Theoretical Study of Atmospheric Pressure Pulse Propagation

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March 1964

Final Report

Contract AF 19(628)-304

Prepared for

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I
INTRODUCTION

This is the final report on Contract No. AF 19(628)-304. It covers the period 1 March 1962 to 29 February 1964. The purpose of this investigation was to formulate a theoretical explanation of the nature and propagation characteristics of atmospheric pressure pulses generated by nuclear explosions. The investigation was based upon the preliminary study conducted by the Radiation Laboratory under Contract No. AF 19(604)-5470 on this subject. A theoretical interpretation has been developed and the analytic work is reported in detail by van Hulsteyn (1964).

The most significant development during the two year period has been the determination of the pressure wave form at a great distance from an explosive source. Numerical and analytic approaches to this problem were considered. The former was used successfully by Weston (1961, 1962) and had proved to be extremely useful. The advantage of this method as opposed to the analytic approach is that it allows a very complicated problem to be treated quickly and with a high degree of accuracy. The mathematical formulation, on the other hand, is limited to special cases, and even then yields results which are only approximately correct.

In spite of its seeming limitation, however, the analytic approach was found to be preferable. The overriding consideration is that this method allows all the quantities to retain their physical identities. Solutions to the radial equation, for example, are found to be Hankel functions whose properties are well known. Their functional behaviors eventually allow us to define the properties of the 'gravity wave' and thus provide a great deal of insight into the physics of the problem.

There must, of course, be some reservations in the use of the analytic method. Most important among these is that a simplified problem which is amenable to mathematical analysis must also conform with physical reality. We must determine, then, whether the assumption of an isothermal temperature distribution will allow us to develop a theoretical pressure wave train which is in good agreement with experimental observations. There might, in addition, be situations for which such a simplification would yield very good results, while in other cases the approximation might be very poor. For example, the isothermal atmosphere will provide good quantitative results for low altitude explosions, but the same statement may not hold when high altitude bursts are considered.

Although the work of van Hulsteyn (1964) contains the final results which were obtained from the work done on this contract, a preponderance of the effort was devoted to investigating the fundamental aspects of the problem. These have been reported upon in previous quarterly reports and so the remaining remarks in this report are confined to a qualitative review.

II

PRESSURE PULSE PROPAGATION

The most perplexing situation in dealing with the problem of the propagation of pressure disturbances in the atmosphere centers on the nature of the atmosphere itself. One would be inclined to suspect that if this medium were assumed to be uniform and isotropic the mathematical difficulties encountered would be few, but the results obtained would be meaningless for practical application because of the unrealistic assumptions. Therefore, much of the early research on atmosphere disturbances was concerned with the possibility of obtaining an accurate description of the temperature variations in the atmospheric structure.

From our studies over the past several years we know this to be an extremely complicated problem, due to the fact that this structure will vary markedly at different points on the earth's surface and, in fact, is unpredictable at any given point due to the uncertain nature of the mass air circulation. Brunt (1952) presents a relatively simple explanation for some of the phenomena observed but even this does not satisfactorily take into account the time variation of the atmospheric parameters. To make matters even more difficult, very little accurate data is now available for altitudes above a few hundred kilometers. At heights of more than 200 Km the temperatures are allegedly on the order of $1500^{\circ}K$, a feature which is certainly subject to some conjecture, due to the fact that temperature in an extremely rarefied medium loses its macroscopic identity. Whether this value is to be thought of as the statistical energy of a random molecule is a question for which we have now no satisfactory answer. It is readily evident, however, that a statistical treatment of the problem is inordinately difficult, if not impossible, and that some way of escaping the microscopic approach must be utilized.

