NASA CP-2003

FREE DRIFTING BUOYS

AIAA DRIFT BUOY SYMPOSIUM

ponsered by the American Institute of Aeronautics & Astronautics
echnical Committee on Marine Systems and Technologies

lampton, Va. May 22-23, 1974

National
Aeronautics and
Space
Administration

American Institute
of Aeronautics
& Astronautics

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The text of this publication was prepared from audio tapes of the symposium. The tapes were transcribed and a minimum of editing was preformed to retain the atmosphere and informal tone of the meeting and to best convey the thoughts developed during the symposium.

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Sponsored by AIAA Technical Committee on Marine Systems and Technologies

Hosted by
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FOREWORD

The purpose of this symposium was an exchange of information between people directly involved with the development, use, and/or potential use of free-drifting buoys. The AIAA Technical Committee on Marine Systems and Technologies sponsored the meeting which was hosted by the NASA Langley Research Center at Hampton, Virginia.

The symposium was well attended (See table 1.) with an excellent cross-section of free-drifting buoy developers and users represented. Panel members were invited to give prepared comments in their speciality area then the symposium attendees participated in open discussions. Invited panel members included persons from oceanographic institutions, academia, individual researchers, industry, and a number of government agencies.

Four sessions were held including:

TRACKING SYSTEMS AND TECHNIQUES - where methods and accuracy of optical, radio, radar, satellite, and sonic tracking of free-drifting buoys were discussed;

DEPLOYMENT AND RETRIEVAL - covering methods currently used or planned in the deployment and retrieval of free-drifting buoys from boats, ships, helicopters, fixed platforms, and fixed-wing aircraft;

SIMULATION, SENSORS, AND DATA - emphasizing the status of water circulation modeling, sensors useful on free-drifting buoys, and data display and analysis; and

RECENT EXPERIENCE AND PLANS - an exchange of experience and plans in the development and application of free-drifting buoys.

A formal publication of the proceedings was not originally planned, however, as the attendance and interest grew a decision was made to tape the proceedings. A transcript was prepared from the audiotapes made during the symposium. This report was formulated in a conversational style in order to retain the atmosphere and tone of the meeting and to best convey the thoughts developed during the symposium.

This publication does not include all of the material presented and discussed at the symposium. The information deleted included field operations movies, open discussions, and physical demonstrations of equipment and devices which were not suitable for printed proceedings.

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FREE DRIFT

BUOY SYMPOSIUM

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SESSION A

Tracking Systems and Techniques

Chairman: Sam Stevens, NASA Goddard Space Flight Center

THE STRIFFLER "TALKING DRIFT BOTTLE,"
A FREE-DRIFTING BUOY-LOCATION SYSTEM

by
Dean F. Bumpus

Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

For an immoderately long time I have tried to gather the data to provide a general description of the circulation over the continental shelf off the East Coast of the United States. I have used the thoroughly unsophisticated equipment, drift bottles, and seabed drifters. The elegant Eulerian equipment, Richardson current meters and their successors, were not available at the outset of my efforts. As a matter of fact they have begun to be employed only recently in shelf waters by students of the dynamics of the continental shelf, chiefly on the West Coast of Florida, off Oregon and Washington and south of Rhode Island. Sophisticated Lagrangian approaches have also only recently been fielded in shelf waters.

A general description of the East Coast shelf waters has been written and should be off the press most any time now in Progress in Oceanography, vol. 6. Some of you may have copies of the reprint. I'm not going to talk about that.

I want to mention the fact that the description I have produced is wholly inadequate for the evaluations which must be made about the advection of continental shelf waters in general and in specific areas.

We need to know what the forces and dynamics of the shelf are, so as to make appropriate predictions of the advection past offshore power plants, prospective drilling platforms, moored oil terminals, solid and liquid waste dumps, etc., as well as for the intellectual satisfaction of knowing what the processes are that govern the circulation on the shelf.

It appears to me that various geographical areas enjoy different sets of dynamical influences which make them unique from the others. The strong tidal oscillations over the abrupt topography of the Gulf of Maine , together with the dynamical effect of the river runoff, appear to be the controlling factors in the net or residual drift. The wind stress may significantly modify the net flow in winter. On the contrary, in the Middle Atlantic Bight, the dynamic gradient created by the river runoff appears to be the dominant influence on the net drift during the warm half of the year--only slightly modified by the wind stress. But, during the cold half of the year only, wind stress is responsible for large intermittent motions which result in less clearly defined net drift. The South Atlantic Bight represents a third situation in which the frictional drag of the Gulf Stream imparts considerable influence. Actually, meanders in the Gulf Stream riding up over the outer edge of the shelf cause wholesale intrusions and exchanges in the waters off North Carolina. The runnoff cycle imparts only a modest influence on the dynamic gradient and the changes in the seasonal wind stress may be responsible for the alternations in the net drift over the inner parts of the South Atlantic Bight.

This prologue is to remind you that while we are looking at the signals from Lagrangian sensors as we study the continental shelves, we must be alert to the other measurements which need to be made in order to derive some understanding of the dynamic interplay of the relevant forces at work in order to gain the ability to predict from these measurements.

It is pretty obvious to me that my drift bottles have been quite inadequate in providing a basis for an understanding of the shelf for a number of reasons. I'll mention only two of the inadequacies. We obtain extremely few returns from bottles launched during the cold half of the year. This is when the NW wind is predominant and an off-shore component is imparted to the surface drift. Is this drift only slightly different from that of other seasons but sufficiently off shore so bottles do not strand or is it more directly off shore? I don't know. The other major concern is that drift bottles give only an integrated net drift over a fairly long period of time, weeks to months, without details.

Consequently, I have from time to time made noises about the need for an expensive system which might be used, a talking drift bottle system.

We've had various experiences with items which would fit under this label. In the late 1950's we had buoys which responded to a radio signal on which we could DF with a ship or aircraft. The buoys were expensive in cost, $5000 - $6000 a piece, exorbitant in use of ship or aircraft time and only a limited number could be fielded and tracked without losing them.

We had some buoys which would read the Consolan signal and thus tell us their position; these were to be used in the Gulf Stream and beyond. The day we launched them the authorities turned the Consolan signal off. That taught us that one does not get anything for free and if you're depending on anybody else, you've got to maintain good communications with them. We apparently didn't learn this lession the first time. Several years later we launched a buoy on Georges Bank which was tracked by the IRLS satellite. It developed a minor electronic glitch and was lost temporarily. However, it was soon useless as the next IRLS satellite was not compatible with the black box on the buoy. We did not follow up on this one for several reasons: high unit cost($50,000 - $60,000 per buoy and electronics), limited number of buoys which could be interrogated in any one area, and the number of fixes which could be obtained were limited to two per day.

Doug Webb will tell you later of another system which does work.

Now as for the Striffler buoy tracking system: The important considerations in the design were accuracy of fixing, range to which buoy could be tracked, cost of equipment, and amount of ship time needed. I wish I could report that we have a system, tested and ready to go. Not so. So let me give you a general description of the system. The idea is to have buoys sound off at regular intervals, each at its own preset time. An amplitude-modulated HF signal derived from a stable 5-megahertz oscillator is received at each of three portable shore stations regulated by ultrastable oscillators. The relative phase difference of the signal as received at pairs of shore stations provides differences in range from the buoy to the shore stations, i.e., hyperbolic lines of position.

Figure 1 shows the block diagram of the buoy electronics for this system. The signal starts at the stable 5-mega-hertz oscillator. It is counted down to 2441 Hertz and is then used to amplitude-modulate the high frequency transmitter. This frequency is further counted down to provide the on-off signal which controls the transmitter. These buoys are timed multiplexed with an "on" period of 6.7 seconds and an "off" period of 44.8 minutes. This gives 400 separate time slots and over 30 position reports for each buoy per day.

The shore station has a similar stable oscillator and countdown chain and therefore, a similar 2441 Hertz signal. In the original concept, before each buoy is set out, it is synchronized to the shore station electronics, so that the two 2441 Hertz signals are in phase. Now, as the buoy is moved away from the shore station, the relative phase of these two signals increases as viewed at the receiver station, and this phase shift is directly proportional to the range.

The positive zero crossing of these two signals is used to set and reset a flip-flop which is used to gate a one megahertz signal to a counter. This gives the phase difference of the two 2441 Hertz signals in microseconds. Actually the grated one megahertz is counted over 1000 cycles of the 2441 Hertz signals so the printed time in microseconds is an average of 1000 periods. Additionally, this process is repeated four times during the 6.7 second "on" period of the buoy transmission. The receiver station also has logic circuits which select the correct time slots for the particular buoys which might be operating as well as the necessary formatting and printer control logic.

This method requires oscillators with very precise frequency stability, and the best crystal oscillators available have stabilities of 1 part per 10^{10}. We have determined the relative drift rate to the equivalent stability of 1 part per 10^{11}. The expression for relative phase shift versus time is given by

$$t = AT / BT^2$$

where t is the phase shift in microseconds;

T is the time over which the phase change is measured, in days;

A is the factor due to the initial frequency offset, in microseconds per day;

B is the factor due to the frequency drift, in microseconds per day^2.

Because this expression is a quadratic, the phase shift will quickly start to increase at an increasing rate, and along with this the phase shift uncertainty will increase. This method of locating would be limited to less than a 30-day mission using oscillators which are corrected to one part per 10^{11}.

There is an alternative! If the relative phase shift is measured at two shore stations and the difference between these two readings is obtained, we will have the difference in range from the buoy to the two shore stations, a hyperbolic line of position. This is exactly like the lines of positions associated with Loran navigation--the only difference being that the transmitter is on the moving vehicle and the receivers are at the fixed shore stations. Figure 2 shows the geometry of three stations that could cover the George Bank area and three lines of position intersecting at a location. We should expect position accuracies of ± 1 mile.

Thus, it develops that less expensive stable oscillators can be used on the buoys and the ultrastable oscillators are incorporated in the shore stations. These shore station oscillators need to be synchronized about once every 30 days, but even this may be eliminated by providing a low-frequency phase lock receiver at each shore station and synchronize each of the stable oscillators to the U.S. Navy low-frequency station at Cutler, Main.

We expected a range reliability of 250 nautical miles. We have achieved 60 nautical miles with an installation on a ship with inadequate antenna trimming.

The cost of each buoy including floatation package, electronics, battery pack for 90 days, and window shade drogue would be about $1500. The shore stations should cost out at less than $4000 each. The crystal clocks are the expensive parts.

The advantage of a system such as this is that a research vessel can set out the buoys, then go about its work, developing the hydrography, making plankton tows, monitoring the wind systems or what have you, without having to sheep-dog the buoys.

The people in the lab can advise the ship by radio as to the whereabouts of the buoys so they can readily find them when it's time to pick them up. Support for this effort has been provided by the NMFS and the National Data Buoy Center.

I'm sure during these two days we shall hear of other systems which are working and on line. I'm looking forward to hearing about them.

As you probably know there is a move under way to develop plans for IDOE-supported continental shelf dynamics studies on the West Coast of Florida and off Oregon and Washington. There will be a workshop in June in preparation for submitting a proposal at the end of the summer. This will provide an opportunity for a real intermural, multimethod attack on two quite different shelf systems. In addition to the multimethod approach, there will be the advantage that the continental shelf dynamicist will be most active in assisting in the design of the experiments. Hence, there should be an excellent opportunity to test hypothesis, evaluate the inadequacies, redesign, and retest. This will be a real feedback iterative system that should advance our understanding of the dynamics of these two areas. It is most encouraging to me to see so many people interested in determining in one way or another the circulation on the continental shelf. It was pretty lonely there for quite a while. There is a real need for a better understanding of the circulation processes and the forces which govern them.

