary.

2. The same symbols are used in multiple contexts in some instances identified below. We have reviewed the notation and eliminated ambiguous or overlapping symbol usage.

3. The figure labels are not sufficiently descriptive, making it difficult for the reader to understand the figures. In almost all figures (e.g., Figure 5), lines and symbols are labeled a, b, c, etc., and the reader has to look at the caption to know what is meant. This makes it unnecessarily difficult to interpret the figures. Descriptive information should be given in the line/symbol labels instead of a, b, c, etc. All figures have been revised to include descriptive labels directly on the lines and symbols, rather than generic labels such as a, b, c.

4. Often, equations involve terms that are not introduced until many lines after the equation. It is better to introduce terms before they appear in equations. We have reorganized the presentation of equations so that all variables, parameters, and coefficients are introduced and defined before they appear in the equations.

Specific Comments

Lines 1-11. The abstract should be revised to reflect changes suggested below. The main results of scientific interest are the effectiveness and efficiency of a banded-matrix technique and the examination of the effects of ion drag, so these should be the focus of the abstract.

In the abstract we state: *"Particular emphasis is placed on the global matrix method, which exploits the structured form of the multilayer system to achieve high computational efficiency while maintaining numerical accuracy." and "The impact of ion drag on wave characteristics is quantified within this framework."*

Line 14. This sentence should mention that it is talking about upper-atmospheric gravity waves.

We agree and have revised the sentence to explicitly refer to upper-atmospheric gravity waves: *"Time-step methods [1,2,3] are commonly used to solve fully nonlinear sets of governing equations for upper-atmospheric gravity waves, thereby allowing the modeling of wave breaking, secondary wave generation, and weakly nonlinear effects."*

Lines 15-16. Please add Knight et al. (2024) to this list.

We have added Knight et al. (2024) to this list.

Lines 26-34. Knight et al. (2022, Section 1) introduced the term "numerical multilayer method" to replace the term "nonstandard" suggested by Hines (1973). The term "physical multilayer" can be used in place of "standard". The terms "nonstandard" and "standard" are anachronistic and nondescriptive, considering that the supposed "standard" approach is not in use anymore. I encourage the authors to use the terms "numerical multilayer" and "physical multilayer" in place of "nonstandard" and "standard", respectively.

Following the reviewer's suggestion, we have replaced the terms "nonstandard" and "standard" with "numerical multilayer" and "physical multilayer," respectively, as introduced by Knight et al. (2022, Section 1).

Lines 47-52. It should be mentioned here that the assumption of locally constant kinematic molecular viscosity is unrealistic, as discussed in Knight et al. (2019, Section 1). Also, Knight et al. (2024, Section 6.5) describes the effect of relaxing the assumption that $\mu z = 0$, where μ is dynamic molecular viscosity. The effects are small, generally. Knight et al. (2021,2024) allow μ to vary according to the standard Dalgarno and Smith (1962) formula, but terms involving μz are omitted from the vertical structure equations associated with the main results.

We have clarified this point by stating in the main text that *"On the other hand, the assumption of locally constant kinematic viscosity is unrealistic as discussed in Knight et al. [16]."*, and by explaining in Appendix A that *"Essentially, the difference between the model of Vadas and Nicolls [35] and that of Knight et al. [11] is that, in the latter, the derivative of the dynamic viscosity $d\mu_0/dz$ is omitted. In the code, for testing purposes, we included a hard-coded logical flag that selects the linearized model to be used. Our numerical simulations show that there are no significant differences between the general model and that of Vadas and Nicolls [35], and that the effect of the assumption $d\mu_0/dz = 0$ is relatively small. This latter assumption was discussed in detail in Knight et al. [11]."*

Lines 57-65. This discussion of Midgley and Liemohn (1966) seems too detailed. I have not seen this problem involving coupling and critical layers in any other context. Perhaps it is specific to their assumption of locally constant kinematic viscosity.

We agree with the reviewer that the original discussion of Midgley and Liemohn (1966) was overly detailed. Accordingly, we have reduced and streamlined this section. It now reads: *"Midgley and Liemohn [4] employed an iterative method that can be regarded as a Gauss–Seidel group iteration. However, the Gauss–Seidel iteration may fail to converge in certain situations, in particular when gravity and dissipative modes become strongly coupled, as discussed in Refs. [5,6]. Klostermeyer [10] avoids this difficulty by introducing the concept of a transfer matrix, which relates the wave amplitudes at different altitude levels and provides a more robust framework for treating such coupling."*

Line 76. Change direction to directions.

We have changed.

Line 110. Define $\overline{\boldsymbol{\sigma}}$ or give a specific reference for it. Also, what does the prime symbol mean in $(\nabla \cdot \overline{\sigma})'$?

The stress tensor $\overline{\boldsymbol{\sigma}}$ is defined in Appendix A. In an earlier version, the prime symbol in $(\nabla \cdot \overline{\sigma})'$ denoted the perturbation of the stress-divergence term. This notation has been removed in the revised manuscript.

Lines 113-123. It is best to leave out the equations of motion for ions, since this is not being modeled here.

The modeling of the ion equations is described in detail in Appendix B.

Lines 145-182. This discussion should be simplified, given that the ion continuity equation is not modeled in this paper and ionospheric effects are left for future work. I suggest using words rather than equations. Note that Knight et al. (2025) modeled the effects of atmospheric gravity waves on the ionosphere by driving an ionospheric model with the numerical multilayer method of Knight et al. (2024). It was seen in Knight et al. (2025, Section 4.3) that the downward flow of ions from the plasmasphere is needed in order to properly describe plasma fluctuations resulting from gravity waves.

In the revised manuscript, this discussion has been removed from the main text. As noted above, the modeling of the ion equations is described in detail in Appendix B. In addition, the following statement has been added to the conclusions: *"The linearized equations on which the solution methods were tested correspond to ionospheric conditions. The ultimate goal of our research is to develop a comprehensive model for analyzing ionospheric gravity waves using satellite measurements. The approach presented in this paper represents only the first component of such a model. Two options are envisaged for extending it to a more complete formulation.*

1. *Fully coupled neutral–ion model. The linearized hydrodynamic equations would be solved together with the ion equations. In this case, the ion continuity equation would include perturbed production and loss terms, whereas ion inertia and ion–ion collisions would continue to be neglected in the ion momentum equation, and only transport parallel to the magnetic field lines would be retained. The state vector would then be augmented by two additional components, namely the perturbed ion number density and the ion diffusion velocity.*

2. *Two-step coupling strategy. In the first step, the neutral-atmosphere equations are solved using the fast field-aligned diffusion approximation. In the second step, the wave-induced perturbations obtained from the neutral solution are used as input to solve the ionospheric equations for the perturbed O^+ ion density. The ionospheric equations may be solved using the SAMI2 model for low latitudes, where the $\mathbf{E} \times \mathbf{B}$ drift is neglected, or the SAMI3 model at higher altitudes, where the $\mathbf{E} \times \mathbf{B}$ drift is included. In this strategy, priority is given to the ionospheric equations of the SAMI framework, while wave-induced perturbations are handled using the approximate approach developed in the present study. Along similar lines, Knight et al. [34] solved the neutral-atmosphere equations without ion drag in a first step, and subsequently addressed the ionospheric response using the Field-Line Interhemispheric Plasma (FLIP) model [44]."*

Line 192. The opposite sign convention for frequency and horizontal wavenumbers is typical for gravity-wave studies. See, e.g., Knight et al. (2025, eq. 1). This should be mentioned here to prevent confusion.

In the main text, we have added the following remark: *"Note that in some gravity-wave studies, the opposite sign convention for frequency and horizontal wavenumber is used (e.g. [34])."*

Line 196. Give the value for Pr. Also, the numerical formula for thermal conductivity, λ = 6.71 × 10−4 T 0.71, does not look right. Is constant cp assumed? If so, what value? The specific heat at constant pressure varies with composition. The value of the Prandtl number is given in the Numerical simulations section (Pr=0.66).