To this end, all of the previous investigators with the exception of Weston (1962) considered the atmosphere to be isothermal above a given altitude and that Euler's macroscopic equations are valid in all regions. Weston hoped to improve upon the results by postulating the thermosphere model in which the temperature increases linearly above a certain height. This approach is in good agreement with the atmospheric values listed in the "Handbook of Geophysics" (1960) but is again questionable, owing to the nebulous nature of the atmosphere at great altitudes. The difficulty with either of these approaches is that we still have to apply Euler's equations in a region where they are not valid and thus have no way of knowing how much accuracy is lost thereby. One way to investigate this situation is to assume that for a given frequency a wave may propagate to a height where the mean-free path and the wavelengths are approximately the same. Following this approach, van Hulsteyn and Akcasu (1963a, b) have assumed that below such an altitude, Euler's equations are satisfied and that above it, the random behavior of the molecular motion serves to produce an indistinguishable signal. The result is that for frequencies in the gravity wave range the wavelengths are of such magnitude that the 'cutoff' altitude may be taken to be infinite without any significant loss in accuracy.

This analytic result is what one might have expected from an intuitive concept but while resolving one difficulty, it raises a series of new problems which will be discussed in turn. The first of these concerns the so-called upper atmosphere and its effect, if any, upon the phase and group velocity of a propagated wave. The second involves the matter of imposing a necessary boundary condition upon a solution if the atmosphere is considered to be infinite in extent. Finally there is the problem of horizontal variations in the earth's atmosphere, an effect which has been all but neglected in the studies of pressure disturbances. All three of these subjects were considered during the past two years and are interrelated topics.

In dealing with the problem of defining a temperature for the upper atmosphere we are seeking to determine the dispersion relations for any wave propagation through the air. For some researchers, obtaining these relationships represents an end in itself since their goal has been the study of the atmospheric structure rather than the description of the form of a pressure pulse. Among the investigators Pfeffer and Zarachney (1962) and Press and Harkrider (1962) present a relatively simple matrix method for studying the effect of arbitrarily complicated vertical temperature profiles. The approach consists of approximating a given temperature variation by a series of isothermal layers capped by an infinite isothermal half-space. In each of these regions a pair of solutions is obtained. By applying two appropriate boundary conditions and matching the various solutions across the interfaces it is possible to obtain a matrix equation which may be solved numerically to produce the dispersion relations.

There are, however, a few minor limitations which might be noted in regard to these and similar investigations as they apply to the task of this contract. The first involves the imposition of the boundary condition at infinity and will be discussed in the following paragraph. Somewhat more significant from the standpoint of accuracy is the fact that these authors used the flat earth approximation rather than a spherical system. In order to determine the effect of the more accurate geometry, van Hulsteyn (1962c, 1963b) extended the matrix method to spherical coordinate, and, in the process, encountered a number of mathematical difficulties. Although none of these proved insurmountable they illustrate the fact that the various solutions previously obtained are, indeed, approximations. The difficulty is due to the possible existence of a 'turning point' which, for certain frequencies, may be very near the earth's surface. When such a situation occurs, the solutions are not valid and the corresponding points on the dispersion curve have no meaning. Aside from this difficulty, the mathematics proceeds in a straightforward manner and in the limit of the flat earth approximation, our results simply reduce to those of Press and Harkrider. The results which we obtain are presented in Section III.

The significant point raised by these calculations is the question as to just how important is the effect of upper atmospheric temperatures. There are three somewhat heuristic arguments which imply that an appropriately chosen isothermal temperature for the entire atmosphere will yield results which are in good agreement with those obtained from more complicated structures. Since these are discussed at some length by van Hulsteyn (1964), it will be sufficient at this point to give a brief synopsis of the arguments raised there. The first point, which may have been implicit in the presentations of Scorer (1950), Hunt et al (1960) and Yamomoto (1957), for example, deals with the density of the atmosphere. From this consideration it would appear that since the air becomes increasingly rarefied with altitude, the effect of temperature at higher levels will diminish accordingly. Essentially, then, a density averaged temperature should prove a valid approximation to the actual structure for the purpose of obtaining the dispersion curves. There should, admittedly be some difference between the results so obtained and the more accurate values derived from the consideration of more exact temperature models. Since our purpose is to construct the time dependent pressure pulse arising from a nuclear explosion rather than to investigate the atmospheric structure we are willing to sacrifice some accuracy for the mathematical simplicity which this approximation affords us.

