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SHOCK AND VIBRATION BULLETIN

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NO. 5

NAVAL RESEARCH LABORATORY
COMMODORE H. A. SCHADE, USN, DIRECTOR
WASHINGTON, D.C.

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FOREWORD

This Bulletin, like any other technical periodical, is a medium for disseminating ideas and methods which are of interest to a specialized group. The material presented herein is intended to be helpful, in one way or another, to those engaged in the field of shock and vibration under the sponsorship of the Armed Forces. In order to reach the technician who really uses the Bulletin, it is essential (1) that those who select and assemble the material to be distributed know the general type of information which is in current demand; (2) that the information reach the worker promptly; (3) that the recipient of the information become, also, a contributor, either by presenting scientific papers for the publication or by discussing the material which is offered by others.

To that end, and for the mutual benefit of all participating activities, a column is provided on page 72 to determine if and to what extent the Bulletin is fulfilling its mission.

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THE SIXTH SYMPOSIUM

Naval Ordnance Laboratory
17 September 1947

ACCELERATIONS EXPERIENCED BY PROJECTILE COMPONENTS

By

D. E. Marlowe, NOL

Perhaps it is of value to point out to the members of this Symposium that the papers presented at the sixth meeting, under the title "Shocks Peculiar to Ordnance," may be divided into two main classes. The paper is concerned either with the effects of very large accelerations, or with the effects of very long duration accelerations. Both of these classes, particularly the second class, are truly peculiar to ordnance.

The problem of long duration accelerations lies at the very root of many of our difficulties in ordnance design. It results in an impossible cushioning problem (since we are always limited in space), and our designers, therefore, must build devices which will withstand the full value of these accelerations. This is a distinctly different situation from that arising in the design of shipboard equipment, where the shock is of only a few milliseconds duration, and there is often plenty of room for cushioning.

I am pleading, therefore, for a certain understanding which is implicit in the following papers. When, for example, Dr. Wayland speaks of an acceleration of 400 g's when a torpedo strikes water, or, I speak of a projectile acceleration of 50,000 g's in the gun, the reader should insert the mental qualification "against which cushioning is of no avail." Only then will he fully appreciate the problems of the designer of modern ordnance.

I would like to discuss very briefly that part of the field of ordnance in which perhaps the highest forces and accelerations lie. The problem in interior ballistics may be stated quite simply. The designer would be satisfied if he could predict two quantities from his theoretical data alone. These are the maximum pressures in the gun and the muzzle velocity. Needless to say, present theory does not permit any such generalization. The linearizations and the constants of the most modern interior ballistics theory turn out, as new experimental data become available, to be less linear and less constant than the mathematician would desire. As a result, we have to depend still on field trials for the determination of maximum pressure and muzzle velocity of any new gun. There exists, at present, a gap which will be closed only by further improvement in both theory and experiment.

The relationship between gun-barrel pressures and muzzle velocity, and the accelerations with which this conference is concerned is clear. The gun pressures are the source of the acceleration, and the muzzle velocity is the end result or time integral of those accelerations. The designer both fears these accelerations and makes use of them. The same forces which are so large as to cause complete disruption of a poorly designed projectile are sufficiently large that we make use of them most comfortably in the operation of fuzes and arming devices. So the designer today finds himself in an interesting dilemma. He has become accustomed to using the accelerations and spins of the projectile in order to operate his fuzes. He must work, therefore, in the design region which lies below the forces which will cause complete disruption of the projectile, yet above the forces which would be so small that his fuzes could not function. Examples are numerous.

During the war, designers used the accelerations of the projectile to break the battery capsule in the VT fuze and utilized the spin of the projectile in order to remove safety devices in mechanical fuzes. On the other hand, those who have worked with experimental aluminum projectiles are fully aware of the disruption that can be caused by these large forces.

Now, the forces involved are worth a brief consideration. The propellant in a gun is not a high explosive, in the technical sense. It burns rather than explodes, and the reaction may last for something on the order of 10 to 20 milliseconds. In war-time guns, this reaction resulted in chamber pressures upward of 20,000 pounds per square inch which, in turn, gave forces on the order of two or three hundred thousand pounds acting on the base of the projectile. Some of this force is expended in engraving the driving band in order to give sufficient spin to the projectile that it may be stable without fins. The remainder of the force, except for a small amount of friction, accelerates the projectile down the gun barrel. The projectile leaves with sufficient spin to be stable and with sufficient velocity to do harm when it arrives at the target. These muzzle velocities, in war-time guns, ranged in the region from 2500 to 3000 feet per second. Table I illustrates the accelerations which have been calculated for projectiles of various sizes. You will notice that the muzzle velocities of these guns all lie in the region between 2500 and 3000 feet per second. As might be expected, the smaller caliber guns require the higher accelerations to attain these velocities, but as the weight of the projectile is reduced, these accelerations can be obtained at reasonable chamber pressures. It should be noted that, while for a small caliber gun the acceleration will be on the order of 100,000 g's, in the sizes where complicated devices may be carried in the projectile we are working with much more reasonable accelerations, none of them higher than about 15,000 g.

It is necessary to draw a distinction between these accelerations, which last for 5 to 15 milliseconds, and the high frequency accelerations which arise from secondary causes. Even in the large projectile, accelerations on the order of 100,000 g's frequently occur, although these are at relatively high frequencies. Among the secondary causes are: (1) the side slap of the projectile as it travels down the barrel partially balanced on a driving ring, (2) elastic reverberations within the projectile, and (3) the acoustic waves in the propellant gas, as they reflect between the base of the projectile and the base of the breech chamber.

Until the last years of the war, the instrumentation available to measure these forces and accelerations was relatively primitive. Although substantial advantages had been obtained in Diesel engine analysis by the use of modern instrumentation, there had been little effort to apply these techniques to the problem of interior ballistics, in spite of the similarity between the two types of measurements desired. As might be expected, the first measurements were made with crusher gauges of various types. Deformable cylinders and spheres were used for measurements of pressure. Indenter gauges of various types were also used within the projectile for the meas-

TABLE I

GUN PROJECTILE BALLISTICS

CALIBRE	MUZZLE VELOCITY F/S	MAX. BORE PRESSURE IN NEW GUNS (LONG TONS/IN^2)	MAXIMUM SETBACK "FORCE" G'S	SPIN AT MUZZLE R.P.S.	MAX. RETARDATION IN FLIGHT (CREEP "FORCE") G'S
20-MM	2725	24	96,566	1154	...
40-MM	2890	19.5	43,215	734	...
5"/38AAC	2600	18	14,344	208	7.2
6"/47AP	2500	18.5	9,012	200	3.5
8"/55HC	2800	18	7,796	168	3.5
16"/50AP	2500	18	3,003	75	1.1

urement of acceleration. I think that most of this group is aware of the common limitations of crusher type gauges, but there is, in addition, one unique problem in the measurement of projectile accelerations. It is most difficult to get the gauge back after the test has been made. The Applied Physics Laboratory of Johns Hopkins University and the British Armament Department independently developed the technique of vertical recovery to solve this problem. It was found that certain classes of projectiles, when fired vertically, would return to earth tail down. Thus the launching and recovery accelerations, it was found, were in the same direction and of about the same magnitude. This technique was quite successful. It was found that a work crew, well trained in the use of post-hole diggers, could recover about 98 percent of the shots fired. Indenter gauges which would discriminate between the launching and landing accelerations were developed by Professor Mindlin of Columbia University.

There were also a couple of serious attempts to measure the acceleration-time curve of the projectile, by measuring the acceleration-time curve of the gun upon recoil. The accelerations of the projectile are then deduced from the relative masses of gun and projectile. The Taylor Model Basin made a fine attempt to measure the acceleration of the projectile in a 1.1" gun. I think they would be the first to agree that, since the elastic deflections of the gun itself are a somewhat unknown quantity, the accelerations are probably not more accurate than the first significant figure. Similar attempts have been made by the British, with corresponding results.

In the last years of the war there were some serious attempts in the measurements necessary in the field of interior ballistics. The scientists of the Ballistics Research Laboratory, Dr. Curtis' group at the Bureau of Standards (who did his actual tests at the Taylor Model Basin), the Naval Ordnance Laboratory and the personnel of the Carnegie Geophysical Laboratory made serious attempts to measure some of the critical phenomena by modern methods. These measurements included pressures, temperatures and strains, as well as, the accelerations with which we are here concerned. It is not possible within the limits of this paper to give a complete summary of measurements of this type. The measurements discussed here are those with which I was personally acquainted, and it should be recognized that other contributions have been made in this field from widely scattered sources, both American and foreign. The measurements listed here should be considered to be representative rather than comprehensive. Figure I is a record of phenomena in a 6" 47 gun, taken by personnel of the Naval Ordnance Laboratory at the Naval Proving Ground, Dahlgren. Measurements were made of the pressure at three different points in the powder chamber. The pressure time curves obtained are related to the normal gun performance by including on the record such measurements as the closure of the firing switch, the flash of the primer which ignites the propellant, and the recoil of the gun. The techniques used are derived from those of Diesel engine analysis and are typical of the records that have been obtained by modern investigators in this field.

The subject in which this group is most interested, the measurement of the acceleration of the projectile, is fraught with certain peculiar difficulties. The methods which have been most successfully used so far have all been a measurement of the position of the projectile and a deduction of the acceleration curve by a double differentiation of the displacement-time record. Among the measurements of this type which have been made, Dr. Curtis, working at the Taylor Model Basin, set up a microwave interferometer, working with microwaves of the same approximate length as the caliber of the gun. Energy was transmitted down the barrel, reflected off the front of the advancing projectile and compared in phase with the initial radiant energy. From this data

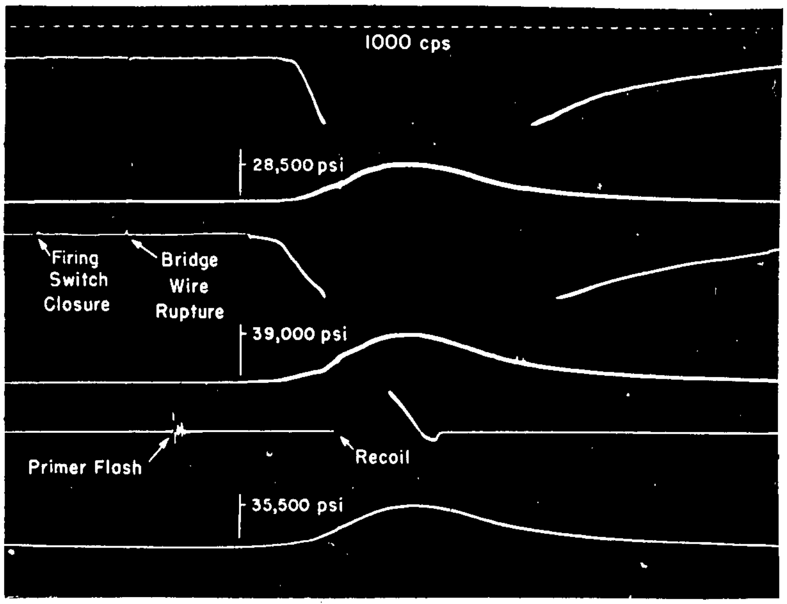


Figure I. Sample Oscillogram

it was possible to determine the position of the projectile to within about an inch. In a somewhat similar attempt, the personnel of the Geophysical Laboratory stretched a taut wire concentric with the bore of the gun from the nose of the projectile to the muzzle. As the projectile was fired, the inertia of the wire kept it in place. The wire was collected in a cup mounted on the nose of the projectile, and the resistance of that portion of the wire suspended in air between the muzzle of the gun and the projectile was measured. The decrease in resistance of this wire was used to determine the position of the projectile within the gun. A third attempt in this field was made by the British, who put strain gauges on the outside of the gun and measured the strain wave which progressed down the gun under the influence of the driving band on the projectile. All of these methods apparently give about equal accuracy and, of course, are suitable only for measuring the main acceleration of the projectile. These methods have been refined until the uncertainties now are of the order of 10 percent. This represents, I believe, a substantial achievement, and it is not likely that a method having the inherent inaccuracy of double differentiation will be much improved beyond this point. It must be clear that any method depending on double differentiation has very severe limitations in frequency resolving power; and that any short period accelerations, due to side slap in the barrel or acoustic waves in the propellant gas, cannot be detected.

Another force which is important in the interior ballistic calculations is the retardation due to friction of the projectile in the gun barrel. Many experimenters have made measurements of this force, and the work of Dr. Curtis on the 90 millimeter gun is, perhaps, typical. There are two forces which must be considered. The first, and larger, is the force required to engrave the driving band of the projectile. The second is the frictional force between the projectile and the gun barrel after the engraving has been completed. Measurements of these forces were made both statically and dynamically. The static measurements consisted of forcing the projectile through the barrel of the gun by ram pressure and measuring the forces required on the ram, first to engrave the band and then to complete the remainder of the travel of the projectile after engraving had been accomplished. The measurements were made dynamically by determining the amount that the forces acting on the base of the projectile, due to powder pressure, would have to be reduced in order to produce the displacement time curve observed in the gun. It was found in tests on the 90 millimeter gun that the bore friction, measured dynamically, was around 10,000 pounds, which was well below the static measurements of 50,000 pounds. On the other hand, the measurements of engraving friction were quite similar in both static and dynamic measurements, both tests yielding a value of about 100,000 pounds.

In this series of tests Dr. Curtis also made an interesting comparison between accelerations of the projectile as measured from the displacement time curve and the recoil accelerations of the gun as measured by a crystal accelerometer. It was found that the acceleration time curve of the main acceleration of the projectile, as obtained by these two methods, could be found to agree to within about 15 percent, which is certainly within the margin of experimental error.

The foregoing selection of representative data from the measurements which have been made by many agencies should, I think, give this audience an understanding of the present state of the art of measurement in interior ballistics. The picture is not a particularly encouraging one inasmuch as relatively few measurements have been made as compared, for example, to the field of internal combustion engines, and yet the expense involved in making such measurements is considerable. It will take an expenditure of much money and effort to obtain additional data, even with the instruments which are available now. At the same time, new problems have arisen since the end of the war which appear to be an order of magnitude beyond the difficulty encountered with war-time guns.

The forces with which we must contend will rise sharply. People in the antiaircraft field are talking about high-velocity guns with muzzle velocities above 6000 feet per second. One of the promising ways to obtain these high velocities is with the use of squeeze bore guns. The forces which are required to deform squeeze bore projectiles, and the influence of these forces upon projectile design and fuze design are quite unknown at the present time. It is only certain that these forces and strains will be very large. In our attempts to develop smokeless and flashless powder we appear to be entering an area in which high-amplitude short-duration oscillations in pressure will occur when the propellant is ignited at low temperature. These pressure oscillations, of course, are transmitted to the projectile and will constitute a new source of high-amplitude accelerations which the projectile and fuze must withstand.

At the same time, we are attempting to put more complicated devices into our projectile. The VT fuze was a complication of at least an order of magnitude, and further complications in this region are already in sight. In addition, we are more and more requiring our fuzes to discriminate between targets. We want the fuze, for example, to look over a target and decide, "Now this is water, so I will not fire until I have delayed for a quarter of a second," or, "This is a piece of steel plate, so I will fire after one thousandth of a second." While such fuzes appear feasible, it is clear that they will not be made of parts which can be turned out by a simple lathe operation.

Since the end of the war, then, we have both increased our requirements on the projectile and on the fuze and have increased the force which these more complicated devices must withstand. Our designers, therefore, are more and more raising the questions - "What are the forces?" - "What are the accelerations?" - What are the possibilities of cushioning?" - and it must be clear by now that we are in a relatively poor position to answer those questions. It is clear that the main acceleration of the projectile which has a time duration of 5 to 20 milliseconds, and a space duration of 5 to 50 feet, cannot be relieved by any form of cushioning. Projectile and components must simply be built sufficiently strong to withstand this acceleration. The accelerations which were listed in Table I, ranging from 1000 to 10,000 g's, cannot be eliminated, therefore; and the designer must simply accept the fact that his devices must withstand these accelerations. However, it appears probable that something can be done about the high frequency acceleration arising from side slap and similar secondary causes. It is likely, therefore, that measurement of projectile accelerations and forces must be made with instruments and equipment which will give insight to these high frequency phenomena.

Now, what do we expect these high frequency phenomena to be in future weapons? It is very difficult to tell. Estimates have been made that we may be interested in pressure fluctuation with peaks as high as 150,000 pounds per square inch and that we may be required to record at 50,000 cycles per second in order to make accurate measurements on these pressure fluctuations. As regards the accelerations, it has been suggested, by the people who are developing vacuum tubes to withstand high accelerations, that tungsten filaments may be particularly susceptible to shock and that, since tungsten is the only known material which is suitable for filaments under these conditions, we may be interested in the phenomena of the propagation of a shock wave along a tungsten filament. It has been suggested that we may have to make measurements in the region of 100,000 g's and record frequencies as high as 400,000 cycles per second. These values are extraordinary and lie well beyond the range of present equipment. As far as the measurements of strain are concerned, we do not yet know what to expect, but it is clear that the strains must lie within the plastic range, and that alone is enough to give grave concern to the designer of end instruments for the measurement of strain. It is most probable that, if these estimates are correct, equipment and techniques not presently available must be developed to meet the problem.

At the present, the Navy and Army are gradually turning their attention to the measurement of these critical phenomena as a function of time, in their most modern and future guns. For the immediate future, we do not foresee radical changes in the equipment required. We shall continue to use piezoelectric, condenser or resistance wire strain gauges as end instruments, choosing the most suitable for any particular application. Portable recorders are available which will give fine results in the region below 50,000 cycles per second. These can be improved if the requirements for better recorders arise. There are, however, two major problems in the instrumentation of guns on which substantial progress must be made. The first of these is concerned with communications between the projectile and the recorder; that is, the transmission of data from the end instrument in the projectile to the recorder which must be stationed outside the gun. This problem is a very difficult one and one for which there does not exist, today, any clear solution. Experiments are now being conducted by the National Bureau of Standards for the purpose of adapting a wire, concentric with the bore of the gun, as a communication channel between the projectile and the gun. This attempt is similar to that tried by the National Geophysical Laboratory in their work. These attempts have just begun, but it appears to be at least a fairly promising method. It has also been suggested that an antenna, concentric with the bore of the gun, projecting from the nose of the projectile might be used to radiate energy to a nearby recorder. This method will probably be explored, although it is questionable whether a signal can be transmitted by such a means without distortion. I am sure that the people working on this problem would be most grateful for any suggestions which the members of this group might have. A second major problem which must be solved may be stated as follows: "Once we have obtained these acceleration time records, what will we do with them?" "How can they assist us in design?" There appear to be, at present, two courses open to us. If we know the accelerations of the projectile and can learn enough about the behavior of materials under shock (and not only materials, but such things as screw threads, gaskets, press fits, etc.), then we may be able to compute the response of any mechanism within the projectile when it is subjected to the accelerations that the projectile experiences. The other possibility, after having obtained acceleration-time records on the projectile, is to develop some apparatus which will play these accelerations back upon the device being tested. There are several experiments under way in this field, of which the High G air gun, which you saw at the Naval Ordnance Laboratory, is one of the most promising.

It must be clear, however, that our present requirements force us to obtain the maximum performance from pickups, communication systems, recorders and playbacks in the field of interior ballistics, and that only a major effort in all of these fields will give us the data that are required to design modern ordnance.

DISCUSSION

MR. SOO-HOO, BuShips

Mr. Marlowe's presentation of the problems in interior ballistics gave me the idea that the study of motion of the projectile in the barrel is still rather rough. It occurs to me that it might be possible to take high-speed movies of the projectile if radium could be used as the radiation source.

IMPACT AND PENETRATION OF WATER

By

Dr. J. H. Wayland,
NOTS, Inyokern, California

Since the tactical use of aircraft-launched weapons, such as torpedoes and mines, may be limited by their ability to withstand the shocks associated with water entry, the study of these forces and accelerations is of vital importance in naval ordnance development. This paper discusses the present status of water entry investigation and simulation, with notes on the theoretical and experimental methods of attack in use and proposed.

Water entry shock problems are not confined to ordnance devices. Probably the first important theoretical treatment of the problem was made in connection with the landing of seaplane floats. Karman's classic work, followed by the work of Wagner, is still referred to frequently by theorists in the field. However, I shall discuss primarily the problems associated with the water entry of torpedoes, particularly the structural problems and those of measuring the impact forces involved when the torpedo goes into the water. I chose torpedoes for two reasons. First, it's the one field in which I have firsthand information and, secondly, because the aircraft torpedo represents a very complex structure. It is a case in which we need a very efficient package to carry a lot of gadgets. The external shell is of no value whatsoever, except to keep the water out. Any weight we carry in a structure that is not absolutely necessary is just extra dead weight to be carried by the propelling vehicle.

The types of failure that we find are several. In the first place, at the point of initial impact we have extremely rapid loading and may find local denting or even local failure, due to too high a rate of loading or merely too much loading. Again, the body as a whole (we have simply a shell) may collapse on itself, and the tail catch up with the nose; then we get an accordian pleat.

We find that the conventional aircraft torpedo on water impact is given a sharp twist (which we call a whip) while the nose is getting through the interface and that the resulting moment may cause failure in bending. Fortunately, most aircraft torpedoes in the past have been extremely strong in the central section, since the heavy air flask must be strong to stand the air pressure, and, consequently, the center section rarely fails. The tail section, which is thin, sometimes tends to double up onto the nose when we get too much whip. This whip tends to strike the back of the fish against the side of the empty cavity which is formed by the nose. When it hits the side of the cavity, we may get local denting or even some structural bending.

There is, of course, another extremely important problem, that of damage to components within the torpedo. We find that we have a short, sharp shock (which is often simulated by an impulsive velocity change) followed by a rather steady deceleration. We have the problem of isolating the effect of the two types of loading on the behavior of various components. Fortunately, there are good laboratory techniques for studying components, particularly at the Naval Ordnance Laboratory with the air guns, but there is still much to be learned about the actual shock pattern associated with water entry.

Having briefly outlined the problem, I would like to show you a little bit about how we at the Underwater Ordnance Division of the Naval Ordnance Test Station (which during the war was part of an OSRD project at the California Institute of Technology) have been attempting to study this problem. I will show you a little bit about facilities, something of our method of approach and some of the results we have obtained. Figure I shows a longitudinal section at the Morris Dam Torpedo Launching Tube. This tube, set at 19 degrees with the horizontal, 22½ inches in diameter and 300 feet long, was constructed for studying the water entry problems associated with aircraft torpedoes. In this tube we have achieved velocities as high as 1350 feet per second by using rather light objects. With a one-ton torpedo we can operate in the neighborhood of 800 feet per second. This gives us the opportunity to study water entry under very accurately controlled conditions.

At times we have been criticized for the accuracy of our control. I think any of you who understand the methods of science realize that accuracy is absolutely essential; otherwise, we have no idea what may be causing trouble. We must control the entry conditions, and we must be able to measure those conditions with precision. It is true that measurement alone is sufficient if it is done well enough, but getting a particular

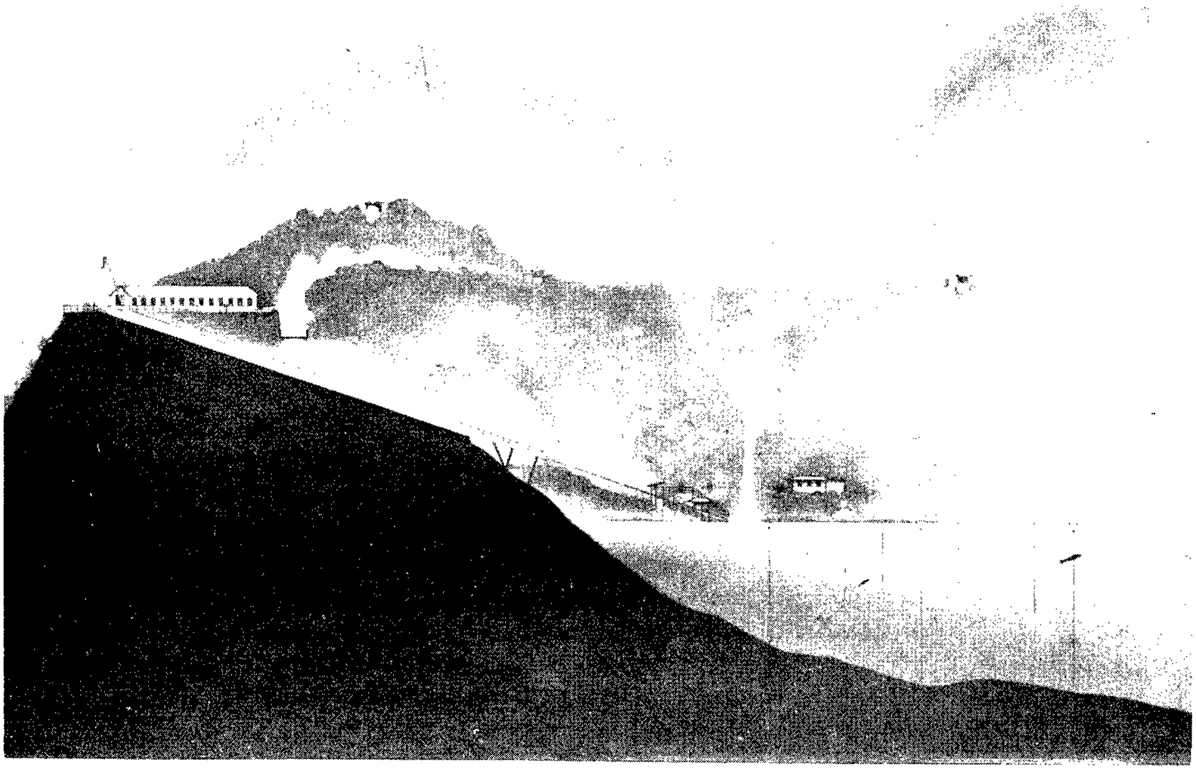


Figure I. Longitudinal Section - Launching Tube value of entry conditions desired by statistical methods, is very slow and very expensive.