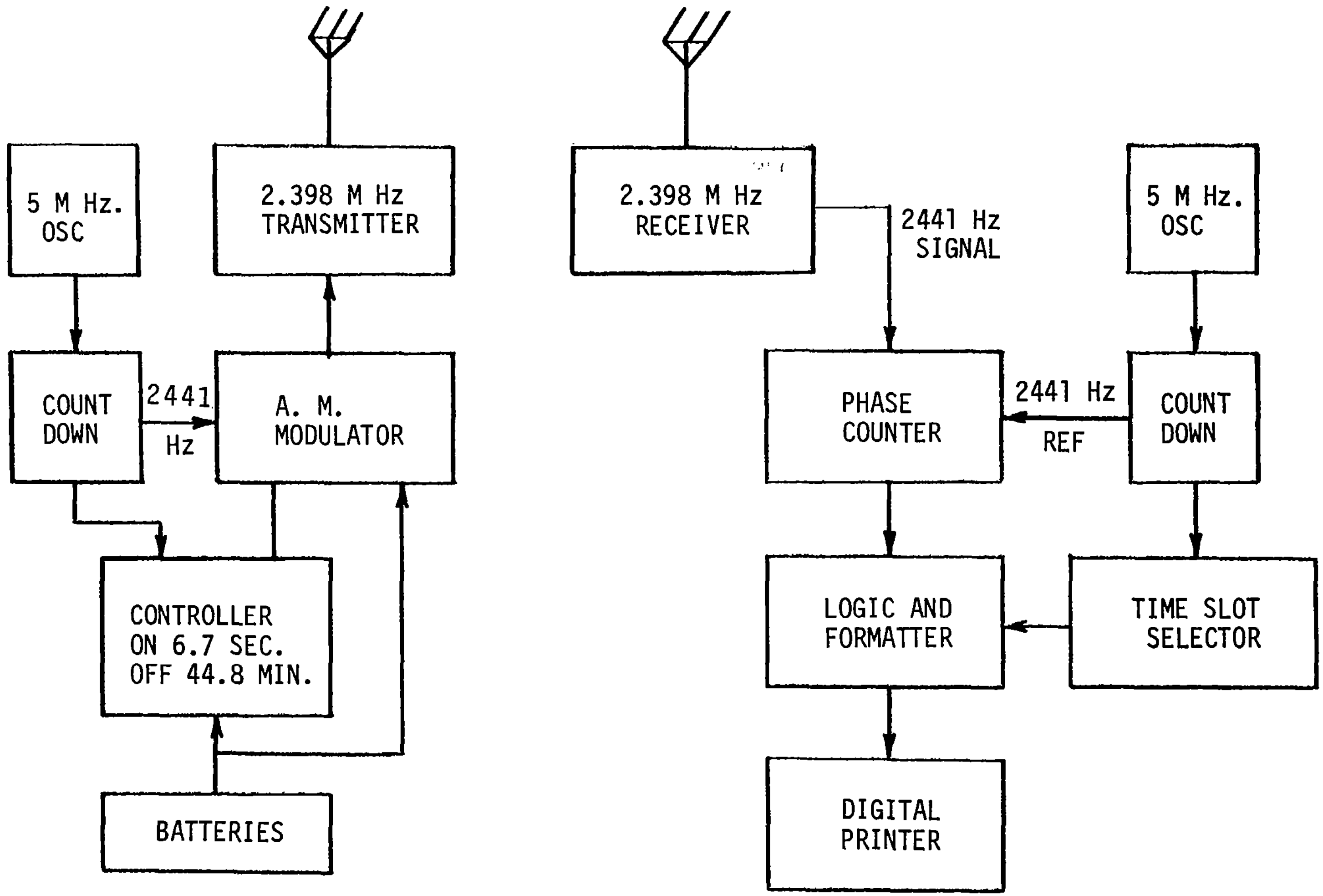


FIGURE 1. BLOCK DIAGRAM OF STRIFFLER BUOY ELECTRONICS

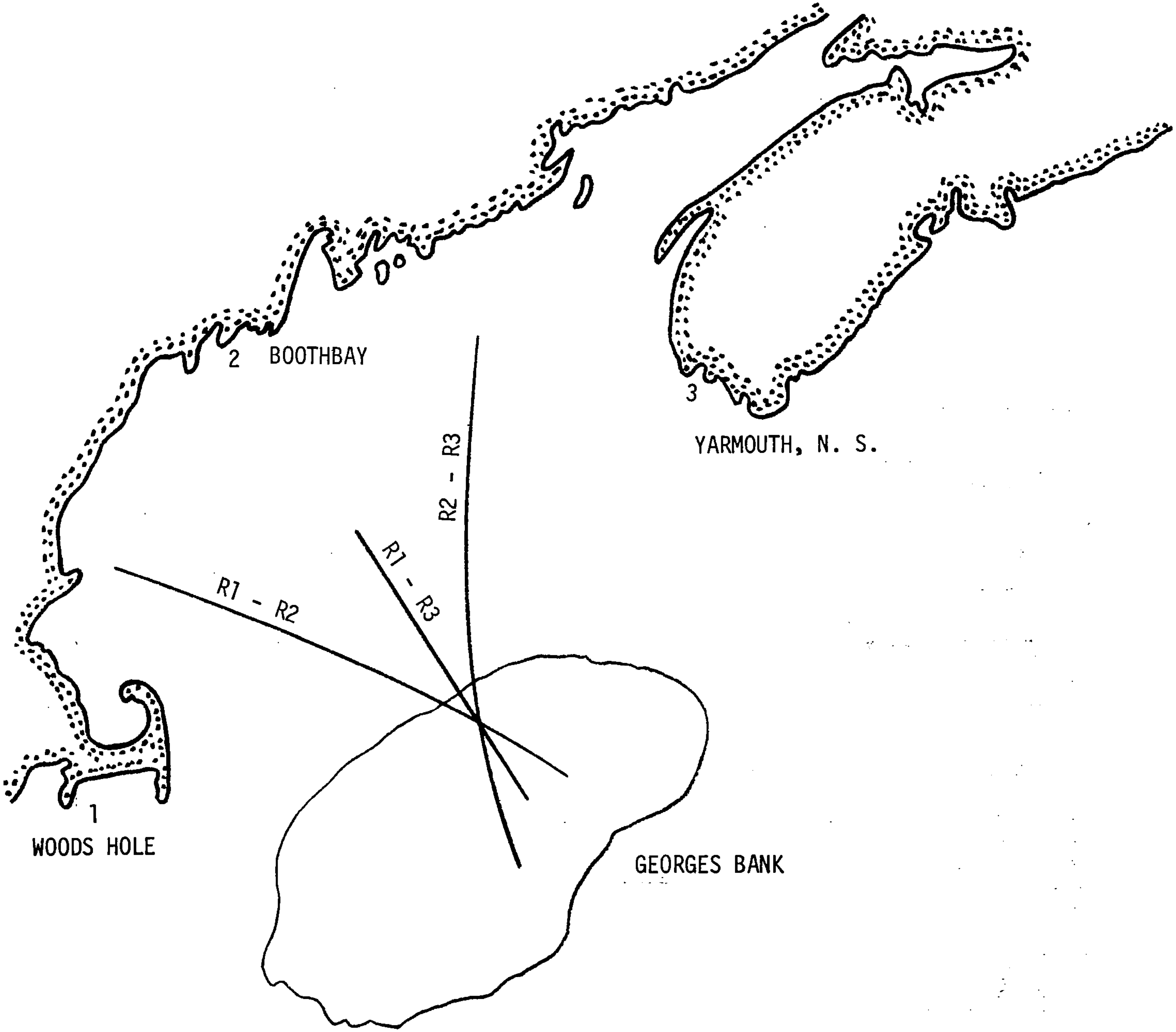


FIGURE 2. TYPICAL LINES OF POSITION OF THE STRIFFLER BUOY TRACKING SYSTEM FOR A POINT ON GEORGES BANK

BUOY TRACKING WITH OVER-THE-HORIZON (OTH) RADAR

by
Craig D. Werner
Barry Research Corporation

The application of Over-the-Horizon Backscatter (OTH-B) radar, for tracking drifting buoys and telemetering data to shore, is an alternative to satellite, fixed-frequency HF, and line-of-sight VHF techniques. An HF OTH radar can provide surface coverage of remote ocean areas, using a single shore-based monitoring facility, at relatively low cost in many cases.

The principle of operation of OTH radar is the same as that of conventional radar, except that the unique refracting property of the Earth's ionosphere in the high-frequency radio wave band is invoked. Conventional radars operating in the VHF, UHF, or microwave bands are strictly line-of-sight systems. With the HF radar however, RF electromagnetic waves, launched by the radar transmitter, propagate to an

The Author: Mr. Werner received his Bachelors Degree in Electrical Engineering from the University of California, Santa Barbara in 1970. He received his Masters Degree in EE from Stanford University in 1974. He is presently a Project Engineer at Barry Research and has acquired experience in construction of HFOTH backscatter radar sounding equipment and is responsible for the design of the interface equipment used between the digital computer and HF radio receiving equipment and an automatic oblique chirpsounder system.

over-the-horizon area of interest, where some of the energy is scattered and returned to the radar receiver by the same ionospheric path. Line-of-sight radars generally operate only at fixed frequencies, but an HF radar periodically adjusts its operating frequency to accommodate the variability in ionospheric propagation conditions. In principle, an HF radar can cover an area of the Earth's surface beginning at 500 km and extending to 3500 km from the radar, by 360 degrees in azimuth. The resulting potential area of coverage is 38 million square kilometers. In practice, present radar antenna arrays will not permit this azimuthal coverage, and during certain hours of the day the range coverage of the radar may be reduced by propagation conditions. However the potential does exist with an HF radar-monitoring system to cover a large area of ocean at a considerably lower overall cost than that of presently used tracking systems.

A drifting buoy installed with an HF transponder can become a "target" for the OTH radar by receiving a signal from the radar transmitter, amplifying this signal, and radiating the signal back to the radar receiver, as shown in figure 1. The OTH radar can then determine the position of the buoy, and data can be telemetered from the buoy, via modulation of the returned signal. If the transponder electronics are broadband, say 12-24 MHz, a variety of propagation and interference conditions can be accommodated by the shore station, which alone determines the operating frequency.

Figure 2 illustrates schematically the operation of the HF radar tracking system. The transponder operates in monostatic fashion, with a T/R gate alternately allowing the reception and retransmission of HF signals from one antenna. The antenna is matched to the electronics by a matching network that is designed to the characteristics of the specific antenna such that a Voltage Standing Wave Ratio (VSWR) of 3:1 is seen by the transponder. A bandpass filter is inserted after the matching network to attenuate signals both transmitted and received, outside the operating band.

The timing cycle of the T/R gate is divided into three intervals--transmit and receive intervals of equal length, and a short interval following the transmit period to allow potential reflections from the antenna to decay. In the receive mode, the incoming signals are routed to a broadband delay line for a length of time equal to the delay time of the line. When the delay line is full, the T/R gate switches to the transmit mode, and the delay line contents are emptied and retransmitted. On the way to the antenna, the signal is DSBSC modulated by the signals from the platform's onboard sensors. The transponder may be programed to operate continuously or to switch on at predetermined intervals. Transmission rates for data are constrained to a maximum of 1-10 bps.

A unique feature of the transponder is a peak signal detection circuit that continually monitors the RF output of the power amplifier, and adjusts the bias supply to the final amplifier so that is can just handle linearly the signal level at the moment. Operating Class A to minimize distortion, the output amplifier could consume needless power if it were always biased to accommodate the largest possible input signals. Here, however, the biasing follows the input signal level, thereby saving much valuable power. The RF output power of the transponder is normally less than 1 watt, and system gain is accomplished in the radar's receive antenna array and subsequent processing.

Figure 3 lists the general characteristics of an OTH radar that would be suitable for a buoy tracking program. An interesting point concerning the range accuracy of the system is that this accuracy is not a function of overall range; the percent range accuracy actually improves with increasing distance from the shore station.

Shown in figure 4 are possible zones of coverage of HF radars. Of the sectors drawn, two are presently covered, one on the West and one on the East Coast. An interesting area of weather activity, the Caribbean and Gulf of Mexico, could be monitored by two stations. When hurricanes accurred, for example, air-droppable buoys with their lightweight transponders could be deployed throughout the area, and monitored from shore.

The remaining slides describe the Air-Droppable, Expendable, Ocean Sensor (ADEOS) buoy, that was developed by Barry Research for use with OTH radars. The impact on buoy design from using the OTH radar tracking method is readily apparent.

Figure 5 shows a line drawing of the ADEOS buoy. The important concepts in the design are:

1. The buoy is air-deployable. Weighing only 37 pounds, the buoy may be air dropped from an aircraft cargo hatch or from an external stores mount. Two important consequences of the air-deployability are that large areas of ocean can be covered with relative economy and that areas can be covered very rapidly after a decision to do so--depending only on the flying time from the base airfield. The cost of an air drop, when compared to deployment by ship--except on an opportunistic basis--is many times less than the cost of the ship deployment.

2. The buoy is expendable. Price is expected to be less than $1K each in quantities of 1000. For the price of purchasing and deploying <u>one</u> of the currently used moored buoys, a large area of ocean could be seeded with ADEOS buoys, and reseeded when they drift out of the area of interest or exhaust their batteries.

3. The buoy can be equipped to measure and relay a variety of parameters, including sea state, sea and air temperature, barometric pressure, wind speed, cloud cover, ocean surface currents, and others.

Figure 6 shows the ADEOS buoy in a simulated deployment situation from the cargo hatch at the rear of a C-130 aircraft. Prior to release the buoy is armed to activate the battery-powered electronics package.

Figure 7 shows a photograph of the ADEOS buoy in preparation for deployment from a light aircraft--carried as an external store under the fuselage. The buoy free-falls without parachutes.

The buoy's main structural element is an alloy steel pipe of 4 cm outside diameter and 4 m in length. At the upper end is a molded flotation collar consisting of polyurethane foam sheathed in fiberglass. The steel pipe supports a fiberglass whip antenna above the water and encloses the battery pack and electronics. The life of the battery pack has been nominally specified as three months, varying with the regularity of interrogation and spectrum occupancy statistics. An alarm signal on the buoy's transmission indicates a low battery voltage condition.

The ADEOS buoy is mechanically designed to withstand air deployment under conditions typically used in C-130 air drops -- that is, from an altitude of 200 feet and a speed of 125 knots. Included in the total weight of 15 kg are 1 kg of electronics and 3 kg of batteries. The flotation collar ensures a dry ride for the antenna in up to sea state 6.

Extensive tests have been made to verify the buoy's integrity in air deployment. Figure 8 shows a drop sequence following release from a light aircraft at an altitude of 200 feet. The plane's speed was 125 knots and subsequent frames show the buoy as it falls. Most of the drag results from the flotation collar, and since this is behind the center of mass, the buoy streamlines to the direction of flight. During a typical drop the buoy enters the water at about 120 mph at an angle of about 45 degrees. Under these conditions, the buoy is submerged for several seconds, rises completely out of the water, and then settles down within one minute to a vertical position.

To measure the buoy's behavior in rough seas, a series of tests were conducted in the North Atlantic in mid-winter. The buoy was launched off the stern of a Coast Guard cutter in rough water and its motion filmed.

In storms with 15 to 25 foot swells and winds up to 50 knots, the buoy's heel angle often approached 45 degrees, but stability was maintained.

To verify the buoy's lifetime at sea, a unit was deployed 1000 miles off the coast of California by a Coast Guard ship en route to Ocean Station November. Figure 9 shows the track of the buoy as it was followed in the subsequent 3 months by an HF radar facility in central California. At the end of this period, the buoy's electronics were still operating normally.

In summary, the advantages and limitations of OTH radar as the tracking means in a drifting buoy program are:

1. Positioning accuracy: the azimuthal component of a buoy's position can generally be determined to an accuracy of ±10 km at 1000 km, and the range component to ±20 km.

2. Geographical factors: the optimum area of coverage of a typical radar facility is a 30 degree wide sector extending from the radar, with a minimum tracking range of about 1500 km, and a maximum range of about 3500 km. Greater sector width can be obtained with increased radar antenna size.

3. Data rate: The OTH radar technique permits data rates to a maximum of 1 - 10 bps. However, data taking is not constrained to a schedule, and buoys may be queried as often as desired.

4. Onboard electronics: the transponder package is considerably simpler than currently used electronics packages, and is smaller, more lightweight, and can be ruggedized for deployment. In addition, the power consumption of the transponder is minimal.

