Review of Revision of Doicu et al., "A numerical model for solving the linearized gravity-wave equations by a multilayer method"

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Overview

The manuscript has been improved significantly. A new technique for including ion drag in gravity-wave models is now described. I have quite a few specific comments on the new manuscript, which is to be expected given the extent of the revisions. The problems with the discussion of causality have largely been corrected, although some things are still not clear, as described below. Some details in the appendix could be removed to reduce the length of the paper, as also described below.

The comments that follow are based on the revision of January 22, 2026.

Specific Comments

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.)"

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.

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".

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.

Page 5: Make h lowercase in "density scale Height".

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.

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"?
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.

Page 9, first paragraph of Section 3: Change "denote" to "denoted" in "… are denote by …". See also the note for page 4 on amplitudes.

Page 10, eq. (33): It says $z_{l+1} \leq z \leq z_l$. Should l be changed to $l + 2$?

Page 10, last line: "Amplitude" occurs again here.

Page 11: Normal modes generally have zero phase speed. Perhaps it would be better just to say "mode".

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.

Page 12 after eq. (47): This would be clearer if it said "for one particular q value" at the end of the sentence.

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).

Page 12, eq. (52): For multiplication of a column vector, $\vec{a}$, from the left, B should be indexed by k, m, not m, k. The same applies to eq. (53) and maybe elsewhere.

Page 13, just after eq. (55): It may be problematic to set $\vec{b}_1 = [1,0,0]^{\mathrm{T}}$ 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.

Page 13, item 3: A reference for LAPACK should be included.

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.

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.

Bottom of page 15 and top of page 16: The right and left sides of these two equations are identical, so they do not seem to be saying anything.

Page 16, first paragraph of Section 4: Correct "it what follows".

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.

Page 17 eq. (81): This condition is problematic at inviscid altitudes for the reason given above.

Page 17 eq. (82): This A looks similar to the A used on page 6.

Page 17 following eq. (82): Correct "s s".

Page 18 eq. (85): Why do you normalize based on the absolute value of the real part? Why not just set f'_{sq0} itself to the value on the right?

Page 18 after eq. (86): It says "as in Section 3, we define $\hat{F}_q(z,\omega)$ …". There was no $\hat{F}_q$ defined in Section 3, so this needs clarification.

Page 19 eq. (92): Again, why do you normalize by the absolute value of the real part?

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.

Page 19 eqs. (97-98): The combination of (86) and (94) gives $\hat{F}_{sq0}(z_1,\omega) = 2\pi A\delta(\omega-\omega_0)$, which is inconsistent with (97). The first part of (98), $\hat{F}_{sq0}(z_1,\omega) = A$ does not make sense if $\hat{F}_{sq0}$ 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.

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.

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.

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. Let $\lambda^+(\omega) = \exp(-\omega^2)$ and $\lambda^-(\omega) = \exp(-\omega^2) + 1/2$. Eq. (108) holds, but there is no σ for which (107) holds.

Page 22 eq. (115): Why is t_0 subtracted from t here but not in (112)? This seems inconsistent.

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?

Bottom of page 22: It says "From these date, we read". This is unclear. Do you mean "data"?

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.

Page 23: Please give a reference for cubic spline interpolation with regularization.

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.

Page 24: When the terms GMMA, SMMA, and GMMN are introduced, refer to the sections in which the corresponding methods are described.

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.

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?

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.

Page 26. "Diffusion velocity" is mentioned. What does this mean? Ion velocity?

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.

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.

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.

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.

Page 28: The purpose of the oval in Figure 2 should be given in the caption or the text.

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.

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 $\mathrm{Im}(k_{z4}) - \mathrm{Im}(k_{z1})$ in logarithmic scale.

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.

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.

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.

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.

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. Let w_j be solution for one of the

δ values, i.e., δ_j, and let j' be the index of the selected δ value. Then you can show $\max_t |w_j(z,t) - w_{j'}(z,t)| \,/\, \max_t |w_{j'}(z,t)|$ for several representative z values. You can also show $\operatorname{mean}_t |w_j(z,t) - w_{j'}(z,t)| \,/\, \operatorname{mean}_t |w_{j'}(z,t)|$. These values will give the reader a good idea of how different the solutions are. If they are small (i.e., less than ~0.01), then there is no problem.

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.

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.

Page 37: The term $\mathbf{g}$ is not defined.

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.

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.

Top of page 38: Define g.

Top of page 40: H_ρ is used in eq. (137) but is not defined until after eq. (140).

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).

Page 43: It is unclear why both sides of (157), (158), and (159) are divided by the same term.

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 μ_{k0} 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.

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.

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.

Page 45 eq. (169): The use of C_1, etc., here conflicts with (162).

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

Page 47: Give a reference for (181), (182), and (183). Also, define $g_{||}$ in (185). Currently, it is defined after eq. (189).

Page 48: Define $\partial/\partial b$ at the first place where it occurs, which is (187).

Page 48: Define **b** in statement after (188).

Page 48: Can you give a reference for this derivation of the ambipolar diffusion coefficient?

Pages 50-51: How do these linearized equations compare with those appearing in previous work, like Volland (1969 "The upper atmosphere as a …"), Yeh and Liu (1974), and Yeh et al. (1975)?

Page 51 towards bottom: It says "We have several options", but only two options are given. "Several" usually means three or more.

Page 51 bottom: It says "diffuse velocity". Should this be ion diffusion velocity?

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.

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.

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.

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.

New references

Broutman, D., Knight, H., & Eckermann, S. D. (2020). Compatibility conditions, complex frequency, and complex vertical wave number for models of gravity waves in the thermosphere. Journal of Geophysical Research: Space Physics,125, e2020JA028011. https://doi.org/10.1029/2020JA028011

Yeh, K. C., C. H. Liu, and M. Y. Youakim (1975), Attenuation of internal gravity waves in model atmospheres, Ann. Geophys., 31, 2, 1975, p. 321-328.