In Appendix A, we define

$$c_{\mathrm{p}} = c_{\mathrm{v}} + R_{\mathrm{M}}, \;\; \gamma = \frac{c_{\mathrm{p}}}{c_{\mathrm{v}}}, \;\; \mathit{\Lambda} = \frac{c_{\mathrm{p}}\mu}{\mathrm{Pr}},$$

where c_{p} is the specific heat at constant pressure, γ the ratio of specific heats, Pr the Prandtl number, $\mathit{\Lambda}$ the coefficient of thermal conductivity, and R_{M} the specific gas constant. Moreover, to derive the heat equation, we assume that c_{p} and Pr are constant, which yields

$$\frac{\partial T}{\partial t} = -\left(\gamma - 1\right) T\nabla \cdot \mathbf{u} - \mathbf{u} \cdot \nabla T + \frac{1}{\rho c_{\mathrm{v}}} \bar{\boldsymbol{\sigma}} : \nabla \mathbf{u} + \frac{\gamma}{\rho \mathrm{Pr}} \nabla \cdot (\mu \nabla T) - \frac{1}{\rho c_{\mathrm{v}}} q_{\mathrm{ID}}.$$

The empirical power-law expression used for $\mathit{\Lambda}$ (as a function of T) is consistent with this closure through $\mathit{\Lambda} = c_{\mathrm{p}}\mu/\mathrm{Pr}$, under the assumption of constant c_{p} and Pr.

Line 226. Say Appendix or Appendix A instead of just A.

“A” has been replaced by “Appendix A.”

Line 227. The notation λn for eigenvalues conflicts with the notation λ for thermal conductivity. This should be fixed.

This has been corrected by denoting the coefficient of thermal conductivity by $\mathit{\Lambda}$.

Line 249. Say “Appendix A.2” instead of just “A”. Say that it is to be expected that the vertical wavenumbers will separate into pairs when kinematic viscosity is assumed to be constant given earlier work, e.g., Volland (1969b).

We reformulate the text as follows: *“As shown in Appendix A, for a constant kinematic viscosity the solutions occur in pairs and correspond to (i) ascending and descending gravity-wave modes, (ii) ascending and descending viscosity-wave modes, and (iii) ascending and descending thermal-conduction wave modes [6,7]. In that appendix, this pairing is explicitly demonstrated by deriving the dispersion relation for the special case of an isothermal (constant background temperature), homogeneous (constant kinematic viscosity), and windless atmosphere without ion drag.“*

Lines 260. Is it really possible to distinguish between viscosity-wave modes and thermal- conduction-wave modes? What is the theoretical basis for this distinction? Presumably, such a distinction would change based on the Prandtl number.

We agree with the reviewer that there is no significant distinction between viscosity and thermal-conduction waves. In the section *Simulation results*, we plot the imaginary part of the vertical wavenumber for all wave types and state: *“The plots reveal a clear distinction between gravity waves and viscosity- and thermal-conduction waves. However, the viscosity and thermal-conduction waves are very close to each other.”*

Line 264. “we can renounce on the vertical wavenumber” does not sound right in English. Perhaps instead say “we can put aside the concept of vertical wavenumber.”

We replaced the phrase with *“we can put aside the concept of the vertical wavenumber.”*

Lines 267-270. You say "providing a more intuitive explanation compared with the analogy with an isothermal and homogeneous atmosphere." This should be reworded. The classification of upgoing and downgoing roots done earlier by Volland, etc. had no theoretical justification other than the wish for dissipation to increase rather than decrease magnitudes. The Knight et al. approach, which will be discussed in Section 5, requires other conditions beyond (45) having to do with causality, and was motivated by theoretical concerns rather than intuition.

We reformulate the text as follows: *"A commonly cited interpretation of condition (20) is that, for increasing z, the exponential term* $exp(k_x \lambda_n z)$ *will tend to be damped more for ascending modes than for descending modes; conversely, for decreasing z, the roles of ascending and descending modes are reversed. However, such a classification of upgoing and downgoing roots (e.g., Volland [6] and related works) was primarily heuristic and lacked a rigorous theoretical justification. By contrast, the approach of Knight et al. [16], which is discussed in Section 4, introduces additional constraints beyond condition (20) that are explicitly related to causality and is therefore grounded in theoretical considerations rather than heuristic arguments."*

Line 299. It would make sense to say "a numerical multilayer method (Knight et al., 2022)" here instead of "nonstandard." Note that the term "nonstandard" does not appear in Klostermeyer (1972).

We replaced "nonstandard" with "a numerical multilayer method [9,15,16]" as suggested.

Lines 313-321. Why is it necessary to apply (61) and (62)? It appears that the method would work setting S 1 to the identity matrix and S 0 to the diagonal matrix of exponential terms or by setting S 1 to the diagonal matrix of inverse exponential terms and S 0 to the identity matrix. What specific problem does applying the condition $\mathrm{Re}(\lambda_{nl}) > 0$ etc., prevent?

In the revised manuscript the scaling matrices are denoted by K_l^1 and K_l^0, and the layer matrices are written as $\mathbb{A}_{l,l+1}^1 = \mathrm{K}_l^1(\mathrm{V}_l^{-1}\mathrm{V}_{l+1})$ and $\mathbb{A}_{l,l+1}^0 = \mathrm{K}_l^0$. To explain the role of the scaling matrices, we included the following text: *"The scaling matrices* K_l^1 and K_l^0 *prevent a possible blow-up of the exponential terms for Re*$(\lambda_{nl}) > 0$ *and Re*$(\lambda_{nl}) \leq 0$*, respectively. Such scaling techniques are standard in radiative transfer theory and are commonly used to obtain stable numerical algorithms for computing the radiance field in multilayered atmospheres [29,30]."* To verify the robustness of the approach, we performed a series of simulations in which the number of altitude levels was reduced from 800 to 400, 200, 100, and 10, corresponding to an increase in the grid spacing Δ_l from 0.5 km to 42 km. In all cases, the results remained stable and reliable.

Line 313 says "To obtain a stable system of equations." Does the banded-matrix method not work without dividing up the diagonal terms this way? This step of dividing up the diagonal terms is not needed with the scattering-matrix approach. It is not done in the Knight et al. references.

In general, the computation of the matrix exponential $\exp(\mathsf{A}x)$ becomes numerically problematic when the argument of the exponential term $\exp(\lambda x)$ is large, that is, when $\lambda x \gg 1$. For small values of λx, no serious numerical difficulties arise. In radiative transfer theory, it is well known that the global (banded) matrix method requires such scaling to ensure numerical stability, whereas the scattering-matrix approach does not. The reason is that, in the latter case, the interaction principle equation is inherently numerically stable.

Lines 348-353. These lines do not need to be included. It says "For the continuity equations and the boundary conditions to be consistent ..." There does not appear to be any need for such consistency. If there is no numerical or mathematical reason for (72) and (73), then there is no need to include them.

These lines have been removed and Eqs. (72) and (73) have been deleted.

Line 354. I suggest beginning by saying something like "Here define an alternative type of lower boundary condition ...".

We revised the text to begin with the sentence: *"An alternative type of lower boundary condition was proposed by Knight et al. [15,16]."*

Line 356. For clarity, it should be stated here that eq. (74) is a generalization of Knight et al. (2022, eq. 2.19), which refers to the first state variable rather than an arbitrary state variable.

For clarity, we now state: "Note that Eq. (47) generalizes Eq. (2.19) in Ref. [15], which is formulated for the first state variable rather than for an arbitrary state variable."

Line 356. The following is for the authors' information. The motivation for this form of the lower boundary condition comes from Knight et al. (2019, eq. 3.5), which is used in the statement of Knight et al. (2019, Theorem 1). For causality results, it is necessary to formulate boundary conditions in terms of state variables rather than modes, since modes are in the frequency domain.Note also that Knight et al. (2022, eq. 2.19) should be used with caution in cases where conditions are nearly inviscid at the lower boundary. See Knight et al. (2024, eq. 5.5).