We have developed techniques whereby we can put pitch and yaw and various parameters of that sort into the entry conditions of the projectile, but I will not go into details in this paper. In order to vary the entry angle, we now have under construction what is known as a variable-angle launcher (Figure 2). The truss is some 300 feet long. On it we can carry a variety of launching devices. We have under construction another tube very much like the one on the fixed-angle launcher. We believe that it would be very worth while to put a slotted-tube catapult on the top to handle devices with wings, so that the problems of wing shedding, etc., associated with the guided missile program, can be studied. The top deck is being reserved for that. There is room for quite a variety of tubes inside the truss, which is about 35 feet high and 24 feet wide. It can be pulled up and down on the slide, while the bottom rests on a barge which is kept from moving sidewise by wind-sway cables. We can vary the entry angle from a few degrees to about 40 degrees. Again you can see that this does not cover the range of angles that may be encountered in mine water entry problems, but it gives good coverage of those angles likely to be met in torpedo and guided missile water

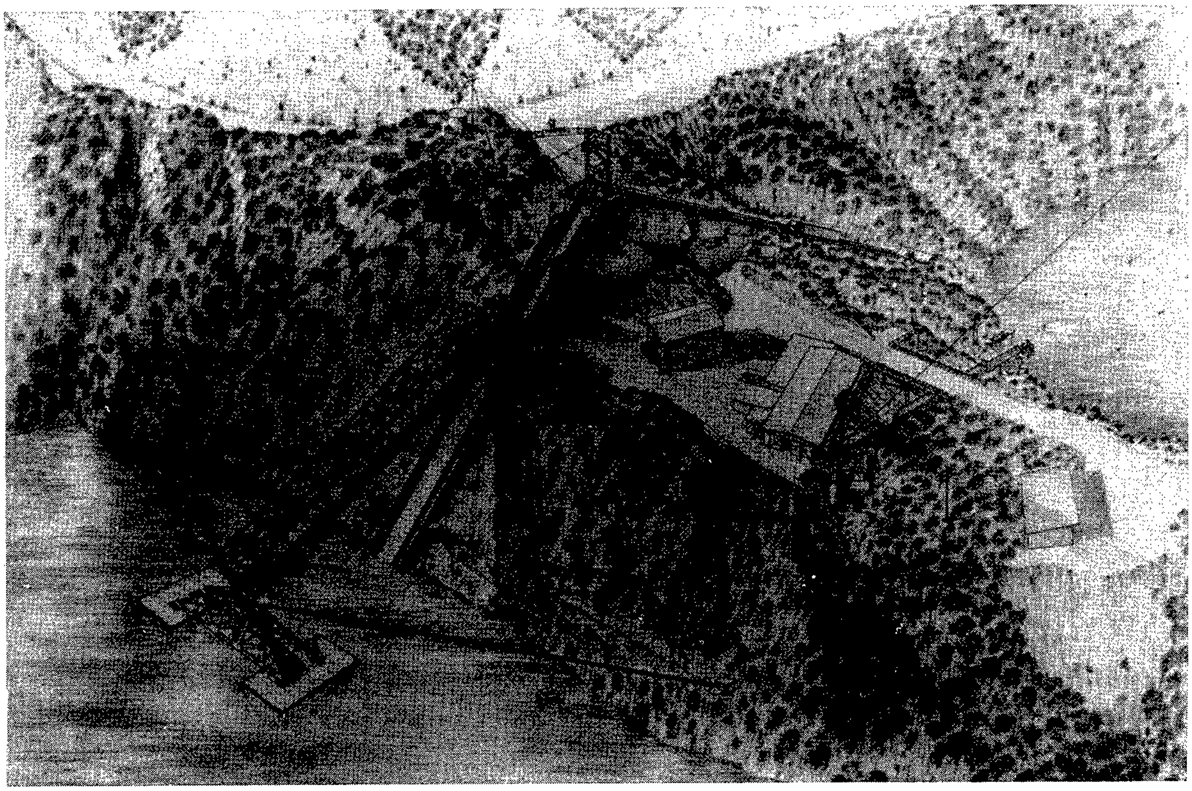


Figure 2. Variable Angle Launcher Morris Dam Torpedo Range

entry problems. It will cover the angles expected in those missiles which are to move forward toward their target. This launcher is to be completed approximately the first of the year.

This type of device gives us opportunity for very precise quantitative measurements. In Figure 3 we have photographs taken at a distance of only 70 feet from the entry point, certainly too close to be during an aircraft drop. From these photographs we can get the various entry conditions. We can measure the exact entry angle, the yaw and the roll. Pitch is obtained by taking the difference between the theoretical trajectory and the actual entry angle. This torpedo, you will observe, has some roll. There are pyrotechnic flares on this missile tail. I will go into the use of these later.

I will show you examples of some of the types of damage that we have actually observed in cases where it has been possible to recover the torpedo. In Figure 4 we have a torpedo which entered the water at rather high velocity. In the upper right-hand photo we see a little trail branching off. This turned out to be the entire head section of our torpedo. It ripped off and landed some 2,000 feet away from the rest of the fish. It had received a very bad bending moment on entry and simply tore loose. In Figure 5 we have a case where the nose section (on the right of the photo) ripped off. We had a "window" located in the nose. On water entry this collapsed, the whole distribution of forces was changed and the nose was simply ripped off. In Figure 6 we have a case where the afterbody tried to catch up with the nose. Notice that

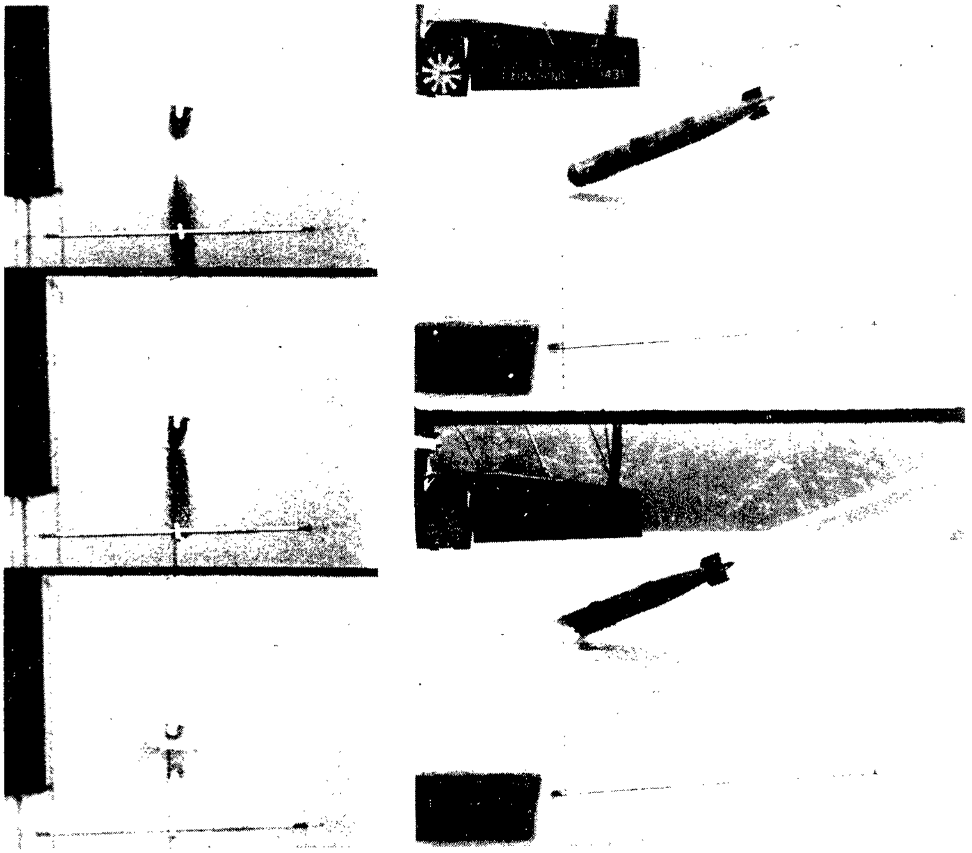


Figure 3

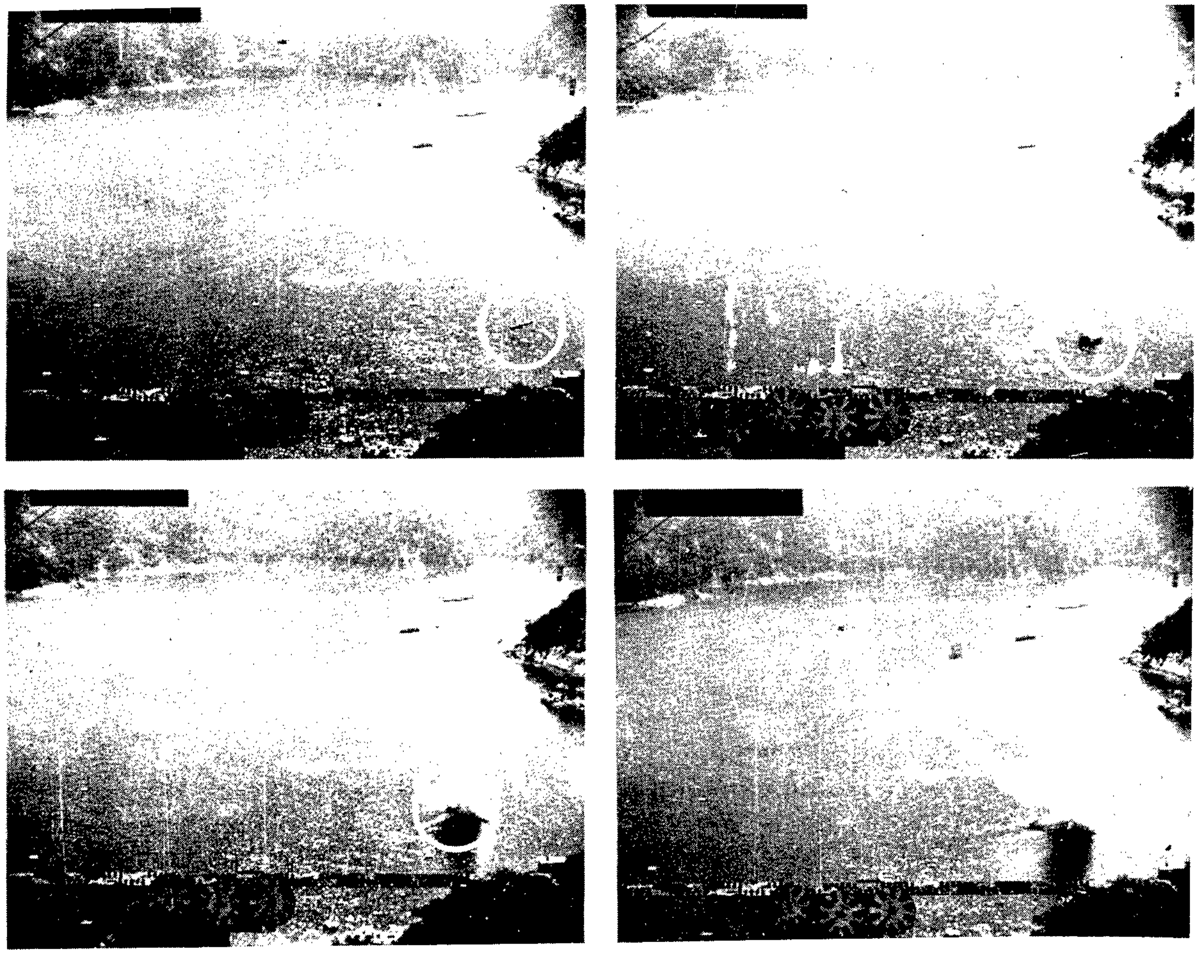


Figure 4

the buckling is somewhat more severe at the top than at the bottom, indicating that there must have been some bending moment also.

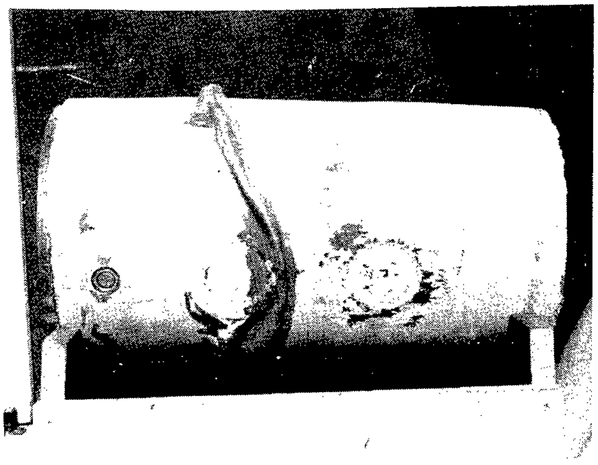


Figure 5

I mentioned the fact that the projectile will sometimes slap into the side of the cavity. In Figure 7 we have some examples. These happen to be two-inch diameter models, but they will illustrate the phenomena. In the upper photo we have an example of a blunt-nosed projectile which is riding right down the center of the cavity, never actually touching the side of the cavity until the cavity has collapsed. In the middle we have a projectile which has what we call a "positive whip"; that is, it goes to the bottom of the cavity. This results in an up-turning trajectory which is desirable in certain circumstances, but the object can strike

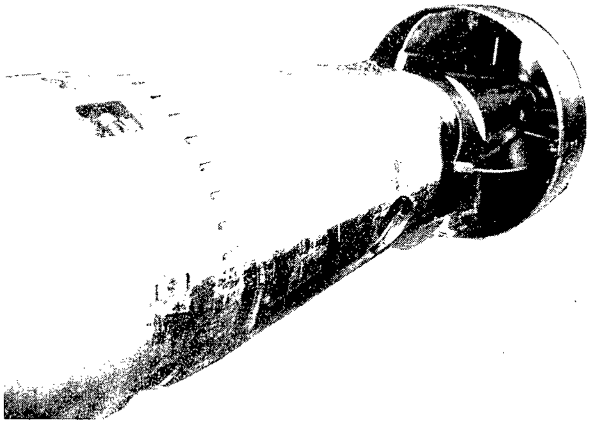


Figure 6

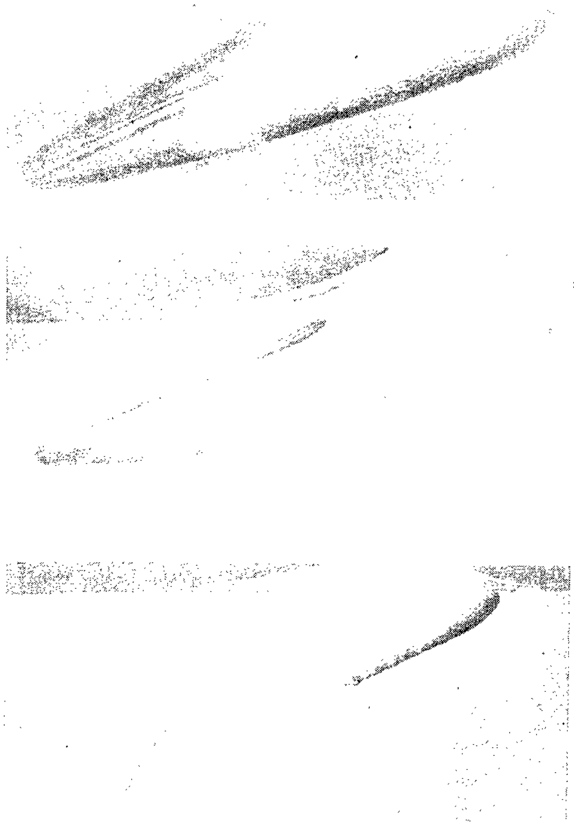


Figure 7

the side of the cavity with rather considerable force. The lower picture shows the opposite case where the projectile "stubs its toe" and strikes against the upper side of the cavity.

As we can see in Figure 8, striking the side of the cavity can also cause damage. Here we have a case of rippled fins. We can explain it in no other way than to say that the torpedo hit the side of the cavity so hard that it rippled and dented the fins. In the vicinity of the gyro-pot in the Mk. 13 standard aircraft torpedo, we have measured pressures as high as 5,000 pounds per square inch due to hitting the side of the cavity. The angular velocity will be in the order of two radians per second and, as we have a large radius, considerable striking velocity can be generated.

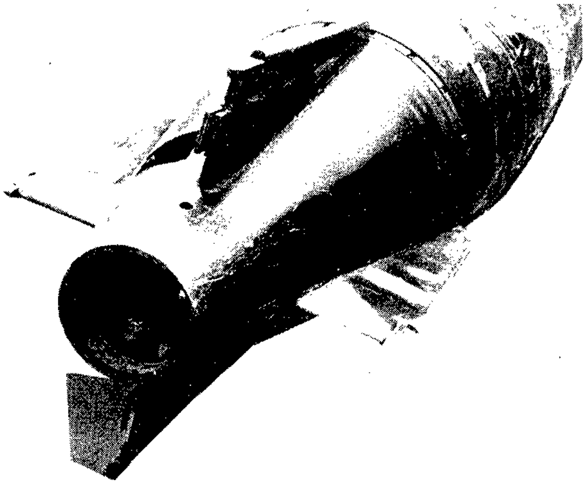
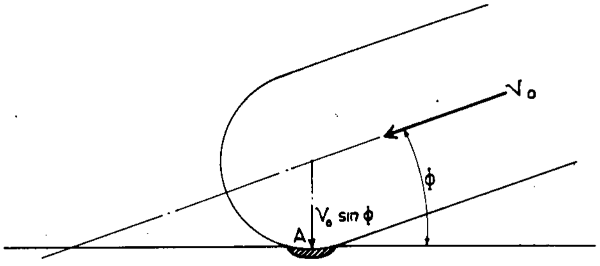


Figure 8

Now, let us look a little more into the quantitative measurements of these pressures. When the projectile nose strikes the surface of the water, it does not know whether it has struck a liquid or a solid. For a small area at the point of contact (See Figure 9) the velocity of recession of the rim of contact is greater than the velocity of sound in the liquid. Consequently, the pressure wave cannot escape, and we have the entire energy of impact concentrated in the parabolic region shown shaded in Figure 9. This is depressed by the surface of the



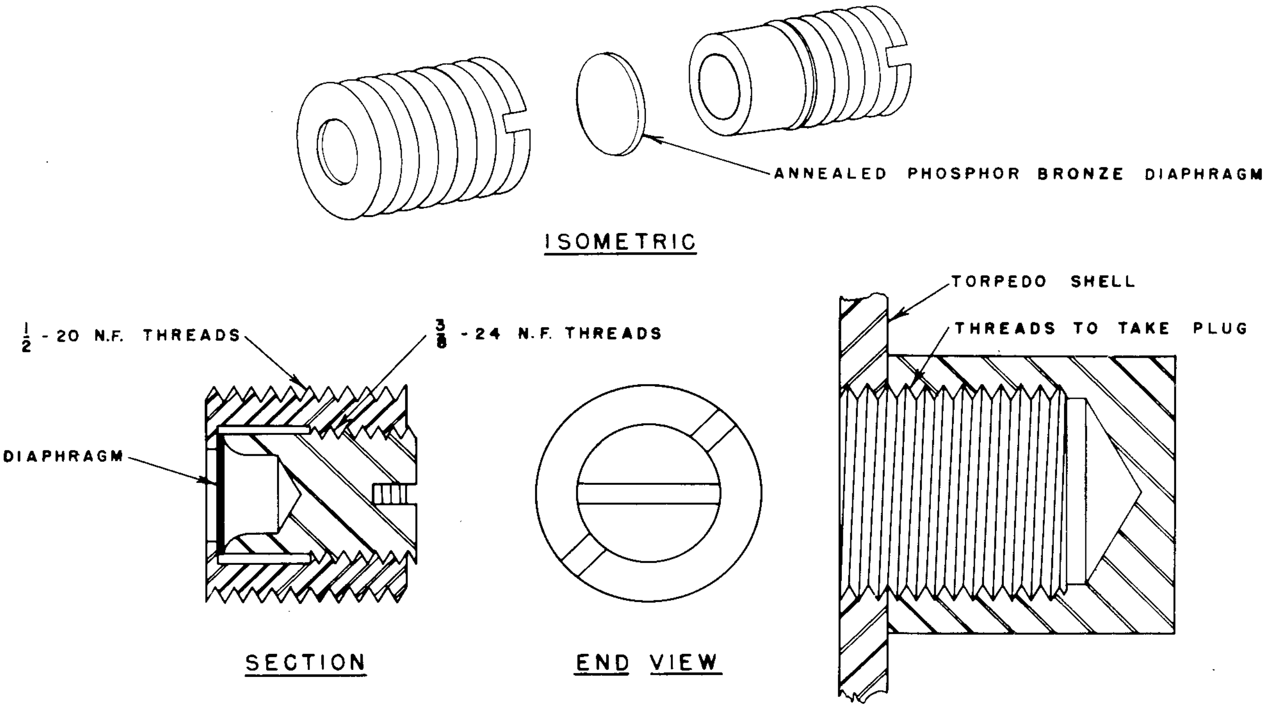
INITIAL PHASE

Figure 9

spherical tip so that we actually have a local compression. The pressures that arise are in the order of ρcv, where "ρ" is the density of the liquid "c" is the velocity of sound in the liquid, and "v" is the normal striking velocity. To measure these pressures we have developed a technique using "pressure plugs" which is very similar to a technique used very successfully here at the Naval Ordnance Laboratory.

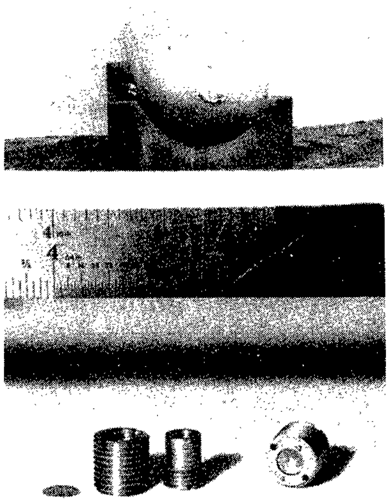
Figure 10 shows one of these "pressure plugs" on which we have an annealed phosphor-bronze disc carried on very accurately machined and contoured shoulders. The indentation of this plug as a function of pressure is calibrated statically. This, of course, introduces potential error. The indentations measured after a drop are then compared with these static data. Figure 11 is a photograph of one of the plugs and a typical pattern carried on the nose of a torpedo exercise head. In the same figure we have an example of a calibration curve for one of the .005 inch thick diaphragms. We make preliminary measurements to estimate in what region we can expect the pressures in a particular area and then use the thickness of diaphragm that will keep us in the linear region.

In the lower left of Figure 12 we have a typical pressure distribution curve. The fish must have been rolled, because of the asymmetrical pressure distribution. We measured a peak of 12,300 pounds per square inch. One interesting thing about these records is that the maximum pressure is somewhat ahead of the point of contact.



HYDRO-PRESSURE PLUG & SUGGESTED MOUNTING METHOD

Figure 10



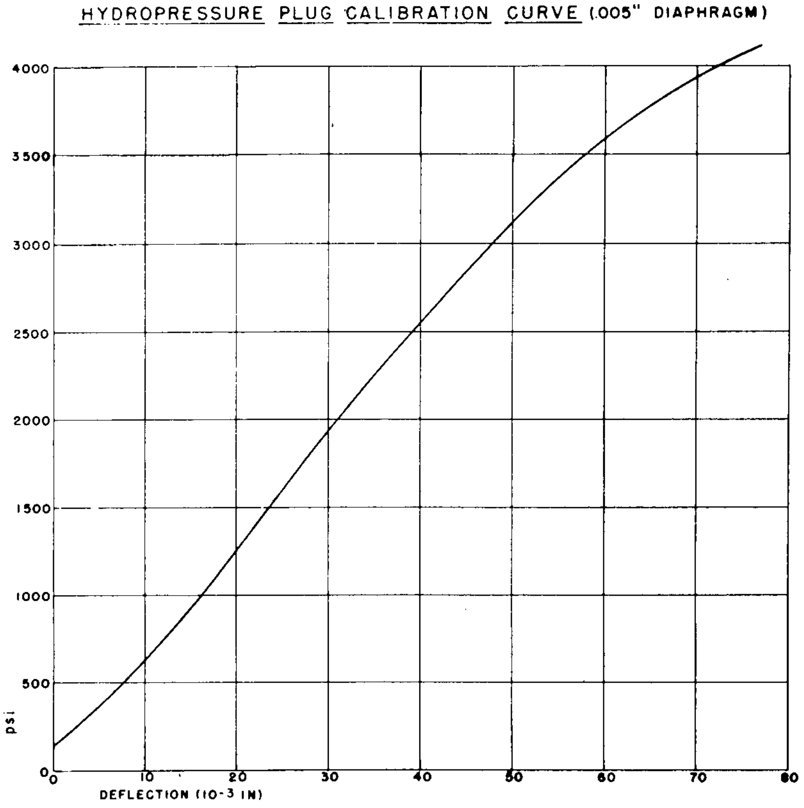


Figure 11

If we ran into absolutely fresh water on the axis, we would have a normal velocity equal to the velocity of the projectile. However, at the point of initial impact we are not hitting with the velocity of the projectile but with a normal velocity equal to the axial velocity times the sine of the entry angle. Consequently, we have a reduced pressure at this point. By the time the point of contact gets to the axis, the water has started to move away. Therefore, the pressure rises and then drops off somewhat as the point of contact approaches the nose.

If we plot the pressure at the point of impact, where we know we are coming into stationary water and, hence, where we know definitely the velocity of impact, we get data of the sort that we see in Figure 13. If we plot the theoretical curve, ρcv sine 0, we get the straight line shown in that figure. We are working at low enough energies so that we do not have to worry about the energy in the shock wave. Certainly the velocity of the shock wave is not markedly greater than that of sound. The low points at the bottom and the high points at the top might make one want to draw an empirical curve somewhat different from the straight line, but the work of Metzelaar here at the Naval Ordnance Laboratory, carrying measurements of a similar type up to much higher velocities, has indicated that this should not be done. His measurements were, in general, slightly above this single theoretical line and slightly below the values calculated by taking into consideration the increased velocity of the shock wave at high energies. In our range, this theory seems quite satisfactory and certainly makes us realize that we are dealing with extremely high pressures.

Fortunately, these pressures do not last very long. The question therefore,

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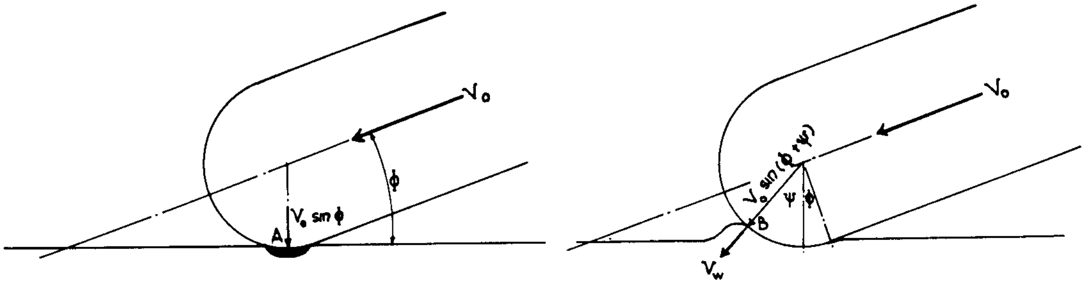
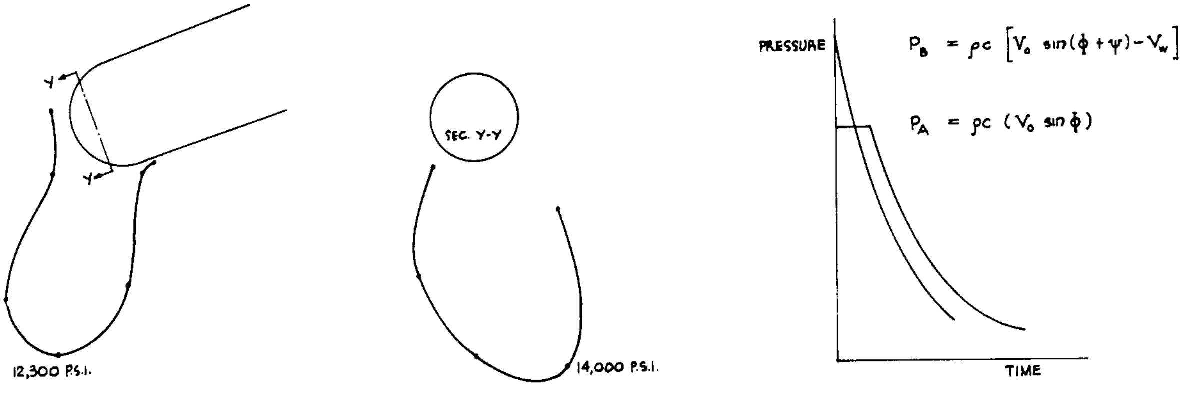


INITIAL PHASE SECOND PHASE



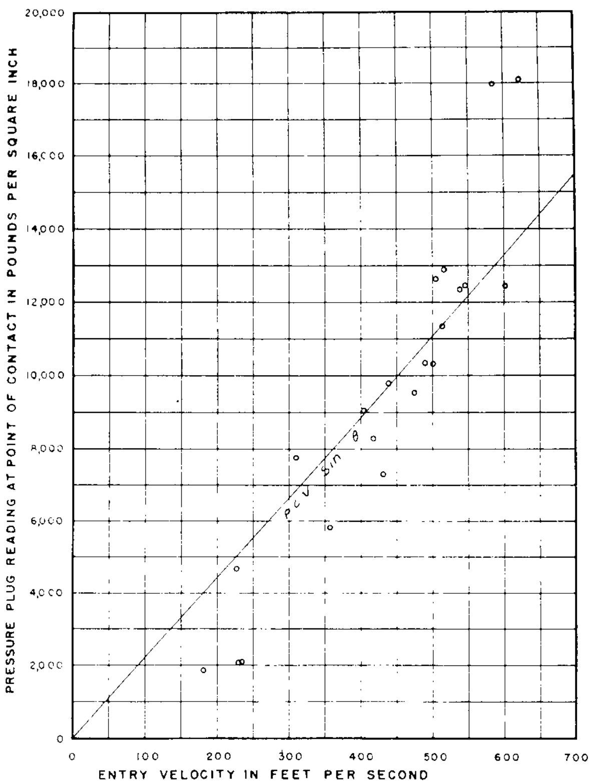
PRESSURE PLUG DATA PRESSURE-TIME RELATIONS

A SUGGESTED EXPLANATION OF PRESSURE PLUG DATA

Figure 12

becomes: "What is their effect upon damage or behavior?" As far as damage is concerned, we believe that they may sometimes, because of the extreme rate of loading involved, cause local damage. When we get local failures, then the whole flow pattern is disrupted. This may then result in disruption of the whole projectile. Actually, we find it very difficult at a 19 degree entry angle to dent the noses of our study dummy torpedoes. The noses are made of sturdy one-half inch steel, and we simply cannot dent them. We find that usually they do not dent at all or else they are completely torn to pieces. We found that the threshhold between no damage at all and complete destruction was over a velocity range of only **25** knots.

