Comments on "The Response of Superpressure Balloons to Gravity Waves"

WILLIAM J. MASSMAN[1]

National Center for Atmospheric Research,[2] Boulder, CO 80307

1 December 1980 and 1 May 1981

Nastrom (1980) and Massman (1978) both discuss a balloon's response to gravity waves, and it seems worthwhile to examine the similarities and differences between our approaches and our results. In our respective expressions for the balloon's equation of motion we both include simultaneous density and vertical wind variations; but our formulations for the density term are somewhat different. The basic difference between the two papers lies not in the density term, but in the approximations which were made. I find that Nastrom's results are probably more general than my own.

Massman's notation is used to show that the two formulations for the density variations are the same. Nastrom denotes the density term as $\frac{2}{3}gR(t) \approx -[2\omega_B^2 w/3\omega]e^{i\pi/2}$. The complex phase angle has been inserted to account for the fact that the density variations and vertical wind variations are in quadrature. I denoted the density term by $\kappa\omega_N^2[Z_a(t) - \bar{Z}_0]$. For a gravity wave the vertical oscillations of the atmospheric isentropic surfaces, denoted by $[Z_a(t) - \bar{Z}_0]$,, are related to the vertical wind variations as $[Z_a(t) - \bar{Z}_0] = -[w(t)/\omega]e^{i\pi/2}$. Furthermore, $\kappa\omega_N^2 = 2\omega_B^2/3$ follows from my definition of κ and ω_N^2, where $\omega_B^2 = g(\Gamma_d - \gamma_0)/T_0$ is the Brunt-Väisälä frequency squared. Thus, the two formulations are identical except for the definition of ω_N^2, wherein lies a subtlety.

Nastrom allows for the expansion of the balloon's volume as it oscillates in the vertical and I did not. In my case this approximation appears to be reasonable as it is supported by the direct observations of the period corresponding to ω_N (Massman, 1978); even allowing for these volume variations would cause no more than a 4% decrease in the value of ω_N predicted by my Eq. (4). Apparently, this approximation is not valid for a lower tropospheric tetroon and probably in other situations as well. However, because the volume variations are so small in my case, generally $\omega_B^2 < \omega_N^2$, I further restrict my solutions to the region where $\omega^2 \ll \omega_B^2$, where ω is the intrinsic frequency of the wave.

Thus, my solutions are a special case of that presented by Nastrom, who concludes that "for large values of $[\omega_N^2]$, the response decreases . . . and the density terms dominate. For long period gravity waves, the balloons asymptotically approach a true isopycnic tracer (with some phase shift) for high static stability, and for low static stability this is not true." I conclude the same thing when I state, "In the limit of $\kappa \gg \epsilon$, the balloon is very nearly a true isopycnic tracer because its movements away from its (equilibrium density surface) are negligibly small." In my paper κ and ϵ are dimensionless parameters which determine the relative importance of the density variations and the vertical wind variations, respectively. There is also an interesting, but minor difference between our respective conclusions. During the TWERL Experiment we had many wave observations in the upper tropical troposphere above Ascension Island where the lapse rate can be as high as 7°C km^{-1} at balloon float altitude. In these cases, accurate simultaneous readings of temperature and pressure showed that the density variations were smaller than could realistically be measured. Thus, it is quite reasonable to assume that the TWERLE balloons are very good isopycnic tracers in the upper tropical troposphere, even though the static stability is often low. In these cases generally $\kappa \approx 4\epsilon - 20\epsilon$. Another minor difference between our papers is the definition of the phase angle. The phase angle for my solutions actually refers to the phase angle between the balloon's oscillations and the oscillations of its equilibrium density surface which is different than Nastrom's definition; but we both conclude that there is some phase shift between the balloon motions and those of its equilibrium density surface.

There is one last difference between our two papers that I wish to discuss. We employ different methods for deriving approximate solutions to our respective equations of motion. For the case where $\kappa \gg \epsilon$, we both conclude that the dominant mode of balloon

[1] Present affiliation: Biology Department, University of Oregon, Eugene 97403.

[2] The National Center for Atmospheric Research is sponsored by the National Science Foundation.

0021-8952/81/091089-02$04.50

oscillation has a frequency equal to the intrinsic frequency of the gravity wave, ω; however, for the second-order terms we have quite different results. I conclude that the second order term is an even harmonic with frequency 2ω (cf. my Eq. (7), and Nastrom concludes that it should be an odd harmonic). My own experience with the TWERLE data suggests that the effects of the second order term could not be observed with any certainty, and hence were of little practical interest.

In summary, while both authors include simultaneous density and vertical wind variations, our respective formulations of the density term are different. Further, I ignore the variations in the balloon's volume, i.e., $(g/V)dV/dZ \ll \omega_N^2$ and I restrict my solutions to the region where $\omega^2 \ll \omega_B^2 < \omega_N^2$. Thus, Nastrom's results are probably more general than my own.

REFERENCES

Massman, W. J., 1978: On the nature of vertical oscillations of constant volume balloons. *J. Appl. Meteor.*, **17**, 1351–1356.

Nastrom, G. D., 1980: The response of superpressure balloons to gravity waves. *J. Appl. Meteor.*, **19**, 1013–1019.