We added the following explanation to the manuscript: *"The boundary condition (48) is a localized condition that prescribes the value of a single state variable while enforcing vanishing slope and curvature at the boundary. It effectively acts as an external driver applied to one variable and is appropriate for non-harmonic source functions. Note that this form of the lower boundary condition is used in the statement of Theorem 1 in Ref. 16. For causality considerations, boundary conditions must be expressed in terms of state variables rather than modal amplitudes, since modes are defined in the frequency domain."*

Line 364. The symbol A is overused in this paper. See lines 267 and 300, for instance. Also, a similar symbol is used for the continuity equation and the global matrix. A different symbol should be used here.

We replaced the symbol A with B to avoid overuse.

Line 377. Move the explanation at lines 383-386 to before eq. (83). Otherwise, the term as is confusing.

In the revised manuscript the index s, which indicates the dependency of a perturbed quantity on the source factor, has been removed in Section 4 (Source function).

Line 381. "The matrix A has 3M − 1 sub- and super-diagonals." Give the numbers of sub- and super- diagonals separately. Are these distinct from the diagonal? 3M − 1 does not seem right. It looks like there should be 4M diagonal bands total.

We revised the text to read: *"The matrix* $\mathbb{A}$ *has* $3M-1$ *sub- and superdiagonals (excluding the main diagonal)..."*

Line 382. Give more specifics on the method used to solve the banded-matrix equation, including the software package and/or a book or article reference. Do you actually invert the banded matrix or do you solve a linear equation of the form $A\boldsymbol{v} = \boldsymbol{b}$ This will make a difference in numerical efficiency. Your global matrix is extremely ill-conditioned due to the very large and very small dissipative wavenumbers towards the inviscid end of the altitude range. One benefit of the scattering-matrix approach is that it avoids this problem, at least as formulated in Knight et al. (2019). It would be interesting to see some discussion of how your banded-matrix method avoids the problem of ill conditioned matrices, especially with a lower boundary at or below 50 km altitude, where kinematic viscosity is very small.

We added the following explanation to the manuscript: *"The matrix* $\mathbb{A}$ *has* $3M-1$ *sub- and 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. The routine ZGBTRF performs an LU factorization with partial pivoting of the complex band matrix, and ZGBTRS subsequently uses this factorization to solve the linear system for the prescribed right-hand side. In this approach, the inverse of the full system matrix is not computed explicitly, which improves the computational efficiency."* Moreover, for lower boundary conditions imposed at 40, 45, 50, 60, 70, 75, and 80 km, we found that all solution methods yield nearly identical results. Accordingly, the maximum values of the perturbed vertical velocity are 50.86, 20.35, 14.57, 14.19, 16.16, 8.39, and 3.44 m/s. In these simulations: the horizontal wavelength is $\lambda_{\mathrm{x}} = 400$ km, the wave period is $\lambda_{\mathrm{t}} = 40$ min, the imaginary frequency shift for a single-frequency wave is 10^{-6} s^{-1}, and the lower boundary conditions are imposed on the vertical velocity with $f_{\mathrm{b2}} = 1 \times 10^{-2}\,\mathrm{ms}^{-1}$.

Lines 393-395. It is not clear why the authors feel the need to state this condition. Generally, in discussion of linear methods, it goes without saying that the approximation is valid in the asymptotic limit of small perturbations. It does not appear that the authors have an actual estimate of the range of validity of their linear equations, so why bother stating eq. (88)? Knight et al. (2024, eq. 6.2) give a condition for wave breaking, but weakly nonlinear effects can occur at smaller amplitudes. Unless the authors can give a good reason for including eq. (88) and the related discussion, it should be deleted.

In the revised manuscript, we clarified that the amplitude of the source function is specified through a normalization condition rather than as a strict criterion for linear validity. Specifically, we determine the amplitude of the source function by imposing a lower-boundary value on the amplitude of the perturbed temperature, horizontal velocity, or vertical velocity. We added the following text in Section 4: *"The amplitude of the source function* A *is specified by imposing the normalization condition:* $|Re\{f'_{sq_0}(x=0, z_1, t_0)\}| = f_{bq_0}$*, for* *some prescribed boundary value $f_{bq_0} > 0$. For example, in the case $q_0 = 1$, f_{bq_0} may be chosen as a fraction of the maximum horizontal velocity of neutrals in the south direction over the altitude range, whereas in the case $q_0 = 3$, f_{bq_0} may be chosen as a fraction of the maximum temperature of neutrals over the altitude range."*

Lines 399-400. This statement is problematic because s is in the frequency domain up to here, while in Section 5 it becomes a time-dependent function. I suggest rewording so that the term s is not explicitly mentioned.

As noted above, in the revised manuscript the source factor s appears only in Section 4. For time-dependent wave packets, we now state: *"Applying the Fourier transform to Eq. (81) and using Eqs. (79) and (80), we obtain the following boundary conditions in the frequency domain (note that $AS(\omega)$ is the Fourier transform of $As(t)$):*

$$\widehat{F}_{sq_0}(z_1, \omega) = AS(\omega), \;\; \frac{\partial \widehat{F}_{sq_0}}{\partial z}(z_1, \omega) = 0, \;\; \frac{\partial^2 \widehat{F}_{sq_0}}{\partial z^2}(z_1, \omega) = 0.$$

Comparing Eqs. (86) and (48), we see that the latter corresponds to the choice $b_{1,2} = b_{1,3} = 0$. In this case, the source factor $s = b_{1,1}$ can be identified with $AS(\omega)$." Similarly, for a single-frequency wave, we state: *"Accordingly, the localized boundary conditions are imposed directly on the harmonic amplitudes at ω_0, namely*

$$\widehat{F}_{sq_0}(z_1, \omega_0) = A, \;\; \frac{\partial \widehat{F}_{sq_0}}{\partial z}(z_1, \omega_0) = 0, \;\; \frac{\partial^2 \widehat{F}_{sq_0}}{\partial z^2}(z_1, \omega_0) = 0.$$

Again, by comparing Eqs. (98) and (48), we see that the source factor $s = b_{1,1}$ can be identified with A."

Line 403. Do 1 and 2 correspond to + and -? State this earlier.

We clarified this point by adding the following explanation: *"We consider the continuity equation (38) and partition the matrices $\mathbb{A}^i_{l,l+1}$, with $i = 0, 1$, as..."*

Lines 406-408. There is a notational conflict with eqs. (61-62), since this reuses the symbol S.

The scaling matrices are now denoted by K_l^1 and K_l^0

Line 410. Where does the term "interaction principle" come from? Line 414. Give a reference for this formulation of the scattering matrix. Lines 424-427. Give a reference for these equations. They are similar to Knight et al. (2019, eqs. 4.30-33), for instance. Line 102. Give a reference for this equation. It is similar to Knight et al. (2019, eq. 4.34), for instance.

The term "interaction principle equation," the concept of a "stack," as well as the term "adding formulas" are standard in radiative transfer theory. As examples, we indicated Refs. [31,32]. We also note: *"Note that Eqs. (68)–(71) are mathematically equivalent to Eqs. (4.30)–(4.33) in Ref. [16]".*

Line 441. I find Section 4.2 problematic and think it should be deleted. In Section 6, it is seen that there is no advantage in using the formulations in Section 4.2, so why add all of this unnecessary detail? If you think it is important, then summarize this alternative in a few sentences, including the finding in Section 6 that it offers no advantage. That aside, the use of the word "discrete" in the title of this section is unclear. In what sense is it discrete? How is it any more discrete than the approach of Section 4.1? Discrete ordinates are mentioned in the introduction, but that does not appear to be related.

We agree with the reviewer that Section 4.2 does not offer a practical advantage. Accordingly, Section 4.2 has been deleted from the main text. The solution methods described there, which we consider to be of purely theoretical interest, have been moved to Appendix C, entitled Solution methods for the grid-point values of the state vector. In addition, the term "discrete values" has been replaced by "grid-point values" to avoid ambiguity.

Line 554. Shouldn't $b1,k$ depend on ω?

Yes, $b_{1,1}$ depend on ω; this is clarified in our response to the comment referring to Lines 399–400.