As far as ballistic behavior is concerned, this initial entry shock will probably introduce a small impulsive velocity change. According to various techniques in measuring and estimating, it should be in the range of from one to five feet per second. We have come to the conclusion that the most promising method for exploring this initial impulsive velocity change is to telemeter the information from the torpedo back to a receiver on shore, where we can use really sensitive recording devices. The Naval Ordnance Laboratory has had considerable success in developing a cathode ray recorder to be carried in a projectile. During the time we are most interested in this particular thing (which is in the first few milliseconds -- in fact, the



PRESSURE PLUG READINGS AT POINT OF CONTACT AS A FUNCTION OF ENTRY VELOCITY

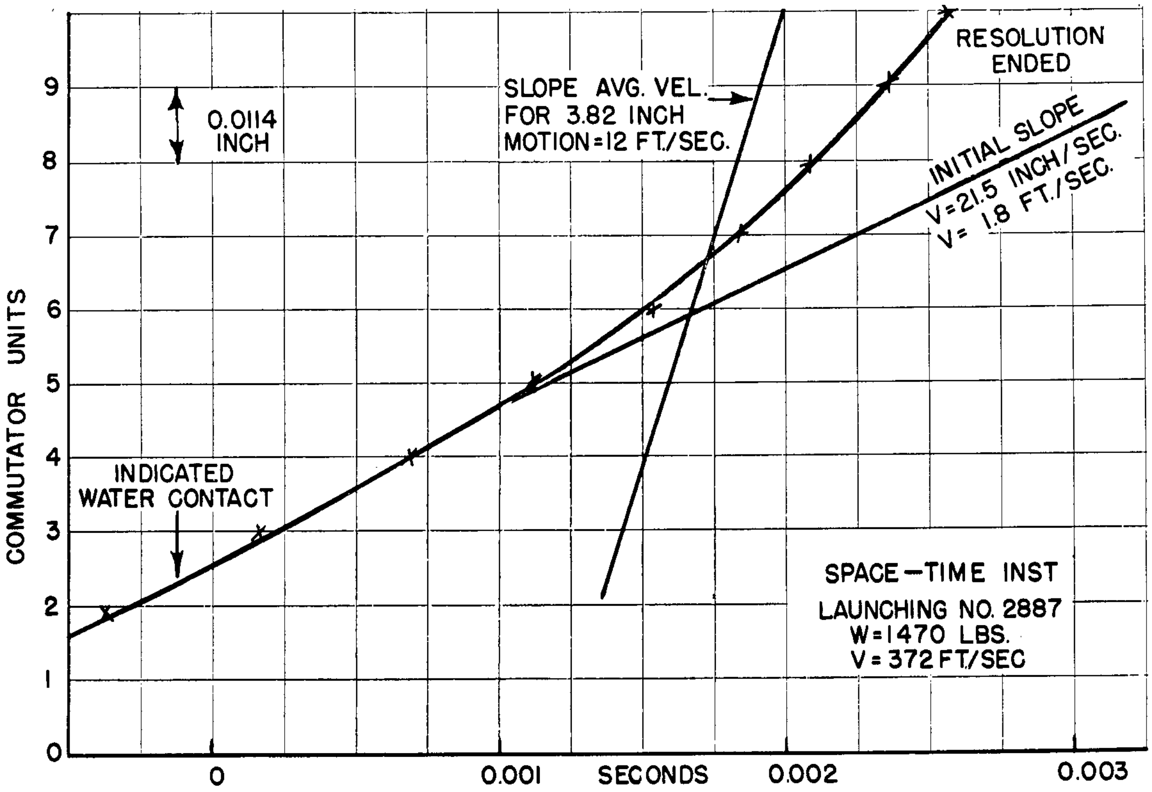
Figure 13

first shock occurs in a period of a fraction of a millisecond), we are afraid of microphonics in the recording system. We feel it is better to have a very good recorder on shore and to try to telemeter the information to it. We shall probably have to shock-mount our telemetering outfit and let it move during this important period; then, if it bottoms later, that is all right because we will already have the information we want. The information plotted in Figure 14 was obtained from such a telemetering device. The antenna was stuck out the back of the torpedo, and we had a receiving antenna ashore. We used a simple coil of wire connected at various points to give us a signal to the telemetering transmitter. We had a slug of metal with electrical contacts made as frictionless as possible, which would keep moving as the torpedo slowed down so that we could get a measure of that early impulsive velocity change. Because of friction, it will inherently measure somewhat low.

We are very pleased that the telemetering technique is showing promise, and we believe that it is applicable to telemetering pressure data in which the pressure plugs can be replaced with piezoelectric gauges. Then we can telemeter back the information of the actual pressure-time distribution on the nose of the fish. We also believe that it will be possible to put strain gauges on various parts of the weapon and telemeter back actual strain-time relationships. This is one of the really fundamental problems that needs to be studied in the matter of structural damage associated with water entry.

To go back to one of our older and very well proved techniques, we have used what we call a flare camera to get the steady entry deceleration. As we saw in the side view of the torpedo entering the water (Figure 3), there are two pyrotechnic flares on the tail. These flares are extremely bright -- so bright that they can be photographed in an extremely short time. What we do is put a plate camera in a fixed place and rotate a slotted disc in front of the camera (Figure 15). The speed is such that we get a thousand slots passing in front of the camera per second. The exposure time per slot is of the order of one thirty-thousandth of a second. This is sufficient to pick up a good image of the flare (Figure 16).

Here we see a series of striations. The torpedo is moving so fast that it cannot be seen. On the other hand, the background is integrated because the light from it appears in each flash of the camera. This gives us sufficient definition to enable us to find certain landmarks to establish scale and to establish the horizon. By measuring the distance between the flare marks and knowing that the time is one-thousandth of a second, we are able to get a distance-time curve.


COMMUTATOR UNITS
0
1
2
3
4
5
6
7
8
9
0.0114 INCH
SLOPE AVG. VEL. FOR 3.82 INCH MOTION=12 FT./SEC.
RESOLUTION ENDED
INITIAL SLOPE
V=21.5 INCH/SEC.
V= 1.8 FT./SEC.
INDICATED WATER CONTACT
SPACE—TIME INST
LAUNCHING NO. 2887
W=1470 LBS.
V=372 FT/SEC
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SECONDS
0.002
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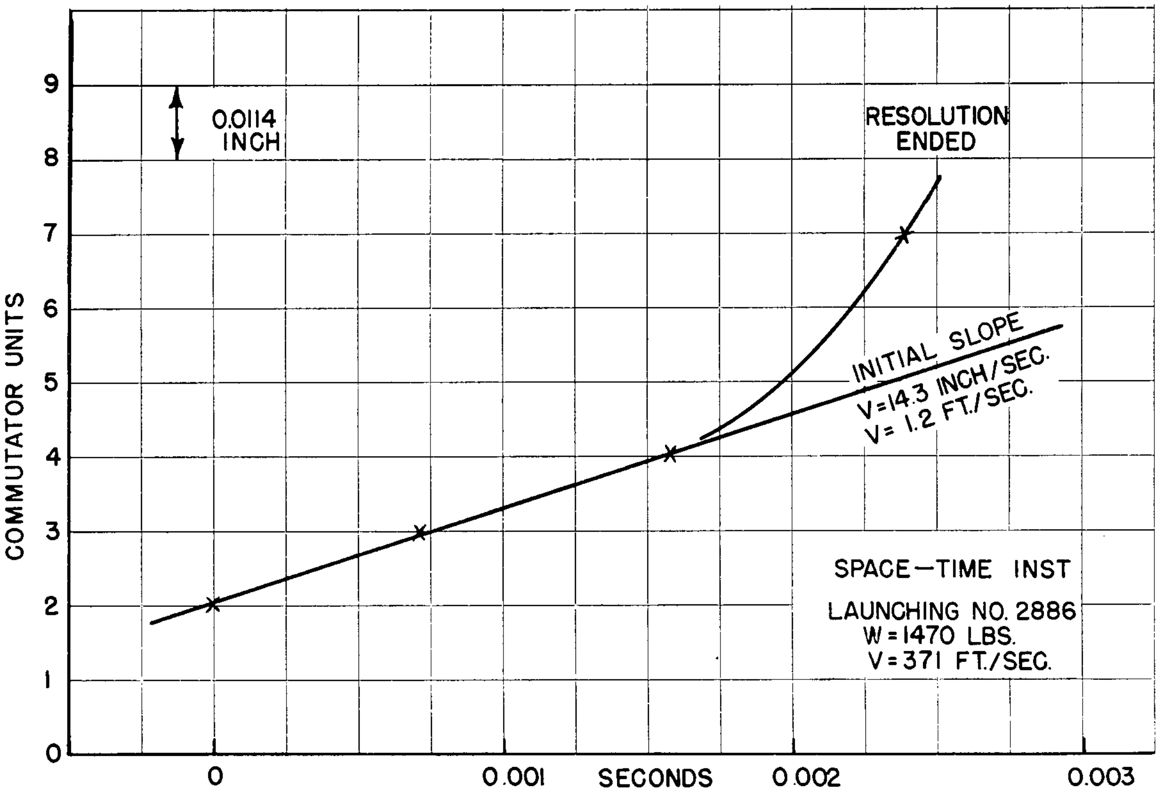
COMMUTATOR UNITS
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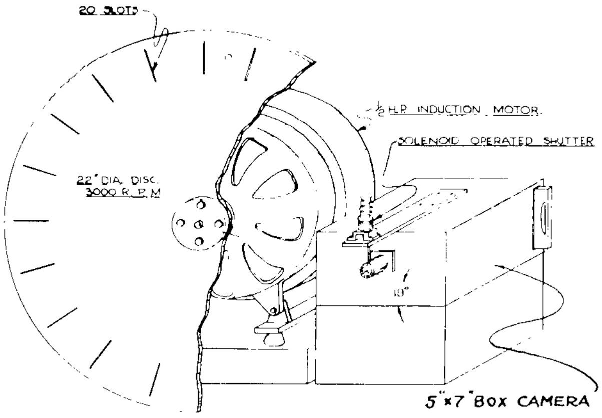
0.0114 INCH
RESOLUTION ENDED
INITIAL SLOPE
V=14.3 INCH/SEC.
V= 1.2 FT./SEC.
SPACE—TIME INST
LAUNCHING NO. 2886
W=1470 LBS.
V=371 FT./SEC.
0
0.001
SECONDS
0.002
0.003

Figure 14



FLARE CAMERA

Figure 15

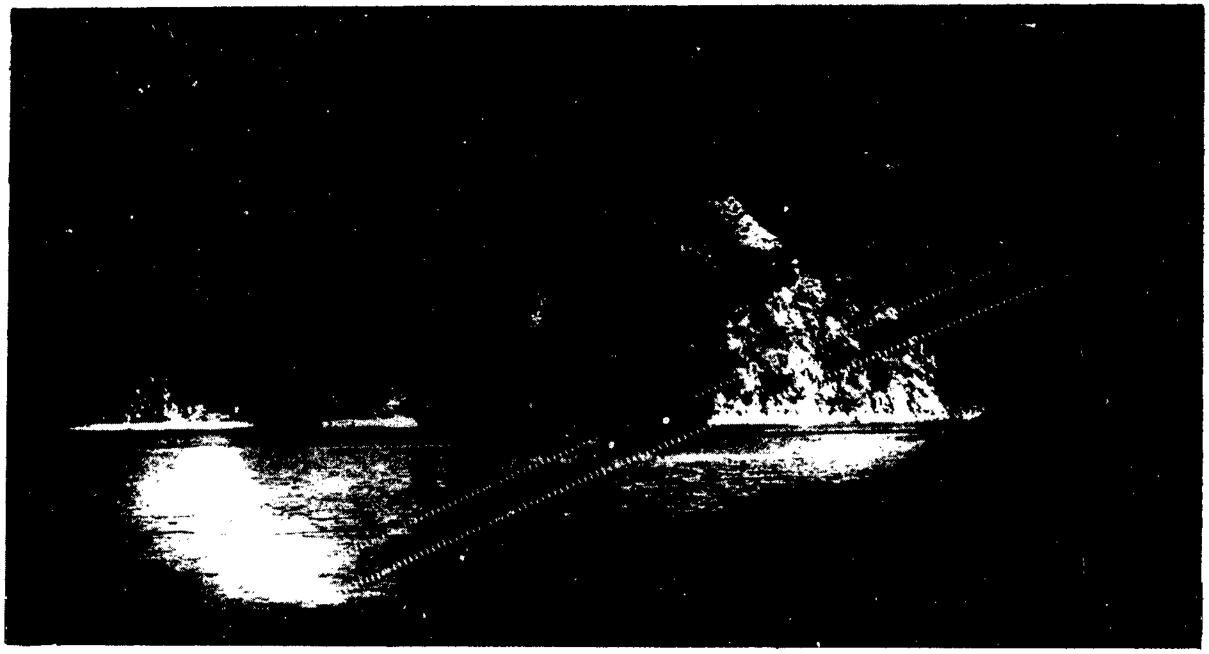


Figure 16

The data are sufficiently precise that we normally plot first differences. By measuring the angle between corresponding opposite flares, we are able to get a time-angle record. In the upper curve of Figure 17 we see first differences plotted against time. This gives us essentially a time-velocity curve. You will notice the zero is suppressed in the plot. The scatter is extremely small. This method is very precise. A theoretical curve is plotted with a drag coefficient of 0.25. This is the mean drag which we have obtained over a large number of launchings on the torpedo Mk. 13. In the lower curve we have angle data plotted. Making suitable corrections for apparent angles which are associated with roll of the torpedo, we find that the whip associated with water entry or the change in angular velocity is readily measured from the plate by routine analysis.

In the left-hand plot of Figure 18 we have the average deceleration in g. plotted against the entry velocity in feet per second for a spherical nose. In this case we find on log-log paper a straight line with slope two, which indicates that the deceleration is proportional to the square of the velocity. This is what we assume when we assume that a drag coefficient actually exists, since we define C_d as proportional to V^2. This demonstrates experimentally that our assumption is a rational one. We also assume that the drag in the case of a

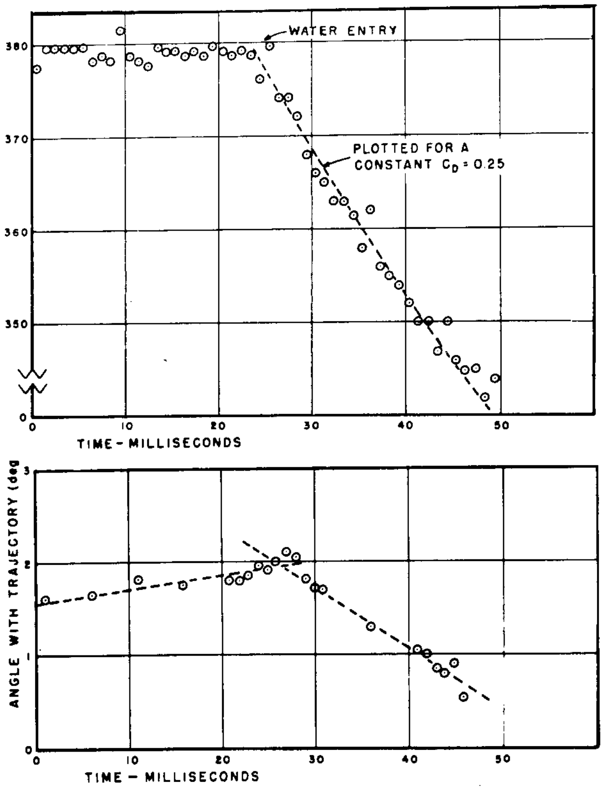
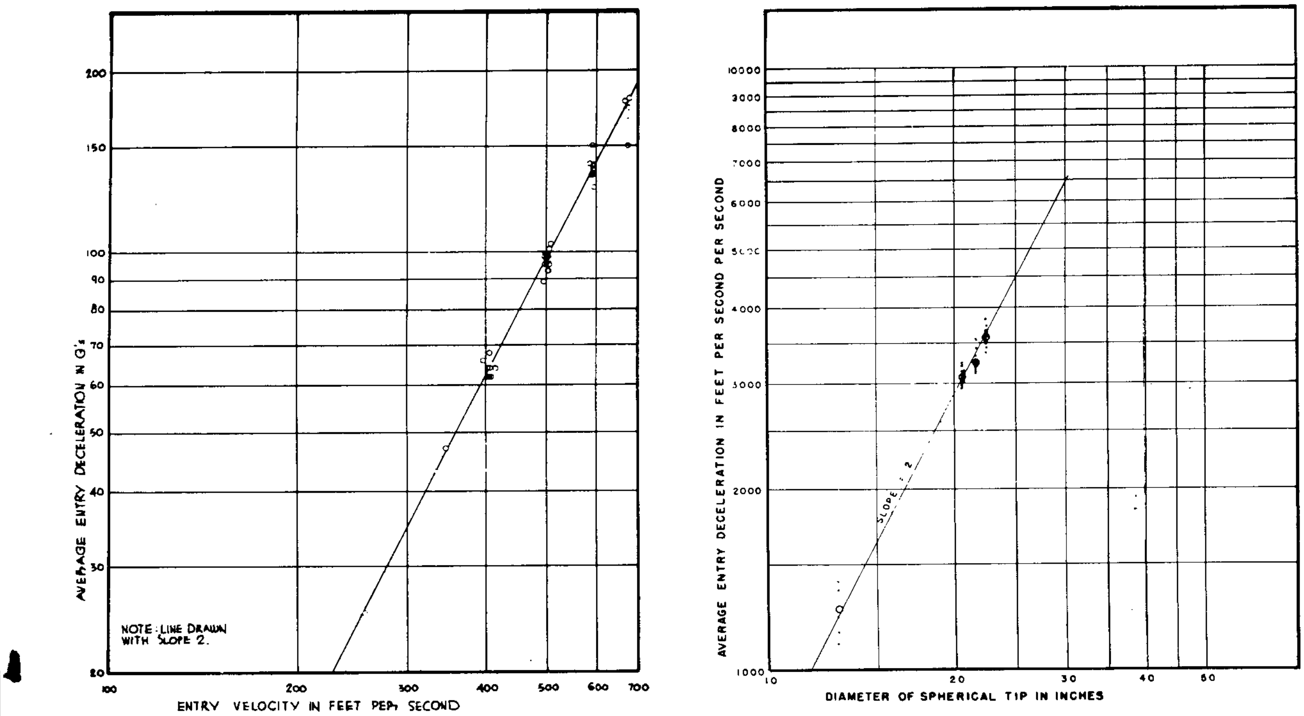


Figure 17. Velocities and Displacement of Torpedo Tail During Water Entry

number of objects of similar shape but different size should depend upon the cross-sectional area.

In the right-hand curve we have plotted entry deceleration for objects of different diameters. Here, also, we find a straight line of slope two, showing that the area law is applicable; at least over the range at which our tests have been run. Nothing startling perhaps, but it is rather comfortable to know that we can reasonably assume a drag coefficient. We can then make tests in such devices as the air guns that will be significant, because we know what sort of forces are going to be met.

In addition to the telemetering, we have recently developed another new technique using high-frequency sound. We had originally planned to telemeter information on a sound beam, but we found that a radio beam was much better because of the wider band which could be trans-



AVERAGE ENTRY DECELERATION AS A FUNCTION OF ENTRY VELOCITY FOR MK-13 HEAD SHAPE (HEAD "F")

AVERAGE ENTRY DECELERATION AS A FUNCTION OF NOSE TIP DIAMETER REDUCED TO 500 FT./SEC ENTRY VELOCITY. ENTRY ANGLE 20°

Figure 18

mitted. In the upper left of Figure 19 we have a transducer in the nose, driven by a 60 kc oscillator shown on the right. One nice feature is that we have an air-driven generator so that we do not have to depend on batteries. This system was developed for us by the Pasadena Physics Section of the Naval Ordnance Test Station.

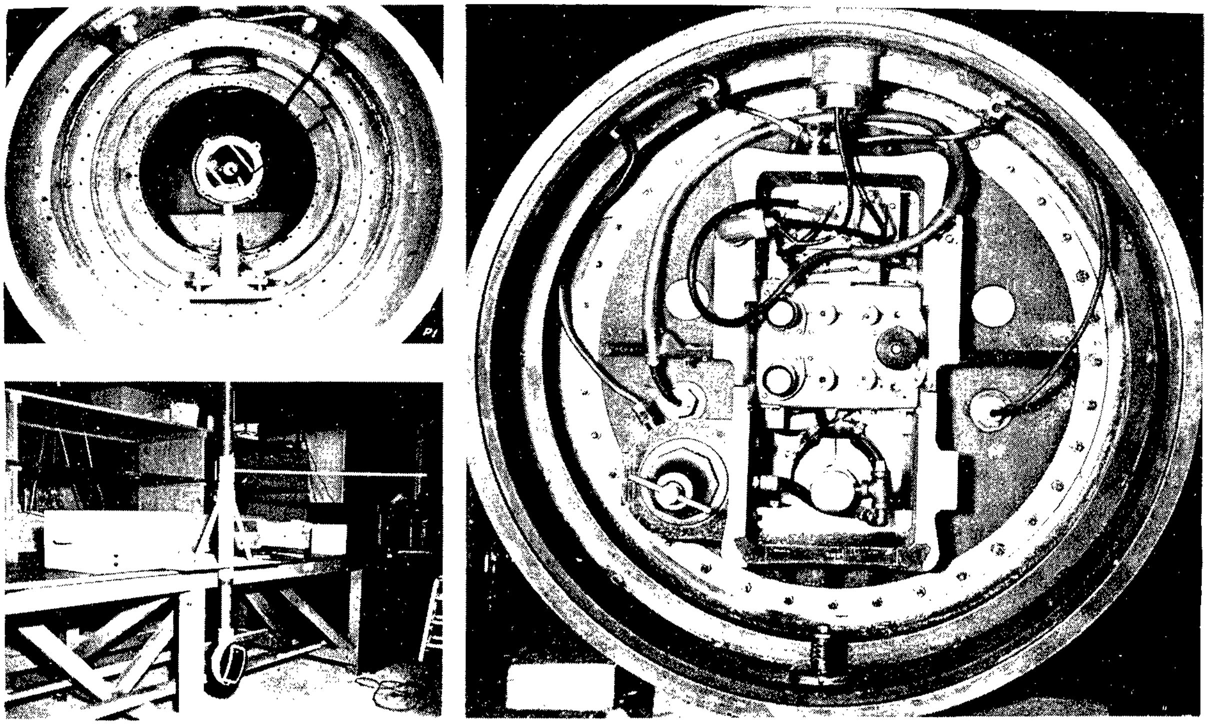


Figure 19

In the lower left is shown a hydrophone which can be put down from a barge. There is another oscillator on the barge which produces 60 kc, also, to compare with the signals from the torpedo. A mixer circuit compares the frequency of the two signals as the projectile travels toward the hydrophone. The frequency was so chosen that we had approximately one beat for every inch of travel of the torpedo. By recording the beats on an oscillograph, it is easy to get a velocity-time picture of the torpedo motion.

The method was developed primarily to study the steady running state. However, we have recorded the conditions at water entry with this apparatus. The hydrophone was 60 feet from entry point. The torpedo followed the path shown on the diagram in Figure 20. We were very happy when it went under the barge rather than broaching into it. What we actually measured was the distance from the trajectory of the fish to the hydrophone, rather than the travel along the trajectory. A correction had to be made.

The record from such a shot looks like a lot of wiggles on an oscillograph (Figure 21). It can be evaluated but the process is quite laborious. We believe, therefore, that in the steady running phase the record will be most useful as a method of calibrating other types of apparatus, rather than as something which would be used on every run. The solid line in the upper part of Figure 21 is a theoretical curve for a drag coefficient of 0.25. Actually, it was necessary to correct the theoretical drag to the experimental data rather than the reverse, because this had to be done in a hurry (the run was made only a week before this paper was presented). Now, if the drag coefficient was constant at 0.25, one would expect to get the lower curve with the knee in it, going to zero as it passed under the barge. The agreement, I think, is quite good. There are some refinements to be made, but we believe that we have a useful method for extending the deceleration measurements beyond the point where our flares go into the water.