5. The economics: a suitable OTH radar facility can be made operational for much less than the cost of satellite or aircraft tracking systems. The buoy electronics package costs on the order of $500 in quantity.

The OTH radar technique of tracking and monitoring drifting buoys may prove to be a viable alternative in measurement programs where simultaneous data is required from many sensors over a large area. An airdroppable, expendable, drifting buoy, with an on-board transponder, has been designed during 1973, and preliminary testing of the OTH radar tracking technique has been performed successfully.

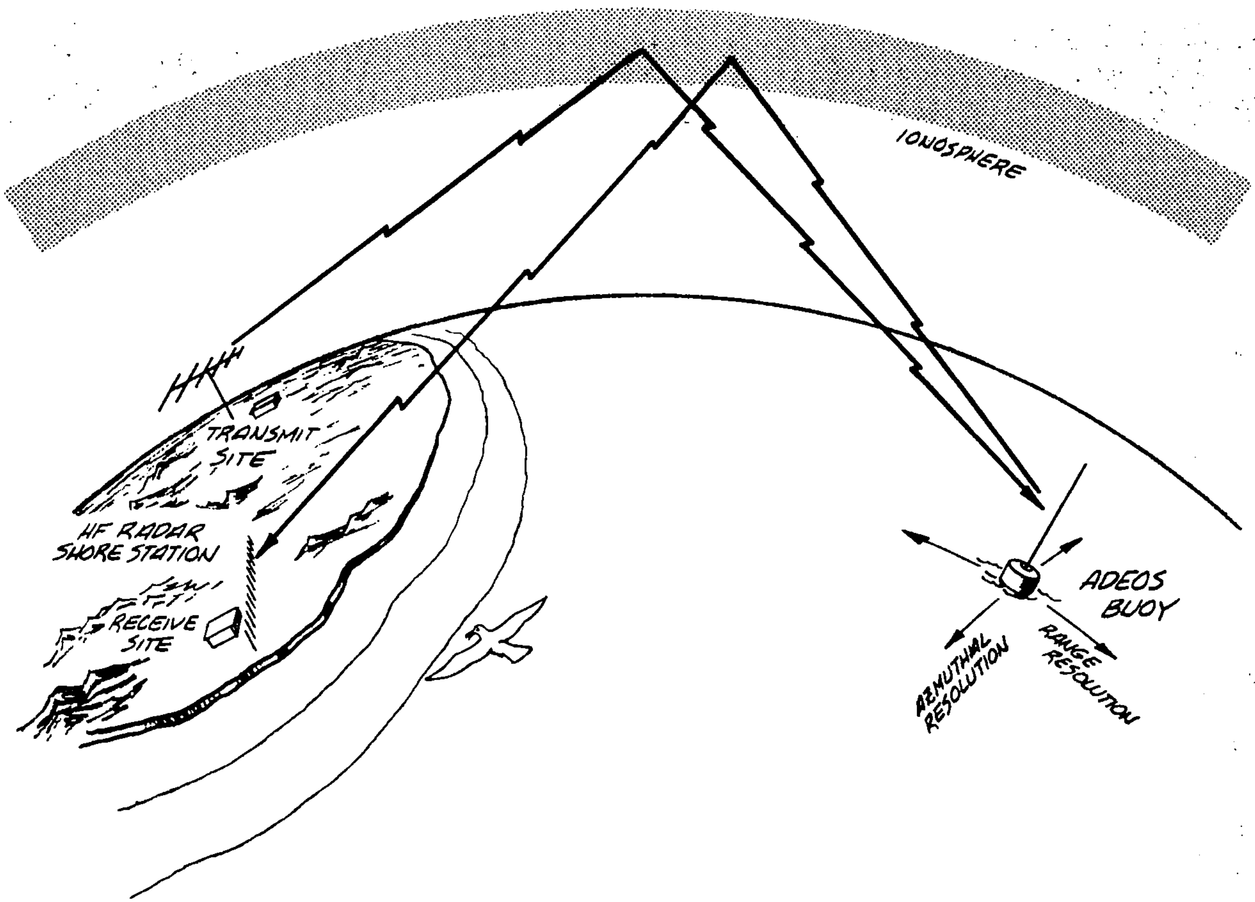


Figure 1. HF RADAR MONITORING DRIFTING BUOY

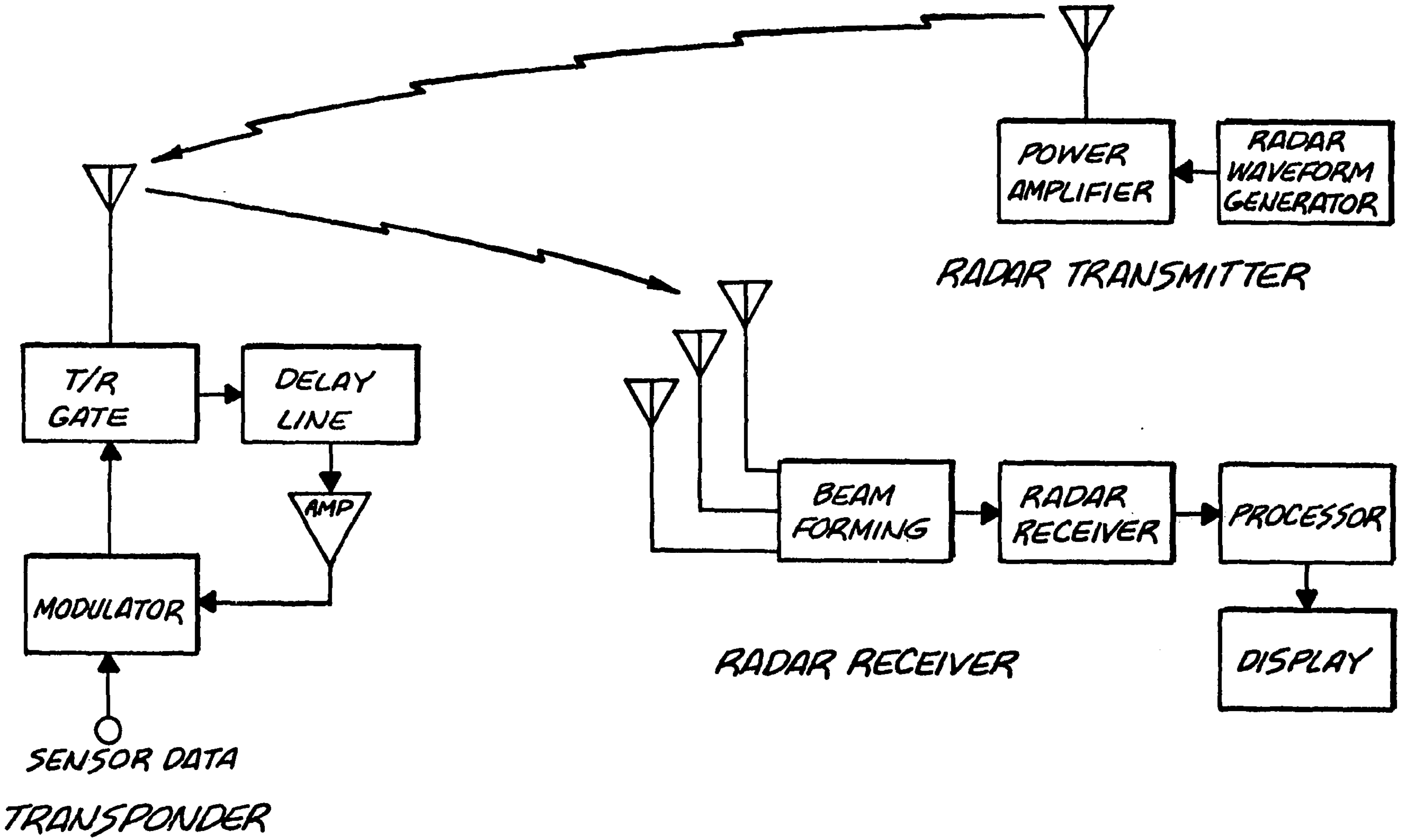


Figure 2. OPERATION OF THE HF RADAR TRACKING SYSTEM

HF OTH-B RADAR SUITABLE FOR BUOY TRACKING

OPERATING FREQUENCY RANGE	4-30MHz
TRANSMITTED WAVEFORM	FM/CW, 8kHz BANDWIDTH
RADIATED POWER	10-20kW
TRANSMIT-RECEIVE SITE SPACING	GREATER THAN 30km
TRANSMITTING ANTENNA	BROADBAND, 16dBi GAIN, e.g. LPA ARRAY
RECEIVING ANTENNA	LINEAR ARRAY: 30° SECTOR CIRCULAR ARRAY: 360° SECTOR ELEMENTS: WHIPS OR LOOPS APERTURE : 1000ft
RANGE ACCURACY	± 20km
AZIMUTHAL ACCURACY	±8km AT 1000km