Lines 563-565. This sentence about the separable case appears to be unnecessary.

The sentence has been deleted.

Line 572. It should be mentioned that in Knight et al. (2024, eq. 2.29) a relaxed condition is given, in which the bounds are allowed to vary with attitude.

In Section 4, we added the following text: *"Knight et al. [11] subsequently relaxed the requirement that (106) hold for all layers l for a fixed σ. The new condition, which we refer to as the Layerwise Causality (LC) condition, requires that at each layer l there is a σ_l such that*

$$Re[\lambda_{1l}^{+}(\omega - j\delta)] < \sigma_l < Re[\lambda_{1l}^{-}(\omega - j\delta)], \tag{1}$$

for all $\omega \in \mathbb{R}$. Equivalently, if at each layer l,

$$d_l(\omega) = Re[\lambda_{1l}^{-}(\omega - j\delta)] - Re[\lambda_{1l}^{+}(\omega - j\delta)] > 0, \tag{2}$$

for all $\omega \in \mathbb{R}$, then the multilayer algorithm will still preserve causality. The LC condition requires a strict separation between the two eigenvalue families within each layer, but it does not require that the same separator works for all layers. Thus, each layer may have its own separating value σ_l. Equivalently, $d_l(\omega) > 0$ means that, in layer l, the real parts of the eigenvalues associated with the ascending and descending gravity waves remain separated (and therefore do not cross) as functions of the real frequency ω after the shift."

Lines 577-594. Since imaginary frequency shifting is not used in this work, much of this summary should be deleted. Regardless, the explanation given here is incomplete, in that there is no indication of how δ was selected. Methods for determining the minimum sufficient δ are described in Knight et al. (2019,2021,2022). Instead of giving this summary, the paper should say that the imaginary frequency shifting technique was not applied and that further study is needed to determine the effect. Also, below I will suggest a new figure for Section 6 that will give a good indication of whether problematic branch points are present. If there are problematic branch points, they will primarily affect nearby frequencies.

Lines 590-594. The problem of the numerical blowup associated with the exponential growth term is discussed at length in Knight et al. (2021, App. B) and should be cited here. Numerical blowup is especially a problem for large time domains. If, in future work, you are unable to obtain results without the blowup, then I suggest reducing the size of the time domain and considering narrower time wave packets. Care is needed in selecting δ. It should be large enough to prevent the crossing seen in Knight et al. (2019, Fig 2b), but not much greater than that. Rigorous methods are discussed in Knight et al. (2019,2021,2022), but it would suffice to look for the curve-crossing issue. I recommend replacing the discussion from line 577 to 594 with brief references to Knight et al. (2019, Section 3.4) and Knight et al. (2021, Section 2.4). You can mention that you encountered the numerical blowup problem mentioned in Knight et al. (2021, App. B) and that further study is needed to resolve this issue. It is good that you introduce imaginary frequency shifting in lines 570-675, however, since that allows you to explore the effect of δ on the root-crossing issue illustrated in Knight et al. (2019, Fig. 2b).

In the new algorithm implementation, we employ an imaginary frequency shift. In Subsection 4.3, we summarize the imaginary frequency shift approach and conclude the discussion with the following text: *"A potential numerical issue with this approach is that a large frequency shift δ, while ensuring the causality condition, may amplify rounding errors when recovering f'_{sq} from $f'_{\delta sq}$ through the exponential term exp$[\delta(t-t_0)]$. For large t, this may lead to an uncontrolled growth of the right-hand side of Eq. (115). Therefore, care is required in selecting δ: it must be large enough to ensure the layerwise causality condition , but not significantly larger than that. Rigorous methods for determining the minimum sufficient δ were described by Knight et al. in Refs. [15,16,17], while the numerical blow-up associated with the exponential growth term was discussed in Appendix B of Ref. [17]. In our implementation we employ a heuristic approach that combines (i) the Layerwise Causality (LC) condition applied at selected altitude levels, and (ii) a Source-Function Reconstruction (SFR) test. First, an admissible interval $[\delta_{\min}, \delta_{\max}]$ is constructed by enforcing the SFR criterion, and within this interval, the LC condition is applied at selected altitude levels to obtain a refined lower bound $\delta_{\min}$. In the final selection step, a discrete set of candidate shifts is evaluated, and for each candidate the LC condition is checked over the entire altitude range. Among all shifts that satisfy causality at all altitudes, the algorithm selects the one whose maximum-amplitude vector is closest to the center of mass of the admissible solutions. A detailed description of this approach is provided in Appendix D."* Thus, the imaginary frequency shift approach is discussed in Appendix D. In Section 6 (Numerical Simulations), we illustrate how this algorithm operates in practice.

Lines 594-607. This alternative approach should not be included in the paper. In practice, it is impossible to verify (157) rigorously without Titchmarsh's theorem (Knight et al., 2019, Section 2). A concise statement of the causality condition is given in the short paragraph following the proof of Lemma 1 in Knight et al. (2019, Section 2). Using the notation given there, the condition $\beta(t) = 0$ for $t < 0$ can be required for the lower boundary condition β, but

Titchmarsh's theorem is needed to establish it for w (Knight et al., 2019, eq. 2.4).

We agree with the reviewer and have therefore removed this alternative approach from the manuscript.

Line 608. It does not appear that the horizontal wavenumber or wavelength is ever given in Section 6. It is important to specify this.

In Section 6, we have now specified the values of the horizontal wavelength and the wave period.

Lines 613-616. Methods 3, 4, and 5 should not be included here, given that they offer no advantages in accuracy or efficiency. The derivations in Section 4.2 do not seem scientifically interesting, given that they mostly rearrange terms from Section 4.1. (The Pade approximation would be of interest if it actually provided advantages, but it does not, so there is no apparent need to mention it except perhaps very briefly.) Method 2 is of interest because a related method is currently in use for atmospheric gravity waves (referring to the Knight et al. work).

In the code, we implemented the following methods: the Global Matrix Method for the Amplitudes (GMMA) of the characteristic solutions; the Scattering Matrix Method for the Amplitudes (SMMA) of the characteristic solutions; and the Global Matrix Method for the Nodal (grid-point) values (GMMN) of the state vector. The first two methods are described in the main text, whereas the third method is described in Appendix C. We do not wish to omit the third method, because it is the approach commonly used in radiative transfer theory, where the solution is formulated in terms of the grid-point values of the radiance.

Line 620. This does not look like a complete list of background parameters. It might be complete with $p0$, cv , and Pr added.

In Section 6, we have now specified the value of the Prandtl number and added the following text: "*Background atmospheric parameters. The code provides altitude-dependent profiles of the input parameters used in the numerical model. These include the temperature, mass density, pressure, southward horizontal velocity, atmospheric scale height, density scale height, specific heat capacity, ratio of specific heats, sound speed, number density of O^+ ions, neutral–ion collision frequency, ion–neutral collision frequency, and the diffusion velocity. In addition, altitude derivatives of the temperature, horizontal velocity, mass density, pressure, density scale height, and ion number density are also provided. As an illustrative example, Fig. 1 shows the background temperature T_0, horizontal velocity u_0 and the ion number density n_{i0}, together with their corresponding altitude derivatives.*"

Lines 520-525. What are the input parameters for SAMI2 and HWM?

