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Geoscientific Model Development Discussions

Interactive comment on "Correct boundary conditions for DNS models of nonlinear acoustic-gravity waves forced by atmospheric pressure variations" *by* Yuliya Kurdyaeva et al.

Anonymous Referee #2

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General comments:

This paper examines boundary condition for linear wave solutions including sound and inertia-gravity waves, motivated by use of high-temporal resolution surface pressure observation. However, the experimental design is not clearly stated in the paper, and the purpose of the numerical experiments are not clear. The numerical results are not understandable. The reviewer suggests that the paper requires fundamentally major revision before rendering further reviews.

Because sufficient information for numerical experiments is not provided in the text, the numerical results are not understandable. In Section 4.1 and 4.2, a simple wave

solution is discussed. However, in general, IGW must have a reflection at the surface, and two waves are generally required to satisfy the boundary condition w=0. I do not think the solution given by these sections satisfy the linear system. Please check by showing figures of w. The authors seems to apply a realistic case in Section 4.4. However, the initial condition is zero and idealized, so that the solution shown by Fig. 4 must be very different from the reality. This means that the purpose of Section 4.4 in unclear.

In general, to obtain a solution satisfying (surface pressure) observations, the assimilation technique is adapted. Because of error in observations, we do not need to find a solution which exactly satisfy observed values at the surface. The authors should clarify the distinction of the purpose of this study compared to the assimilation approach.

It is not clear what the problem of the surface boundary. Please clarify this is common problem already discussed by previous studies, or the problem the authors first point out. Clarify the contribution of this paper to solve the problem (the uniqueness).

This paper does not mention studies on dynamical cores of widely used meteorological models. Most of the non-hydrostatic models are elastic, and comprise sound and gravity waves. The authors must review the current status of the meteorological models and clarify the differences and advantages of the present model.

Specific comments

p. 1, L33, "excited at tropospheric heights": This is not a familiar phrase. Where are specific heights? Or should be rewritten as "excited within the troposphere".

p. 2, L3, "…Zhang, 2014;)": Remove ";"

p. 2, L4, "propagate from tropospheric height": "propagate from the troposphere"

p. 2, L8, "Cumulus clouds" should be "cumulus clouds" (also, p. 10, L10)

p. 2, L12: The authors should refer to examples of numerical models for atmospheric models. General elastic non-hydrostatic models can simulate sound and gravity waves.

p. 2, L19-20: “Yu et al. (2009) . . .”: The authors should clarify the purpose of this paper examined, instead of just mentioning development of 2D models for AGWs.

p. 2, L32, “3D”: spell out.

p. 3, L7, “infrasound waves”: This term is unfamiliar. Please explain.

p. 3, L18-10, “However, the specifying the surface pressure as lower boundary conditions in the nonlinear numerical AGW models raise some problems not adequately studied in the past.”: Are there any studies which raised problems of the specifying the surface pressure as lower boundary conditions in the nonlinear numerical AGW models? Please refer to the previous studies if exist. This problem is not well understood in general. The authors should clearly define the problem in the introduction.

p. 3, L33, “and available online”: should be changed as “which is available online”.

p. 4, 2.1: Please specify the target the ATMOSYM model is used, such as for the f-plane and DNS system.

p. 4, L19: “the heat equation”: It is the internal energy equation, and can be rewritten as the pressure equation under the assumption of the ideal gas state.

p. 4, L20: “(T’)”: Omit parenthesis.

p. 5, L8, “AGWSYM” should be “ATOMSYM”?

p. 5, L11, “Vertical profiles of the background temperature T0(z)”: Most of the readers are unfamiliar to NRL-MSISE-00. The authors should give a figure of the temperature profile.

p. 5, L11-24: This paragraph describes experimental setup. However, only insufficient information is provided. What are spatial resolution and time step of the numerical model? What is the domain size? Is this a DNS model and then why background

turbulent viscosity is introduced (L20)?

p. 6, L5, “These boundary conditions are usually valid at high molecular viscosity and thermal conduction at high altitudes”: I do not understand this statement for the condition w|z=h = 0. This is an artificial condition, and cannot be validated in nature.

p. 6, L18, “inclinations” should be “deviations”.

p. 6, L22-24, “Changes in the boundary condition for the other hydrodynamic variables may lead to non-correct mathematical problems.”: I do not understand the meaning of this sentence. Please explain more.

p. 7, L13: Please give definition of N2.

p. 7, L28-29, Theorem I: Surface boundary conditions for u and v are not required to derive the theory?

p. 8, L2-6, Consequence: If this consequence is derived, what is the meaning of uniqueness? If R and Ït’ can be arbitrarily added (as long as R + Ït’ is the same at the surface) to the solution, then the solution for the system is not unique.

P, 8, L7-11: I do not understand this paragraph. What the authors imply for the “jumps in R and Ït’ near the lower boundary”? What is the meaning of “mathematical wave modes non-existing in the nature”?

p. 8, L15-18, Eq. (14): Please check the equations. What is ςς(z) and κκ(z)? The right hand side of the equation for Ït’ is confusing.

p. 9, L4-8, “As far as AGW amplitudes are generally small near the ground”: It is not clear in the real atmosphere, the amplitude of AGW is small enough. Please discuss representative values of the amplitude and quantify how much the amplitude of AGW should be small enough for linearization.

p. 9, L17, Eq. (18): I did not check whether these equations satisfy the set of linearized equations (10), but there seems something contradicting. What is H0? Why the Coriolis factor ωz is not included? Does this solution satisfy the surface boundary condition, particularly for w=0? In general, two waves must be required to satisfy this condition, with incoming and reflecting waves.

p. 10, L10, Eq. (21): “mz” should not appear on the right hand side because z=0.

p. 10, L14-16, “In this regime, we can expect good agreement between the numerical results and the analytical solution”: This comparison is meaningless unless details of the experimental setting is presented. What are the spatial and the temporal resolutions? If they are small enough, it is reasonable that the numerical solution converges to the analytic solution.

p. 10, L17-22: What are the initial condition for R and Ïť?

p. 10, L23-27: Already defined H0=8km. What is the value of D?

p. 10, L27: The font for zero should be corrected. “o” is used, and in many places elsewhere including the appendix.

p. 10, L28-32: Please show whether the solution shown in Figure 1 really satisfies the surface boundary condition w=0. In general, reflection IGW exists.

p. 11, Section 4.4: This section should be 4.3. This is a case for a realistic observation. However, the initial condition is idealistic with zero perturbation. The solution shown by Fig. 4 is growth of disturbances from the initial state. This does not resemble the real atmosphere.

p. 11, L9, Eq. (22): “exp[(x-x0)2 . . .” should be “exp[-(x-x0)2 . . .”.

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