It is not too surprising that our dispersion curve obtained from the isothermal model agrees remarkably well with the results presented by previous authors. More significant is the fact that these curves are certainly good approximations to the empirical group velocities plots presented by Donn and Ewing (1962). On this basis it is definitely felt that a more exact temperature representation would not be necessary from the standpoint of proceeding with the mathematical investigation of the pressure pulse. The final proof of the validity of this approach arises from the form of the theoretically derived wave train. Using the isothermal model we obtain (van Hulsteyn, 1964) the gravity wave portion of the pressure pulse which is in exceptionally good agreement with the empirical data reported by several observers. This, we feel, is the decisive argument as far as the accuracy of our methods is concerned. (We shall return to this point in the conclusion of this report, however, since a situation might arise which would obviate our results).

The problem of the boundary condition is a feature which heretofore has been either misunderstood or misrepresented. This is discussed thoroughly (van Hulsteyn, 1964), but since much confusion has arisen on this point, we feel that this is a matter which cannot be overstressed. Traditionally, it has always been assumed that the frequency dependent kinetic energy in an infinite column must be finite. This is one way of expressing the fact that of the two solutions existing in the upper region only one is acceptable. Hocking (1962) felt that placing a restriction on the kinetic energy is unacceptable since this still allows for velocity terms whose magnitudes are unbounded as the altitude becomes infinite and these terms violate the condition under which the hydrodynamic equations are linearized. This interesting observation, though correct in theory is wrong in practice. We shall return to his treatment of the problem in a paragraph to follow but will first give what we feel to be the correct statement of the boundary condition.

Essentially, in the process of linearizing the hydrodynamic equations we require that the time dependent quantities be much less than their steady state counterparts. We observe, then, that our solutions must be such that

$$\lim_{r \to \infty} \frac{p(\underline{r}, t)}{p_o(r)} \ll 1$$

from which it also follows that the velocity terms must go to zero. In a sense, then, Hocking's statement was correct, but he was applying the restriction to the frequency dependent, rather than the time dependent quantities. The condition upon the pressures as stated above may be made only after the frequency-dependent solutions have been determined and the inverse Fourier transformation has been performed.

It is interesting to note that there is apparently no such quantity as a cutoff frequency, at least in the concept to which the term has been previously applied. Scorer, Hunt et al, and a whole host of others have argued that for frequencies above the so-called cutoff value the kinetic energy in an infinite column becomes infinite and must therefore not be allowable. What we find, on the other hand, is that for frequencies above this value, most of the energy is radiated into space. This, then, lends physical credence to the term gravity wave, which is in essence the very low frequency portion of the spectrum. For this portion, all of the energy is trapped by the atmosphere and thus propagates around the earth rather than radiating into space.

In this manner we avoid the concept of the cutoff frequency as it was originally postulated and the difficulty of conjuring up a mechanism to explain the absorption of unallowable frequencies no longer confronts us. These hitherto non-existent values are actually excited by an atmospheric disturbance but normally make only minor contributions to the detected pulse form due to the fact that so little of their energy is propagated around the earth.

Returning now to Hocking's (1962) idea, there might be some merit in considering the effects of atmospheric viscosity. He simplified the situation greatly by choosing the isothermal, rather than the adiabatic energy condition. With $\gamma = 1$ from this assumption, the analysis is not so complicated but neither is it entirely realistic. At any rate, he shows, through a somewhat qualitative procedure, how one can obtain frequency-dependent velocity terms which do indeed go to zero at infinite altitudes. From the explanations of the preceding paragraph, however, we note that this is a sufficient but not a necessary condition. It is questionable then as to how much significance may be attributed to the inclusion of the viscosity parameter into the equations of motion. This is known to be an effect which becomes increasingly great as the atmosphere becomes correspondingly rarefied. Hence, the kinematic coefficient of viscosity is negligible near the earth's surface but increases exponentially with altitude as the inverse of the steady state density. One might expect, then, that its effect would be great, and yet, from an earlier argument we feel that the dense air at low altitudes is the major factor in determining the behavior of wave propagation. This dilemma is one which has not been satisfactorily resolved from a quantitative standpoint. An attempt was made by van Hulsteyn (1962a, 1962b) to continue Hocking's method, and he derived an extraordinarily complicated fourth order eqaution and four rather involved boundary conditions.

