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

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Overview

This paper describes a new modeling technique for atmospheric gravity waves (GWs) and examines the effect of ion drag on GW propagation and dissipation in the thermosphere. The authors find that a banded-matrix technique can be applied to thermospheric GW modeling problems with the same effectiveness as scattering-matrix techniques but with greater computational efficiency. While the aforementioned results are of scientific interest, some of the material in the paper is unnecessary and should be deleted, especially given the length of the paper. Also, further elaboration is needed in some instances identified below.

I am happy to see that the authors include a discussion of results from Knight et al. (2019,2021,2022), which are relevant to their work, but their discussion of causality reflects some misunderstandings and should be substantially revised, as indicated below.

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.
2. The same symbols are used in multiple contexts in some instances identified below.
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.
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.

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.

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

Lines 15-16. Please add 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.

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 μ_z are omitted from the vertical structure equations associated with the main results.

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.

Line 76. Change direction to directions.

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

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

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.

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.

Line 196. Give the value for Pr. Also, the numerical formula for thermal conductivity, $\lambda = 6.71 \times 10^{-4}\, T^{0.71}$, does not look right. Is constant c_p assumed? If so, what value? The specific heat at constant pressure varies with composition.

Line 220. Something to consider for future work is that there are disadvantages in converting to nondimensional quantities. It makes it impossible to check equations in terms of units, and it leads to more complicated formulas as in the Appendix.

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

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

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).

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.

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.”

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.

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).

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? 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.

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.

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

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.

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).

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.

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

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.

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 $A\,\vec{v} = \vec{b}$. 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.

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.

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.

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

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

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.

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.

Line 554. Shouldn’t $b_{1,k}$ depend on ω?

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

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.

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).

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).

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

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).

Line 620. This does not look like a complete list of background parameters. It might be complete with p_0, c_v, and Pr added.

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

Line 627. p_0, c_v, and ρ_0 should also be shown in a figure. The density scale height, $H = -\rho_0/(\partial\rho_0/\partial z)$, should also be shown.

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.

Line 658. It would be more helpful to state that $\kappa_\omega = 0.8$ when such results are discussed, both in the text and in the figures and/or figure captions.

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 $A\,\vec{v} = \vec{b}$ 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.

Lines 690-701. It is not clear what is gained by merely comparing results for different values of κ_ω. 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 κ_ω 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 κ_ω? 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 κ_ω, 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 κ_ω 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.

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.

Also, Figure 7 shows altitude in the x-axis, but it is standard to put altitude in the y-axis.

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

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 κ_ω 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.

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.

Line 729. Say Knight (2019, Section 6.1).

Lines 729-732. I do not see the scientific interest of this. One would expect the results to be similar.

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.

Figure 14 is unnecessary, since the information is already conveyed by Figure 13.

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, a single-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 ω_0? This should be stated.

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.

Line 751. Should (14) be (149)?

Line 751. Units should be given for δ.

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.

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.

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.

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.

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