Figure 3. CHARACTERISTICS OF AN OTH RADAR SUITABLE FOR BUOY TRACKING

MONITORING BUOYS WITH OTH RADAR

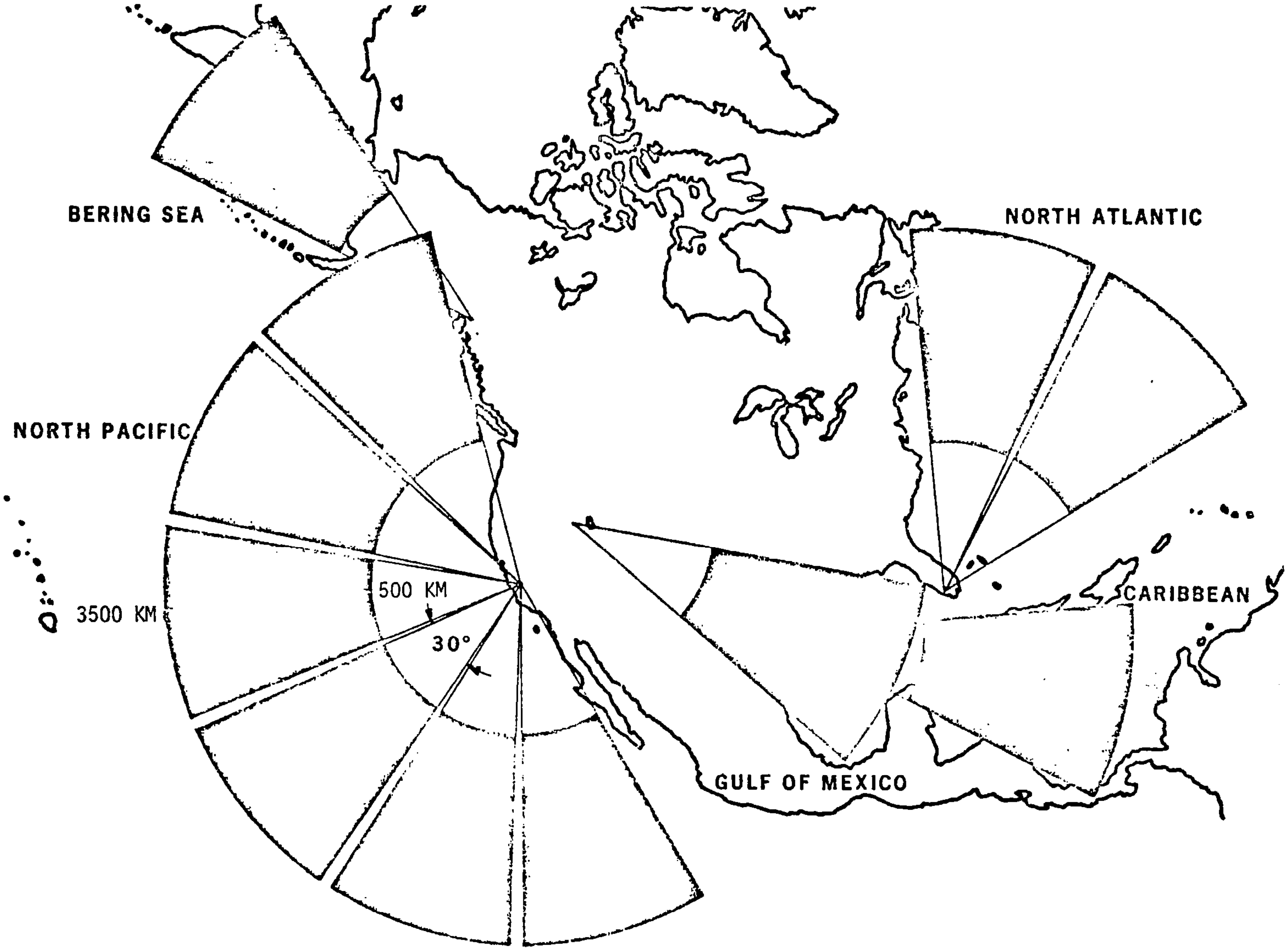


Figure 4. POSSIBLE ZONES OF COVERAGE OF HF RADARS

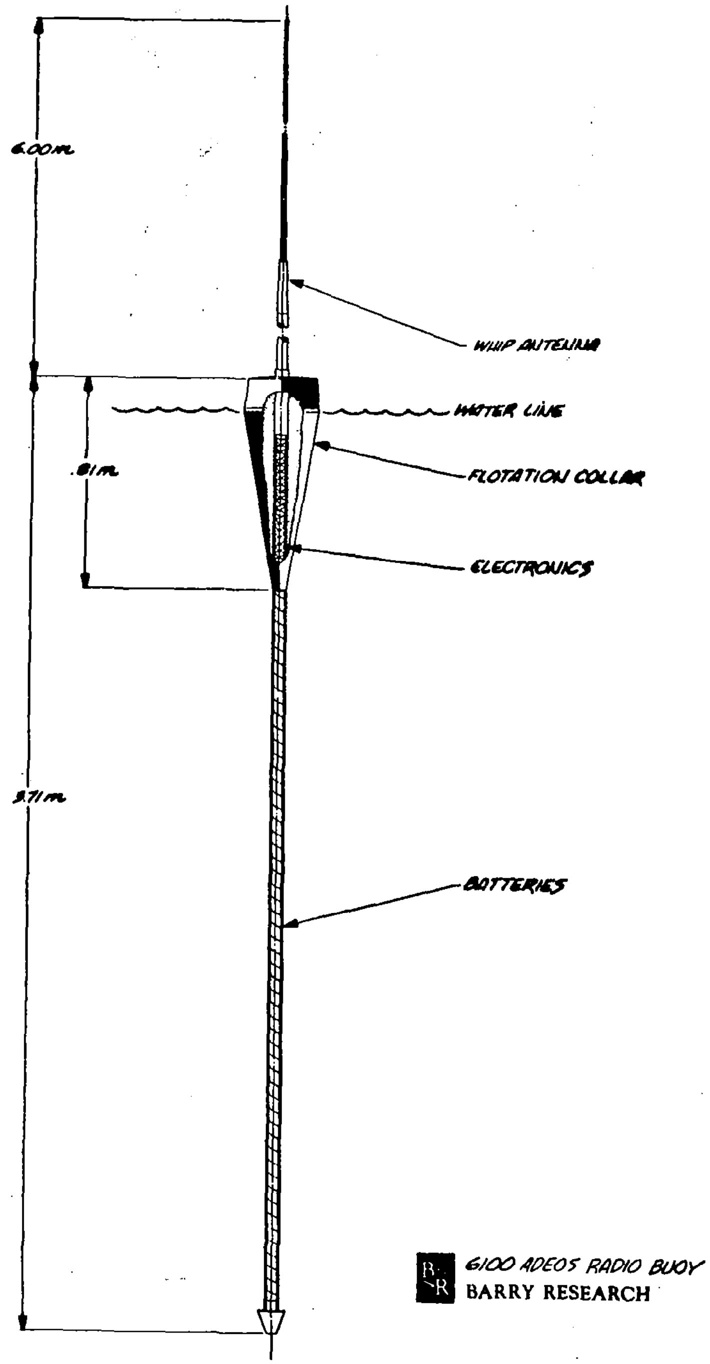


Figure 5. ADEOS BUOY

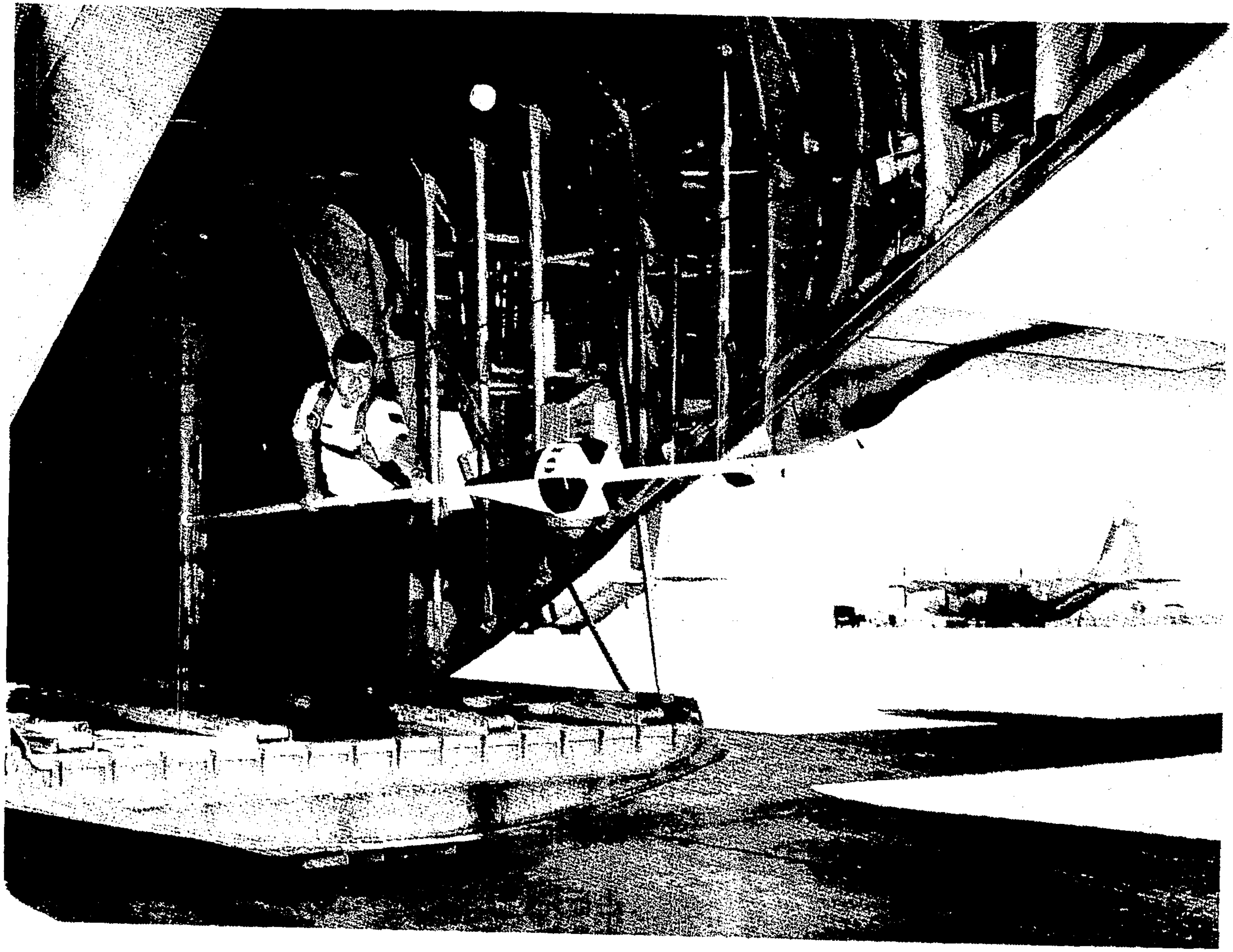


Figure 6. DEPLOYMENT SITUATION FROM THE CARGO HATCH OF A C-130 AIRCRAFT

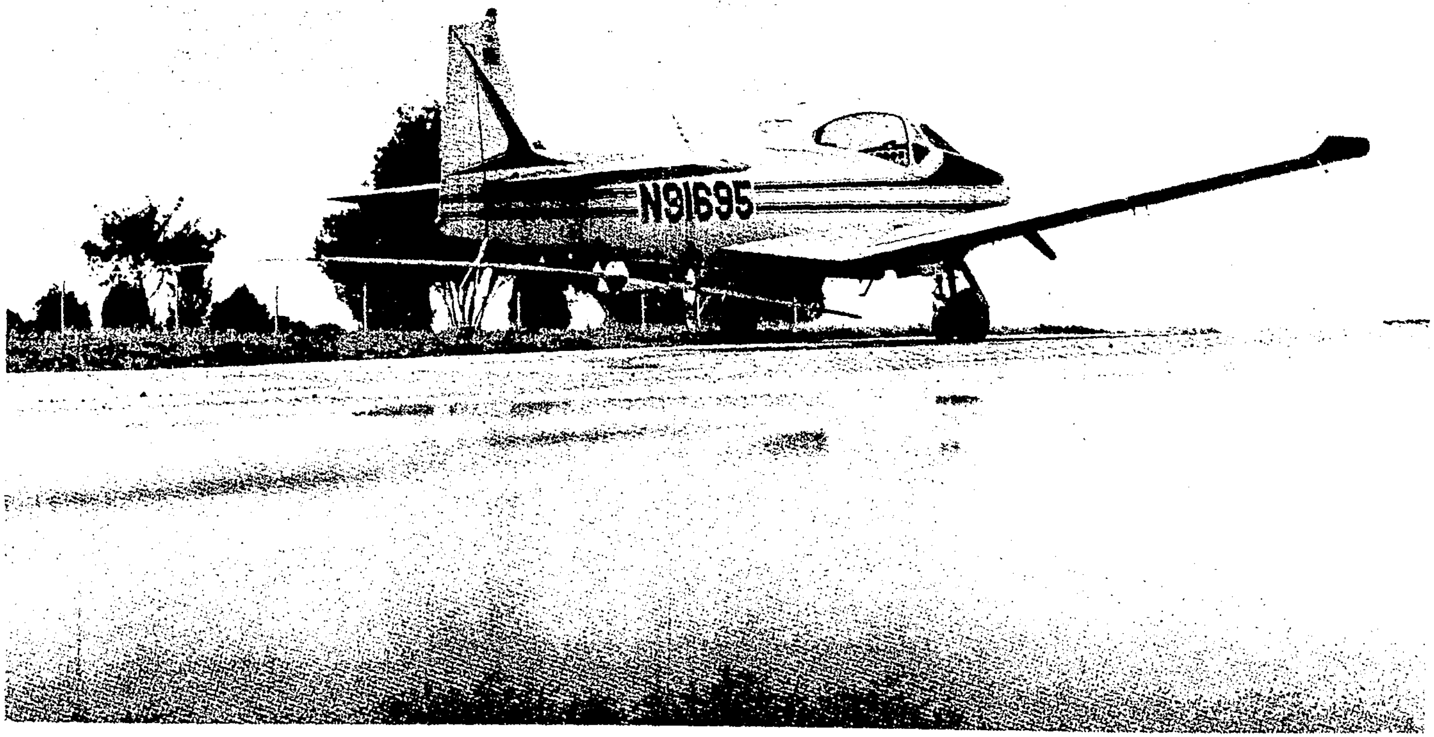


Figure 7. DEPLOYMENT TECHNIQUE FROM A LIGHT AIRCRAFT

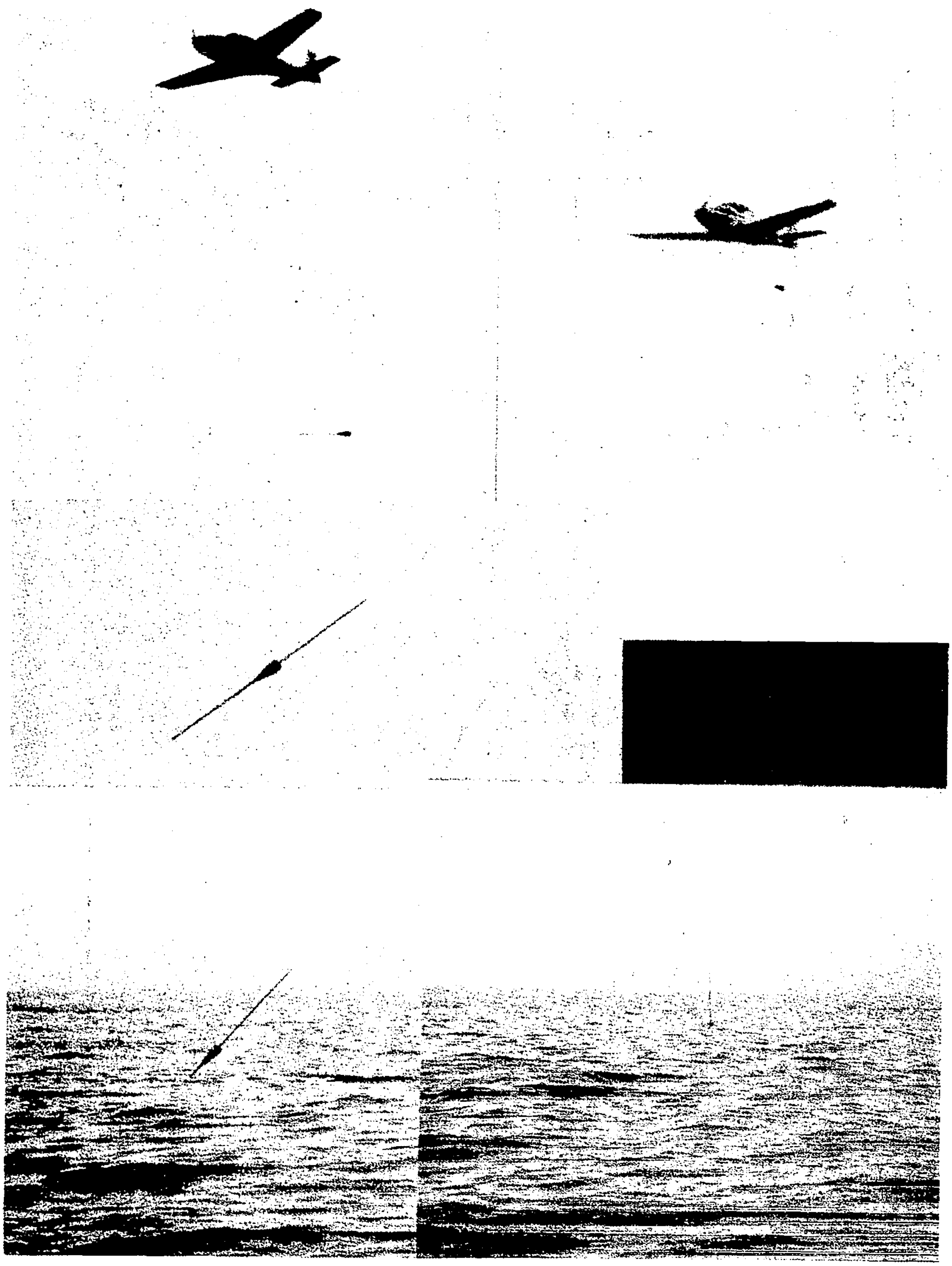


Figure 8. DROP SEQUENCE FROM A LIGHT AIRCRAFT

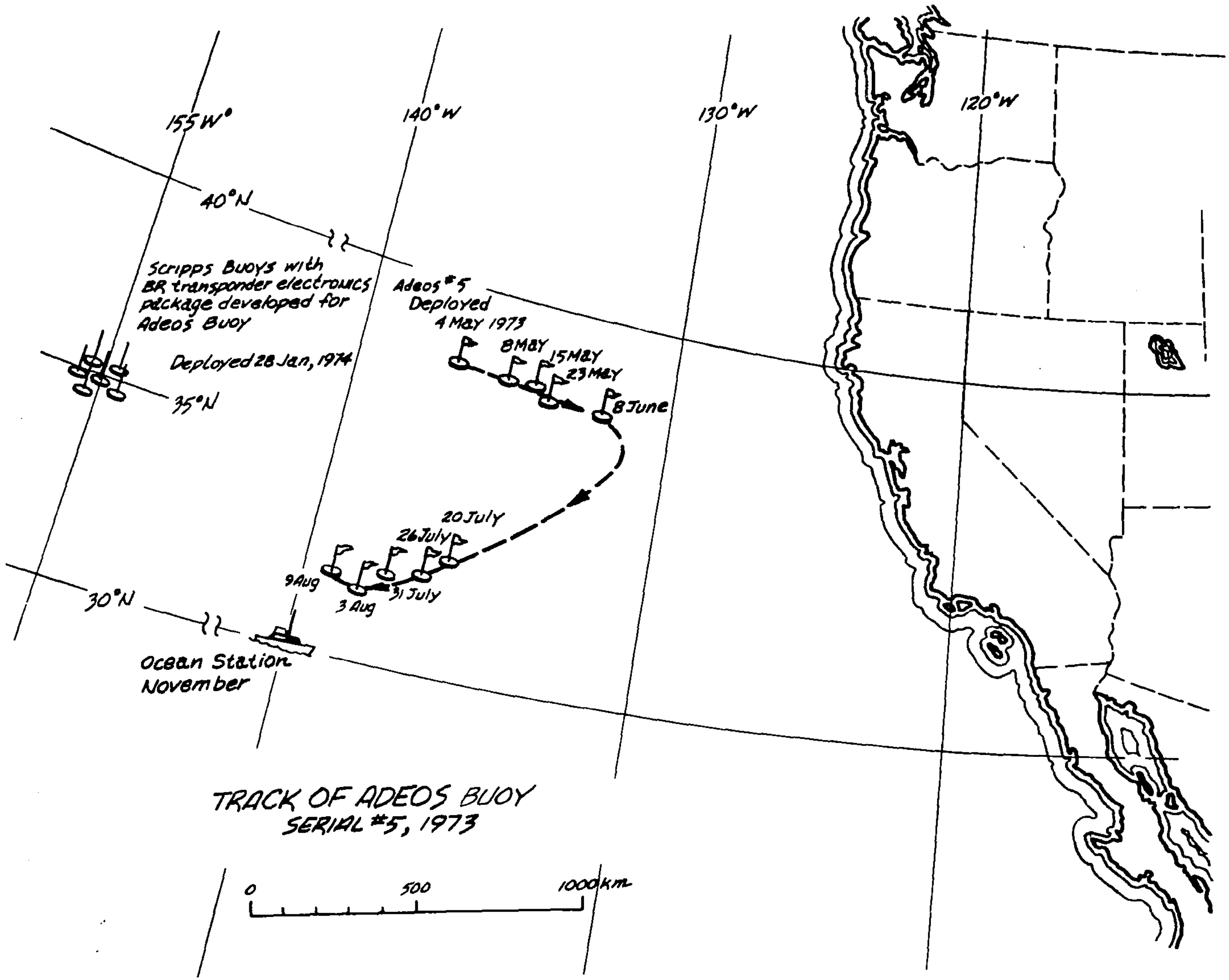


Figure 9. TRACK OF BUOY IN PACIFIC

A NEAR SHORE CIRCULATION STUDY OF THE CHESAPEAKE BAY ENTRANCE USING RADAR TRACKED BUOYS

by

Dr. Ronald E. Johnson
Old Dominion University

Near shore is the right word for it. We're probably within 6 or 7 miles from the shore. The initial system was set up with the existing technology to satisfy a desire to learn something about the local circulation in the entrance to Chesapeake Bay, which is in our own backyard here. Budget, of course, required that there be little development cost, and so existing technologies were used for this. This is a cooperative venture between the Institute of Oceanography at Old Dominion University and the NASA Langley Research Center, with the cooperation of Wallops Flight Center. We've been using S-Band Radar to track four free-drifting buoys in the south side of Chesapeake Bay. The Wallops people have provided the radar tracking van, a MPS 19 S-Band Radar, in the 2700 to 2900 megahertz range. Langley Research Center has provided the free-drifting buoys that have battery life in the range of 30 to 40 hours.

Figure 1, unfortunately, is a picture taken on a dark December day last winter. This is in the entrance of Chesapeake Bay, showing the radar van on the right and an FM antenna for communication with the ship that was out with the buoys. The black blob in the center is the 50 kilowatt generator, and the little trailer that housed the digital output and also protected us from the weather is on the left (since it was the kind of blustery day that has been described in a certain story).

The second figure (figure 2.) shows the weak link in the system at present. We get a digital readout of range and azimuth which then are manually recorded and plotted at the present time. It's a fun job. We take fixes approximately every 5 minutes to every 15 minutes and the accuracies here are within 5 meters out to a range of about 15 nautical miles on a line-of-sight basis. If the buoys get around the corner of Cape Henry, as they have done on numerous occasions, they are out of sight and then our research vessel would have to go try to find them and pick them up. We have done something like four data runs with up to four buoys in the entrance to the bay.

I have been trying to establish the circulation around the tip of the south side of the bay to try to help out with the sediment erosion problem along the Virginia Beach shoreline. The goal is to ultimately establish whether or not the circulation pattern implied by the drift of these buoys indeed has an onshore component that could somehow be utilized in connection with dumping of sand in an offshore environment and allowing it then to be slowly carried by the circulation pattern onto shore, perhaps even aided by the northeast winds in the wintertime. Our research vessel, LINWOOD HOLTON, has been releasing seabed drifters and surface drifters at the same time the buoys are released. If you're interested in data from within 5 miles of the shoreline, we've gotten recoveries of over 50 percent on all of these drifters. All of them do seem to indicate that there is a net in-shore pattern and also that once they get inside the bay they seem to be carried across the bay to the eastern shore side of the bay, which might support things that Coriolis once determined.

Figure 3 is a chart showing the entrance of Chesapeake Bay, with the Bay Bridge Tunnel shown inside the entrance by the solid lines. The triangle near Cape Henry is the location of the radar van. Rudee Inlet is further down the beach and indicates the southern limit of any of our tracking. We cannot see around Cape Henry very far with the radar van positioned as shown, and also it is a line-of-sight device located near sea level. We have positioned the radar van near 44th Street on the beach; but, again, cannot see around Cape Henry into the bay.

Figure 3 also shows the smoothed track of one buoy over a tidal cycle on August 8-9, 1973. The start of ebb is at position 1. The start of flood is at position 41, with the start of the next ebb at position 55. For this particular run, then, the ebb excursion was about 7 nautical miles and about 1½ on the flood. I should mention here that the buoys are coupled at a depth of 20 feet to a crossed dragplate of 2.5 feet by 5 feet dimensions, so we are looking mostly at subsurface circulation. We are currently studying the coupling effect as a separate project. The most southerly points on this figure are not connected because we were getting only momentary fixes as the buoy was almost out of sight. The buoy drift pattern is quite interesting, however. Note that the track is approaching a streamline flow pattern--that of flow in and out of an orifice. Also, a very narrow clockwise motion with an onshore component is evident in the cape area. We have yet to get buoy runs that can be tracked completely all the way around Cape Henry.

Figure 4 shows a much enlarged scale of a drift inside the bay near the Bay Bridge Tunnel on May 22-23, 1973. The drift started at position 1 with flow reversals noted by positions 11, 33, 56, 79, and 101. Position 110 indicates buoy recovery and not a flow reversal point. The flood excursions seem to be around 4 nautical miles in length with somewhat shorter ebb cycle lengths. Again, clockwise tidal cycles are noted with a difinite cross-bay component. Note the sweep around the shoal area just west of the bridge. This area is less than 20 feet in depth, less than dragplate depth. We were afraid the buoy would go aground, but things followed basic laws and the drift went around the shoal and back out through the bridge tunnel. One of the buoys did ground next to the fishing pier on the west side of the southerly bridge tunnel island around position 50.

I might admit here that one of the complications to the tracking is that most of the available current information used in predicting where the buoys will go and, hence, where to initially release them, comes from the tidal current tables. This is primarily surface information; but the circulation in this area is approximately two-layered, with a subsurface tide change usually occurring before the surface flow reversal. With this in mind, we carefully predict where the buoys are supposed to go, put them out; and, sure enough, they don't go the way we've predicted. On this particular day, the tide at depth reversed some 2 hours before the surface change and also had an unpredictably long flood compared to ebb. This caused a study of tidal circulation around the bridge instead of around the cape as originally planned.