In Figure 22 we have a curve obtained from earlier sound range data at which the recorded points were quite a distance

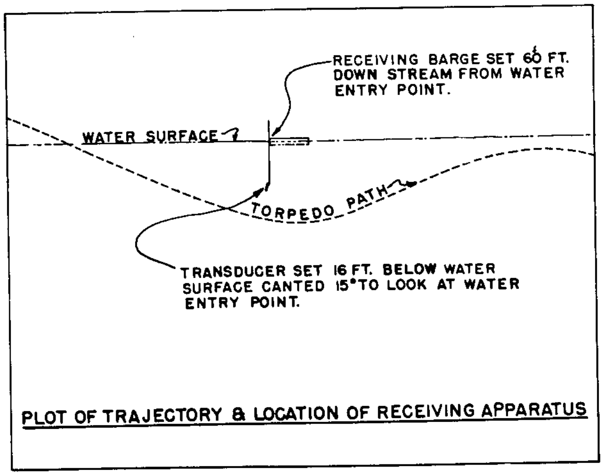


Figure 20

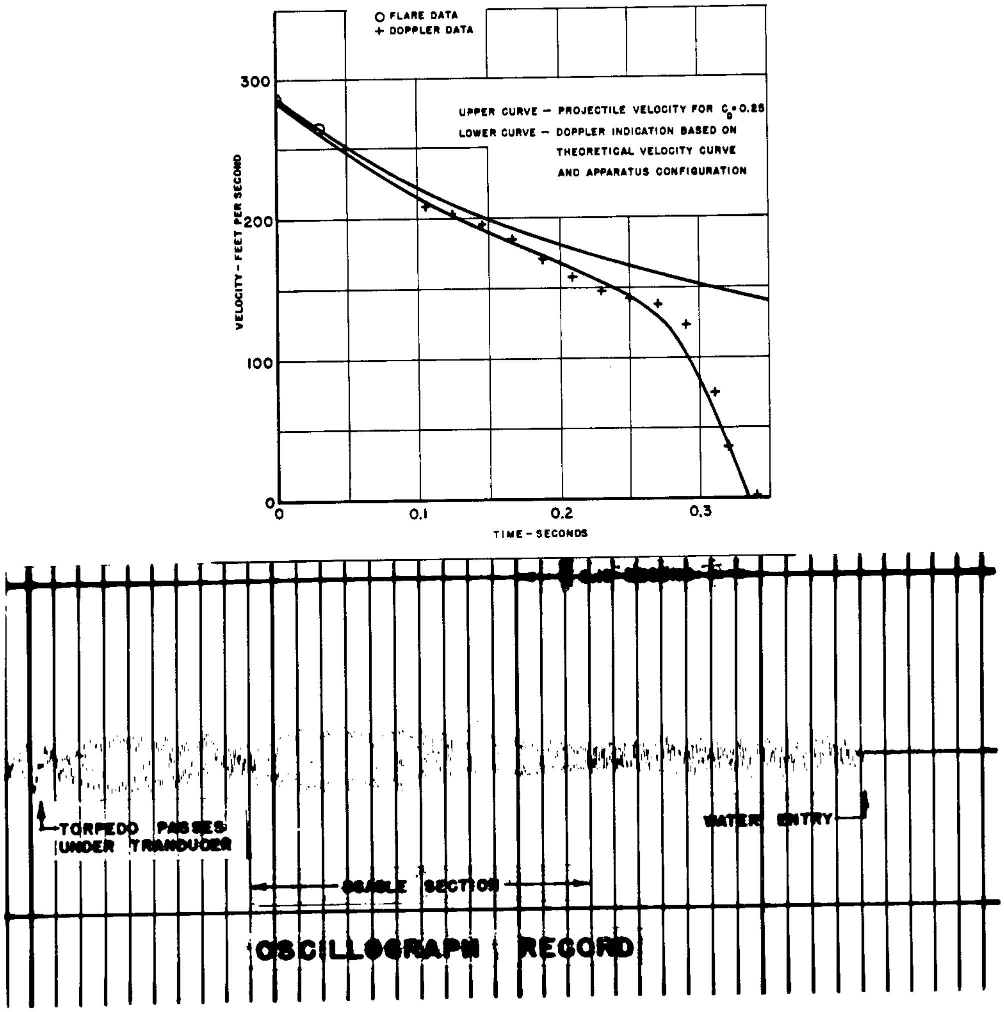
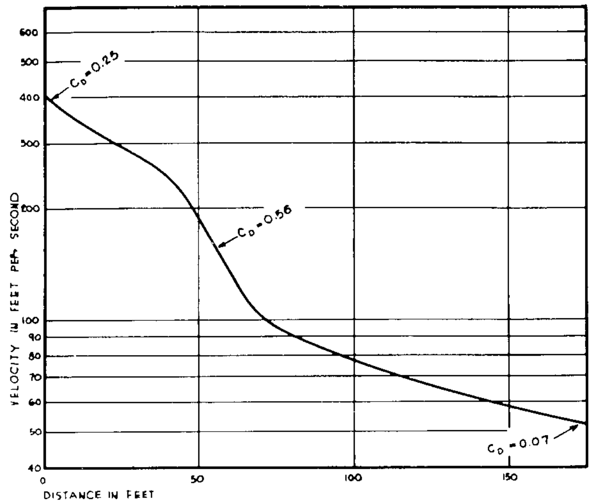


Figure 21 Velocity Time Data from Doppler Device



VELOCITY – DISTANCE CURVE
C.I.T. FULL SCALE DUMMY AIRCRAFT TORPEDO

SLOPE PROPORTIONAL TO C_D

Figure 22

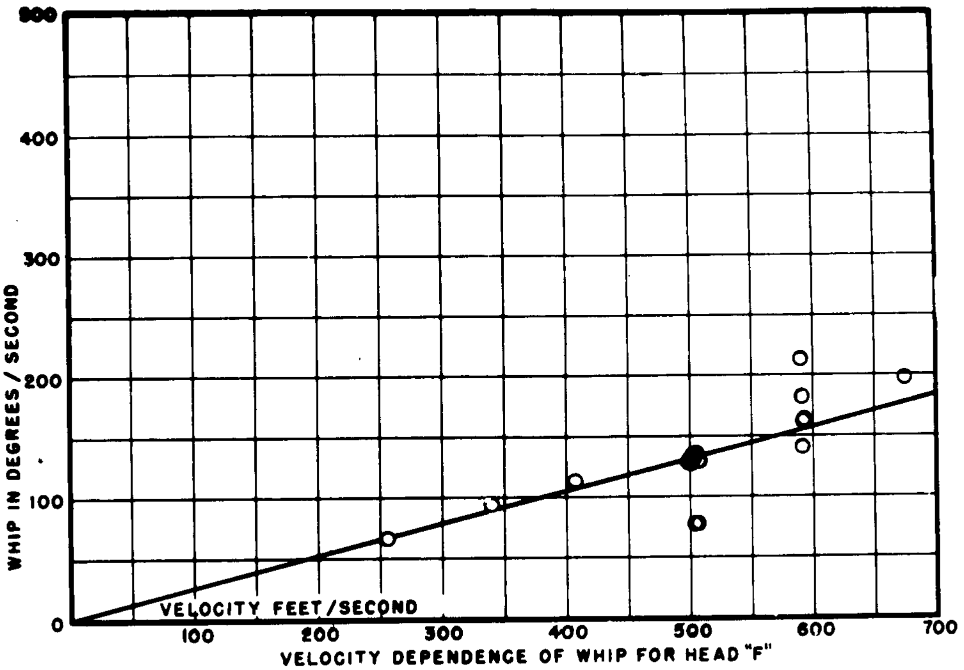
apart. The drag coefficient here is proportional to the slope of the curve. The steeper the curve the higher the drag coefficient. You will notice that there is quite a change in the middle, the drag increasing markedly at about 40 feet from the entry point. This is rather an unusual shot: usually the break is not so sharp. We believe that this increase is due to the fact that by the time the torpedo gets into the side of the cavity, it is slewing and, therefore, is presenting a much bigger area and, hence, slowing down much more rapidly. As the bubble is shed, the drag coefficient drops off approximately to its steady value of about 0.07, which is the viscous or skin drag on such an object.

Now, when we have such slewing, we see that we may have forces to contend with that are fairly considerable and are transverse to the axis of the torpedo. Therefore, internal components must be designed to take care of the transverse forces. These forces are of a steady nature and not just of the impulsive nature associated with initial entry when the projectile slaps the side of the cavity. If we do not need an upturning trajectory, we may be able to avoid these large transverse accelerations.

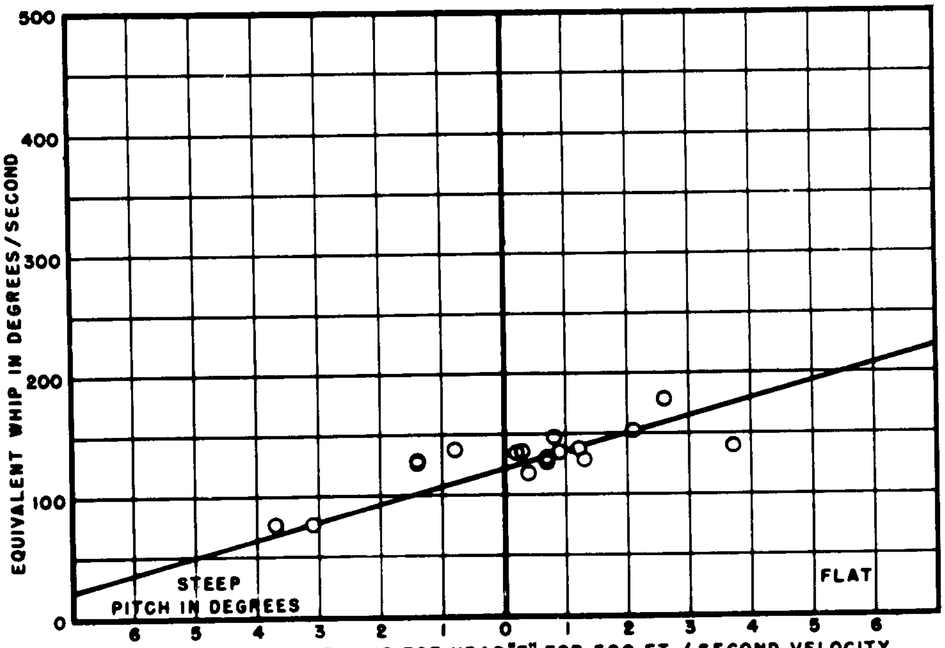
Let us now consider the matter of whip in more detail. We have done considerable work to determine the relation of this whip to various factors. For a simple theoretical reason, whip should depend directly on velocity. The forces causing whip are assumed to be proportional to V^2, since they are associated with the steady deceleration rather than with the impulsive shock. Whip actually occurs between the time that we get impact and symmetric separation on the two sides of the nose. The distance over which this occurs is constant, regardless of the velocity. The time that this represents is thus inversely proportional to the velocity. Since the whip depends upon the impulse -- i.e., force times time, it should be proportional to the velocity to the first power on similar noses.

In the upper curve of Figure 23 we have fairly good experimental conformation. The data have not been corrected for pitch, yaw and other secondary effects. The fact that there is some scatter could be reduced by taking these factors into account. The correlation coefficient has been worked out and is extremely good. In the lower curve we show the relation between pitch and whip. By "pitch" I mean the angle between the axis of the projectile and the tangent to the trajectory. This shows that the flatter the pitch the more the whip and, consequently, the greater the likelihood of damage due to striking the side of the cavity.

Recently we have been winding up the study of a family of sphere ogives. Professor Knapp, in the C. I. T. water tunnel, found some rather interesting results on such a family. He found that on an ogive nose as we approach cavitation we get gross cavitation; that is, we get big "gobs" of cavitation. In the case of an ogive we also get rather corrugated cavities, whereas with a sphere we get a sharp



VELOCITY DEPENDENCE OF WHIP FOR HEAD "F"



PITCH DEPENDENCE OF WHIP FOR HEAD "F" FOR 500 FT. / SECOND VELOCITY

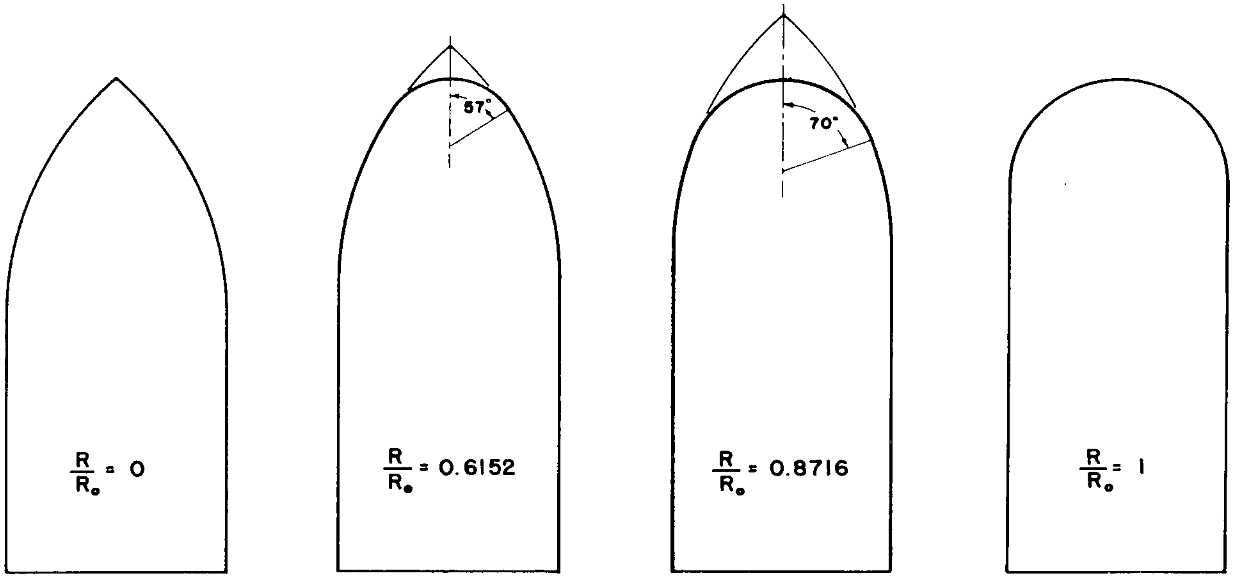
WHIP RELATIONS FOR HEAD "F"

CIT FULL SCALE DUMMY AIRCRAFT TORPEDO

Figure 23

cavity. Now, if we flatten ogives with a spherical nose section to an increasing extent, we eventually reach the point where the point of separation during entry would tend to occur right at the joint between the sphere and the ogives.

We got interesting results on the drag and pitch sensitivity tests with the various shapes shown in Figure 24, which give us an approach toward a method of design of the nose. In Figure 25 we see some of the results; we have quite a change in pitch sensitivity, shown by quite a change in slope with the various noses. In Figure 26 we have a summary of the results. As we reduce the area of the hemisphere we would expect the whip-producing forces to go down as the area, that is, as the square of the radius. The straight ogive, in which larger distance must occur between impact and the time of symmetric separation, might be expected to have a larger whip, and it does. In one shape tested, water tunnel tests indicated that the separation should occur right at the juncture between the ogive and the sphere. We should expect this to be just about on the sphere curve as well as on the ogive curve shown, and this is approximately the case. Therefore, we are able to choose, as a result of water tunnel test, the shape which will give us the least whip.



1.5 DIAM. OGIVE

1.5 DIAM. OGIVE
57° HALF SPHERE ANGLE

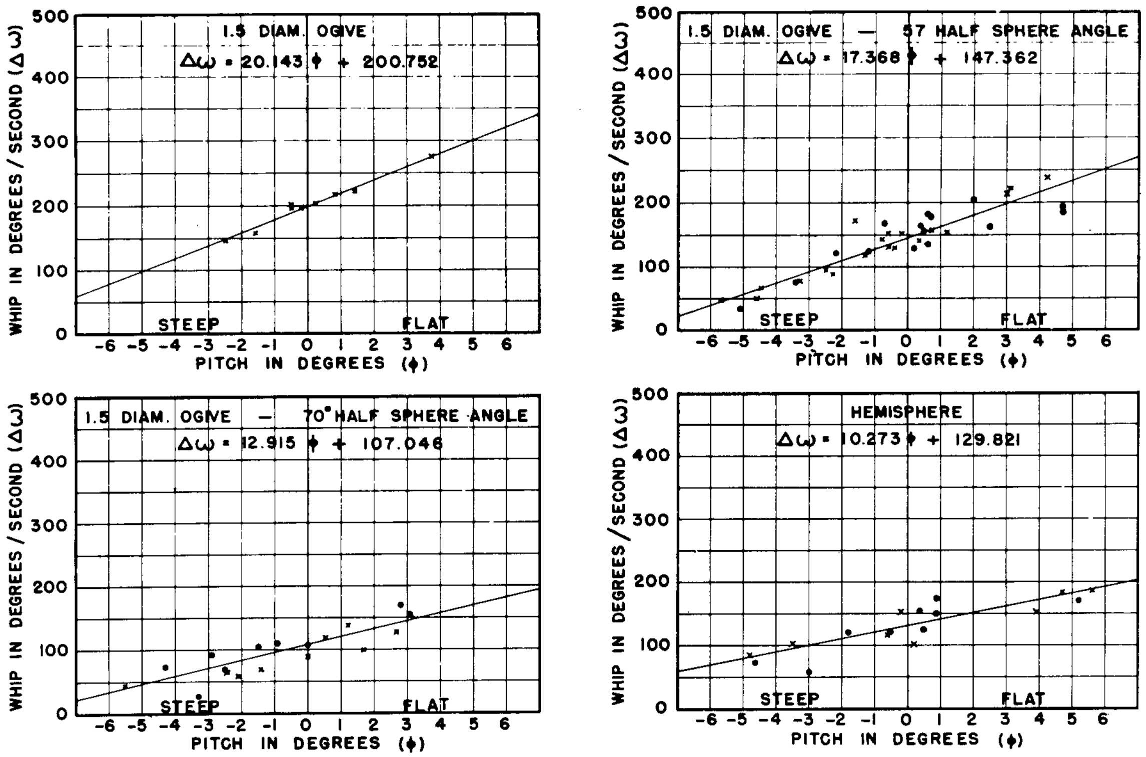
1.5 DIAM. OGIVE
70° HALF SPHERE ANGLE

HEMISPHERE

Figure 24

The "whip" curve to the right represents pitch sensitivity; that is, how much the whip varies per degree of pitch change. The ogive has a very high pitch sensitivity, while the sphere has the least.

When we consider the axial drag forces, we have another condition which could be predicted. The ogive has the lowest drag forces and the sphere the highest, but the ogive sphere combination upon which separation occurs on the ogive has almost as low drag as the pure ogive. This confirms the theory that the amount of drag is very closely proportional to the size of the cavity that is blasted in the water. It is very comforting to have such theories confirmed by objects 22 inches in diameter rather than merely on small models, where aerodynamic questions are often as important as the hydrodynamic ones.



PITCH DEPENDENCE OF WHIP FOR 500 FT./SEC. VELOCITY

WHIP CORRECTED TO 500 FT./SEC. ENTRY VELOCITY AND 800 SLUG FT.^{2} MOMENT OF INERTIA.

• APPROX. 500 FT./SEC. ENTRY VELOCITY.
× APPROX. 400 FT./ SEC. ENTRY VELOCITY.

Figure 25

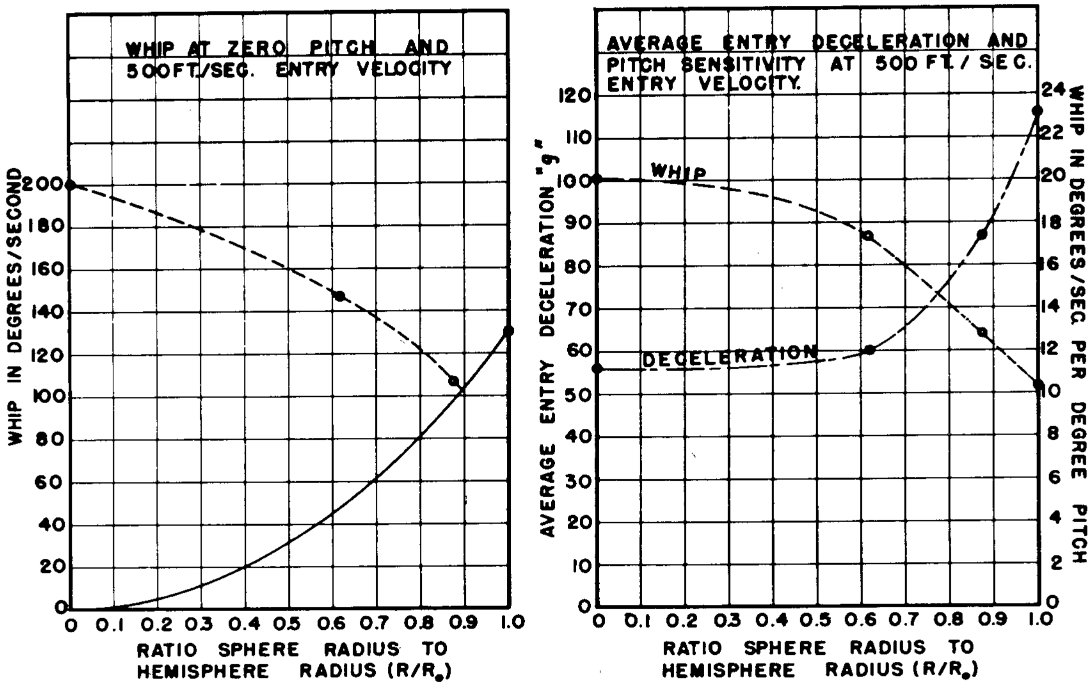
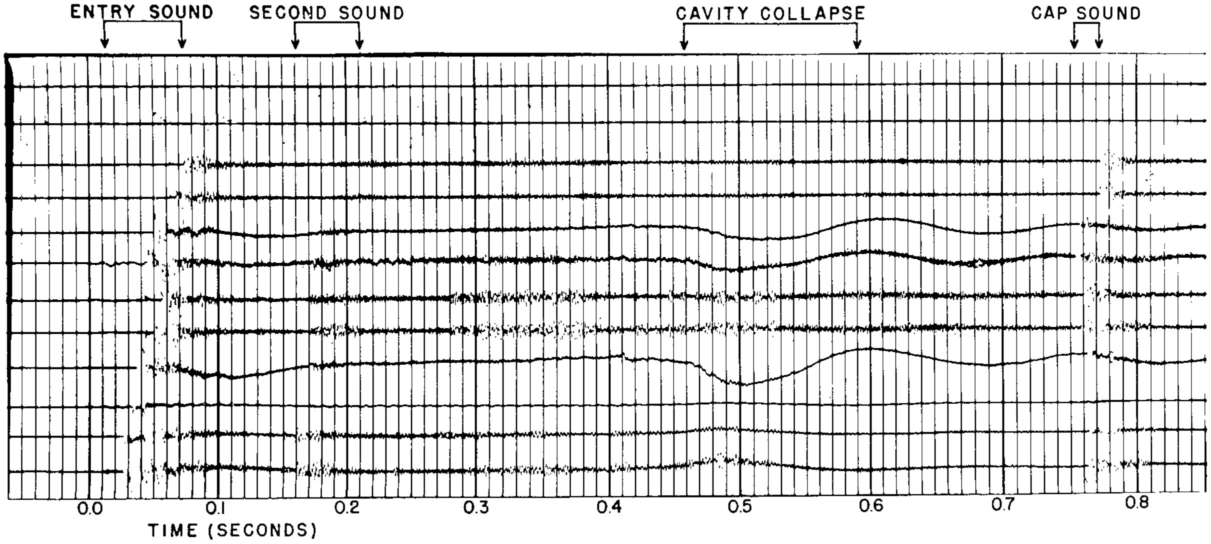


Figure 26

Figure 27 is a typical record made on a sound range used primarily to obtain the trajectory and distance-time data. Each trace represents the information picked up by a separate hydrophone. The hydrophones are arranged in a rectangular parallelepipedal array. Detonating caps are fired in sequence on the torpedo give records such as the one shown on the right of the figure. From the arrival times of the sound at four microphones it is possible to compute the position and time at which the cap was fired. These records show certain other information which has been of great value. On the left entry sound is recorded. The signals marked "second sound" are associated with the projectile striking the side of the cavity. The low frequency recorded at about 0.5 seconds has been correlated with the collapse of the cavity surrounding the projectile.



SOUND RANGE RECORD

Figure 27

We have found the use of indenter gauges rather disappointing, largely because of the shock history of the torpedo before it is launched. The torpedo rattles down the tube, experiencing short sharp shocks which put spurious signals on the gauges and make the results difficult to interpret. The use of preloaded indenters was out of the question in our case. The only way we could get results was with an indenter which actually moved to a fresh surface between each separate shock. Due to the freedom from shock during air flight, it had time to come to equilibrium in the new position and, hence, was able to take the record at water entry on a clean surface.

Indenter gauges are very susceptible to frequency troubles and unless one uses a series of gauges of widely different natural frequencies, so that it is possible to calculate out the frequency effects, the data are not easy to interpret. With a gauge preloaded almost to the point which it will record, one can get away from this frequency trouble; but this is not practical in our case, because of our launching method.

We, therefore, use what we call a "step accelerometer". A diagram of one element is shown in Figure 28. We have a cantilever spring which we preload by means of a screw. When a certain value of acceleration is reached, the contact is pulled away from the spring. By having a series of these beams with varying preloads, we can get a record of the wave

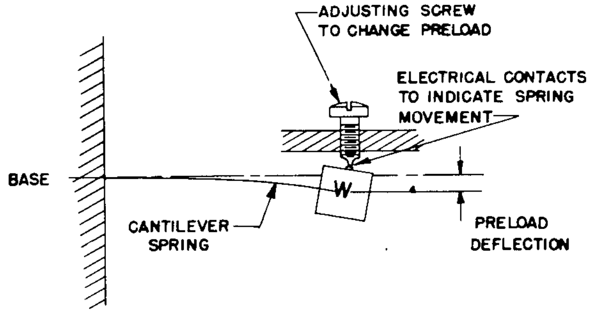


Figure 28. Representative CIT accelerometer spring element.

front, although the tail of the wave is not so well recorded.

The recording arrangement is shown in Figure 29. The contact from each accelerometer element is connected to a separate neon tube. An optical system is so arranged that the images of the tube are accurately aligned across the film. By moving the film rapidly, with one tube modulated by a 500-cycle timing oscillator, the time of break of any contact can be determined to about 1/20,000 second.