Whether or not these conditions are necessary is not known, but they were found to be insufficient and no means has yet been devised for performing the numerical integration on a digital computer. Upon discovery of the fallacy in Hocking's argument, the investigation was dropped. The problem of the viscous effect, then, is one which might be of interest to some purist who, at some later date, feels that we were somewhat hasty in abandoning this idea.

This brings us to the final atmospheric consideration, namely the effect of horizontal as well as vertical variations. Since we have observed that low altitude contributions predominate we would expect that winds and temperature variations along great circle paths might have a more pronounced effect than had been previously supposed. One good reason for ignoring these variations is that in all but the simplest case we are forced to deal with a non-separable equation. Weston and van Hulsteyn (1962) did consider this special example, namely the local wind effect, and did find that a slight difference is introduced into the dispersion curves.

A method for handling the global wind problem was offered by van Hulsteyn (1963a) in which it is shown how the density and velocity terms may be eliminated from the equations of motion, leaving a non-separable equation for the pressure. One approach to handling this problem might be to subdivide the earth's surface into a number of local regions. In each of these a dispersion curve can be obtained, and it might be possible to match solutions across the boundaries of these regions.

It is not difficult to visualize intuitively the manner in which the horizontal variations might accelerate or retard the arrival times of a signal at a given point, but there is some doubt as to whether the cumulative effect of global winds along a path of travel will significantly affect the period of oscillation of the signal at the observer's location. This unresolved question is one which we definitely feel might be worth pursuing. Most of the theoretical groundwork for the analysis has already been performed, and all that remains is to devise a scheme whereby the numerical computations may be accomplished.

This completes discussion of the atmospheric problems which have been encountered during the past two years in the process of studying the detection of nuclear signals. It is now felt that perhaps too much attention was focussed on this portion of the problem, yet it would have been impossible to proceed meaningfully without some awareness of the pitfalls that do exist. It is perhaps ironic, in retrospect, that in the analysis of surface bursts we should revert to the simplest of atmospheric temperature models. At the same time, had we proceeded in such a fashion at the beginning we would have been guilty of unjustified assumptions and our methods would definitely have been subject to criticism. As it now stands, we feel that a fairly exhaustive investigation of various possibilities has been performed, and the fact that the theoretical results in van Hulsteyn (1964) correspond very closely to empirical data cannot be passed off as fortuitous.

The final point which we wish to discuss as far as the methods employed are concerned is the relationship between the energy of an explosion and the amplitude of the detected signal. This also is mentioned in van Hulsteyn (1964) and represents insofar as we know the first attempt to relate these quantities. The basic idea in its inception is analogous to an approach presented by Weston (1962), in which we assume the temporal behavior of the pressure on some surface enclosing the source is known. Whereas Weston introduced the clever though artificial process of "shrinking" the surface to zero, we found it more fruitful to leave the surface intact. In this manner we can compute the energy efflux across this surface and thus determine the yield of the explosive source. This in many respects represents the key to the entire problem, since the behavior of the pressure on this surface enables us to determine the pressure at any exterior point. A knowledge of the total energy flow across the surface, then, makes it a relatively simple matter to specify the amplitude of the signal that an observer will detect at some distance from the explosion. The analysis and results are given in great detail in van Hulsteyn (1964).