The last figure (figure 5.) shows one buoy track near the entrance. This drift indicates the strong dominance on circulation due to wind. The buoys this day, December 5-6, 1973, were expected to come into the bay and exit in a clockwise loop in a northwest to southeast fashion. The wind picked up to approximately 25 knots shortly after buoy release from the southwest causing the northeasterly motion. The tidal cycles are still evident, with flow reversals indicated by positions 55, 103, and 130. It appears that the surface circulation due to wind stress was altered at least down to 20 feet during this condition. The other figures showed buoy drifts on light wind days.

All of the figures presented here have shown only one buoy drift track. The others, ranging from one to three more, all showed visual agreement as to flow reversal and shape of flow path. The additional buoy information allows computations to be made including correlations between buoys, centers of rotation of individual buoys, as well as the tidal water mass, and current components including correlations and cross correlations. I hope that Dr. Tim Barnett will talk tomorrow about some of the data analysis that has been done to date.

I'm hopeful that additional work in the entrance may be done with smaller portable radar units and with additional buoys, say up to 12, so that we might do the entire bay entrance. The four buoys presently being used now are very expensive, approximately four thousand dollars each, and we all get very nervous about putting them in the entrance, which has--as you probably know--one of the heaviest concentrations of ship traffic that may be found anywhere in the world. We have yet to have one of these run over, but we've lost contact with them several times when 'big blobs' go by on the radar screen.

The present operation itself is one that involves at least 20 people every time we go out. There are three agencies involved (Institute of Oceanography at ODU, LaRC Systems Development Section, and Wallops Flight Center) plus a fourth 'agency', weather, which has cancelled us out a couple of times. All of the above have severely limited the number of times we've been able to operate.

Are there any questions?

Speaker Unidentified:

Ron, what time of year was that last figure?

Dr. Johnson:

The last one was December 5 and 6 of last year (1973). The one in the entrance of the bay. The one up around the bridge tunnel was taken

May 22-23, 1973. These have just been done when we can get everyone together to get out. I have not been able to plan these drifts for seasons yet.

Speaker Unidentified:

Ron, it isn't clear to me. Were you using the dragplate at various levels, and if so, what depths were these?

Dr. Johnson:

Earlier, I mentioned 20 feet for one drift; we've kept them all at 20 feet. The one day we did lose a dragplate, we put the equivalent amount of weight underneath the buoy and left it on the surface just to see what would happen to it. The surface circulation, indeed, was much different than at 20 feet. The buoy excursion was twice that of what it was at 20 feet. In fact, it went so far south that we were getting nervous that we were going to lose it into some near shore drift to the south rather than the tidal circulation in the bay.

Question:

Dr. Johnson:

No, this has just been an initial attempt to see if we can do this operation in the entrance of the bay from a fixed shore station. It appears we can, but once we start getting over a couple of tidal cycles the buoys may get out of radar range simply because we're going around corners and trying to see over and around things. We need a little longer range perhaps. Maybe a radar set up on a light tower might be a good platform.

Speaker Unidentified:

I'd be interested to know what makes the buoys so expensive. Are they passive?

Dr Johnson:

No, these are active. They have a battery life of some 30 to 40 hours. They're interrogated by the radar van, but they are active. John McFall can tell you a lot more about the radars. This is his operation.

John McFall, LaRC:

The reason the transponders are so expensive is that they are aerospace qualified equipment, which means they are flight articles which are over-qualified for these missions. However, they are in-house off-the-shelf-items. This means we could put something together quickly to respond to local needs.

Speaker Unidentified:

What was the cost of the buoy package?

Dr. Johnson:

Around $4000-- replacement cost.

Speaker Unidentified:

Is that really overly expensive, when you consider the cost of the 20 men that it takes to operate the system?

Dr. Johnson:

If we would have been able to use recording equipment, the system would take only one or two operators. But because it was not available, several students and myself were doing the recording by hand. This wasn't so expensive but it was a lot of manpower involved.

Speaker Unidentified:

How many buoys do you think you can differentiate between, using this system?

Dr. Johnson:

There's really no limit on that, because each one has a different pulse rate for interrogation. The buoy senses that the radar is asking for it to respond. The ones we used has a 2 to 12 microsecond pulse delay. The buoys are all receiving on the same frequency, but they know when it's their turn to be interrogated due to the delay range in the double pulse. However, in the entrance of Chesapeake Bay the currents are so swift and we're not sure exactly where they're going to go all of the time, even though I make 'predictions,' that I would be hesitant right now with the existing radar to track more than four.

Speaker Unidentified:

One other question Ron. I'm interested in the system itself--in the buoy itself. Had you made any attempt to correct for the drag of the surface float or does the dragplate so dominate the system, that this isn't important?

Dr. Johnson:

This has been a problem that we have recognized. A separate small contract has been initiated to try to analyse the coupling effect of various buoy/drogue combinations. This is still underway. As I mentioned, the one day when we did lose the dragplate we just put the buoy out with an equivalent weight right underneath the buoy. The speed of that buoy was twice that of the subsurface current, so there has to be an effect on the buoy/drogue system. The surface buoy is something in the order of 2-1/2 feet in diameter by 4-feet deep, so it's a massive can that we've got out there.

Speaker Unidentified:

What is the size of your dragplate, compared to that?

Dr. Johnson:

They are 2-1/2 feet by 5-feet plates crossed, centered at 20 feet.