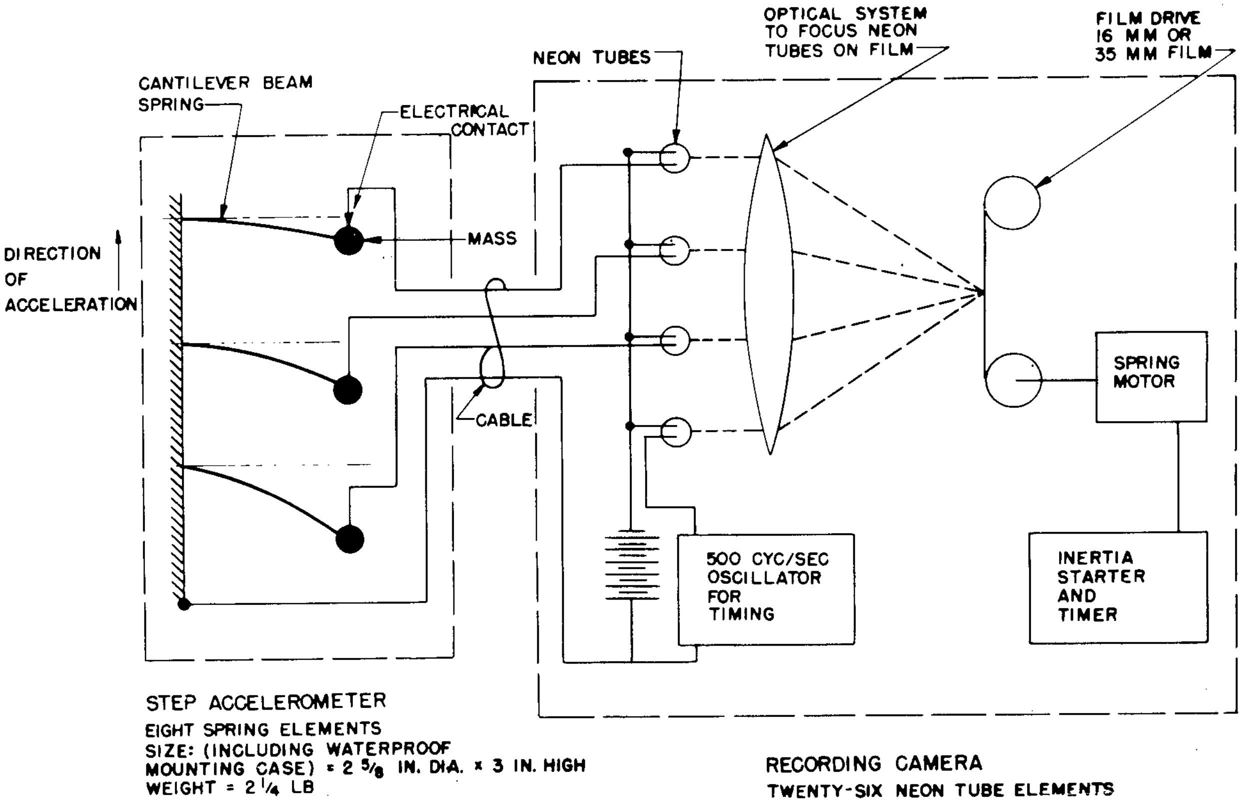


Figure 29

Figure 30 shows one of the step accelerometers with eight sensitive elements, and Figure 31 shows one of the neon tube cameras. The assembly on the lower right carries the neon tube. This camera is very rugged and has withstood launchings up to 500 feet per second.

An actual launching with three accelerometers recording on the same camera is shown in Figure 32. One is mounted near the nose, one very close to the point of initial impact and one at the gyro pot. We find that the one nearest the point of impact opened first, the nose one shortly thereafter and the one near the gyro pot still later. After about two torpedo lengths of travel, when the tail slaps the cavity, we get another fairly sharp shock at the tail location. When

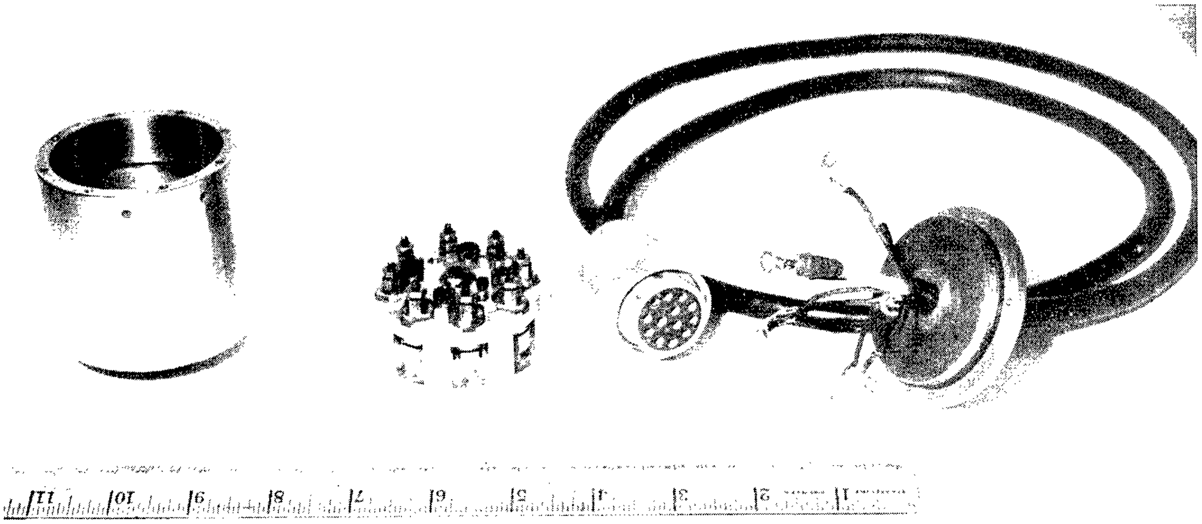


Figure 30

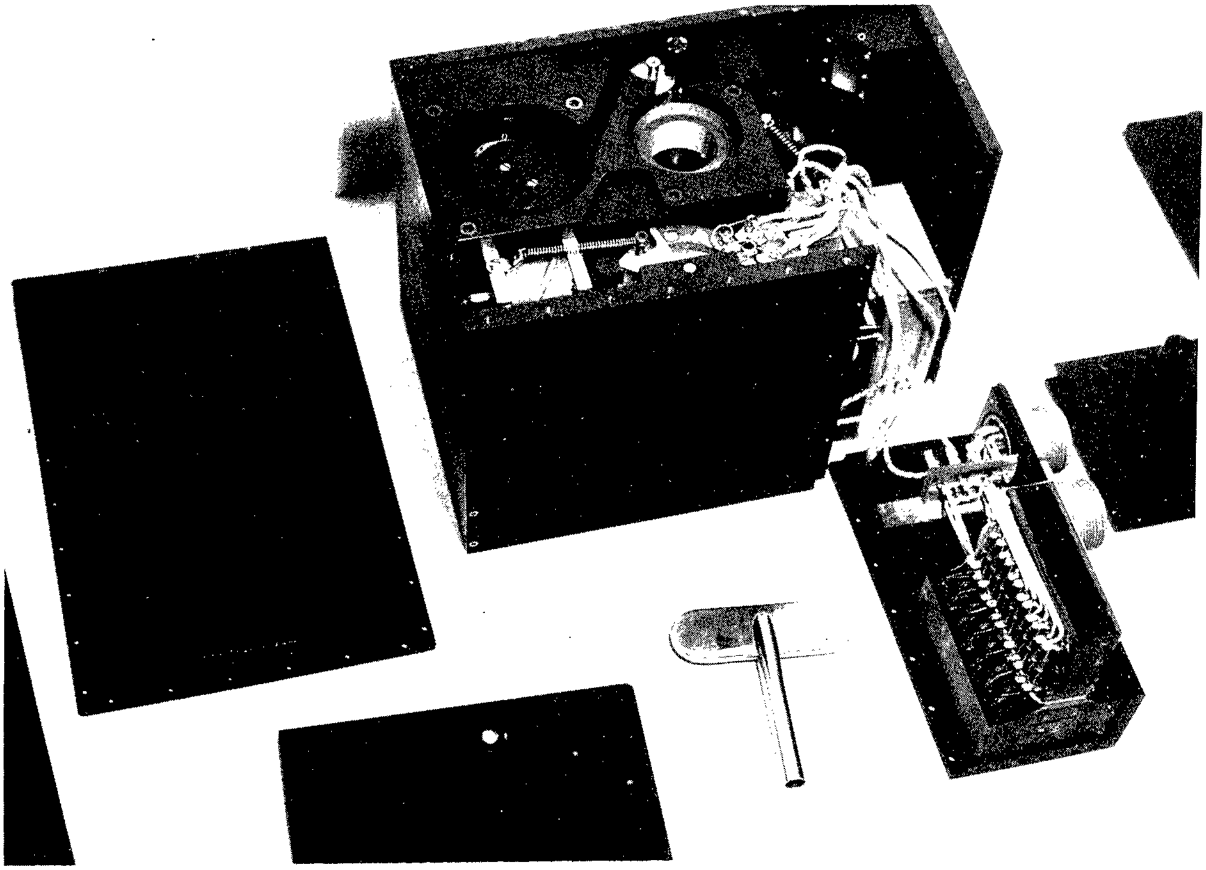
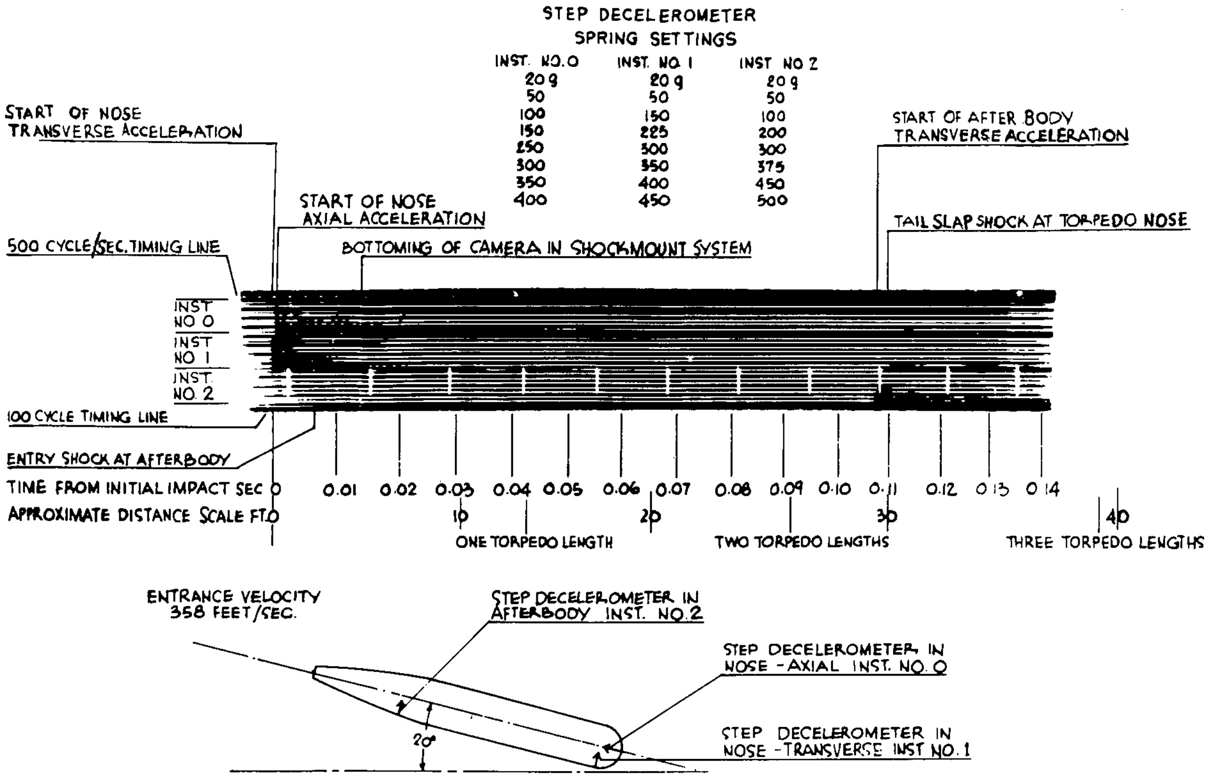


Figure 31



STEP DECELEROMETER SPRING SETTINGS

INST. NO. O	INST. NO. I	INST NO 2
20 g	20 g	20 g
50	50	50
100	150	100
150	225	200
250	300	300
300	350	375
350	400	450
400	450	500

TYPICAL RECORD FROM STEP DECELEROMETER INSTALLATION
MARK 13-2 TORPEDO; W = 1500 LB.
LAUNCHING NO. 240

Figure 32

we calculate the theoretical velocity of propagation of a shock wave in a beam of this sort, we find that these delay times are in fairly good agreement.

The fact that we find such high accelerations indicates that we are measuring local accelerations and not accelerations of the whole body. Calculation will show that the accelerations we record here could not possibly be those of the torpedo as a whole. These are the types of shock which could cause local denting and which, therefore, would be of interest in connection with components to be placed directly in the nose. These figures are useful in evaluating the likelihood of local damage but not in connection with the ballistics of the whole body. This must be kept in mind very carefully in instrumenting devices of this source. Ballistically, the fact that we do have short accelerations of 1,000 g. means very little.

Let us now look at some laboratory work on forces occurring right after the initial shock stage. The nose has quite a job blasting a cavity into the water. The water is standing still when first hit, and it has to be accelerated. Energy must also be put into maintaining the cavity, which actually abosrbs most of the energy. While the cavity is being established, we find accelerations that are somewhat above the steady values. In Figure 33 we have a shadowgraph of a sphere dropped into water with an accelerometer aboard. This photo was taken with two successive flashes. The first

Figure 33

shot showed the undisturbed water surface, and the second showed the sphere after it had penetrated some distance.

Figure 34 shows some of the data from a series of these tests. The technique loses precision for penetrations greater than 20% of the diameter, but, in general, it was found that there were accelerations during this entry phase about three times the steady drag accelerations. This peak occurred very early in the entry. The penetration is only about

DRAG COEFFICIENT BASED ON PRESSURE DISTRIBUTION

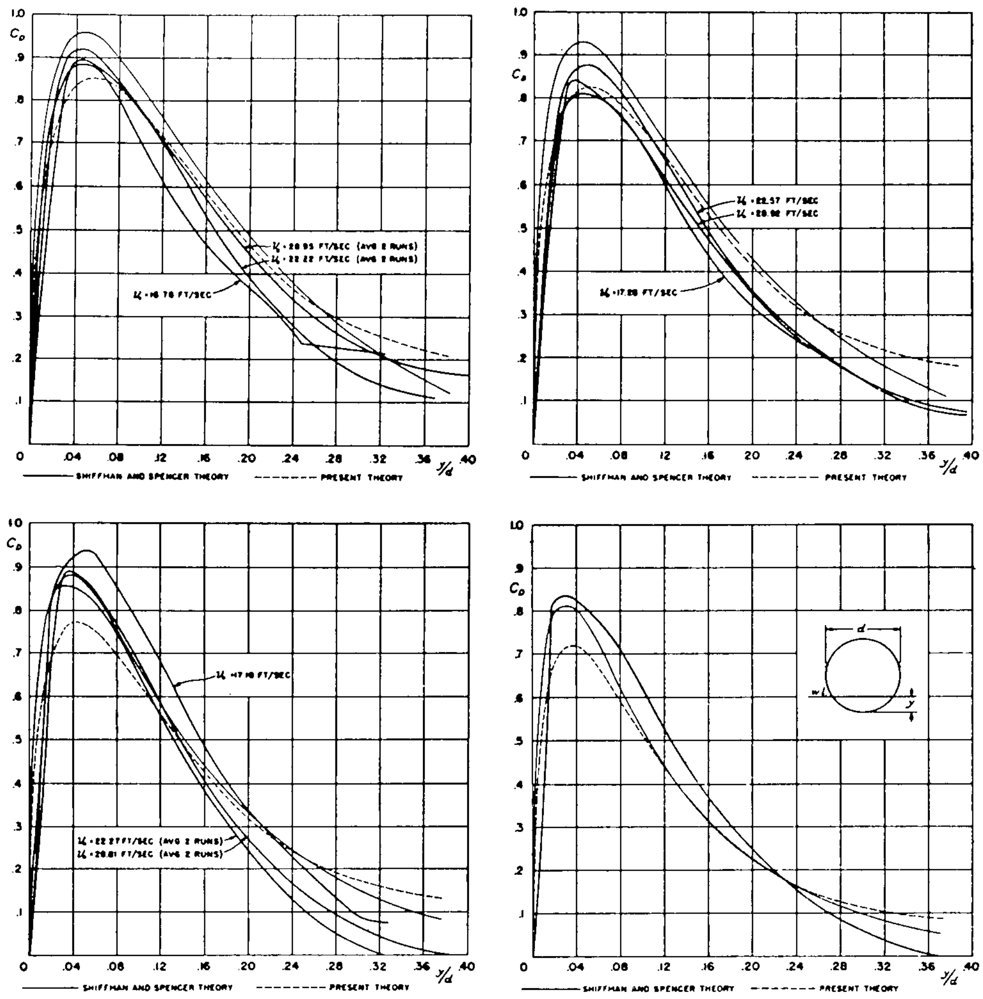


Figure 34. *Upper left:* impact drag coefficient of sphere (σ = 1.082).
Upper right: impact drag coefficient of sphere (σ = 0.782).
Lower left: impact drag coefficient of sphere (σ = 0.483).
Lower right: impact drag coefficient of sphere (σ = 0.296).

.04 of the diameter. These data are all for vertical entry. For oblique entry the time taken to establish steady flow is longer, and the peak is certainly not so great. The best available assumption is that the total area under these curves is equal for both normal and oblique entry. This has not been checked, either experimentally or theoretically, and poses one of the unsolved problems in the field. Theoretical results for normal entry have been attained both by Shifman and Spencer at New York University and, more elegantly, by Chou at the California Institute of Technology. Fairly good theoretical agreement has been attained with the curves as seen in Figure 34.

Now, I will show you some of the damage-measuring instruments developed during the war at C.I.T. and which were not used widely as they should have been. Figure 35 is a tension device in which a mass restrained to move only in one direction is pulling on a tension specimen. This is mounted in a projectile and furnishes the way of studying the forces peculiar to that particular section of the weapon. The big problem, of course, is to avoid friction effects.

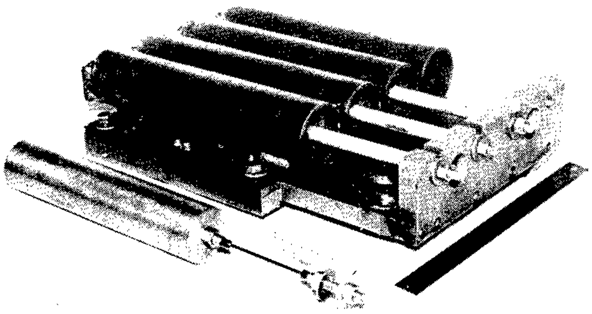


Figure 35

Figure 36 is a shear device in which the cylindrical masses sheared various sizes of pins. This device was actually used in determining whether or not it would be feasible to use a shear pin as a method of starting an igniter upon impact. The igniter would start the engine of the torpedo.

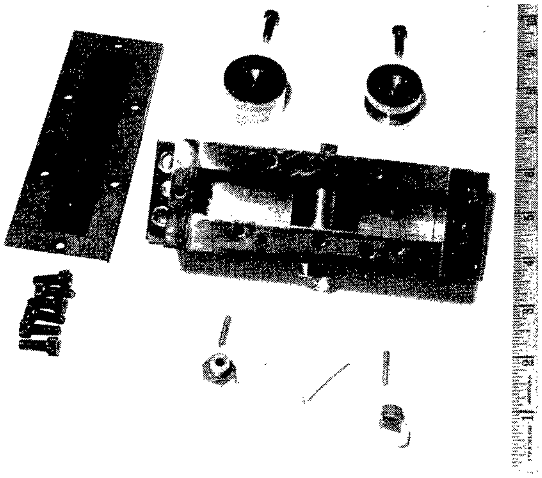


Figure 36

In Figure 37 we have a bending device in which are cantilever beams of various natural frequencies with various methods of loading. It can be placed in various parts of the torpedo to find out which of various types of internal structures might withstand the shock.

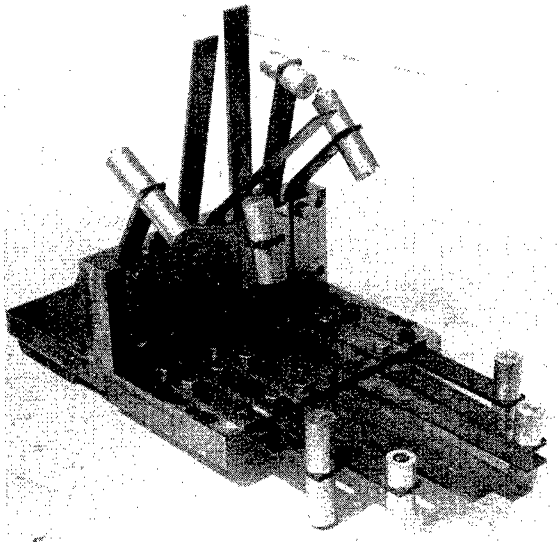


Figure 37

Now, just what do we expect to get out of such a program? The scientist, of course, would like to gain an understanding of just what goes on, but the military man wants this understanding as a means of designing projectiles and weapons to do various specific things.

In one case, the Germans arrived at a design which had very useful structural characteristics. That was the bomb torpedo, shown in Figure 38. This has a flat nose. It is, however, a rather small flat nose so that it does not introduce too much whip. A large flat nose is unstable: it will "stub its toe" and strike against the top of the cavity. Within the cavity, however, it has positive stability. If the flat nose is very small, there is very little whip tendency, and the forces are almost entirely in compression. They were able, therefore, to make these things of cast iron and even to launch them onto ship decks, they claimed, although I have found no record of their ever having been used in service. This is an example of good structural design.

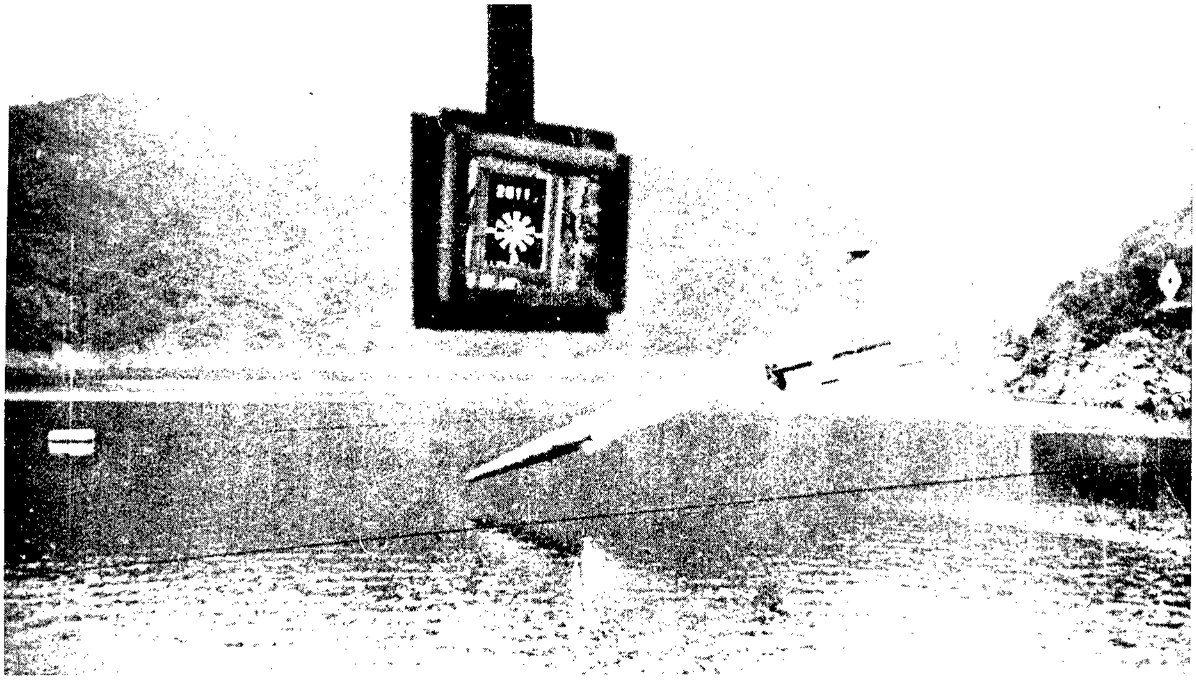


Figure 38

We always have to make a compromise, of course, between the structural design and the design that is best for ballistic purposes. In our laboratory in Pasadena we have been trying to set up a program of investigation into some of the fundamental aspects of water entry phenomena. For example, the problem of the response of a thick hemispherical shell under load has never been fully understood, even for static loading. Under dynamic loading even less is known. In the first place, we don't actually know what the loading is. Therefore, the first thing to do is to record the dynamic pressures so that we will have some idea of the type of loading. We need the actual pressure-time relationships. We hope to be able to apply the telemetering techniques, mentioned earlier, to make these measurements. We must also measure the response of the structure to those force conditions, and here the telemetering comes in again.

We have an ultimate goal of being able to design structures from theoretical considerations and then to perform our test work in the laboratory. We would like to be able to test them in a structures laboratory and then be sure that when they are put in the field, they will stand the gaff.

The problem of trying to put a projectile into the water at supersonic velocities is also far from solved. We can probably put a solid slug into the water, and with that we may be able to poke a hole in the side of a merchant ship, something as in the case of the British shark. When we are trying to put in devices that will know their target (such as influence exploders) or in cases where we will want a maximum charge-to-weight-ratio, we must know the fundamentals of impact loading and the response, not only of materials, but of complete structures.

Professor Clark at C.I.T. has done a great deal of work on impact loading effects on materials, but the whole field of impact effects on structures is one which requires a great deal of study. We have recently re-established the drop table which was in use during the war, in somewhat rebuilt form. This will be useful for studying components under sharp shocks. The air guns at the Naval Ordnance Laboratory are particularly good for simulating the effects of the more nearly constant loading after the initial shock phase. We are just completing an impact tensile test machine which will permit tests at velocities up to 800 feet per second, which is about four times the maximum used by Professor Clark. This machine will be adaptable to impact tests of structures at high velocities. Until we know the actual force-time pattern to which structures are going to be subjected, we are going to have a hard time arriving at a design. We need measurements: fundamental measurements, a large number of measurements, quantitative measurements.

PHYSICAL ASPECTS OF COUNTERMINING

By

R. E. Hightower, NOL

The phenomena of exploding relatively large charges within a neighborhood surrounding underwater ordnance devices are discussed. Various physical aspects are described in which changes may be measured in operation, state or condition of the ordnance devices involved.

While the primary function of ordnance containing an explosive charge is to inflict damage, either to enemy ships or underwater obstacles, it must be apparent that in instances where multiple units of ordnance are used, such as mine fields, torpedoes in salvo, pattern-fired depth charges and demolition outfits, the firing of a single weapon unit may impart tremendous shock and subsequent damage to neighboring weapons. This shock may inflict damage to underwater ordnance in a number of ways.

1). When the explosion is relatively near (15 feet or less), sympathetic detonation of the explosive charge may result.

2). At somewhat greater distances, mechanical damage, ranging from the crushing of steel cases to rendering gaskets ineffectual, may result. This effect is noted to a few hundred feet, depending upon the size of the charge.(Figures 1,2,&3)

3). At distances up to a mile from the explosion, closure of relays and switches in ordnance has been recorded, as well as the effects associated with magnetic shakedown in steel cases.

4). Acoustic mechanisms in ordnance devices may be affected up to 80 or 100 miles from an explosion.

The principle upon which a particular weapon depends for its efficacy may be one that is most vulnerable to explosive shock.

1). Mechanical movements of parts are produced by sudden accelerations or transient vibrations from the shock wave. Many of the components contained in ordnance devices are purposely made as delicate as the planting conditions will allow to gain sensitivity, long battery life, or for the sake of size. Often these factors make the weapon respond to mechanical motion imparted by a nearby explosive charge.

2). Magnetic signals are produced mainly by shakedown of ferromagnetic cases and by magnetostrictive causes. The induction mine consists essentially of a mine case containing a search coil for detecting changes in the earth's magnetic field, a firing mechanism for detonating the mine for a selected type or sequence of signals received by the search coil, an explosive charge and accessories for arming, safety, etc. The mine case is usually fabricated from steel, its history following the usual manufacturing processes of rolling, welding, hammering, filing, grinding, machining, etc. These processes impart a magnetic state to the mine case, with each mine being both complex and individual in this regard. It is not surprising, then, to find when such a case is subjected to the elaborate mechanism of a countermining shock, that a change of magnetic state occurs in the case, that a flux change through the search coil coupled to the case takes place, and that the firing device operating from the search coil, may be actuated.