We have added a new section, namely Section 5 (Numerical Implementation), in which the input data and their use are described in detail. We now state: *"The code uses as input the data file produced by the International Reference Ionosphere (IRI) code available at https://ccmc.gsfc.nasa.gov/models/IRI~2016/ is used. From these date, we read*

1. *the date (year, month, and day) and the time,*
2. *the geographic latitude and longitude,*
3. *the magnetic dip angle,*
4. *the solar radio flux f10.7 and its 81-day average,*
5. *the number density of O^+ ions as a function of altitude.*

In its present implementation, the IRI data files correspond to the locations summarized in Table 1. The altitude grid extends from $z_{\min} = 80$ *km to* $z_{\max} = 500$ *km with a step size of* $dz = 1.0$ *km. Users may generate custom data files by running the IRI code and specifying the corresponding file names in the input namelist.*

The IRI data are subsequently used in a manner analogous to that in the SAMI2 model of the Naval Research Laboratory (https://github.com/NRL-Plasma-Physics-Division/SAMI2). In the present implementation, the ionospheric equations follow the SAMI2 framework originally developed by Huba et al. [40] and described in detail by Huba [41]. In SAMI2, the neutral atmospheric parameters–namely the neutral number density, total mass density, and temperature–are specified using the MSIS family of models. In this study, these parameters are based on the MSIS formulation of Hedin [42], while we note that more recent updates are provided by the NRLMSIS 2.0 model of Emmert et al. [43]. The meridional and zonal winds are specified using the Horizontal Wind Model. In the present implementation, we follow the formulation of Hedin et al. [44], while more recent updates are described by Drob et al. [45]." In addition, at the beginning of Section 6 we now specify the numerical input used in the simulations: *"The simulations are performed using, as input, an IRI data file corresponding to the EISCAT Tromsø (auroral) location on 11 February 2012 at 10:00. The solar zenith angle is 84.2°, the magnetic inclination angle is 78.28°, the daily solar radio flux F10.7 is 109.4 sfu, and the 81-day averaged solar radio flux is 116.9 sfu, where* $1\,sfu = 10^{-22}\ Wm^{-2}Hz^{-1}$*. In the simulations, the Prandtl number is 0.66, the magnetic index is 7.0, and the horizontal wind model 14 implemented in SAMI2 is used. The altitude grid extends from 80 km to 500 km and contains 801 grid points. The lower boundary can, in principle, be set to smaller altitudes (e.g., 50 km), but we choose 80 km because this level is typically adopted as the lower boundary for ionospheric equations. Unless stated otherwise, the horizontal wavelength is* $\lambda_x = 400$ *km, the wave period is* $\lambda_t = 40$ *min, and the imaginary frequency shift for a single-frequency wave is* $10^{-6}\ s^{-1}$*. The lower boundary conditions are imposed on the vertical velocity with* $f_{b2} = 5 \times 10^{-2}\ ms^{-1}$ *in Eqs. (85) and (101)."*

Line 627. p_0 , c_v , and ρ_0 should also be shown in a figure. The density scale height H, should also be shown.

The freely available code computes these quantities, among others, as altitude-dependent input parameters. In order to avoid an overabundance of figures showing background profiles, we decided to display only the background temperature, the horizontal velocity, and the ion number density, together with their corresponding altitude derivatives, as representative examples of the input data.

Lines 649-652. I suggest omitting the Fourier transform in the altitude dimension. There are several reasons for this: 1. It is confusing, since it means that there are two types of vertical wavenumbers, one obtained from the vertical structure equations and one obtained directly from the Fourier transform. 2. It creates notational ambiguity, since the same notation is used for both types of vertical wavenumbers. 3. Taking the Fourier transform in the vertical dimension does not make sense given that the vertical wavenumbers coming from the vertical structure equations include both real and imaginary parts. See Knight et al. (2025, Section 4.2, first paragraph). Vertical wavelengths are not defined, strictly speaking, when significant dissipation is occurring. 4. The Fourier transform makes the most sense with periodic or unbounded domains, neither of which applies to the altitude dimension. Aside from that, the values given here are difficult to interpret. If the vertical Fourier transform is left in the paper (which I recommend against), then the actual value for Δk_z should be given, and $N_k \Delta k_z$ should approximately equal 450 km. Line 655. Give a reference for the nonuniform Fourier transform.

We agree with the reviewer's assessment and have removed the consideration of the Fourier transform in the altitude dimension from the revised manuscript.

Lines 665-672. There is no need to include this discussion, and it should be deleted, along with Figure 2. Figure 2 merely confirms that the derivations in Appendix A.2 are correct, and it should go without saying that they are correct.

Lines 673-689. Again, methods 3, 4, and 5 should be omitted, making Figure 3 unnecessary. Continuing with lines 673-689, Figure 4 probably becomes unnecessary if it is just a comparison of the first two methods. Given $M = 3$, I would expect about factor of three ratio of processing times between methods 2 and 1, assuming that the banded-matrix method solves the linear equation $A\boldsymbol{v} = \boldsymbol{b}$ directly rather than inverting A. This is because the scattering-matrix approach effectively solves for a general three-dimensional lower-boundary condition, meaning that it does more computations than would be needed for a specific lower-boundary condition, in principle. The ratio in Figure 2 is more like a factor of five. This makes me wonder whether your scattering-matrix computations are done as efficiently as they could be. Rigorous analysis of the computational steps involved with methods 1 and 2 would be needed to clarify this. I am not suggesting that such analysis be included in the current paper, but I would like for your paper to mention that more rigorous analysis is needed to make the result definite.

We agree with the reviewer's assessment. In the revised manuscript, Figures 2, 3, and 4 have been removed, and the corresponding discussion has been deleted. Instead, we now include the following concise summary: "*Accuracy and efficiency of the solution methods. Taking the global matrix method for amplitudes as a reference, we find that the relative root-mean-square errors in the perturbed temperature, vertical velocity, and horizontal velocity obtained with the other two solution methods are smaller than* 10^{-6}. *Thus, all methods ex-*

hibit comparable accuracy. On the other hand, we find that the scattering matrix method is more time-consuming than the global matrix methods, particularly for time-dependent wave packets. This is because the scattering matrix approach requires numerous matrix operations in each layer, whereas solving a system of equations compressed into band storage is computationally less expensive." We deliberately leave open the possibility for users to further improve the implementation of the scattering-matrix method and, eventually, its computational efficiency. Instead of these figures, we have included Fig. 3, which shows the altitude profiles of the perturbed temperature, vertical velocity, and horizontal velocity computed using the general and simplified models.

Lines 690-701. It is not clear what is gained by merely comparing results for different values of $\kappa\omega$. This is because there is no way of knowing to what extent differences in neutral-atmospheric dynamics are contributing to the differences. To clarify this, I recommend combining the results in the upper panels of Figure 5 with the results shown in Figure 8 in the same figure (maybe a different figure for each state variable) and discussing these results together. I would give results without ion damping for each of the three $\kappa\omega$ values so they can be compared in each case. Why are the apparent vertical wavelengths in the upper panels of Figure 5 so similar for the three $\kappa\omega$? As mentioned above, I could not see where you specified the horizontal wavelength. The vertical wavelength coming from the vertical structure equations should change with $\kappa\omega$, assuming that the horizontal wavelength is kept fixed. These issues need to be clarified in Section 6. As indicated above, I recommend omitting the type of analysis shown in the lower panels of Figure 5 and in Figure 6. You can replace it with a comparison of results for the three $\kappa\omega$ values, with and without ion damping, as described above. If you want to talk about vertical wavenumbers, I recommend looking at vertical profiles of the upgoing gravity-wave roots and interpreting differences in model results in terms of those. It would also be good to include discussion of previous analysis of the effects of ion damping on gravity waves and relate it to your present work.