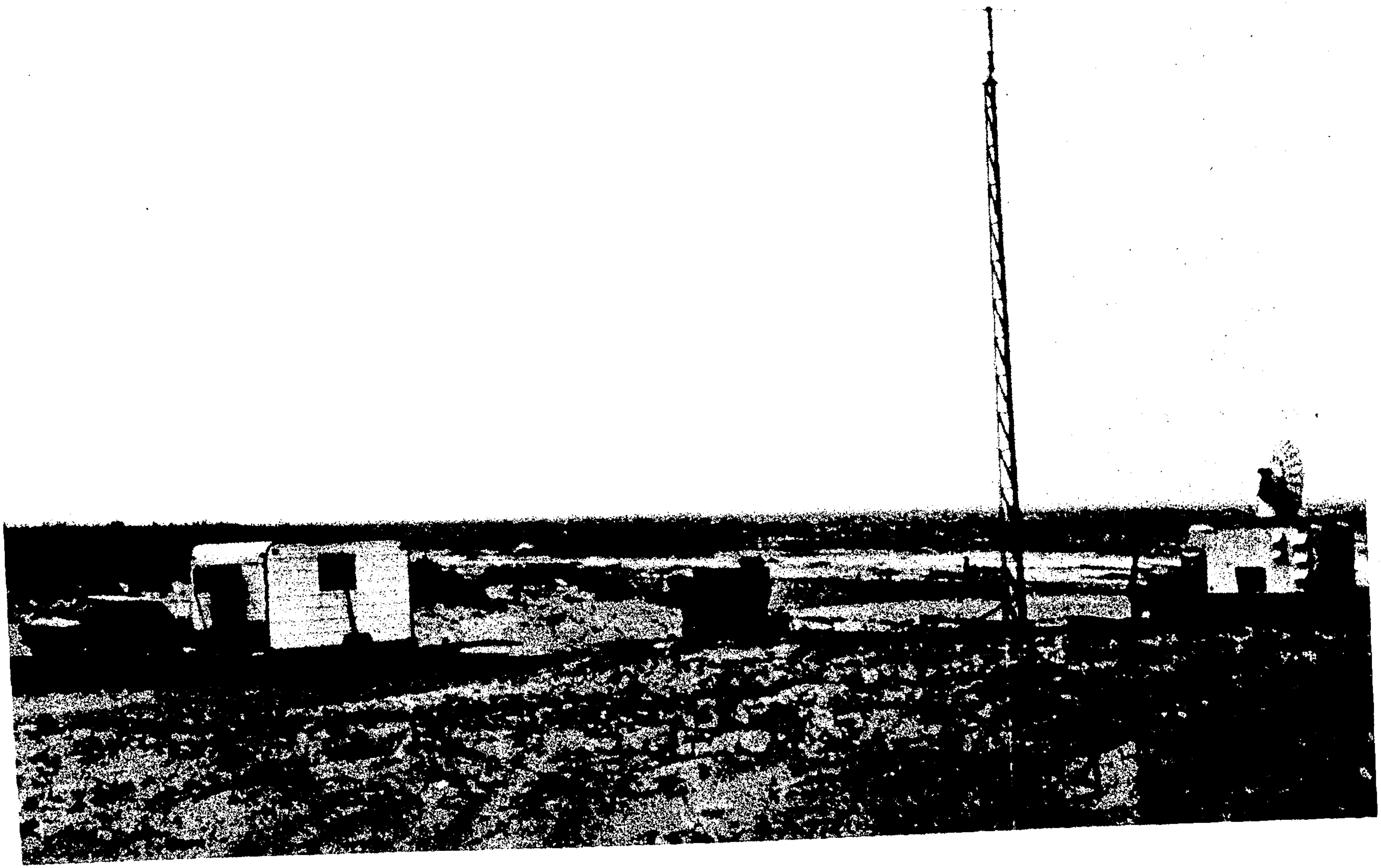


FIGURE 1. RADAR VAN, 50 KW GENERATOR AND SUPPORT TRAILER

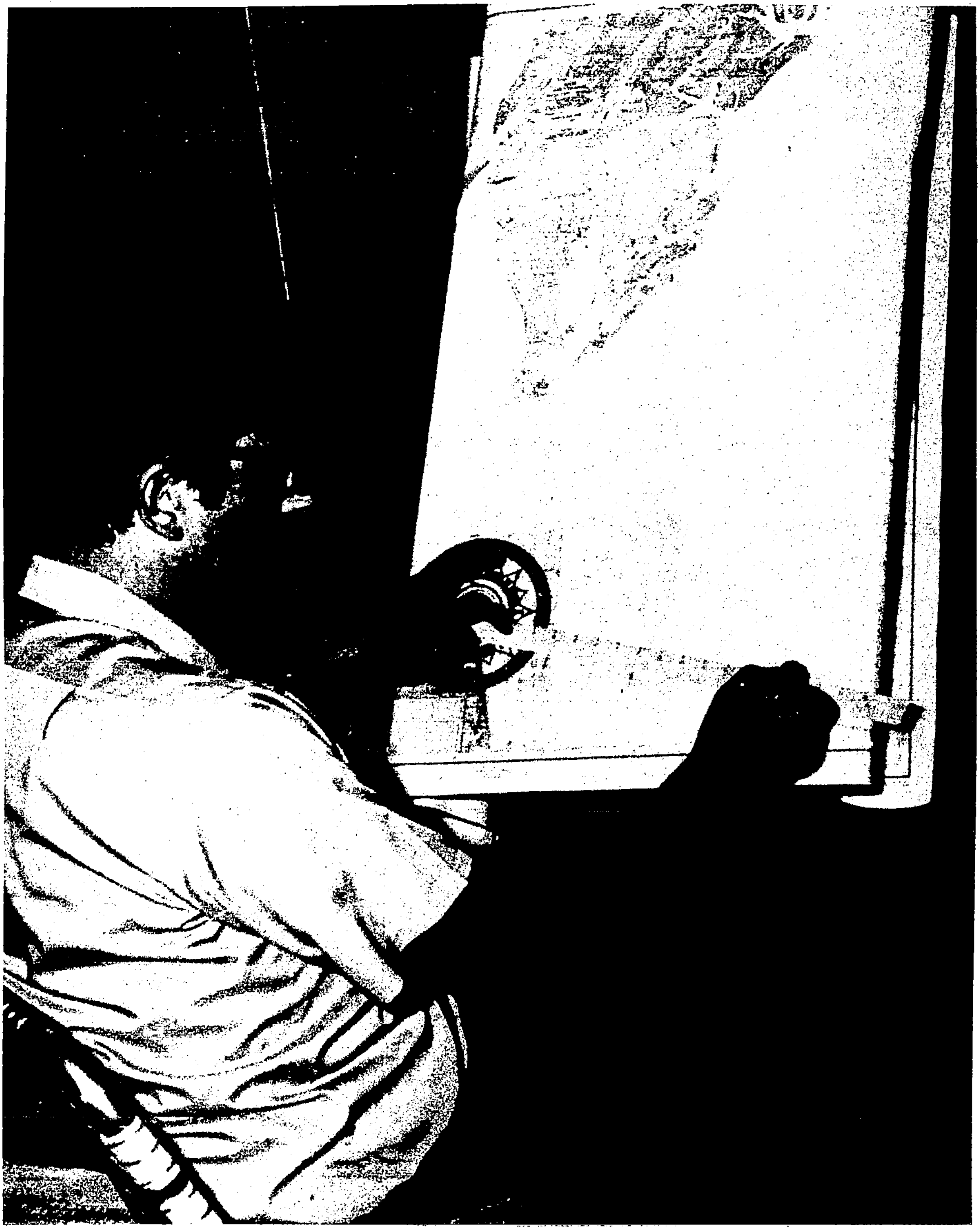


FIGURE 2. MANUAL RECORDING TECHNIQUE

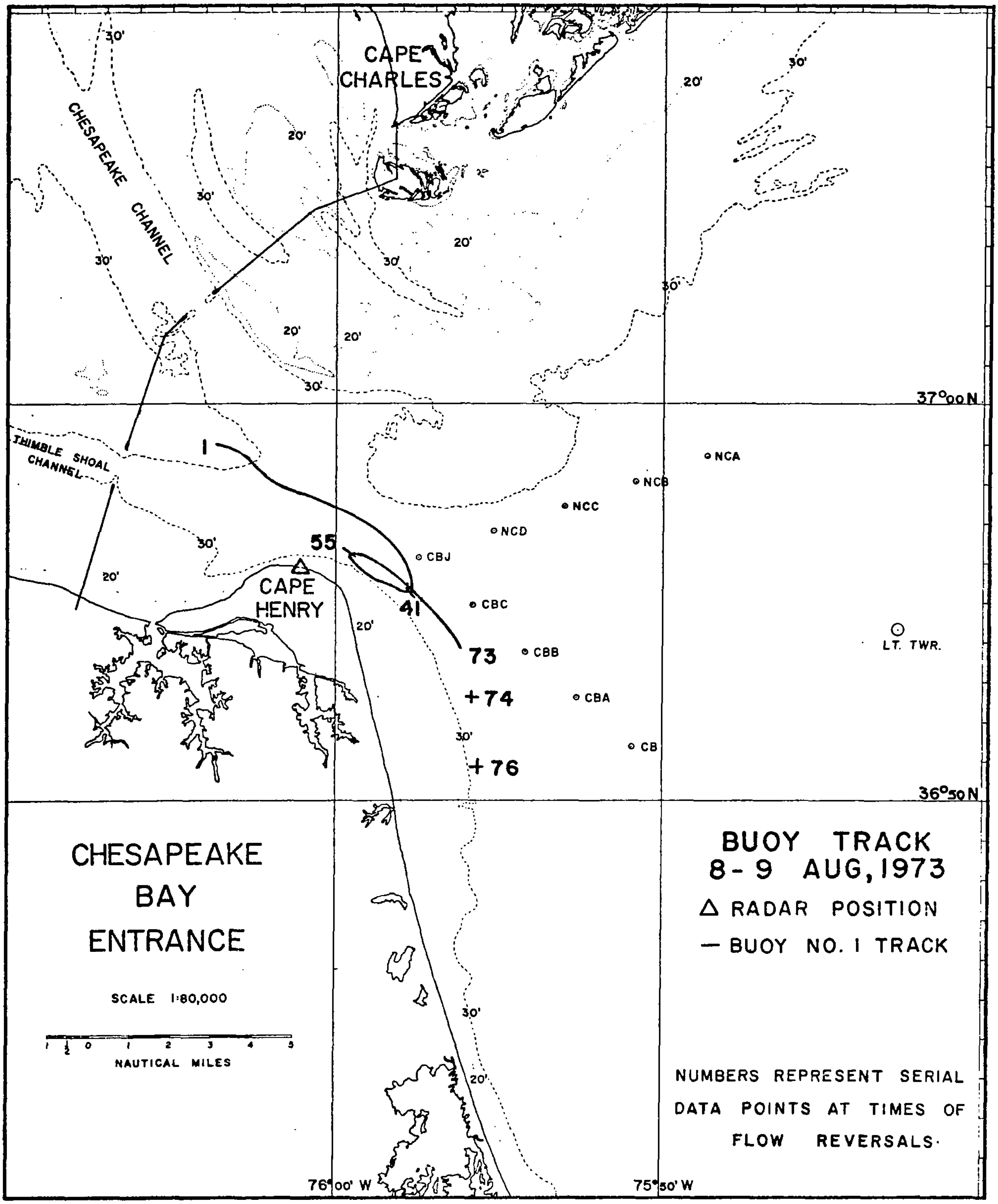

CAPE CHARLES
CHESAPEAKE CHANNEL
THIMBLE SHOAL CHANNEL
CAPE HENRY
37°00N
36°50N
76°00' W
75°50' W
NCA
NCB
NCC
NCD
CBJ
CBC
CBB
CBA
CB
LT. TWR.
1
55
41
73
+74
+76
20'
30'
CHESAPEAKE
BAY
ENTRANCE
SCALE 1:80,000
NAUTICAL MILES
BUOY TRACK
8-9 AUG,1973
△ RADAR POSITION
— BUOY NO. I TRACK
NUMBERS REPRESENT SERIAL DATA POINTS AT TIMES OF FLOW REVERSALS.

FIGURE 3.