3). Acoustic signals are produced by the shock wave, either direct, reflected from bottom or surface, or refracted great distances through the ground. The acoustic signals produced by countermining are responsible for the effects over the greatest distances and the widest variety of phenomena. Crystal type hydrophones have been known to generate sufficient voltage from explosions to cause arcing within a vacuum tube used as an amplifier following the hydrophone and to burn out the filament of the tube. The actuation of acoustic mine mechanisms has occurred at distances up to 100 miles. It is believed that refraction of the acoustic energy from the countermining explosion may be partly responsible for this phenomena (see Figure 4).

The range of damage or influence depends not only upon the type of actuating signal but upon the type of weapon. Mines are either ground, moored or floating. Torpedoes move horizontally at from six to twenty foot depths, sometimes in a fan spread. Depth charges move vertically and are usually dropped in patterns. Demolition outfits are usually on the bottom and spaced as closely as possible but beyond the crushing range. With the wide diversity of possible weapon locations, shocks have produced phenomena not always anticipated from the layout of the test, while undergoing countermining.

With favorable bottom conditions and placement of charge and weapon, to encourage the reinforcement of a direct and reflected wave, it is possible to experi-

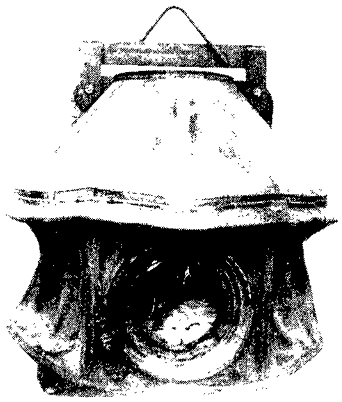


Figure 1. Army Gorund Mine T5, Results of Countermining at 120 ft. with a 3000-lb. Charge of Granular TNT

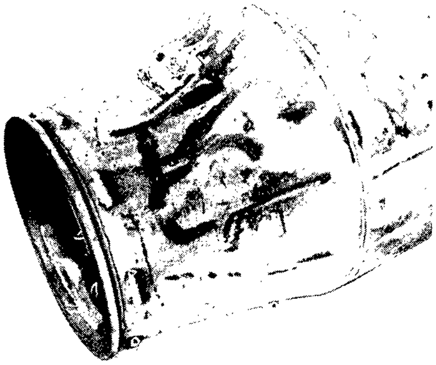


Figure 2. Damage to Mine MK 25 Mod 1 from 30-lb. TNT shot 15 ft. away 10 ft. off the bottom

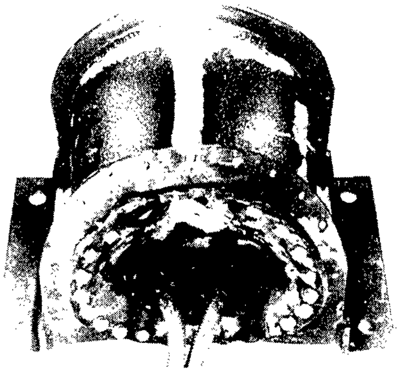


Figure 3. Mod 1 Demolition Firing Device, Damage from 7000-lb TNT Explosion 50 ft. away in 20 ft. of Water

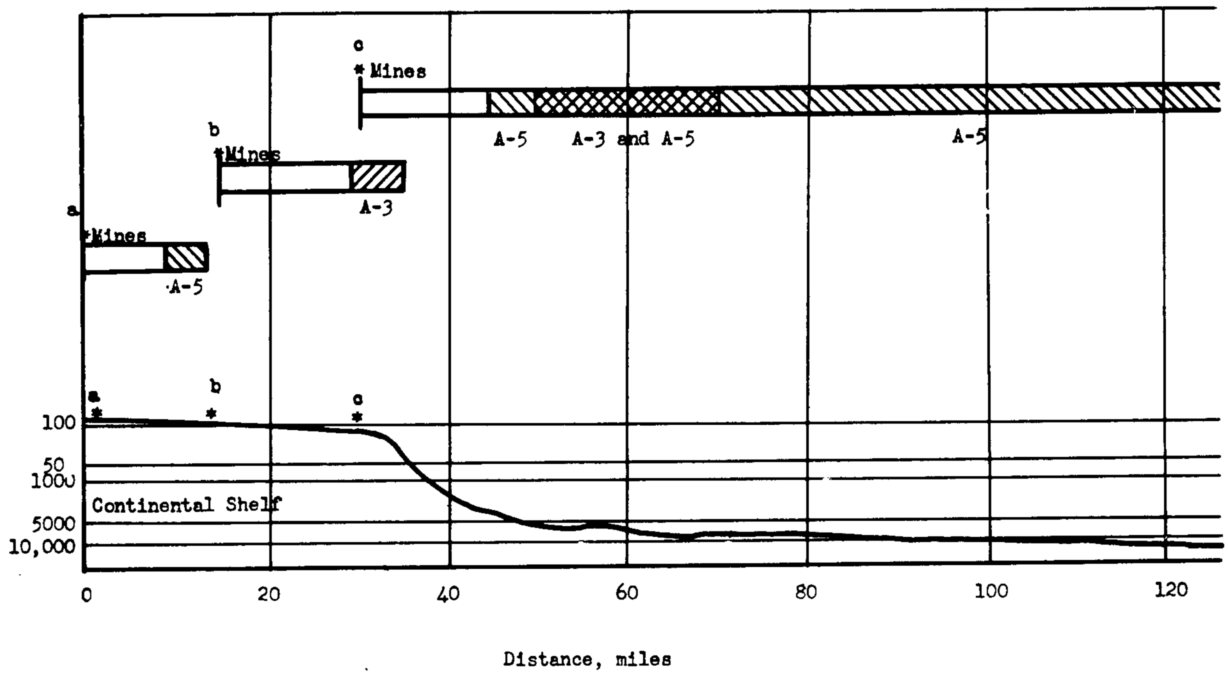


Figure 4. Zones of Actuation of Acoustic Mines from Underwater Explosions of 50-lb TNT and 630-lb TPX Charges at Various Depths

ence a higher peak pressure acting upon the weapon than would be normally expected. The converse is equally true when the energies transmitted by the direct wave and a reflected wave cancel. Mines planted on the bottom may be located in a so-called "shadow zone" with respect to the origin of the countermine shock and actually experience somewhat milder pressure at low angles than might be anticipated (see Figure 5).

For the most part, in the past countermining has been investigated under full-scale conditions in the field. Shock tests are possible in the laboratory, but these tests are a better simulation of shock imparted as a result of planting from aircraft than of the countermining phenomena. In a like manner, laboratory vibration studies best simulate transportation conditions. It would be of tremendous value if the scaling of charge size and distance could be successfully accomplished to allow for accurate laboratory investigation of the countermine phenomena. While it is true that a smaller charge at a lesser distance will yield the same peak pressure as a larger charge at

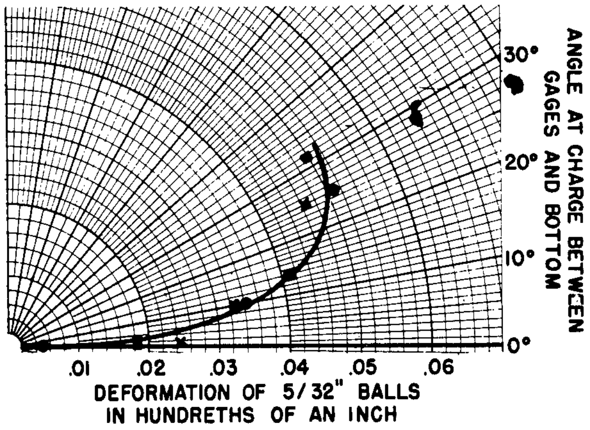


Figure 5. Effectiveness of Explosives at Small Angles from Charges Fired on a Mud Bottom

greater distances, the time constant does not lend itself readily to scaling. Until such a technique is developed, work to investigate the effects of countermining phenomena on weapons will continue under full-scale conditions.

ACCELERATIONS PRODUCED BY CONTROL OF GUIDED MISSILES

by

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In the "Bumblebee" program, the designer is confronted with many types of accelerations and vibrations. Empirical equations are here developed to represent certain control conditions. The problems of boosting accelerations and excessive vibrations are still being attacked.

The present paper presents a consideration of the acceleration of guided vehicles, due to guidance and control, that has been experienced in the "Bumblebee" program. It will be recalled that our experience has been connected with the subsonic guidance test vehicle CTV, and the supersonic guidance test vehicle STV-2.

In the simplest cases, the response of a missile in flight under control is described by one of the following two types of equations:

Type A: $(p^2 + 2ap + a^2 + \omega^2)\ \phi = C$ (1)

Type B: $(p^2 + 2ap + a^2 + \omega^2)\ A = k\,\delta$ (2)

where $p = \dfrac{d}{dt}$

a, ω, C, k = constants

δ = wing or flap displacement

ϕ, A = dynamic responses

ACCELERATIONS ARISING UNDER TYPE A.

Since the displacements of a missile under complete control are restrained to form a dynamically stable vibrating system, the equation of Type A can be interpreted as the dynamic response of the missile displacements about its c.g. position. $C \equiv 0$ corresponds to the free response while if $C \neq 0$, the response to a steadily applied torque is obtained. The variable ϕ may be interpreted as displacement about the pitch, yaw, or roll axes.

ROLL ACCELERATIONS: Type A C = 0.

If we assume initial conditions $\phi = \phi_0$, $\dot{\varphi} = 0$, $t = 0$, the solution of (1) is

$$\phi = \frac{\phi_0}{\omega} e^{-at} (\omega \cos \omega t + a \sin \omega t) \qquad (3)$$

When we consider ϕ to represent roll displacement then two kinds of accelerations are of interest, namely, the centrifugal acceleration at each point and the acceleration tangential to the circle during oscillatory response.

Values of these two accelerations are dominated by the following formulas for which the attenuation factor, e^{-at}, has been neglected.

$$A_n(\max) = r(\dot{\phi}_{\max})^2 \leq r\ \phi_0{}^2\ (\omega^2 + a^2) \qquad (4)$$

$$A_t(\max) = r(\ddot{\phi}_{\max}) \leq r\ \phi_0\ \omega(\omega^2 + a^2) \qquad (5)$$

where r = radius of the point under consideration from the axis of rotation.

As a typical example for a missile (CTV, approximately) we assume the following values of the parameters.

$$a = 5 \text{ sec}^{-1}, \quad \omega = 6 \text{ sec}^{-2}, \quad r = 1/2 \text{ ft}$$

then (4) and (5) become

$$A_n(\max) < 32\, \phi_o^{\,2} \qquad (6)$$

$$A_t(\max) \angle 183\, \phi_o \qquad (7)$$

If the initial displacement $\phi_o = 1$ radian, the maximum accelerations of (6) and (7) are less than 1 g and 6 g respectively. From this calculation we can conclude that components mounted near the periphery of a missile can experience comparatively large accelerations.

STEADY OSCILLATIONS IN PITCH AND YAW

Although a beam riding steering system is functioning properly according to its design in its responses to intelligence signals, experience has shown the existence of steady undamped oscillations in pitch and yaw. These are due to many factors which may include backlash, dead space, wing twist and so on. These oscillations may be described empirically by the equation (1) with $a \equiv C \equiv 0$. In this case the maximum lateral acceleration is of the form

$$A(\max) = d\, \omega^2, \qquad (8)$$

where $\underline{d}$ is the amplitude of the oscillator and ω is the response frequency. Furthermore, ω can be expressed in terms of the x-component V_x of the linear velocity, assumed to be constant, as

$$\omega = b\, V_x \qquad (9)$$

so that the path equations are

$$y = a \sin bx, \quad x = V_x t \qquad (10)$$

For oscillations of amplitude $d = 100$ feet, the maximum lateral acceleration is

$$A(\max) = 100\, \omega^2 .$$

An angular frequency of 1 radian per second, equivalent to a period of approximately six seconds, produces a maximum acceleration of slightly more than 3 g's. If on the other hand the resonant frequency of these noise components goes up to $\omega = 2$ radians per second (period of approximately three seconds), the maximum acceleration will exceed 12 g. The corresponding length of a complete cycle for speeds of 600 feet per second and 1800 feet per second are given in the following table:

$d = 100.$

$V_x = 600$ ft/sec, $\omega = 1$ rad/sec, $b = \dfrac{1}{600}$, $x_\sim = 3800$ ft.

$\omega = 2$ rad/sec, $b = \dfrac{1}{300}$, $x_\sim = 1900$ ft.

$V_x = 1800$ ft/sec, $\omega = 1$ rad/sec, $b = \dfrac{1}{1800}$, $x_\sim = 11{,}400$ ft.

$\omega = 2$ rad/sec, $b = \dfrac{1}{600}$, $x_\sim = 5{,}700$ ft.

One can therefore conclude that oscillations of this type can lead to relatively high accelerations since an oscillation of a missile moving at 1800 feet per second with an amplitude of 100 feet will produce a maximum acceleration of 12 g in a cycle which is approximately one mile long.

ACCELERATIONS CHARACTERIZED BY TYPE B

A perfectly designed guided missile will fly its prescribed path in transient or steady state conditions. The transient phases must be sufficiently damped to generate the steady state condition quickly. The maximum lateral acceleration in any oscillations in the transient period will be less for a damped system than for the corresponding undamped case. Hence the transients have an upper limit given by a formula of the form (8).

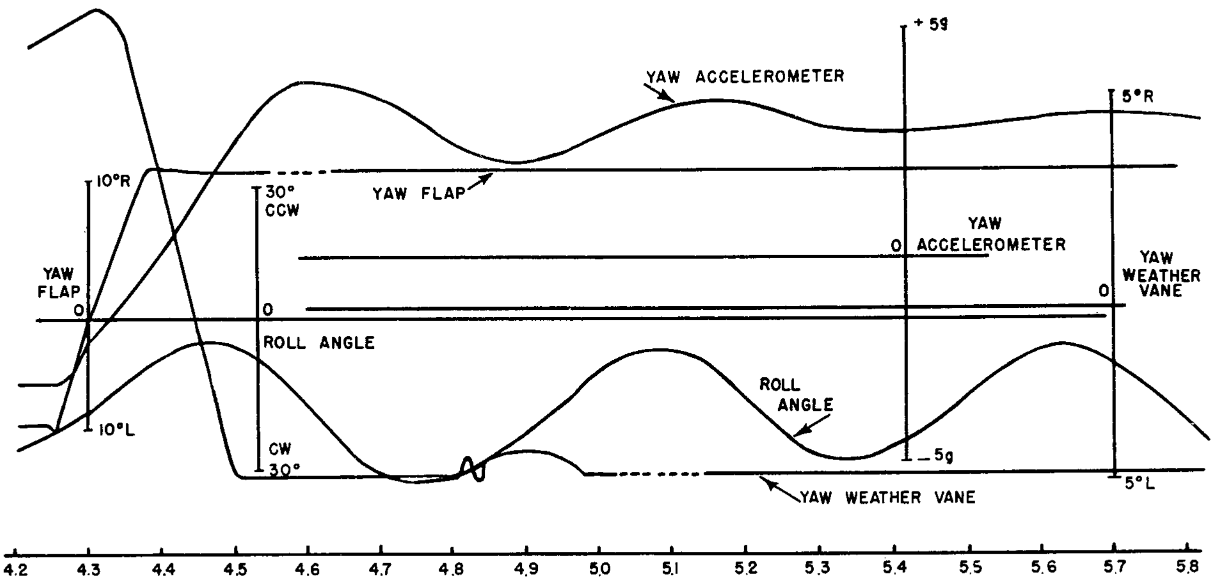


Figure 1. Telemetering Record
CTV-2 #67 Mark 7 Mod 3

The steady state condition will depend upon the way δ is controlled during the steering period. Some insight can be obtained on these phenomenon by considering the lateral accelerations produced by step inputs in δ. Flight tests of this type have been performed on roll stabilized CTV's and STV-2's. Performance curves for the two types of test vehicles are shown in Figures 1 and 2.

Empirical equations which represent these acceleration responses to the first order of approximation are:

CTV (670 feet per second):

$$(p^2 + 6p + 150)A = 1460\ \delta\ ,\quad (\delta \text{ in degrees}) \tag{11}$$

STV-2(1525 feet per second):

$$(p^2 + 6p + 825)A = 29500\ \delta,\quad (\delta \text{ in degrees}) \tag{12}$$

For a step input wing deflection, say

$$\delta = 0,\ t \angle 0,\ \delta = \delta_o,\ t > 0 \tag{13}$$

and initial conditions

$$A \equiv A = 0,\ t = 0, \tag{14}$$

the solution of (12) becomes

$$A = 36.5\,\delta_0{}^\circ \left[1 - e^{-3t} (\cos 28.6t - 3 \sin 28.6t) \right],$$
δ_0 in degrees. (15)

The graph for (15) is shown in Figure 3. When compared with the experimental curve of Figure 2 it is seen that the attenuation, steady state response for $\delta_0 = 1\frac{1}{2}$ degree, and response frequency check rather well. The amplitude of the transients differ by a factor of 2. Later in the flight, these amplitudes also check. The deviation in the response during the initial flap setting from the empirical formula derived from later deflections is not explained.

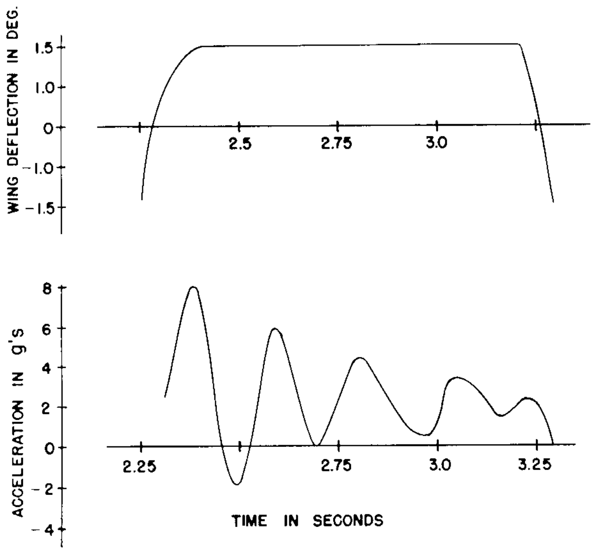
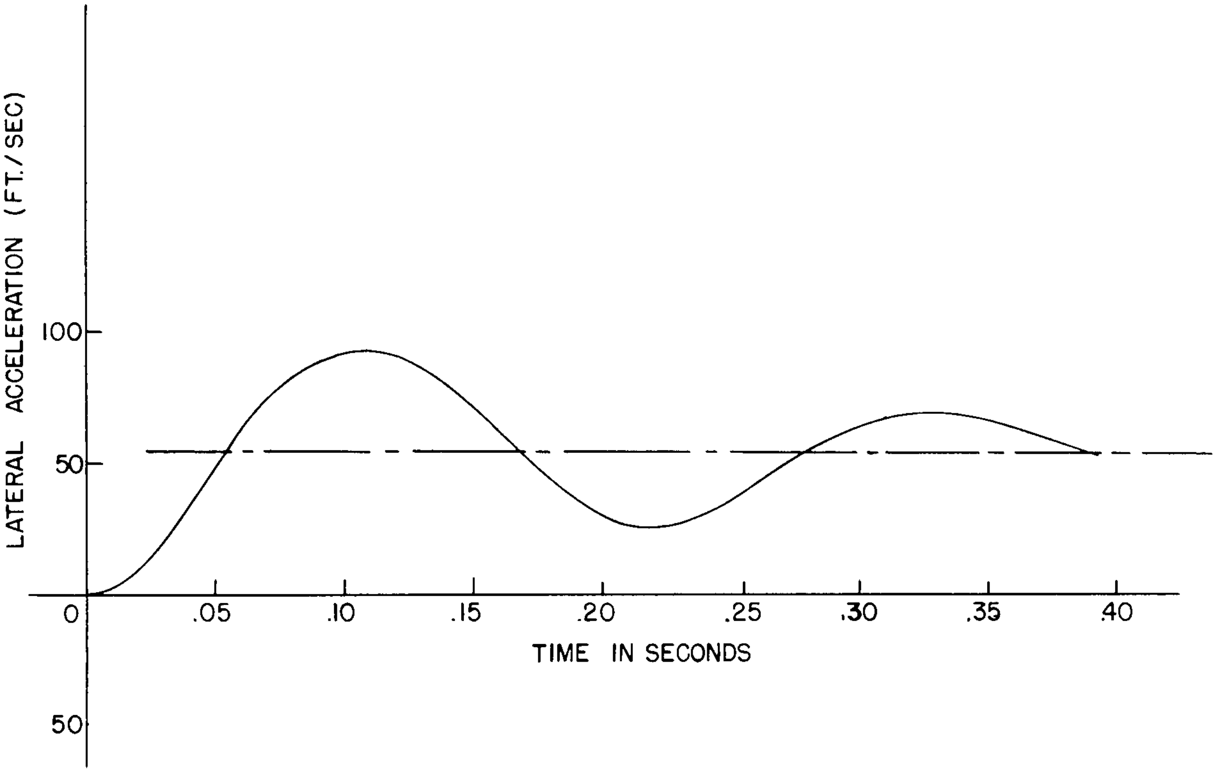


Figure 2. Flap Motion and Lateral Acceleration vs. Time
STV-2A #26



$$(p^2+6p+825)A = 29500\,\delta$$
$$\delta = 0,\ t<0, \quad \delta = 1.5^\circ,\ t \geq 0$$
$$A = pA = 0, \quad t = 0$$

Figure 3. Lateral Acceleration vs. Time with Step Wing Motion
STV-2

"BUMBLEBEE" PROGRAM ON ACCELERATIONS AND VIBRATIONS

The two major problems to be considered in a guided missile program are (a) the design of the components, including the airframe for peak accelerations due to the boosting and steering operations and (b) the design of these same components to withstand the sustained vibrations present in the missile or to avoid the presence of these vibrations. Except for the airframe the problem under (a) is rather simple. Airframes are being designed in the "Bumblebee" program to withstand approximately $7\frac{1}{2}$ g of lateral acceleration. The maximum boosting acceleration of approximately **50** g turns out to be the determining factor for component specification.

The problem of microphonics and excessive accelerations arising from vibrations of mounting bases for components has not been solved for the "Bumblebee" missiles. The main reason why this has not been done is that the ram jet motor in the missile prototype size has not been flown and it is fairly certain that the vibrations in the air can not easily be duplicated on the ground because of the way in which the ground test must be anchored. The expected excessive vibrations arising from the ram jet burner will be considered when measurements become available on the amplitude and frequency as a function of position inside the airframe.

During the development stage of components, microphonics are considered from only a qualative standpoint. These components will require further development if the vibrations on the prototype are extreme and can not be eliminated.

DISCUSSION

MR. CUNNINGHAM, NRL

In connection with the subject of vibration in guided missiles, I think this group would be interested in vibration data obtained from V-2 rockets. I would like to discuss several paragraphs from two preliminary reports which summarize the data obtained to the present time.

Three type 4-102 Consolidated Engineering Corp. vibration velocity pick-ups were installed in the warhead of a V-2 rocket fired 7 March 1947. The pick-ups were arranged to measure vibration in each of three mutually perpendicular planes; parallel and perpendicular to the longitudinal axis of the rocket.

Observation of the recorded data shows that the vibrations perpendicular to the longitudinal axis of the rocket were so small as not to be capable of measurement with the present equipment (less than **0.002** inch). Vibrations parallel to the axis of flight of the rocket had measured frequencies varying between 42 and 56 cps. Peak vibrational velocities varying between 1.06 and 1.82 inches per second were observed.

Assuming an average frequency of 50 cps. the velocity was integrated to compute the vibrational excursion encountered. Computed data showed that the peak (double amplitude) excursion varied from 0.0068 to 0.116 inch. It should be noted that these peak excursions represent the most severe conditions. The vibrations waxed and waned in a manner similar to that occurring on board ship. A considerable amount of third harmonic content was also observed with a frequency of approximately 150 cps. but with an extremely small amplitude. The vibration was present only during the time the rocket motor was firing, indicating that the motor probably was the cause of the vibration.

A Consolidated Engineering Corp. Type 4-102 vibration pick-up was mounted in the control chamber of a V-2 rocket, fired on 10 July 1947. The pick-up was oriented to measure longitudinal vibration along the line of flight of the rocket.

An analysis of the record disclosed vibrational frequencies between 36 and 48 cycles per second and a peak velocity of 1.785 inches per second. Assuming a vibrational frequency of 40 cps. the double amplitude motion was computed to be 0.014 inch. The corresponding peak acceleration was computed to be 1.17 g. These values represent almost continuous vibration at amplitudes somewhat smaller than the maximum. The vibration was apparent only while the rocket motor was firing.

It was reported that this rocket did function properly and the rocket motor was cut off after 32 seconds of flight. It is not known whether the above vibrational values are truly representative. The error in the above data is accurate to plus or minus 20 percent.

REVIEW OF METALLURGICAL FACTORS INVOLVED IN THE FAILURE OF STRUCTURES OR COMPONENTS BY SHOCK OR VIBRATION

By

Dr. I. G. Slater
British Admiralty Delegation

The lack of basic knowledge on failures due to shock and vibration are discussed and some of the difficulties in obtaining such information are pointed out. The importance of design and fabrication is emphasized. Failures due to shock are usually associated with:

(a) the presence of a stress raiser of appreciable magnitude;
(b) a "susceptible" steel;
(c) suitable temperature condition;
(d) locked-up stresses;
(e) an "energiser" to set off rupture.

The effect on fatigue life or failure due to vibration by such factors as frequency of vibration, temperature, cold work, over-and under-stressing, residual stresses, surface finish and corrosion are briefly discussed.

INTRODUCTION

I intend to deal in this paper with metallurgical problems which are evident in the catastrophic failure of structures or components in ships subjected to shock or vibration and to outline investigations and avenues of thought which are current in the United Kingdom. From a study of the many papers presented to these symposia, it is apparent that the development of techniques for measuring the various physical phenomena involved in shock or vibration has greatly outstripped advances in our basic knowledge of how and why the structures or components actually fail. It is further evident that the path ahead of us in this matter is going to be far from straight and smooth since we have to contend not only with the enunciation of new physical concepts in the properties of materials but also the fact that our present materials of construction are far from being isotropic or homogeneous. Methods of construction share parallel features of irregularity in that local departures from the acceptably satisfactory are frequently encountered. The practical significance of this lack of regularity has long since harassed the engineer in his efforts to insure acceptable standards by way of specifications, to use the present known characteristics of materials to best advantage in his designs and particularly, to bridge the gap in our knowledge between behavior in practice and current theory.

"IRREGULARITIES" OF MATERIALS AND METHODS

The metallurgical literature is full of references to these matters which seem to be inexhaustible in their varieties and ramifications. It will be useful to classify some of them as follow:

1). Uniformity as influenced by relative degree of freedom from blowholes, shrinkage cavities, inclusions, segregation and other discontinuities.

2). Variation in physical and mechanical properties with direction, as influenced by the methods of manufacture as in rolled plate.

3). Changes which occur consequent on plastic straining such as strain age embrittlement.

4). The effects of temperature on properties and in particular, the catastrophic rupture of certain mild steels at lower temperatures.

5). Effects of surface condition such as its relative roughness or smoothness consequent on machining operations, decarburisation as may be evidenced in steel castings or plate surface damage by corrosion.

6). Imposition of high local stresses and strains in manufacturing or constructing operations such as those which occur around rivet holes, stresses locked up in welding, bending to shape, defects in welds.