In the previous implementation, an error was present in the coefficient of thermal conductivity, which was set to $\lambda_0 = 6.71 \times 10^{-7} T_0^{0.71}$ instead of the correct value $\lambda_0 = 6.71 \times 10^{-4} T_0^{0.71}$, thereby substantially reducing the effect of thermal conduction in favor of ion-drag effects. After correcting this error, we have revised the numerical analysis accordingly. In the revised manuscript, Fig. 8 of the previous version has been replaced by a new Fig. 4, which focuses explicitly on the effect of ion drag. The new Fig. 4 includes the following explanation: "*Ion Drag. The effect of ion drag on the perturbed temperature, vertical velocity, and horizontal velocity is illustrated in Fig. 4. A moderate attenuation is observed in the altitude range from 180 to 350 km, where the ion number density is relatively high. When* $\mathbf{E} \times \mathbf{B}$ *drifts are not included, ion drag does not exhibit the classical regime in which auroral convection strongly drives the neutral atmosphere. Instead, ion drag mainly arises from diffusion- and pressure-gradient-driven ion motion along the magnetic field, as well as from any relative ion–neutral motion induced by neutral winds. Consequently, ion drag does not constitute the dominant forcing mechanism for the neutral* *perturbations in this configuration."*

After Fig. 4, we included a new figure, namely Fig. 5, illustrating the influence of the horizontal wavelength λ_x and the wave period λ_t on the perturbed quantities. We found these effects to be of interest and summarize them as follows:

1. *"Horizontal wavelength. As the horizontal wavelength increases, horizontal pressure gradients become weaker, which reduces the driving of the wave motion. This also weakens the coupling between horizontal and vertical motions, resulting in smaller gravity-wave amplitudes."*

2. *"Wave period. As the wave period increases, the buoyancy restoring force acts more slowly, leading to weaker oscillations for a given forcing. In addition, dissipative processes such as viscosity and thermal diffusion act more effectively on low-frequency waves, further reducing their amplitudes."*

Lines 702-722. Pairwise classification of vertical wavenumbers is less important than being able to divide the roots into separated upgoing and downgoing sets. Figure 7 should include more descriptive titles and labels giving the meaning of the panels. The figure caption is difficult to interpret because it merely refers to equation numbers without reminding the reader of the meaning. While Figure 7 illustrates the differences between two governing-equation assumptions (i.e., locally varying and constant kinematic viscosity) in their effect on vertical wavenumbers, which is of some value, it does not say much about whether the roots can be separated into upgoing and downgoing sets. To do this, one would need to look at how the roots vary with frequency. This applies even for fixed-frequency cases. I recommend giving a figure like Knight et al. (2019, Fig. 2b) for several different altitudes, e.g., 150, 250, 350, and 450 km. If any of the roots cross like in Knight et al. (2019, Fig. 2b), it means that there is a problematic branch point nearby. Generally, there is no problem for single-frequency results provided that the frequency is far from the problematic branch point. Even though the global method does not explicitly require upgoing and downgoing modes to be defined at intermediate altitudes, the solution still may not be valid without appropriate imaginary frequency shifting for problematic branch points occurring over the entire altitude range. I hope to write a paper clarifying these issues in the future.

In the revised manuscript, Fig. 2 is now the counterpart of Fig. 7 from the previous version and has been substantially revised, including clearer titles, labels, and an expanded caption. The new Fig. 2 includes the following explanation: *"In the upper and middle panels of Fig. 2, we plot the imaginary part of the vertical wavenumber for ascending (k_{z1}) and descending (k_{z4}) gravity waves, computed using the general and the simplified models, respectively. The plots demonstrate that only in the latter case do the vertical wavenumbers appear in pairs. In the former case, a problematic altitude range for gravity waves is observed between 80 km and 120 km, where the imaginary parts of the vertical wavenumbers for the ascending and descending modes are nearly identical, being either both positive or both negative. In the lower panel of Fig. 2,* *we show the imaginary part of the vertical wavenumber k_z for all wave types ($k_{zn}, n = 1, \ldots, 6$), computed using the general model. The plots reveal a clear distinction between gravity waves and viscosity- and thermal-conduction waves. However, the viscosity and thermal-conduction waves are very close to each other. In the upper panel of Fig. 2, we also compare the imaginary part of the vertical wavenumber computed with and without ion drag. No pronounced effect of ion drag on the vertical wavenumber is observed. A small effect appears in the altitude range from 180 to 300 km, where the ion number density is relatively high. This finding is consistent with the results of Shibata [43], who showed that, for gravity waves, plasma diffusion is of minor importance with respect to the vertical wavenumber, which is mainly controlled by dissipation due to viscosity and thermal conduction in the neutral gas."* We emphasize that, in Fig. 2, the pairwise classification of vertical wavenumbers for the simplified model is included primarily to support the statements in Section 2 and the dispersion equation in Appendix A. The crossing of the curves corresponding to the real parts of the eigenvalues associated with ascending and descending gravity waves as functions of frequency is encapsulated in the Layerwise Causality (LC) condition. In the code, this condition is checked first at selected altitude levels and, in a second step, over the entire altitude range. If the LC condition is not satisfied, an error message is issued and, in the second case, the corresponding solution is considered invalid. Finally, we note that a routine for plotting the eigenvalue curves as functions of frequency could, in principle, be incorporated into the code without difficulty. However, we do not regard this as necessary for the purposes of the present study (code description), since any user with access to the freely available source code can readily generate such plots and carry out this analysis independently.

Line 718. "The ion-drag is important for time frequencies ..." Give a specific reference for this.

This sentence has been removed from the revised manuscript.

Lines 718-722. As mentioned above, this discussion should be combined with the discussion in lines 690-701. Also, the results discussed here are puzzling. It says there is complete agreement between results with and without ion drag for $\kappa\omega = 1.2$. This does not seem possible. Surely, ion drag would have some effect. The authors should double-check this and provide further explanation if there really is no effect. In particular, they should look at the vertical wavenumbers (obtained from the vertical structure equations) and see whether there is any difference. Aside from this, the caption of Figure 8 is puzzling. Case (a) is with ion drag excluded. What is $\kappa\omega$ for (a)? If $\kappa\omega = 0.8$ for (a), then the similarity between results for (a) and (b) makes even less sense, given that $\kappa\omega = 1.2$ for (b). The wording here and in the text should be made clearer, and errors, if any, should be corrected.

The numerical analysis related to the effects of ion drag has been revised. We refer to our response to the comments on Lines 690–701 for a detailed description of the corrections and the revised interpretation.

Lines 723-726. These lines should be deleted. Figure 9 gives a comparison of nearly identical results, and if the results are identical there is most likely a trivial reason for it, so the discussion, along with Figure 9, does not need to be included.

Lines 723–726 and Fig. 9 have been deleted from the revised manuscript.

Lines 733-738. This is similar to some previous work, which should be cited. Knight et al. (2019) defines the "transmission-only" approximation, which is similar to your eq. (118), and Knight et al. (2021) discusses a single-mode approximation, which is related to the transmission-only approximation. Additionally, Knight et al. (2019, Section 6) shows the upgoing and downgoing contributions to a wavefield. Although (118) is introduced in Section 4.2, which I recommend deleting, it should be possible to give very similar definition in Section 4.1.2.

We have revised this part of the manuscript and now explicitly relate our formulation to the work of Knight et al. In particular, the new Fig. 6 addresses the computation of ascending and descending wave modes and is explained as follows: *"The ascending and descending solution modes in layer l, denoted by $\mathbf{e}_l^+$ and $\mathbf{e}_l^-$, respectively, can be computed using the GMMA through Eq. (61) or using the GMMN via the recurrence relations (269) and (270). The total solution mode $\mathbf{e}_l$, obtained from Eq. (59) in the GMMA formulation and by solving Eq. (266) in the GMMN formulation, should satisfy the relation $\mathbf{e}_l = \mathbf{e}_l^+ + \mathbf{e}_l^-$. In all our simulations, this identity is satisfied. Furthermore, the results shown in Fig. 6 indicate that the ascending mode is dominant, except in the altitude range between 120 km and 180 km in the case of the general model. This finding, which is consistent with the results presented by Knight et al. [16] (see their Fig. 6), suggests that in a simplified model one may assume the ascending modes to be dominant at altitudes above 200 km, that is, $\mathbf{e}_l \approx \mathbf{e}_l^+$ for $l = 1, \ldots, L$. Under this assumption, the state vector can be computed using the upward recurrence relation (269). In Ref. [17], this approach was referred to as the transmission-only approximation, whereas in Ref. [16] a related single-mode approximation was introduced."*

Lines 747-748. This reflects a naïve view of causality. Causality is really about whether upgoing and downgoing modes are defined and valid. For frequencies near problematic branch points, asingle-frequency solution will be incorrect, regardless of whether the peak in amplitude seems to be moving with altitude.