III

NUMERICAL RESULTS

From the standpoint of time and effort versus results, our efforts to obtain dispersion curves by using the matrix method of Press and Harkrider were the most frustrating. The theoretical and numerical approaches are presented in van Hulsteyn (1962c, 1963b); from these we were able to obtain the points shown in Fig. 1. For the higher frequencies there seems to be a definite pattern consisting of three modes. These, however, do not connect with the points at lower frequencies in any identifiable manner. Attempts to find intermediate points were thwarted by the fact that some of the parameters in the dispersion equation became imaginary for all trial values of the wavelength.

One might tend to think, then, that there exist "forbidden" frequencies, in the sense that they do not correspond to any wave propagation. An alternate possibility is the fact that these frequencies bring us too close to a turning point, with the result that our approximate solutions are not valid. The method of Press and Harkrider (1962) is different from that pursued under this study. By choosing the simple geometry they excluded the complications that we encountered. The trial values that we used as starting points in our calculations were the values they obtained as final results. We can, in a sense, conclude that in certain regions of the frequency spectrum, their values were not valid. However, we did not succeed in determining the proper answers.

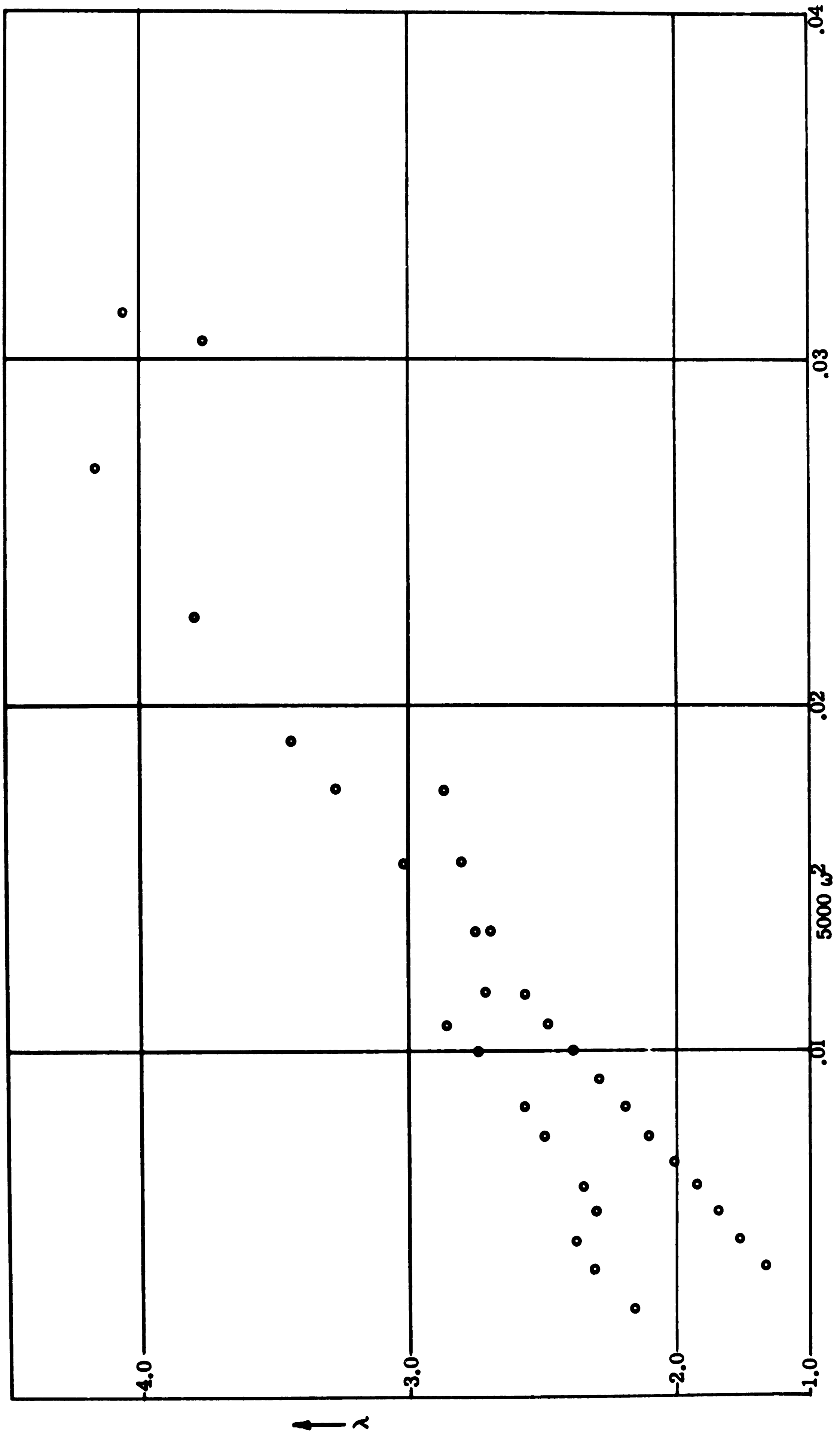

FIG. 1: PLOT OF VALUES OF INVERSE PHASE VELOCITY (λ) AS FUNCTION OF 5000 ω^2 (WHERE ω IS FREQUENCY)

IV
RECOMMENDATIONS FOR FURTHER STUDY

In conclusion, after having presented the advances that have been made in the treatment of the atmospheric disturbance problem, it might be worth mentioning that certain areas of investigation still remain to be considered. The most important unresolved difficulty deals with the handling of the high altitude nuclear explosion which has certain inherent difficulties not manifested in the particular problem we have solved. To be more specific, let us consider the situation in which a nuclear device is detonated at an altitude of 300 Km, where the air is very rarefied and the temperature is an ill-defined quantity. In this region, the mean free path of the molecules is on the order of 1 Km and the validity of the hydrodynamic equation is therefore certainly suspect. On the supposition, however, that these do hold, the problem of ascribing an atmospheric temperature distribution becomes important. We would be tempted, by virtue of our earlier arguments, to assume