The practical significance of each of these irregularities is a long way from being fully enenciated. Experience has indicated that a substantial number of them constitute hazards of relatively rare occurence while others are a constant source of speculation. From the physicist's point of view where accurate measurement is a prime requirement, the task of giving even a first approximation to the solution of many of these problems is indeed a big one. Briefly then, the state of the art is much more qualitative than quantitative.

FRACTURE RESULTING FROM SHOCK

Mechanisms of failure under conditions of shock can be classified in terms of whether the fracture is "brittle" or "ductile". Brittle fracture denotes catastrophic failure under the particular system of stressing involved and is obviously a complex characteristic about which we have much to learn. The fact that, given a suitable stressing system, many materials which show good ductility according to normal specification tests will fail in a brittle manner, merits particular emphasis. This aspect was brought into great prominence consequent on failures encountered in the earlier designs of Liberty ships. At the same time, instances of brittle fracture in many other types of structures or components are far from uncommon and will be discussed later on.

Ductile fracture denotes that considerable amounts of plastic deformation have occured before actual rupture. In practice this implies that the hazards consequent on overstressing are much less serious in that in many cases the whole of the energy expended in the shock can be taken up by distortion of the structure. The manifestation of ductility in a structure is thus an achievement depending in part on the inherent qualities of the materials and fabrication methods employed and in part on the skill of the designer. In the latter connection it is worthy of note that the possibilities of using qualitatively the plasticity of mild steel in design show many attractions and some of us may still have memories of air raid shelters where consideration of such factors yielded safe dividends.

For the purpose in view, problems associated with brittle fracture are thus of immediate concern.

PHYSICAL CONCEPTS OF BRITTLE FRACTURE IN METALS

In recent years substantial contributions have been made to our concepts of brittleness, notably by E. Orowan, N. F. Mott, L. Bragg and G. I. Taylor and it will be useful to summarize briefly some of their views.

The ultimate criterion whether a metal shows a brittle or ductile fracture depends on the relative magnitudes of the yield stress and of the so-called brittle or cleavage strength. With a smooth test-piece of normally ductile material, plastic deformation continues to a point of rupture before the tensile stress reaches the value of the brittle strength. In a test piece carrying a suitable notch or crack, however, a state of triaxial tension exists around these stress raisers so that the maximum tension at yielding can reach much higher values than in uniaxial stressing. If such higher values reach the brittle strength level, fracture occurs without significant plastic deformation. Orowan has indicated that the highest tension likely to be encountered in a notched specimen is of the order of two to four times that of the ordinary yield stress, depending on the material and probably reasonably independent of the notch contour. The evaluation of brittle strength is thus a matter of considerable significance and promises useful lines of investigation.

Another significant factor in this line of argument is the fact that the yield stress of a material usually increases with the rate of deformation and it is clearly possible to envisage rates of loading at which the yield stress may rise so high that the brittle strength level is attained with consequent brittle rupture. Even with materials where ductile crack propagation normally occurs, it may happen that a local discontinuity such as a cavity or crystal of particularly low cleavage strength gives rise to sudden splitting instead of yielding. This change to high velocity rupture, although initially local, may well switch the mode of crack propagation from the ductile to the brittle type.

A further process by which stresses exceeding the normal yield stress can arise is the phenomenon of elastic superstressing which is evident at the roots of notches and other stress concentrations of small linear dimensions. It has been shown that such stresses may reach values of up to three times that of the normal yield stress at sharp notches. Bragg has indicated a quantitative relationship between the yield stress and the volume to which plastic deformation is confined.

To summarize, it may be stated that the tendency of a material to notch brittleness is evidenced when the brittle strength is less than about three times the ordinary yield stress. Enhanced tensions may occur at notches or other forms of stress raisers by the presence of triaxial tension or superstressing or by high velocity effects.

Many other features are emerging which merit close attention and among these it may be appropriate to mention concepts as to the apparent greater liability for thicker plates to suffer brittle fracture, the significance of the propagation of plastic waves in metal and their velocity relative to that of the impact imposed by the extraneous source of stressing.

METALLURGICAL CHARACTERISTICS OF INSTANCES OF BRITTLE FRACTURE NOTED IN SERVICE

Most of the catastrophic fractures encountered in service have been in various types of mild steel and undoubtedly the most spectacular of these have occurred in ships plating where welding had been employed for construction purposes. The steels concerned have usually shown reasonable ductility in normal specification tests and investigations as to the underlying causes of apparent brittleness have been vigorously pursued on both sides of the Atlantic. It will be useful to summarize the general findings to date under a number of headings according to the nature of the structure or component concerned.

Failure in Mild Steel Ship's Plating.- The point has clearly emerged from the failures examined that the inherent quality of the steel is a predominant factor in determining the relative susceptibility of the material to catastrophic failure. The nature of this inherent quality has not been fully explored but it is evident in a more practical way that mild steels of higher carbon content or of lower manganese content are more hazardous materials. Additionally, all these steels show very low impact strengths at lower temperatures - temperatures at which the failures occurred in service. This relationship between susceptibility to notch brittleness and temperature is the significant feature for various types of steel as illustrated graphically in Figure I.

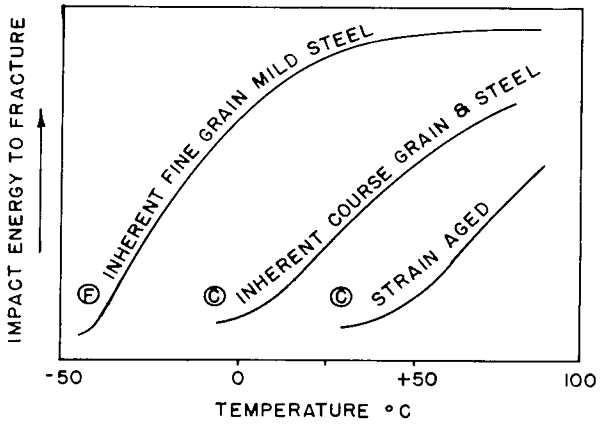


Figure I. Change in Impact Resistance with Temperature.

In most if not all cases, the origin of the fracture coincided with some form or another of stress raiser such as a sharp corner or an area of defective welding. Loading conditions at the time of failure appear to have varied considerably - from purely static loading involving stresses imposed in the structure by virture of its configuration and technique of fabrication to shock loading of reasonable magnitude consequent on the movement of the ship at sea.

Metallographical examination reveals that the path of these brittle fractures is, as a rule, transgranular running across the ferrite and often changing in direction when passing from one grain to another. Certain evidence has been accumulated to suggest that the fractures are coincident with the cube faces (indices 100) of the iron lattice which is the cleavage plane. Associated with the main fracture there are often small secondary cracks exhibiting similar features. Neumann bands are also evident in some cases near the fractures. The presence of such bands is normal only in iron and steel which has undergone not more than a very slight amount of previous plastic deformation. The bands are characteristic of shock conditions.

Failures in Boiler Drums. - These failures, which are rather out of the run of normal experience, involved the tube plates of the steam drums of admiralty pattern water tube boilers. These boilers had seen twenty years intermittent service and the failures all occurred in the early days of the war while they were being refitted in the colder winter months at an ambient temperature of about 40 degrees F. Fracture accompanied by a very loud report occurred while old tubes were being hammered out and comprised a split running athwartships across the base of the drum from a tube hole on the right side to a tube hole on the left side. (See Figure 2.) The split opened up a few thousandths of an inch suggesting a fair amount of internal stress in the tube plate and the appearance of a typical fracture is given in Figure 3. This shows the characteristics of a typical cleavage fracture and the origin of rupture at the tube hole.

Detailed examination of the several instances encountered showed that the origin of the fracture was at the base of

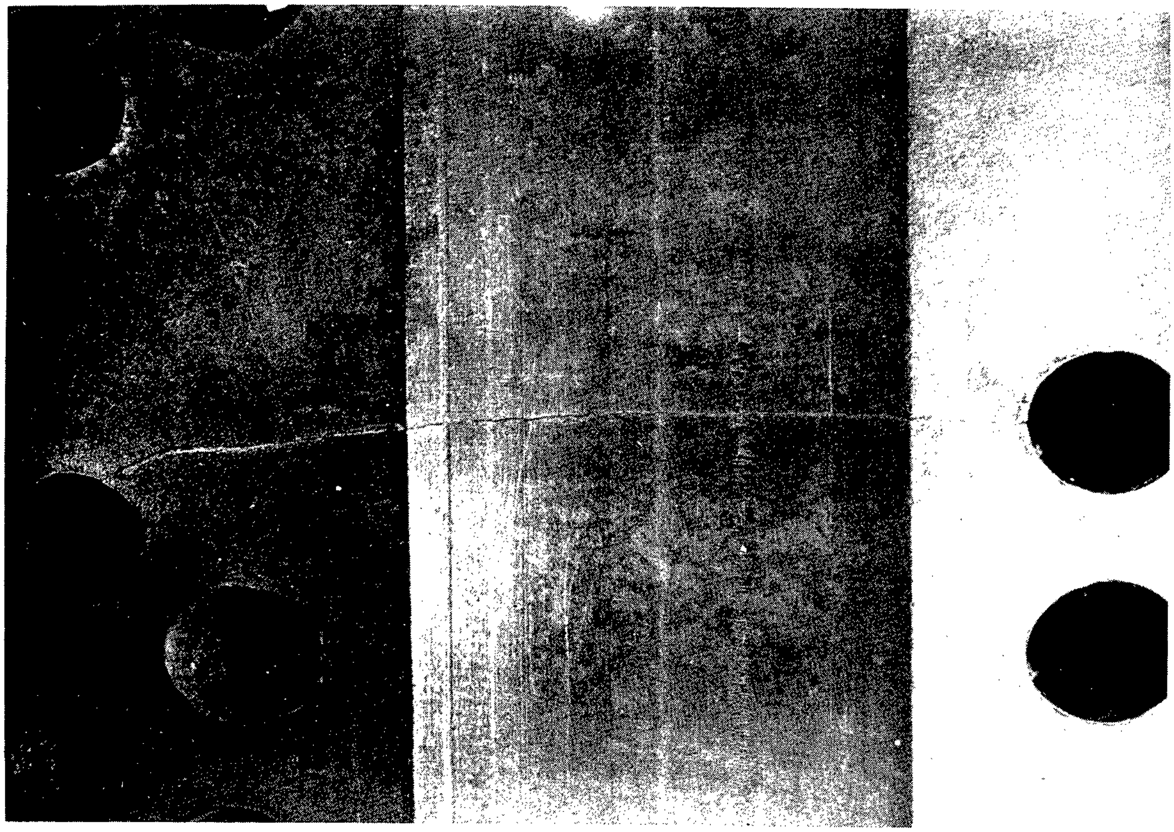


Figure 2. Base of tube plate in boiler drum showing crack athwartships between tube holes ($\frac{1}{2}$ x)

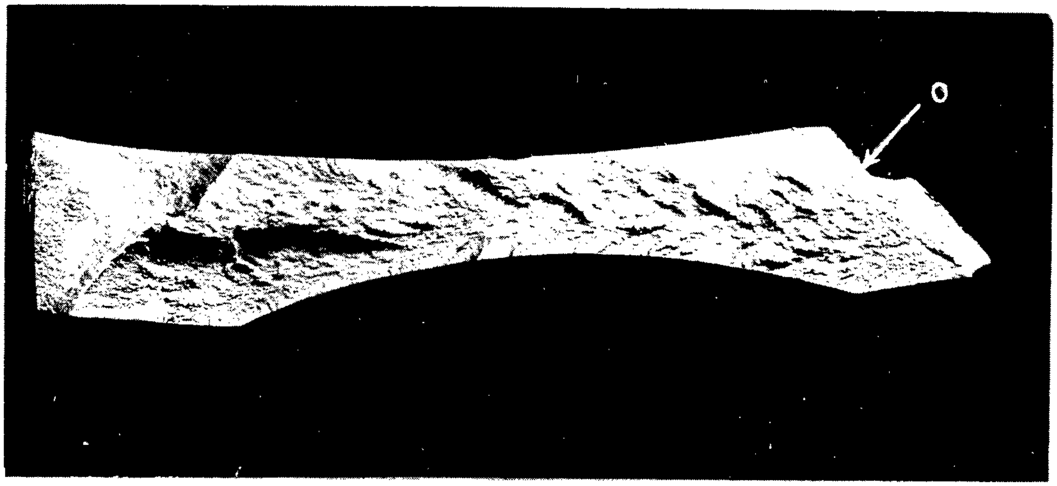


Figure 3. Appearance of fractured surface: note arrowhead markings pointing to origin of fracture O ($\frac{1}{2}$ x)

a corrosion pit in the arboured tube hole as illustrated in Figure 4.

Figure 4. Photomicrograph of area including origin of fracture at root of corrosion pit (100 x)

Such corrosion pits are of common occurrence in older drums and exist often in the form of small grooves radiating from the tube hole as shown in Figure 5.

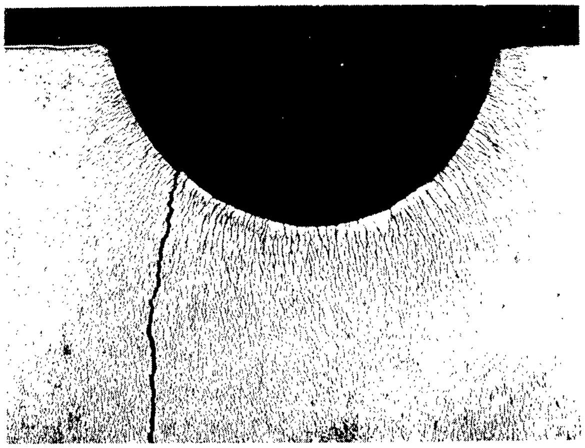


Figure 5. Outer surface of tube plate showing corrosion grooves radiating from tube hole (2 x)

Hardness surveys of a cross section embracing the tube hole are given in Figure 6.

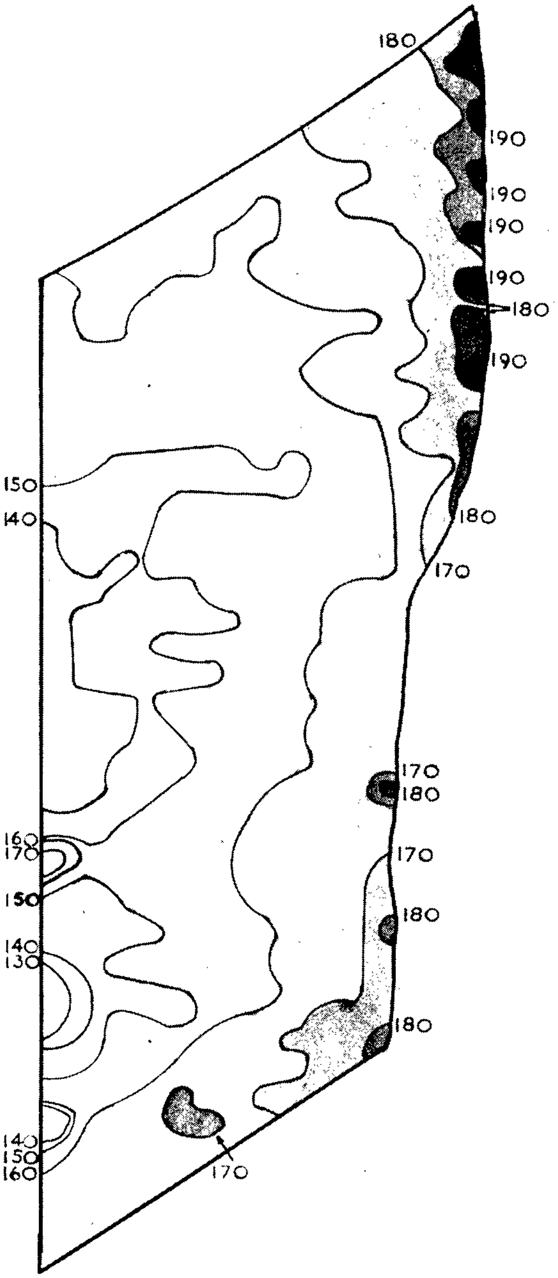


Figure 6. Hardness surveys of a cross-section embracing the tube hole

This clearly demonstrates the intense work hardening which has occurred at the surface consequent on initial machining and in tubing and detubing operations. The steels used for these tube plates are of the inherent coarse grained type and controlled strain age tests show them to become very brittle at hardness levels of the magnitude shown in Figure 6. Further relevant data on the composition and mechanical properties of the steels concerned in five typical failures are given in Table I.

TABLE I

		Boiler Tube Plate No.				
		1	2	3	4	5
Composition %	C	0.16		0.15	0.16	
	Si	0.08		0.29	0.015	
	Mn	0.53		0.54	0.71	
	S	0.07		0.06	0.04	
	P	0.06		0.06	0.05	
	N				0.006	0.008
Yield: tons per square inch		17.2	16.4	15.0	19.4	20.8
U.T.S. tons per square inch		26.8	27.8	25.1	27.1	27
Elong. %		36	35	34.3	34	25.5
R. of A. %		63.5	57.3	59	55	52
Izod Impact ft/lbs - Longitudinal		8.5	7.5; 6.0	3.4; 10.0	35; 60.5	32; 22; 18
- Transverse		11.0; 6.5 20.0; 9.0 25.0	8.5; 13.0 7.0	10.0 19.5; 18	19	

The above tests were made at room temperature (approx. 60 degrees F).

Further test data of interest relates to slow speed notch bend tests made on the steel from tube plate No. 4. Test pieces were taken:

1). from the body of the plate remote from the surface;

2). similar to (1) but after strain aging (12 percent strain and aged ½ hour at 250 degrees C - a tensile test piece was employed for this and subsequently machined into a notch bend test piece.);

3). from the surface of the plate.

The load extension curves obtained are given in the graphs in Figure 7 which include calculated data of the energies to initiate the first crack (Ec) and the total energy to fracture (Et). The energy to initiate cracking is the same in all cases but the energy absorbed in propagating the crack is substantially different. Of singular interest is the graph

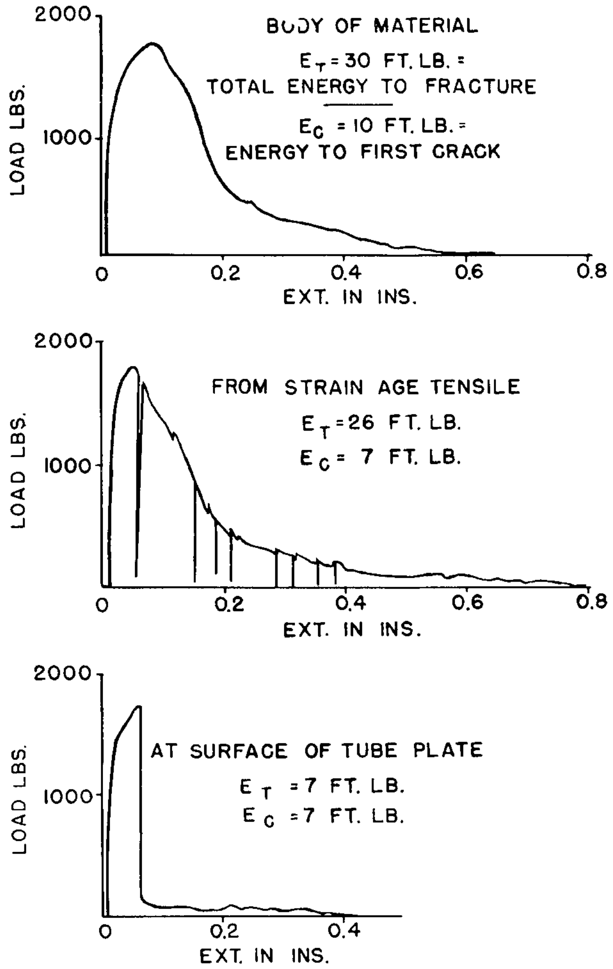


Figure 7. Load/extension curves of notched bend test pieces cut from tube plate

for the strain aged test piece which threatened initially to fracture in much the same way as the brittle specimen taken at the surface of the tube plate. Subsequent microscopic examination revealed the fact that the easy path of fracture had been diverted by a number of large silicate inclusions. It may well be that other microstructural features will influence the path of fracture and energy absorption in a similar manner.

Significant features emerging from the foregoing which are also evidently common to many failures in ship plates include:

1). the presence of a stress raiser of appreciable magnitude
2). a "susceptible" steel,
3). suitable temperature conditions,
4). possibility of locked-up stresses,
5). an "energizer" to set off rupture.

The "energizer" in the foregoing cases was the shock of the blows of the boilermaker's hammer in driving out tubes. Such blows in a hollow drum might well be associated with resonant effects in the tube plate giving rise to the build up of stress of very considerable magnitude at local areas.

Failures in Other Components.- Other instances of failures involving cleavage fractures which merit attention are given in the succeeding paragraphs.

During fitting in cold weather a large, mild steel, super-heater header forging fell on to a hard deck from a height of several feet and ruptured violently across a diameter (see Figure 8).

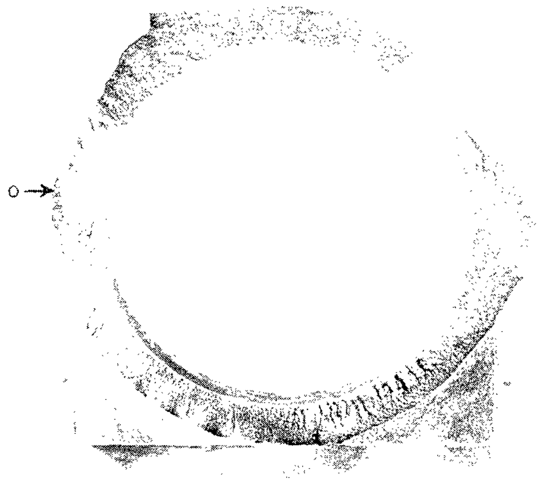


Figure 8. Fractured surface of superheater header forging

The origin of the fracture coincided with a sharp corner formed by a machining operation and also coincident with this, a large sulphide segregate was present in the steel. These two features evidently acted as a stress raiser of considerable magnitude.

The rupture of chain links particularly in cold weather is frequently associated with brittle fracture and the instance of two heavy hauling chains which fractured during "normal" usage can be usefully cited. Links were manufactured from U shaped bars by flashbutt or resistance welding and some of the flash had been hammered over, (see Figure 9) thereby producing a serious defect. Additionally, the material was a low carbon rimming steel which had been formed to U shape by cold bending with the result that the steel had embrittled in consequence of strain aging.

Figure 9. Photomicrograph showing defect on outer side of chain link and cold work (100 x)

Mild steel studs used for various fittings in ships often fracture in a brittle manner particularly during refitting operations in cold weather. In many cases examined, strain age embrittlement has occurred consequent on excessive tightening up or excessive loading in service. A typical example of the evidence of plastic straining as revealed by etching for Luders lines in a fractured stud is given in Figure 10.

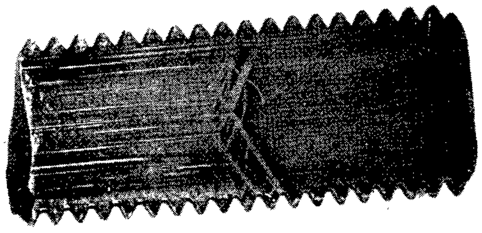


Figure 10. Longitudinal section through stud etched to show Luders lines. Fracture at F (I x)

The notched and embrittled bolt, when cold, readily succumbs to a rapidly applied stress such as a jerk or a hammer blow on the end of a spanner.

ENUNCIATION OF FACTORS WHICH ENHANCE LIABILITY TO BRITTLENESS IN STEELS

Three factors appear to be predominant, namely, the nature of the stress raiser, the energizer to initiate fracture and the relative susceptibility of the steel. The first of these may originate in some form of surface defect either in the original material or arising from the method of fabrication employed. Additionally, lack of perception in design must not be overlooked. The energizer to initiate fracture is less easily analyzed. Temperature appears all important in revealing the relative susceptibility of a steel to brittle fracture and it will be useful to classify "steel" factors which may be significant into:

I). Inherent factors

a). Chemical composition including oxygen and nitrogen content

b). Deoxidation practice in steel making c). Inherent grain size

2). Structural Factors

a). State of heat treatment including micro and macrostructural features
b). Non-metallic inclusions, their nature, size and distribution.
c). Surface condition
d). Cold work

Our knowledge on the relative significance of all these factors is far from complete, but most catastrophic failures encountered have been in inherently coarse grained steels, which are known to have relatively high transition temperatures. Such steels, additionally, are susceptible to marked strain age embrittlement so that fortuitous local damage to the structure will result in the production of severely embrittled zones. Unfortunately, the inherently fine grained steels (involving a piping ingot) are much more expensive to produce or otherwise their extensive use for many structures or components would be apparent.

From a more fundamental viewpoint, a major issue which requires evaluation is whether the relative susceptibility of a steel depends upon some property of the ferrite or whether it is associated with the nature and state of aggregation of the pearlite. Brittle fractures probably occur along the 100 planes, whereas plastic deformation takes place along the 110 and 112 planes in soft iron. This observation may well support the contention that the nature of the ferrite crystals is the responsible factor. More data on the crystallography of fractures is required and there is an evident need for skillful experimentation in this connection by X-ray diffraction or other methods. If the pearlite is significant, it is necessary to determine to what extent the transition temperature is dependent on the carbon content and the spacing of the pearlite lamellae. Work on these more fundamental aspects is in progress in England.

FRACTURE RESULTING FROM VIBRATION

The term "fatigue" in its application to metals has long since been so familiar as to merit but little description. The capacity of a structure to withstand repeatedly applied stresses without fracture depends upon a number of factors, most important of which are the magnitude and nature of the stresses applied, the number of repetitions of stress, the surface condition at the area of maximum loading and environment. Comprehensive data exist on the endurance limits of many of our more common materials of construction when tested in the form of conventional test pieces and it will be useful to summarize the methods of expressing experimental results in Figures 11 and 12. Figure 11 shows the relationship between the number of reversals of stress and the range of stress in cases where the stress is completely reversed in each cycle. In many cases of machine design, varying stresses are encountered which are not completely reversed. Endurance limits under these conditions are illustrated in Figure 12.

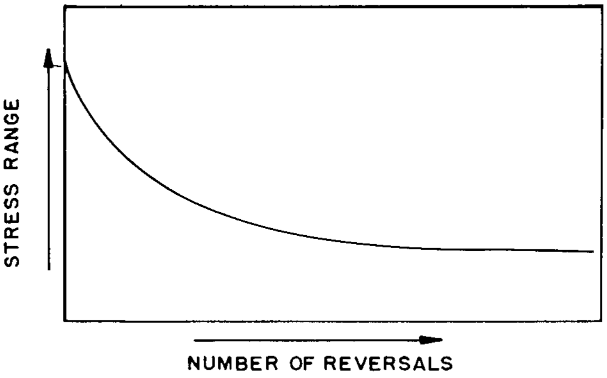


Figure 11. Relationship between number of reversals of stress and range of stress

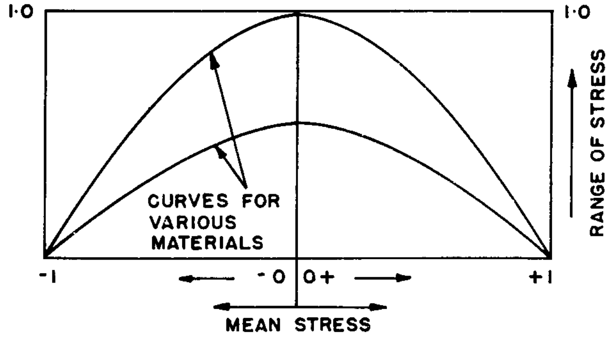


Figure 12. .Relationship between range of stress and mean stress

In practical problems, the stressing system may involve other than the uni-axial conditions which have been illustrated in the foregoing and it is important to know endurance limits under combined stress conditions. A simple case is that of alternating torsion where tests have indicated the endurance limit to be of the order of half that of alternating tension - compression.