Lines 749-756. This discussion is problematic. Firstly, Figure 15 is the wrong type of plot for analyzing issues with causality, i.e., whether upgoing and downgoing roots are valid. What is needed is a figure like I described for lines 702-722 above, showing the imaginary parts vs. frequency. There is no indication of how the δ value was selected. Note how in Fig 2b of Knight et al. (2019), two roots cross, while in Fig. 2d they do not cross. This indicates that the δ value used in Fig. 2d was sufficient. If δ is not large enough to prevent the roots from crossing, then it will not work. The bottom three panels of Figure 15 should be omitted. To really assess the effect of problematic branch points, you need a solution that is known to be correct. Just observing that the solution is small before $t = 0$ is not sufficient. Regarding Figure 15, are the eigenvalues specific to $\omega 0$? This should be stated.

We are grateful to the reviewer for these comments, which clarified our understanding of the concept of causality and the role of problematic branch points. In the revised manuscript, the previous Fig. 15 has been removed, and the corresponding discussion has been deleted. The new Fig. 7 is now the counterpart of Fig. 13 from the previous version.

Line 747. The extreme difference in computation time between methods 1 and 2 is very puzzling given that only a factor of five difference was seen for the single-frequency case. What possible reason could there be for this? It seems like this must be a mistake.

Lines 773-774. As discussed above for line 747, there is no apparent reason why there should be a difference in relative efficiency between single-frequency and time-varying cases.

In the revised manuscript, we no longer report the computation times of the solution methods. Instead, we leave it to the user to assess the efficiency of the implemented methods and, if desired, to further optimize their performance.

Lines 768-769. "The amplitude of the source function can be computed ..." This is unclear. Why would one want to compute the amplitude of the source function? Generally, one starts with the source and computes the wavefield from that.

We agree with the reviewer's comment. However, as explained in our response to the comments on Lines 393–395, in our approach the amplitude of the source function is determined indirectly by imposing a lower-boundary value on the amplitude of the perturbed temperature, horizontal velocity, or vertical velocity. In other words, we prescribe the effect at the lower boundary and infer the corresponding source amplitude, rather than prescribing the source amplitude a priori, thereby prioritizing the effect over the cause.

Appendix A. Converting to non-dimensional form makes the equations more complicated than they would be otherwise, and it also makes it impossible to check equations via units.

Line 793. "A1-A4" is unclear. Does this mean eqs. (A1-4)? Maybe say "eqs. A1-A4 below".

Lines 916-917. This statement is redundant with discussion in the main text.

Line 927. Say whether this is density or pressure scale height.

Line 940. Say "$\mu 0 = \mu k =$ constant", etc., here.

Lines 997-1037. These lines would belong in a separate section, but I do not think they should be included in the paper at all. If you have fresh insights into Hines' criticism, I suggest describing them briefly in the main text without any additional equations.

Appendix A has been completely reformulated, and all of the reviewer's comments have been taken into account. In particular, the notation has been clarified, redundant statements have been removed, ambiguous definitions have been specified, and the extended discussion in Lines 997–1037 has been deleted.

Final comment: It would be advantageous for the authors to show that they can reproduce a previous result. To this end, they could apply their method 1 to the case illustrated by Figure 2a in Knight et al. (2022). It would be interesting to hear whether they get similar results, although it would not be necessary to add a figure for this.

Our code is designed to operate with realistic, altitude-dependent background parameters, and for this reason it is not straightforward to reproduce the results presented in Knight et al. (2022), which are based on a more idealized configuration. However, we are able to reproduce the results reported in Knight et al. (2024), which are formulated within a framework more consistent with the present model.

New References Knight, H., Broutman, D., & Eckermann, S. (2024). Compressible and anelastic governing- equation solution methods for thermospheric gravity waves with realistic background parameters. Theoretical and Computational Fluid Dynamics, 38(4), 479–509. https://doi.org/10.1007/s00162-024-00709-x Knight, H. K., Richards, P. G., Martinis, C. R., & Goncharenko, L. P. (2025). Modeling MSTIDs produced by gravity waves with parameters obtained from all-sky imager observations and comparisons to incoherent scatter radar observations. Journal of Geophysical Research: Space Physics, 130, e2025JA033906. https://doi.org/10.1029/2025JA033906.

We have included these references in the revised list of references.

Reviewer 2

We are grateful to Stephan Buchert for his insightful and pertinent comments, especially those concerning ion drag.

My review focuses on the issue of the ion drag, how it is introduced and discussed.

Comment 1)

Lines 144-165: This discussion is about taking into account ion drag in the neutral dynamics. It is based on equation (13) which seems to have appeared 1st in Klostermeyer (1972). The implications of (13) are not entirely clear to me, so I try to rephrase my understanding of the issue: In the direction parallel to the magnetic field B the ions are dragged with the neutrals, and the unperturbed velocities u|| and ui,|| are approximately the same (in lines 172-177 an approach by Shibata (1983) which includes also diffusion and gravity is mentioned, but then not applied). But the interesting yet in the manuscript not elaborated aspect is what happens in the directions perpendicular to B?

Equations (14) and (15) suggest that the background ion velocity ui,⊥ is assumed to be zero in the reference frame where the neutral wind u is given (presumably a co-rotating frame). The drag force fID and frictional heat PID can only depend on the difference between ion and neutral velocity. So obviously ui,⊥ = 0 is assumed. At high latitudes this assumption is quite unrealistic. The aurora zone ion convection is dominated by coupling to the magnetosphere as described by Dungey (1961). The Weimer (2005) model, among others, could be used in combination with the HWM for the background difference u⊥ - uui,⊥. At mid-latitudes there is inter-hemispheric coupling which determines the ion velocity depending on inter-hemispheric wind differences, for example according to the HWM (Laundal et al., 2025; Buchert, 2020). The SAMI2 model, as far as I understand, includes inter-hemispheric coupling by particle transport, but not electrodynamically, i.e. without a mid-latitude Sq current system.

This study is about gravity waves and linearized perturbations. According to equations (16) and (17) the background state has an effect also on the perturbed fID' and PID'. My guess is that quantitatively it is quite negligible. Also reviewer 1 remarked on the very small difference that seems to arise from the ion drag. An update to a more realistic background ion velocity model could be done, and at high latitudes the effect should then become larger than observed in the present draft. Finally I would like to remark that the perturbations by gravity waves themselves should also affect the ion velocities, which would then affect the perturbed ion drag. Equation (16) and (17) are incomplete, as it is assumed that ui,⊥ faithfully remains unaffected by the perturbed u⊥', or that ui,⊥' = 0 as well. To abandon this assumption and solve the ion momentum equation (7) would be complicated and is understandably avoided in this work. However, ion drag forcing and dissipation by gravity wave perturbations might well be important and have comparable or even larger effect than a realistic background state via the perturbed densities in the 2nd term of equations (16) and (17). Even if a complete, linearized solution were available for both neutrals and ions, the very high mobility of electrons along magnetic field lines should have the effect that these are electric equipotential also within gravity waves. This seems not guaranteed with a complete neutral and ion solution unless additional constraints are introduced.

Alternatively, a possible assumption is that the ions are completely dragged by the neutrals, ui = u also in the perpendicular directions. This is equivalent to ignoring the ions for neutral dynamics. Compared to the effective assumption ui,⊥ = 0, which is chosen in the draft, this seems to me slightly preferable. The above mentioned condition that magnetic field-lines are electric equipotential also under perturbations by gravity waves would then generally be violated.

In summary, the authors have followed the approach in previous works when taking into account the ion-neutral coupling in atmospheric dynamics which is fine. My complaint is that the implications of the chosen approach had not been made clear up to now. Therefore I recommend to explicitly state that the Klostermeyer (1972) equation (13) implies that the ion velocities perpendicular to B, both background and perturbed ones are assumed to be zero, and this might be unrealistic in some situations. If possible, give quantitative estimates about an expected inaccuracy related to this assumption.