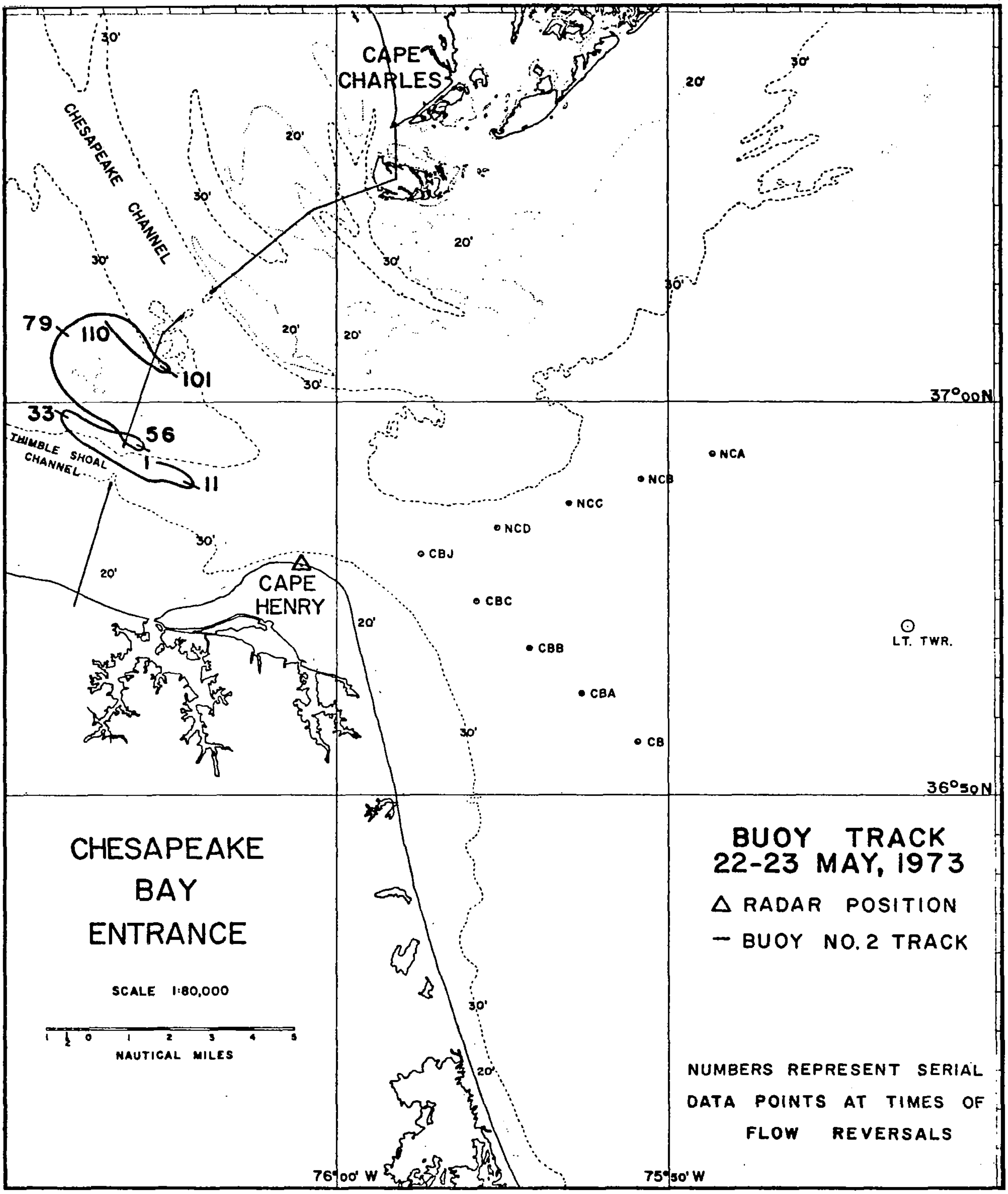

CAPE CHARLES
CHESAPEAKE CHANNEL
THIMBLE SHOAL CHANNEL
CAPE HENRY
79
110
101
33
56
1
11
NCA
NCB
NCC
NCD
CBJ
CBC
CBB
CBA
CB
LT. TWR.
37°00N
36°50N
76°00' W
75°50' W
20'
30'
CHESAPEAKE
BAY
ENTRANCE
SCALE 1:80,000
NAUTICAL MILES
BUOY TRACK
22-23 MAY, 1973
△ RADAR POSITION
— BUOY NO. 2 TRACK
NUMBERS REPRESENT SERIAL
DATA POINTS AT TIMES OF
FLOW REVERSALS

FIGURE 4.

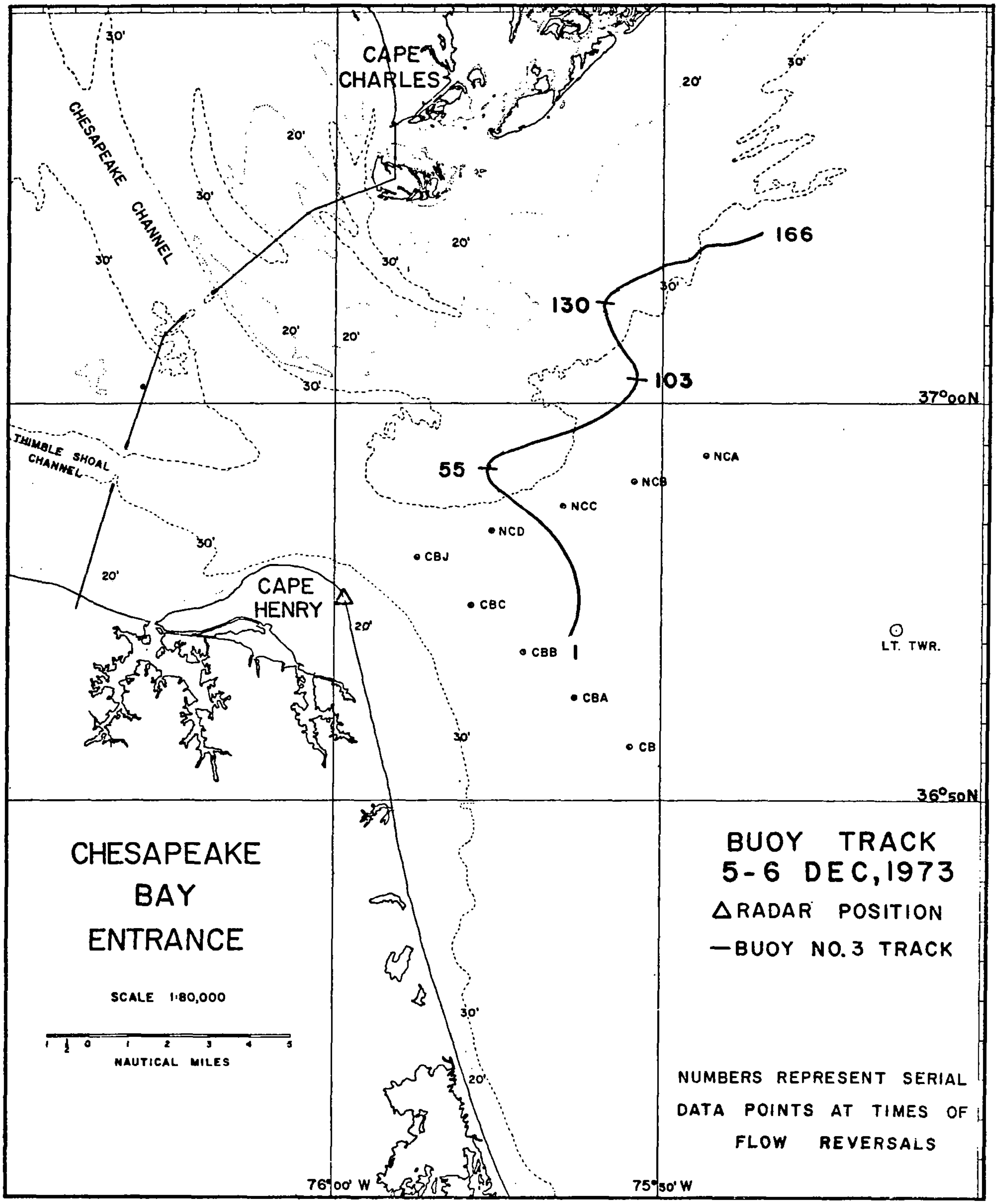

CAPE CHARLES
CHESAPEAKE CHANNEL
THIMBLE SHOAL CHANNEL
CAPE HENRY
166
130
103
55
1
NCA
NCB
NCC
NCD
CBJ
CBC
CBB
CBA
CB
LT. TWR.
37°00'N
36°50'N
76°00' W
75°50' W
CHESAPEAKE BAY ENTRANCE
SCALE 1:80,000
NAUTICAL MILES
BUOY TRACK
5-6 DEC, 1973
△ RADAR POSITION
— BUOY NO. 3 TRACK
NUMBERS REPRESENT SERIAL DATA POINTS AT TIMES OF FLOW REVERSALS

FIGURE 5.

MEASUREMENT OF OPEN OCEAN CIRCULATION USING NEUTRALLY BUOYANT FLOATS

by
Douglas C. Webb
Woods Hole Oceanographic Institute

I am going to summarize three approaches to the measurement of open-ocean circulation using neutrally buoyant floats. I think some of you are not familiar with what is meant by neutrally buoyant floats. These are devices which can be arranged to come into equilibrium at any subsurface depth in the ocean. This is done by making the whole instrument less compressible than seawater; and if it is carefully ballasted, it will sink in the ocean until it comes into equilibrium with water of a certain density. Since the in situ density in the deep ocean is primarily dependent on pressure to a very close approximation, we can think of these as constant depth devices. Since we are able to occupy any part of the water column and have no physical connection with the surface, all of the tracking that I will speak about will be done sonically.

I am going to discuss three separate kinds of experiments, and these are the things which are common to all three neutrally buoyant float experiments; I will describe them to you. All operate at most ocean depths. All are recoverable on an acoustic command which causes them to jettison a weight and rise to the surface. All are constructed of aluminum alloy, and use sonic tracking. The aluminum alloy may seem a little irrelevant, but to meet the structural requirements for compressibility, there is a structural design problem involved. The depth is sought out in a completely passive way. There is no active mechanism to maintain them at constant depth. For figure 1, I have drawn a picture of the instrument to kind of focus our thinking about the whole experimental approach. We call these vertical current meters. On the figure are summarized the vital statistics. You can visualize this as a cylindrical instrument, equilibrated out at certain depths in the ocean, with a set of inclined vanes at the equator. A vertical translation of the water past the instrument is going to act on those vanes and cause it to rotate. These instruments are all fitted with internal magnetic compasses which measure the rotation, that is, the angular position which is a direct measurement of the displacement of water. Together with a record of the temperature and pressure it permits us to make certain specialized studies of both horizontal and vertical motion of deep water. The sonic signal range is set by the local refraction pattern. One can track approximately six of these simultaneously; in general, the horizontal accuracy of measurement is about +/- 200 meters with respect to the ship. The accuracy of presetting the depth is about +/-70 meters. On many of these instruments we have fitted small ballast pumps which permit us to have a command control of depth, and we are able to place them within approximately 2 meters of target. The sound system is used for relocation and also telemeters the environmental data. These are not suited for broad scale open ocean studies because they require a ship to stay with them. You cannot leave the area for more than 3 or 4 days or you are not likely to find them when you get back. However, they are very suited to specific, clear-cut scientific investigations. Over the past number of years together with Arthur Voorhis at the Woods Hole Oceanographic Institution, we have set out to measure vertical and horizontal displacements in several studies, such as the partition of energy, (what fraction of the local horizontal kinetic energy is carried in the vertical potential energy and internal waves?), studies of large-scale convection mixing taking place at the break between the slope and the shelf, studies of upwelling, and studies of thermal microstructure.

The next instrument, or approach, I would like to show you is the work of John Swallow in England (figure 2). He was anxious to be able to work with a greater range from the ship, with many more floats and with much more convenient and faster tracking. During the MODE I experiment, he took about 30 of his floats to sea and had approximately 22 of them simultaneously in the water. The approach he took was to cause each of these floats to be transponding. He took the view that he was obliged to take hydrographic casts during the experiment, so he fitted the end of the cable with an interrogator which would call up all the floats, and they would reply. Moving around the area that he was studying each day, he would get three or four ranges to the floats. From these he could keep a running set of positions of all of his equipment. The maximum range is approximately 70 kilometers, and this is possible because the deep interrogator is much less sensitive to the local refraction pattern. The frequency is approximately the same as the vertical current meter. He obtained horizontal fixes, which are good to +/- 1 kilometer and part of that error is, of course, the fact that the ship moved between times that he was able to get a line of position.

In the United States we were dissatisfied with the performance that we were obtaining with the vertical current meter type instruments, and we set our sights on something quite different. Tom Rossby, at Yale, and myself wanted to avoid the costly ship tracking. We wanted to obtain frequent and accurate fixes from many floats in the ocean at the same time, and we hoped to obtain quite a long endurance. For the kind of experiment that we wished to do, it seemed to us that the gravest limit was the short time over which one could get records. So we set out to make the same kind of neutrally buoyant device which was able to signal, by sound, to land-based stations. Figure 3 is a sketch of the instrument which we used during the MODE I experiment. We operate these instruments at 1500 meters depth and use the deep sound channel for transmission during MODE I. We obtained ranges of 1500 kilometers. To avoid severe attenuation of the sound over such ranges, the gravest technical problem was to lower the frequency to 270 Hz. Endurance was to be 18 months. We think that it is possible to track up to 200 simultaneously; we have, however, only tracked 20 simultaneously. The horizontal accuracy and the absolute accuracy is about +/- 1 kilometer. Figure 4 shows the April to December 1973 tracks. Much of the inaccuracy is due to an ignorance of the sound speed over the long path to the receiving station. However, over a period of several days, the sound speed is stable so that scatter between success of fixes is smaller. During the MODE I experiment a number of these instruments were fitted with vanes and internal recorders which measure temperature, pressure, and the angular position giving us vertical current. In fact, we started putting these in the ocean in March of 1972, and nine of them are still working and giving us their position every 4 hours so that we have a number of trajectories in excess of a year's length. These floats are not able to operate over the whole water column because we rely on sound transmission by the deep sound channel, and we think that the operating depth is limited to approximately the range between 600 and 3,000 meters. These floats are quite large, but they are comparatively simple.

All of the American work on this and the vertical current meter was supported either by the Office of Naval Research or the National Science Foundation.

Figure 5 will give you an idea of what can be done with the vertical current meter. This is a record taken during the overturn of Mediterranean water in the Gulf of Lyons in February. You normally think of the ocean as quite strongly stratified, but it obviously has to break down some places sometime. We thought that this would be a place where it would, and here we have a direct measurement of vertical displacement of the water past the instrument. You can see that water is displaced here in excess of 500 meters vertically past the instrument. It is not the instrument moving because the lower record, the one marked meter, is the pressure record from the float and shows that it remained fairly close to the same depth, although it is pushed off its equilibrium position in the order of 100 meters by the vigor of this vertical flow.

DISCUSSION

Peter Hacken - Johns Hopkins University:

Have you ever thought about using some kind of a neutrally buoyant float on the shelf? Now I know there are problems in shallow water about getting a float to be a certain depth, but let us say you made something that sort of bounced or rolled along the bottom. Can you acoustically track such a thing from shore stations? Do you have enough signal if you have a small transponder on the float, up in the surface waters, in the mixed layer and close to a seasonal thermocline, could you track something over a distance of maybe 100 kilometers, in a depth of water that was perhaps 20 or 30 meters deep?

Douglas Webb - Woods Hole:

Well, Peter, I would not like to say 'no', but we are going to try and do the easy experiments first I think.

Peter Hacken - Johns Hopkins University:

Do you have any feel at all for how far, what kind of a range you can get with the power that your sources have now in surface waters?

Douglas Webb - Woods Hole:

I do not think it's a problem of power, Peter, I think it is a matter of getting an acoustic path. I would guess that it is very variable and uncertain, although I have not looked at that closely. We have been very interested in deep-water studies, like MODE, the mid-ocean dynamics, on a fairly large scale. I am sorry I cannot give you a better answer.