Many design and material factors must be taken into account in evaluating probable endurance limits. Limitations due to irregularities in materials and methods of construction have been outlined above in "Irregularities Of Materials And Metnods" p.47, other factors in the following:

1). Frequency of Repetitions of Stress. - Experimental evidence indicates that frequencies up to say 20,000 cycles per minute have little effect on endurance limits. At higher frequencies, a substantial increase in endurance limit may be shown and a figure of 30 percent increase has been quoted for iron and aluminum at frequencies of 1,000,000 cycles per second.

2). Temperature. - In a general way, endurance limits fall off with increase in temperature in a rather similar manner to other mechanical properties. Steels usually show slightly better endurance limits at 300 to 400 degrees C. than at room temperatures but with a rather,rapid fall-off at higher temperatures.

3). Cold Work. - Cold work within limits, increases the endurance limit of many materials, steels showing a greater increment than non-ferrous alloys as a rule. Shot peening by virtue of its surface hardening effects, can have a significant effect in increasing the endurance limit in springs. Excessive cold work may result in a decrease in endurance limit.

4). Overstressing and Understressing. - There appears to be a limiting number of cycles of overstress, depending upon the magnitude of overstress, above which the endurance limit is decreased. A so-called damage curve can be produced correlating the maximum stresses of the cycles of overstress and the limiting numbers of these cycles.

In some materials, notably mild steel, it is possible to raise the endurance limit by understressing in the first instance and gradually increasing the magnitude of the stress.

5). Residual Stresses. - In some cases, the application of cycles of reversed stresses results in a very substantial reduction in the magnitude of the residual stresses and the effect on the endurance limit is negligible. Such effects have been noted in welded components and in heat treated steel specimens.

6). Surface Finish and Stress Concentrations. - Machine tool marks can have a very substantial effect in reducing the endurance limit and in the case of mild steel, the reduction may amount to 20 percent or more. Stress concentrations consequent on the configuration of the component may produce marked reduction in endurance limit as is evidenced by the frequent failure of machine components by fatigue at poorly radiused fillets and the like. The relative sensitivity of a ma-

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CALIBRATION METHODS FOR VIBRATION PICKUPS

By

S. P. Thompson

A survey of methods for calibrating vibration pickups has been made in the literature. The methods found have been classified, briefly described, and evaluated. References are appended.

Of the various phases of Navy shock and vibration work, none is of greater fundamental importance than the calibration of the measuring instruments employed in experimental investigations. Unfortunately, it is difficult to obtain information on pickup calibration in any one place in the literature. This paper, the result of a survey of the literature available at NRL, is an attempt to compile, classify, and compare methods in current use. The methods discussed here were devised for the calibration of electromechanical pickups, but in most cases are capable of being used on devices whose outputs are not electrical.

Most useful pickups are linear, i.e., the output is proportional to the input at a given frequency. The complete calibration of such a device is specified by two functions of frequency, one of amplitude of electrical output for unit amplitude mechanical input, the other of phase of electrical output for a mechanical input of a specified reference phase. Most of the methods discussed yield no phase information and are hence incomplete. In many cases, phase information is of little consequence, however, and the incompleteness of the calibration does not detract from its utility. In any case, the calibration of course holds for the motion of measured system when loaded by the pickup. Because mechanical "media" are nonhomogeneous and nonisotropic it is impossible to correct the calibration for the disturbing effect of the measuring instrument itself.

If one adequately calibrated device were available, others might be calibrated by comparison with it by methods which are obvious extensions of those to be described. The problem of calibrating a comparison standard in absolute units has therefore occupied the attention of most vibration instrument workers. The survey at NRL has disclosed four fundamentally different approaches to the problem.

1. Calculation

2. Measurement of a mechanical quantity, of an electrical quantity, and the use of some assumption about linearity of response versus frequency.

3. Measurement of a mechanical quantity, of an electrical quantity, of a frequency, and the use of a sinusoidal exciting device to produce vibration of the pickup.

4. Reciprocity.

1. The simplest method of obtaining an absolute calibration involves calculation of the response of the device from known or measured parameters describing its behavior. Accelerometers are the transducers most often treated in this manner, although King (1)* mentions use of this method to determine the response of velo-

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Numerals in parens refer to references at the conclusion of this paper.

city and displacement units. Calculation of the response of a crystal accelerometer from the loading mass and the crystal properties is described briefly by Fehr (2) and in more detail by McCarthy (3). The latter compared the calibration obtained from calculations using a statically determined value of crystal constant with that obtained by a dynamic method to be discussed later, and concluded that experimental difficulties prohibit realization of an accurate calibration by calculations based on the properties of a crystal accelerometer. His conclusion agrees with the general observations of King (1), that while calculation methods yield useful predictions of approximate behavior, they are no substitute for detailed experimental absolute determination.

2. If an instrument is approximately flat with frequency for some mechanical variable, it suffices to determine its response at one or more frequencies to obtain a calibration over the spectrum, valid to the approximation made. In this method, a mechanical quantity and an electrical quantity are measured; the frequencies involved are usually known.

Strain gages in particular are usually calibrated by this method. Roberts (4) describes devices and techniques for such measurements, and comments particularly on the inaccuracies of methods employing beams in static flexure, preferring the use of calibrating mechanisms whose members are assumed to be rigid.

Accelerometers may be treated by static methods wherein a force deflecting the compliance is applied by either gravitational or inertial action on the seismic mass. Gravitational forces were used by Shrader (5) and by Ramberg (6) in this way. Centrifuges are commonly in use at NOL (7), and were used by Shrader (5) to provide high inertial forces. Static methods are inapplicable to crystal units because of leakage. Furthermore such methods suffer from two fundamental disadvantages:

a) The action of damping does not enter the calibration experiment, and

b) The mechanical impedance of the seismic system does not enter the calibration experiment, merely the resonant frequency.

Measurements may be made simultaneously over a band of frequencies instead of one at a time by a method due to Vigness (8) which has been applied to the calibration of crystal accelerometers. A hammer pendulum is allowed to strike a pendulous anvil to which is attached the unit to be calibrated. The resulting linear velocity of the anvil is related through the calibration constant to the integral of the accelerometer voltage pulse. This method is free from the objections common to static determinations, and yields calibrations in reasonable agreement with those obtained by methods to be described.

3. The methods already described depend either on a knowledge of the internal construction of the device to be calibrated, or on a knowledge of the behavior of the device with frequency. Methods free from such restrictions are generally considered more accurate. The most commonly used methods employ a sinusoidal exciting device to vibrate the pickup, and a mechanical quantity, an electrical quantity, and a frequency are measured. The measurement of frequency is capable of much greater precision than is necessary. The measurement of the required mechanical quantity is performed in either of two ways:

a) The exciting device is constructed in such a manner that the amplitude of displacement of the pickup may be calculated from static mechanical measurements made on the exciting device. One of the simplest of such devices is the plucked cantilever beam used by Vigness (8) to produce a slightly damped vibration of initial displacement amplitude equal to the static displacement of plucking. A highly refined device in general use is the Sperry calibrator, described by Draper, Bentley, and Willis (9). The writers consulted agree that exciting devices of calculable amplitude are confined to use at low frequencies. King (1) mentions an upper limit of 25 cycles at a single amplitude of 0.25 inches, giving an acceleration of 16 g., and the Glenn L. Martin Company (10) an upper limit of 40 cycles at a single amplitude of 0.5 inches, giving an acceleration of 90g. Baumzweiger (11) has used a device producing an amplitude of displacement of 0.5 inches up to 30 cycles for the calibration of crystal transducer, and Scott (12) reports that a similar device is used for the calibration of the General Radio vibration meter.

Roberts (4) discusses the precautions to be observed in the use of these devices, and particularly points out the the danger of resonances internal to the devices at higher frequencies.

b) The required mechanical quantity is measured dynamically, and is usually the amplitude of displacement at the point of attachment of the pickup. Usually the mechanical quantity is measured optically, but Shrader (5) describes a mechanically resonant system consisting of a plucked beam in flexure to which is attached a stylus marking a waxed paper strip driven at constant speed. Shrader's remarks on the suppression of higher modes of vibration in a plucked beam are appropriate both to this method and to the plucked beam of Vigness described in (a). Roberts (4) discusses the use of exciting devices arranged to be internally resonant at the frequency of measurement, but gives no details of recommended optical systems. Madvell (13), McCarthy and Wiggins (14), Hull (15), and Baumzweiger (11) report optical measurement of displacement amplitude, but do not describe the technique employed.

The traveling microscope is the most frequently used device for the measurement of displacement amplitude. King (1) and McCarthy (3) report its use, the latter with intermittent illumination of the point of observation by means of a strobotac. Fehr (2) concludes that the upper frequency limit using a microscope is around 1000 cycles and 300 g., because of the upper acceleration limit of vibration motors and the lower limit of accurate displacement measurements performed with a traveling microscope. Vigness (8) has used a microscope to observe the motion of a free-free bar in flexure, the motion of the bar being maintained either by electromagnetic or compressed air excitation coupled loosely. Accelerations of 500g. at 900 cycles were obtained by this means.

The calibration methods already described, employing a sinusoidal exciting device and involving measurement of amplitude of displacement dynamically, are open to the fewest theoretical objections, since no assumptions about the internal construction of either the pickup or the exciting device are necessary. Unless refined methods are used, however, experimental difficulties in the accurate dynamical measurement of displacement amplitude may lead to a calibration of low accuracy at higher frequencies. Stansfield (16) describes the use of movable electrical contacts adjusted to just fail to close a detecting circuit at each extreme of displacement.

Most of the methods discussed in the literature are, however, optical. Hunt (17) discusses the problem of optical magnification to increase displacement sensitivity, and points out the undesirability of ordinary magnifying systems which increase the dimensions of the point of observation by the same factor of multiplacation as the displacement. By the use of prisms, he has devised a method free from this difficulty.

A simpler optical system achieving greater displacement magnification is described by King (1) in which a short arm, carrying a small mirror whose surface is normal to the arm, is pivoted at its ends on a fixed point and on the point whose displacement amplitude is to be measured. Light from a small source is reflected from the mirror and focused on a scale. A magnification of displacement of 1000 is reported to be easily obtainable, but no information is given on the experimental difficulties which one might expect at higher frequencies.

A third refined optical method is described by Vigness (8), who used it in conjunction with a free-free bar in flexure, A lens of short focal length attached to the mechanical point of observation forms a real image of a distant source. This image is reduced in size and has an amplitude of displacement very nearly equal to that of the lens. A second real image, using the first as a source, is formed on a distant screen by a similar lens attached to a motionless point. A magnification of displacement of 1000 is reported possible.

4. Simultaneous use of resonant exciting and refined optical measuring methods extend accurate but incomplete calibrations to higher frequencies. Although ways can be devised to extend these methods to obtain the phase calibration as well as that in amplitude, experimental difficulties have apparently precluded the realization of complete calibrations. A method yielding an inherently complete calibration has been devised by Trent (18). His method is based upon the existence of reciprocity between the receiving and driving sensitivities of a linear and bilateral electromechanical transducer and involves only one mechanical measurement, that of mass. The method does not lend itself easily to the direct calibration of field instruments but affords an absolute calibration of primary standards by comparison with which field instruments may be completely calibrated. There is no inherent restriction on the upper frequency limit of application of the method. A program to verify the theory involved and to discover and minimize unanticipated experimental difficulties is now in progress at NRL.

Information on calibration methods not mentioned in this paper will be appreciated by the writer, and may be sent to the Shock & Vibration Section, Sound Division, Code 475, NRL.

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COMMENTS

. . . FROM THE EDITOR

Immediately after the sixth symposium, a member in attendance remarked, "My first reaction upon receipt of the program was that the Agenda Committee had gone a bit astray from the field of shock and vibration. However, out of curiosity I decided to attend. Now I am very glad that I did, for it was one of the most interesting symposia thus far." This reaction prompted a request for comments from other personnel in attendance. These observations are printed on the next page.

It is the aim of the Committee to present interesting as well as balanced agenda; to focus the attention of the group upon the current and urgent problems; and to provoke thinking and discussion which will benefit the entire program of shock and vibration. To be sure that future symposia are in line with the ideas of the majority in attendance, the Agenda Committee has provided this column "Comments to the Editor." Its purpose is to express opinions regarding the Symposia and the "Shock and Vibration Bulletin" in an informal and unofficial manner.

What are the broad areas in the shock and vibration field which should be explored for the maximum benefit of the Service? Before attempting to answer this question, it is desirable to restate the original aims and objectives of our mission. Our ultimate goal has been, and still is, to supply adequate information to the designer and engineer who develop structures, machines and equipments for the Armed Forces. With modern speeds and mobility, technicians are confronted with shock and vibration problems which were not encountered prior to World War II. Hence, the designer and constructor demand scientific quantitative data--not intuitive guesses--for their plans in future vehicles. In addition, because more and different types of vehicles have become strategically important, the subject of shock and vibration has permeated and expanded into many fields of technology. That means a much wider interest in the causes and effects of shock and vibration, as well as in the methods and means for overcoming these disturbances.

The responsibility for collecting and disseminating this information has been delegated to the Shock and Vibration Centralizing Unit. At the outset, our concern was mainly with shipboard equipments and their evaluations; and, a great deal of technical effort has been concentrated upon propulsion machinery and electronic devices. Indeed, even now a major portion of the shock and vibration problems are centered about an operating ship. However, with the advent of high-speed projectiles, it becomes necessary to solve shock and vibration problems which hitherto seemed hardly within the realm of our subject. That is why Agenda Committee scheduled metallurgy, guided missiles and dynamic stability as part of the last symposium. It contemplates further programs along these lines in the future. By a "Comments to the Editor," you can advise and help orient the Agenda Committee in a proper and profitable direction. Please do!

. . . TO THE EDITOR

SYMPOSIA

"Having been associated with test work for a number of years, Dr. Slater's remarks were not only informative but, also, disturbing. They make us think that perhaps we should review and revise our evaluations and test techniques in the light of the characteristics of materials which the author described."

-- NRL

"The subject of impact and penetration of water was interesting, particularly the instrumentations used in the work. .... It seems to me that the greater portion of the talks should be centered around the most recent developments in techniques, with a much shorter period devoted to general material. The symposium is functioning as a clearing house in shock and vibration. It is not to end with just a few papers presented at regular intervals, but the organization should function by dissemination of information on a variety of problems in the field."

-- TMB

"The Shock and Vibration Symposia have succeeded well in acquainting personnel associated with the Navy with the shock and vibration facilities at various Naval laboratories and in publishing and distributing this information. They are at the present time reviewing many of the branches of shock and vibration together with bringing out plans of future work. It is probable that future meetings will bear more on the theory and specific problems involved in shock and vibration work.

"The stress of desiring to cover many different fields of endeavor within a short time has made it necessary that the symposia meet at quite frequent intervals. It is probable that after the principal subjects have been covered, it would be better to increase the time between meetings so as to have about four per year. It might also be suggested that the meeting agenda be prepared at a time more in advance of the meeting date, so that an abstract or manuscript from the various speakers could be submitted to experts in that field for prepared discussions."

--BuOrd

The Shock and Vibration Symposia have been of great interest to this code. Presentation at the symposia of synopsis of such works as the Cameron Reports has been most helpful to an understanding of the shock problem. The Sixth Symposium was most informative especially with respect to the metallurgical aspects of shock presented by the British representative, Dr. Slater, and the damage data discussed by the Watertown Arsenal personnel.

--BuShips

"............... As would be expected, those reports or parts of reports most valuable to me were the ones which I could relate to some previous theoretical knowledge or practical experience of my own. .... There was a tendency to include too many details, making some reports too long and tiring. Also, certain reports were too general, with insufficient quantitative information."

--TMB

"I would like to suggest that all future symposia contain at least one paper from an allied field which has a direct bearing upon our work. For example, it would be desirable to get information similar to the metallurgical factors presented by Dr. Slater on the properties of plastics, rubber and other materials used in the construction of ships, aircraft, weapons and missiles. In addition, as often as is possible talks should be included on rockets, guided missiles and jet propulsion with regard to expected accelerations and frequency of vibrations."

-- NRL

I have attended each of the six Shock and Vibration Symposia held to date and have read and circulated the four Shock and Vibration Bulletins. I believe that the series has been quite profitable to all concerned and should continue. The symposia provide a convenient means for disseminating technical information on shock and vibration to interested activities and at the same time permits interested personnel to exchange ideas and comments in a relatively direct and informal manner. The short inspection trips to observe shock and vibration test equipment in operation were especially educational.

-- BuShips

"Dr. Wayland, Dr. Slater and Mr. Marlowe each presented what I think is a good paper. The background of each problem was presented, their objectives were outlined, and their successes, failures and unfinished work were given, with enough detail so that the audience had an appreciation of their difficulties and yet not so much detail as to make it difficult to follow."

-- TMB

"Let us try to cover the broad fields of shock and vibration so that those whose work restricts them to some narrow portion of the field will have an opportunity to see and hear what is going on in the other branches and in allied fields. This is one way to keep abreast of the times and prepare us for future events."

-- NRL

"The Shock and Vibration Symposia are bringing out the diversity and the unity of problems in this field. They have awakened me to a deficiency in my grasp of the principles. Besides arousing an interest in learning more about the subject, the meetings have given me background for other work. For example, at the last Symposium I got ideas on sources of information on fracture of metals."

-- BuShips

"I think that the meetings should be held less frequently, and that the papers should represent new important developments in the field rather than background material. We should strive for meetings of the caliber of the physical society meetings."

-- NRL

"This section feels that the past Symposia have been very educational for all concerned and that people of all branches have become considerably enlightened on the subject of shock and its effects on vehicles, equipment and personnel. The Fifth Symposium held in June at TMB was particularly excellent.

"We, as well as many other sections in the Bureau, are particularly interested in shock effects on equipment aboard ship and to that end we feel that further emphasis should be placed on the problems involved and that one or two future Symposia should be devoted to the subject. Particular emphasis should be placed on proposed full scale tests on combatant vessels. The information desired, problems involved in obtaining the information and the general purpose for conducting the tests should be thoroughly covered.

"It is realized that this becomes more of a specialized problem than it was originally conceived to handle in a Symposium, but we propose it for the reason that it will bring to the minds of all concerned the types of problems involved in obtaining solutions to the "Shock Problem" which is more or less general for all branches of the Armed Services. Furthermore, it is felt that it will be an excellent opportunity to advertise the fact that full scale tests are in order and may result in considerable support for the project from people outside the Bureau of Ships. We need all the support we can possibly obtain and this would be a good chance to obtain it.

"It is felt that these suggestions in addition to those already outlined in the Bulletins and discussed at the Symposia will tend to make a well rounded program of work to be conducted in the future symposia."

-- BuShips

"Mr Marlowe's presentation of the stupendous problem of instrumentation in interior ballistics, that of trying to communicate with a projectile as it travels down a gun barrel, was challenging."

-- NRL

"I would like to hear more short papers on work being done on current problems at each institution participating. These papers should be in the nature of progress reports, so that each group would get a general idea of the problems and methods of attack for each participating activity for *current* problems. Recommended frequency of meetings -- every three months."

-- BuOrd

* * *

S & V BULLETIN

"The material is very informative and presents in useful form summaries of past and present developments for the benefit of people working in the field.

"It is felt the summaries provide information of value in the selection and planning of instrumentation for tests. The material is also of value in anticipated probable test results.

"On the whole, the approach is very good. In certain specific instances, it is thought more quantitative information could be shown.

"It has been suggested that abstracts of British reports might be added in subsequent bulletins.

-- NEL

"We have read each one of these bulletins with much interest. The tabulations of available vibration equipment were especially useful in our work. We have no suggestions for changes."

-- BuStds

INSTRUMENTATIONS IN DEMAND

Suitable instruments for measuring various shock and vibration phenomena are urgently needed. Accurate determinations are not only of scientific interest but are a practical necessity to the modern designer and constructor: therefore, the Committee invites all participating activities to outline their instrumentation problems. By stating clearly the deficiencies in existing instruments, needed improvements can be effected. If the response concerning instrumental problems warrants the scheduling of a Symposium on this subject in the near future, your Committee will formulate plans accordingly. Please use these columns in which to state your problems.

The Agenda Committee

SAVIBULL

GUN BLAST EFFECTS ON PERSONNEL

Comments by TMB

In discussing the paper on "Air Blast Research at the Taylor Model Basin" (see Page 19, S&V Bulletin No. 4) Comdr. Malim outlined the British experiences and viewpoints regarding the problems of establishing blast pressure tolerances for personnel in the vicinity of guns and rockets. He invited the comments of the Taylor Model Basin on this subject. The following remarks have been contributed by Mr. W. J. Sette, Mr. H. L. Rich and Mr. P. Tamarkin, all of whom have been actively participating in the study of gun blast problems for some time.

With regard to paragraph 1 of the comments by Comdr. Malim, Mr. Rich's statement concerning the 7 psi curves, established by use of Williams gages, may be explained by pointing out that the Williams gage is known not to give a true measure of the peak pressure, since its response is also governed by the duration of the blast pulse. In general, it may be said that as the true peak pressure and duration of the blast pulse increase, the Williams gage estimate of peak pressure becomes increasingly high, while for blast pulses with a duration of about five milliseconds or less, the Williams gage peak pressure indication may be somewhat low. A theoretical analysis of the Williams gage is given in an article entitled "Forced and Free Motion of a Mass on an Air Spring", by B. Sussholz, Journal of Applied Mechanics, June 1944.

The accompanying chart, taken from the report entitled "Survey of Research", U.S. Navy Gun Blast Committee CONF Interim Report of January 1946, consists of a series of seven psi personnel safety contours obtained from guns of various calibers by use of the Williams gage. These contours are drawn in full line. For the purpose of comparison there are included dashed contours, which were obtained from a 3-inch 50-caliber gun by use of the Taylor Model Basin diaphragm blast gages. To within 20 or 30 percent, the dashed contours should represent the pressure field of any of the guns listed. The Williams gage readings are too high for almost all of the guns.

From its experience with gun blast, the Taylor Model Basin concurs in the opinion stated in the summary of British experiences and viewpoints to the effect that peak pressure is not the only parameter to be taken into account in assessing the action of blast pressure on personnel. It is felt that the duration and impulse in a blast pulse are also of prime importance.

The Taylor Model Basin further concurs in the general thought expressed in the summary that this is a problem which should be given joint study by both physicists and physiologists to determine the fundamentals of the action of shock waves on personnel, and to establish tolerance blast pressure limits for personnel working

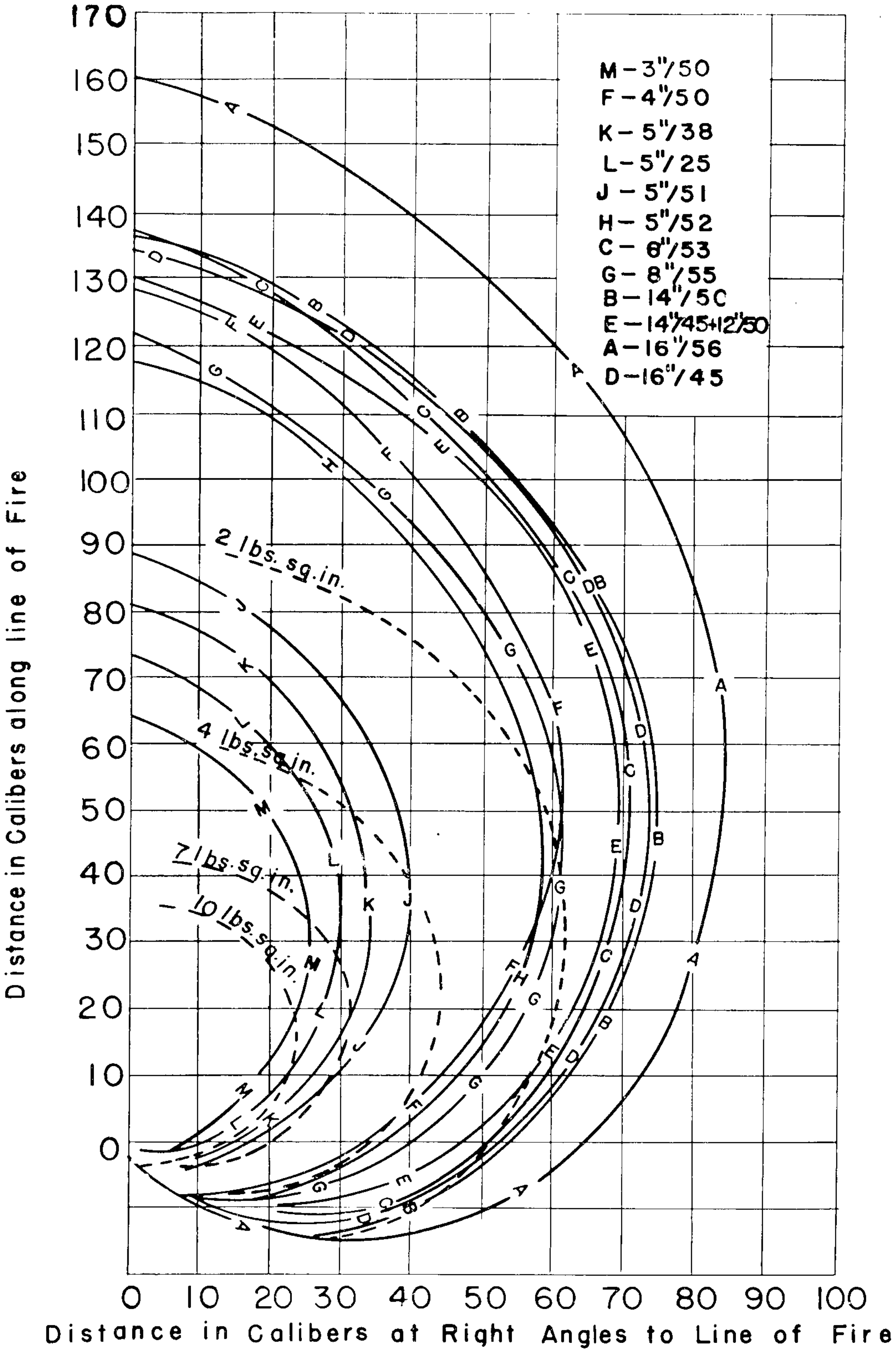

Chart Showing 7 psi Blast Pressure Contours with guns and rockets. Such a program, including arrangements for joint participation of United States and British investigators if desired, would presumably be sponsored by one of the Bureaus of the Navy Department or by the Office of Naval Research. In this connection, a "Gun Blast Committee" was established in 1945 through the efforts of the Navy Department's Coordinator of Research and Development (now the Chief of Naval Research), as an aid to the coordination of work in this field. However, the work of this Committee to date has been principally concerned with the effect of gun blast on structure rather than on personnel.