We fully agree with these comments. In the revised manuscript, we derive in Appendix B the expressions for the ion-drag terms that enter the hydrodynamic equations. In this appendix, we treat the ion equations, derive the linearized system, discuss the assumptions that lead to a decoupled set of equations, and present the corresponding plane-wave solutions. In particular, we now state: *"This approximation is justified when perpendicular ion transport is small compared to the dominant field-aligned diffusion. The resulting formulation captures the leading-order effects of ambipolar diffusion along the magnetic field lines, while deliberately neglecting perpendicular electrodynamic coupling, such as cross-field advection and* $\mathbf{E} \times \mathbf{B}$ *drifts. Consequently, the model is ap-*

plicable to regimes in which field-aligned transport dominates and perpendicular electrodynamic effects play a secondary role. We note, however, that the neglect of the electromagnetic drift velocity is not appropriate for all geophysical regimes. At high latitudes, ion convection is largely controlled by magnetospheric forcing [47], and realistic modeling generally requires externally imposed convection electric fields, for example from empirical models such as Weimer [48]. At mid-latitudes, perpendicular ion motion may be influenced by inter-hemispheric coupling and neutral-wind differences between conjugate hemispheres [49,50]. A fully self-consistent electrodynamic formulation, in which the electric field is obtained from an electrostatic potential Φ *via* $\mathbf{E} = -\nabla\Phi$, *with* Φ *determined from quasi-neutral current continuity and the conductivity-tensor relation [**?**], is therefore beyond the scope of the present study but constitutes an important extension for future work."*

In Section 6 (Numerical Simulations), we now state: "*Ion Drag. The effect of ion drag on the perturbed temperature, vertical velocity, and horizontal velocity is illustrated in Fig. 4. A moderate attenuation is observed in the altitude range from 180 to 350 km, where the ion number density is relatively high. When* $\mathbf{E} \times \mathbf{B}$ *drifts are not included, ion drag does not exhibit the classical regime in which auroral convection strongly drives the neutral atmosphere. Instead, ion drag mainly arises from diffusion- and pressure-gradient-driven ion motion along the magnetic field, as well as from any relative ion–neutral motion induced by neutral winds. Consequently, ion drag does not constitute the dominant forcing mechanism for the neutral perturbations in this configuration."*

In the Conclusions, we now add the following perspective on future extensions of the model: *"The approach presented in this paper represents only the first component of such a model. Two options are envisaged for extending it to a more complete formulation.*

1. *Fully coupled neutral–ion model. The linearized hydrodynamic equations would be solved together with the ion equations. In this case, the ion continuity equation would include perturbed production and loss terms, whereas ion inertia and ion–ion collisions would continue to be neglected in the ion momentum equation, and only transport parallel to the magnetic field lines would be retained. The state vector would then be augmented by two additional components, namely the perturbed ion number density and the ion diffusion velocity.*

2. *Two-step coupling strategy. In the first step, the neutral-atmosphere equations are solved using the fast field-aligned diffusion approximation. In the second step, the wave-induced perturbations obtained from the neutral solution are used as input to solve the ionospheric equations for the perturbed* O^+ *ion density. The ionospheric equations may be solved using the SAMI2 model for low latitudes, where the* $\mathbf{E} \times \mathbf{B}$ *drift is neglected, or the SAMI3 model at higher altitudes, where the* $\mathbf{E} \times \mathbf{B}$ *drift is included. In this strategy, priority is given to the ionospheric equations of the SAMI framework, while wave-induced perturbations are handled using the approximate approach developed in the present study. Along similar lines, Knight et*

al. [34] solved the neutral-atmosphere equations without ion drag in a first step, and subsequently addressed the ionospheric response using the Field-Line Interhemispheric Plasma (FLIP) model [44]."

Comment 2)

Lines 181-182: "This topic will be discussed in more detail in the Conclusions." I cannot find a more detailed discussion of the topic in the Conclusions?

In the previous version of the manuscript, this topic was discussed in the Appendix rather than in the Conclusions. In the revised manuscript, we have clarified this point and now discuss the topic in detail in Appendix B.

Comment 3)

Lines 129-130: "... neglected the Coriolis force ..." The statement is not very specific. According to Klostermeyer (1972) the Earth rotation should be taken into account for wave periods τ>1 hour. So I guess for long period/large scale gravity waves there could be effects from the Coriolis force. How is a limit "... gravity waves with an angular frequency ω > 2Ω, where Ω = 7.3 × 10−5 s−1 is the Earth's angular velocity" justified, what does it mean in terms of small, medium, and large scales?

In Appendix A of the new version of the paper, we use the following argument: *"In the above hydrodynamic equations, the Coriolis force has been neglected. For a two-dimensional wave geometry in which both the background flow and the perturbation velocities are confined to the vertical* (x, z) *plane and all variables are independent of the transverse horizontal coordinate* y*, the Coriolis acceleration associated with the Earth's rotation is directed entirely along the transverse direction and therefore does not enter the momentum equations considered here. More precisely, for* $\mathbf{u} = (u, 0, w)$ *and* $\mathbf{\Omega} = (-\Omega\cos\phi, 0, \Omega\sin\phi)$*, where* ϕ *is the geographic latitude and* $\Omega = 7.29 \times 10^{-5}\, s^{-1}$ *the Earth's angular velocity, the Coriolis force per unit mass is* $\mathbf{f}_C = -2\mathbf{\Omega} \times \mathbf{u} = (0, 2\Omega(\cos\phi w + \sin\phi u), 0)$*. Thus, the Coriolis acceleration is directed entirely along the transverse horizontal direction* y *and does not affect the two-dimensional* (x, z) *momentum equations."* On the other hand, an alternative explanation is the following: *"In the above hydrodynamic equations, the Coriolis force has been neglected because we are interested in gravity waves with frequencies* $\omega > 2\Omega$*, corresponding to wave periods shorter than* $\pi/\Omega \approx 12$ *h,where* $\Omega = 7.3 \times 10^{-5}\, s^{-1}$ *is the Earth's angular rotation rate."*

Comment 4)

Lines 620-624: The references to the SAMI2, MSIS and HWM models are a bit old, updates and newer versions of these models exist. I think that using not the newest of these models is fine and does not significantly affect the results and conclusions of this work. Updating the models is not necessary. But a brief statement about which models exactly were used is recommended. According to my research the latest models would be:

Huba, J. D. (2023). On the development of the SAMI2 ionosphere model. Perspectives of Earth and Space Scientists, 4, e2022CN000195. https://doi.org/10.1029/2022CN000195

with a link to the SAMI2 code on Github, https://github.com/NRL-Plasma-Physics-Division/SAMI2

Emmert, J. T., Drob, D. P., Picone, J. M., Siskind, D. E., Jones, M. Jr., Mlynczak, M. G., et al. (2021). NRLMSIS 2.0: A whole-atmosphere empirical model of temperature and neutral species densities. Earth and Space Science, 8, e2020EA001321. https://doi.org/10.1029/2020EA001321 with link to the public code.

Drob, D. P., J. T. Emmert, J. W. Meriwether, J. J. Makela, E. Doornbos, M. Conde, G. Hernandez, J. Noto, K. A. Zawdie, S. E. McDonald, et al. (2015), An update to the Horizontal Wind Model (HWM): The quiet time thermosphere, Earth and Space Science, 2, 301–319, doi:10.1002/2014EA000089.

We added the references indicated by the reviewer and now state: *"The IRI data are subsequently used in a manner analogous to that in the SAMI2 model of the Naval Research Laboratory (https://github.com/NRL-Plasma-Physics-Division/SAMI2). In the present implementation, the ionospheric equations follow the SAMI2 framework originally developed by Huba et al. [40] and described in detail by Huba [41]. In SAMI2, the neutral atmospheric parameters–namely the neutral number density, total mass density, and temperature–are specified using the MSIS family of models. In this study, these parameters are based on the MSIS formulation of Hedin [42], while we note that more recent updates are provided by the NRLMSIS 2.0 model of Emmert et al. [43]. The meridional and zonal winds are specified using the Horizontal Wind Model. In the present implementation, we follow the formulation of Hedin et al. [44], while more recent updates are described by Drob et al. [45]."*

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We have included these references in the revised list of references.