the same isothermal structure as was employed for the surface burst problem. Apparently, however, the behavior of the dispersion curves as recorded from empirical data differs markedly in the case of high altitude explosions. A partial explanation lies, of course, in the fact that the short wavelength oscillations cannot propagate due to the mean free path spacing of the molecules. Whether this is a sufficient interpretation of the anomalous behavior, however, is not known, since so little data is available on the subject of high altitude explosions.

In considering the high altitude explosions it is apparent that more attention should be focussed on the high frequency "acoustic waves". These were ignored in van Hulsteyn (1964) since our concern in that discussion was limited to the gravity wave mode, but are the subject of Goodrich (1963). The reason for the importance of the role of the acoustic waves in studying high altitude bursts is based upon the absorption of the higher frequency components in a rarefied medium. It is not expected, for instance, that the shapes of the gravity waves from bursts at ground level and at an altitude of 300 Km will differ markedly. The same statement does not apply to the acoustic waves. It seems very likely that this portion of a wave train would be considerably distorted, and this idea makes this facet of the problem worth pursuing.

A final problem which we have been investigating during the past two months is the concept of causality. Our concern about this feature of the problem was mentioned in van Hulsteyn (1964) in regard to the theoretical form of the pressure pulse. It was at first surprising to us that we appeared to be detecting a signal long in advance of its anticipated arrival. The reason for this behavior is due to the fact that our mathematical treatment of the problem considered only one mode of oscillation. The effect of all modes must, of course, be such as to cancel the contribution that arrives prior to time zero.

V
ACKNOWLEDGMENTS

The author wishes to express his appreciation to Dr. Raymond Goodrich for his guidance and consultation on this problem. His contributions, and those of Dr. Z. Akcasu, J. Ducmanis, B. Harrison, R. Hiatt, Prof. L. Hocking, D. Hodgins, H. Hunter and Prof. N. Kazarinoff provided invaluable assistance. I would also like to thank the contract monitor, Mrs. E.F. Iliff, for her patience and cooperation during the period when the contract was in effect. Finally, I wish to acknowledge the assistance rendered by our secretarial and drafting staff.

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