Rip Anderson, Sandia:

We have corresponded before about the Pacific. What accuracies, so far from the SOFAR tracking type approach might you expect, north of Hawaii? Or are you familiar with the SOFAR channels and tracking stations in the Pacific?

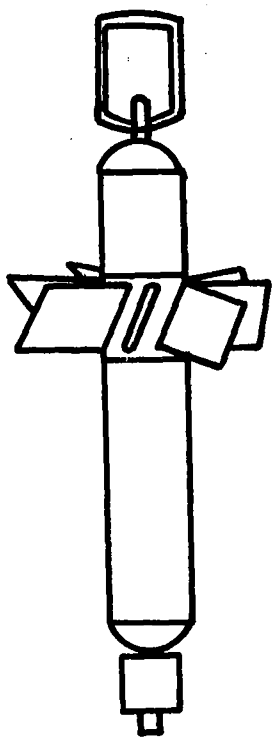
Doug Webb - Woods Hole:

It is hard to know what is the ultimate accuracy achievable, and we have not stressed that, because for questions of large scale circulation, the kind of records we have been getting are quite good. For studies of the fine-scale motion the relative accuracies are adequate. However, it is my understanding that problems that one would face if you sought maximum accuracy is primarily your uncertainty of the speed of sound over the whole path, and I think each situation you would have to look at for stability, from the historical data, how stable it looks and if you would squeeze it harder, I think you would just have to go out and measure it. Possibly some differential methods would help.

Jack Kane - Office of Naval Research:

I am not part of your community, but I control that Canary Island, Hydro Acoustic Station. I would like to see you later, and anyone else that is interested in possible uses. There is a change in status. I have managed to salvage it. It is going into housekeeping for 12 months while I give what I call the Hydro Acoustic Community a chance to get organized and see if we have any real uses for it. I would also like to talk with anyone interested in addition to the radar (over the horizon) work at HS, I also control other HS radars, as long ago as 4 years I had a NOMAD anchored in 18,000 ft. of water or 6,000 meters. Up to 40 North, it broke loose, we tracked this thing for about 30 days, that is we kept touch with it. I sent the Wanda River out of Miami, she stopped at Bermuda and picked up Carl Hardigan and on a 95 ft. hull he went up in those 30 ft. seas and recovered the NOMAD buoy. I put him within one mile, when he saw the light. We were about to tell him to come about, so we would like to talk--I did not come prepared to give a paper, but I would like to talk with people that are interested. We have facilities that we can make available to you.

VERTICAL CURRENT METER



RANGE: TO 25 KM

FREQUENCY: 5 KHZ

ENDURANCE: 4 WEEKS

MAX. NO.: APPROX 6

ACCURACY:

HORIZONTAL $\approx \pm 200^{m}$

VERTICAL $\approx \pm 70^{m}$

4' LONG
7" OD
75 POUNDS

FIGURE I

MINI-MODE FLOAT - J.C. SWALLOW

RANGE: TO 70 KM

FREQUENCY: 5.4 TO 6.8 KHZ

ENDURANCE: 3 WEEKS

MAX.NO.: 18

ACCURACY:

HORIZONTAL $\pm 1000^m$

VERTICAL $\pm 70^m$

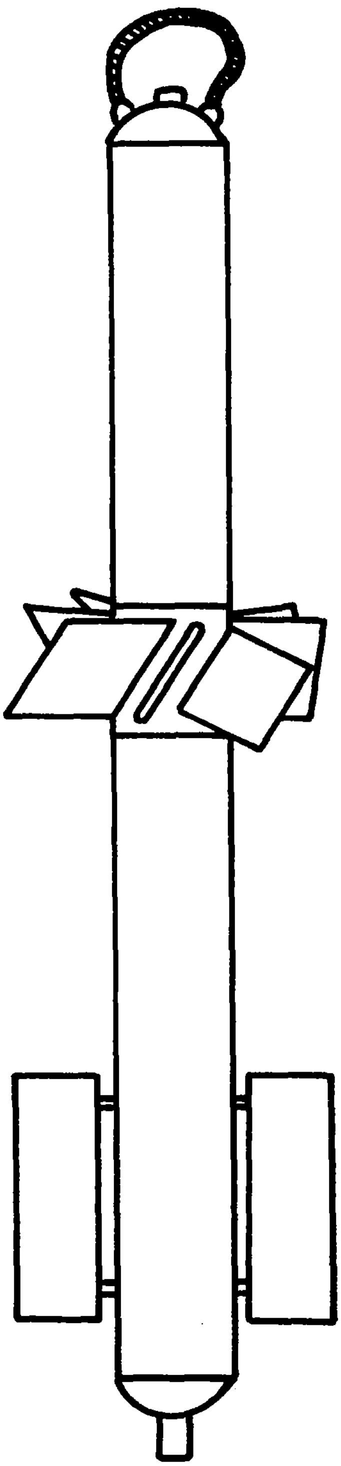
14' LONG

5" OD

130 POUNDS

FIGURE 2

MODE I SOFAR FLOAT



RANGE: 1500 KM OR MORE

FREQUENCY: 270 HZ

ENDURANCE: 18 MONTHS

MAX. NO.: 200

ACCURACY:

HORIZONTAL $\pm 1000^m$

VERTICAL $\pm 70^m$

19' LONG
12" OD
1000#

RANGE OF OPERATING
DEPTH IS LIMITED
-APPROX. 600 TO 3000^m

FIGURE 3

THE NAVY NAVIGATION SATELLITE SYSTEM FOR BUOY LOCATION

by

Stanley Turner

Navy Underwater Systems Center

We have an application in the Navy for location of distressed submarines. I am here to explain a system which will accomplish this task and may be useful in other areas; however, at the present it is still quite expensive. If you have any interest in the progress of the system, I would like to ask you to write to me. My address is:

Stan Turner
The Navy Underwater Systems Center
New London, Conn.
Telephone: 203/442-0771

I will be glad to forward you the progress.

How would you like to be able to walk into a room and there on a lighted wall display see the precise location of a free drifting buoy, or the locations of 30 free drifting buoys scattered across the Atlantic and also to see their individual tracks being displaced as you watch. We haven't done it yet, but we're just about to.

The system I will describe is designed to track and keep histories on the locations of 32 individual buoys. The readout tracks and histories are presented ashore. Also presented are the date, the time, the buoy latitude and longitude, the drift rate and the direction. This is displayed in tabular form on a TV screen ashore and printed on a piece of paper. Also, range and bearing between pairs of buoys that are separated hundreds of miles are displayed. We are just starting to plot out on a chart the tracks of these receivers and print out and display on the TV screen oceanographic data that are transmitted along with the positional information. Figure 1 is a simplified diagram of the system. The tiny navigation receiver is placed in as many as 32 buoys. There is also a communication satellite terminal transmitter in the buoy. It could also contain a conventional HF transmitter. As a navigation satellite passes, the receiver tunes onto the signal, counts and stores the doppler and also stores the navigation satellite message about the satellite's own location. After the pass, the stored data are transmitted via a synchronous communication satellite ashore for processing and display of the latitude and longitude. These buoys can be located anywhere in the Atlantic, using the particular satellite that I have been using or in the Mediterranean and a single shore base can locate them. Figure 2 shows a blocked diagram representing a conventional navigation receiver. There are four basic parts: a receiver for the doppler velocity data and orbital parameter data, a data digitizer, a computer which converts the satellite data to longitude and latitude, and a display unit.

The Applied Physics Laboratory of Johns Hopkins divided the receiver at the computer. They were able to store the doppler information and add the satellite's positional information ashore (figure 3).

Our system (figure 4) processes and stores both the satellite message and the doppler information and transmits them together. We store the location information in 1420 bits. The operation of the system is automatic; it does not require any operator other than initially setting up the shore base. Ours has been operating for months, with the receiver at a fixed shore location, and we've been getting 300 ft. accuracy 50 percent of the time, 500 ft. accuracy 67% of the time and 900 ft. accuracy all of the time. This is a single satellite pass on the average.

Now for the equipment itself. The receiver portion is approximately 4 inches in diameter and 12 inches long, and weighs about 5 lbs. It contains an RF portion and a digital portion, which store the information.

The transmitter can be a satellite transmitter or an HF transmitter, or it can also be hard wired into the shore station. The transmitter I've used successfully was made for a buoy which would be used in a distressed submarine situation. Information from the submarine about its best known location is stored in the memory and as soon as the buoy goes to the surface the transmitter transmits out that location, and the identity of the submarine. These data are transmitted several hundred times during the life of the buoy. The transmitter I have used is a 100 watt transmitter and weighs one pound. If a 5 watt transmitter would be adequate the lower 3 inches would not be necessary which would make it even smaller and lighter.

Improvements which I intend to make are to miniaturize it until it is just slightly larger than a coffee cup. This should bring the price down. At the present time the development cost on these has been approximately $13,000. We have been assured that ultimately the cost could come down to about $1,500, which is still pretty expensive, but they could be used for some other applications. Figures 5 - 8 show other applications with figure 8 showing the application for which it was designed.

DISCUSSION

<u>Bob Molinari, NOAA-AOML</u>:

I was interested in knowing how you determined your accuracy from the navigation satellite systems on the ship. They usually figure about a 1/4 of a mile to 1/2 of a mile accuracy.

<u>Stan Turner, NUSC</u>:

Well, this hasn't been tried at sea yet, so this is to be determined. One of the reasons for this development was to find out what kind of response the antenna would have at sea state or sea level zero, and dancing around..

Bob Molinari, NOAA-AOML:

And in terms of buoys, what is the endurance that you might expect if you put one of these in a buoy?

Stan Turner, NUSC:

Well, this depends on the battery and the battery life, but in order for you to evaluate this you should know this has been designed to date with accuracy in mind. I felt that if it were not the most accurate receiver that we could develop people would criticize it severely, so I made it without regard so much to the amount of power it consumed. Right now it does consume a lot of power--6 watts and it's a 12 volt system. But I think that we'll give more regard to the power consumption when we miniaturize it, for example the digital portion of it, we intend right now to put this in a square--1/4 inch by 1/4 inch by about 1/16 inch high. We'll do this work at the navy electronics lab. I should say the NELC in San Diego, where they have a nice large scale integration facility and I hope that we can cut the power down.

Fred Vokuvich, Research Triangle Institute:

What is the availability of your Navy satellite for civilian scientific work?

Stan Turner, NUSC:

For work that is being conducted by the U.S. and other nations which share the satellite, corporations perhaps like your own can use it. They usually have gone through organizations like mine as sort of a cooperative venture, but my contractors have used the satellite. So for availability, get hold of somebody like me and work with them. And now as far as how frequently you can use it, it's generally assigned in blocks of 4 hours each. But you can have block after block after block until you get really baggy eyed, like I am. I've used it, in fact in the earlier days, the satellites were my very own practically, using them for months and months on end. Nobody else was using them.

John McFall, LaRC:

I'm about ready to adjourn the conference and just start buying those things. They sound so good, and I've heard so many good ones over the past years. Now I'm going overboard a little bit in what I'm saying, but a lot of times we've heard these 'one-of-a-kind' type of things, so the question I have is, what kind of a time frame do you see for general availability of this type device and also what kind of realistic cost within the next 5 to 10 years?

Stan Turner, NUSC:

Well, here's what I've done. I felt that I've operated on a budget that's really a starvation type, and I felt that if I couldn't continue this, I'd like to see the world have it. What I intend to do is publish, in great detail, the information--schematic values and what have you on this and let everybody have it. Then the competition and the brains of the nation would improve it. It's a lot of fun-this little navigation satellite receiver. It's just a two conversion type of receiver and the digital portion of it is straightforward, and if I can understand it anybody can. It's really cheap--I think it'll get down to the point where they can be made as cheaply as Japanese radios.

John McFall, LaRC:

That sounds great. A couple of years ago, I started talking to a satellite positioning company--anybody here from there--the cost was prohibitive and we didn't pursue it any further.

Stan Turner, NUSC:

Oh yes, listen this thing here, that's one of the reasons why I wanted to bring it out. So that people could use it. Because the Navy purchased theirs at $100,000 to begin with and then 75 and 50 and $35,000 is sort of the cost now, but of course that includes a computer and so forth, whereas, one of the other developments that I'm gonna try to do is to program some of the little micro - mini computers that are coming out now and put that back in the buoy. It's very attractive to me and the one thing it would do is to reduce the number of bits transmitted from 1420 to about 60 for the latitude and longitude and for identity.

John McFall, LaRC:

Then there are other costs aside from the transmitter. Can you give an idea of those?

Stan Turner, NUSC:

Sure. The antenna, for example, being on a starvation diet, I got from Chris Craft. It cost $280 and I had it designed so that it would go up the elevator at the Navy Electronics Building. It's something that they just send to you UPS. It weighs maybe 40 lbs. and I wanted to install it in the Navy Electronics Building in Washington, to put on the first demonstration before the oceanographer of the Navy. The receiver, which again is a fairly inexpensive receiver--I'd say it cost, well it cost $1,500. It has a crystal IF amplifier filter and the rate at which we transmit the information just fits inside of that IF band width of about 400 hertz. Now it transmits up at 302 megs and transmits down from the communication satellite at 250. And the ability of our transmitting oscillator is--I'm just going to say without putting some figures on it--well, it holds within about 2 cycles. The reason I did that is because I didn't want to have a preamp before the transmitted message. So that,when this turns on, it just goes right down the center of that IF band and the data are immediately received. There's no space lock loop scanning circuit involved. There it would have to scan during the time the preamp was on and lock onto it. I've used this up in Thule, Greenland, as far as 76.5 degrees North latitude and down the coast of the United States for demonstrations and it works beautifully. Now that costs about $1,500. I have a television display, which cost another, with the character generator another $1,000. You can imagine that the shore base is not very expensive. It's all developed, you're welcome to it. I also feel however, that for the navigation satellite system, I use a Nova 1200 computer, and 8000 bits of memory to do a number of things. And it has a typewriter type of display. A printer output. Nova costs around $3,000 or $4,000 plus a couple of thousand dollars for memory, and the typewriter costs a thousand dollars and the TV again--well the TV monitor itself costs about $300. But really once you get that you're in business.

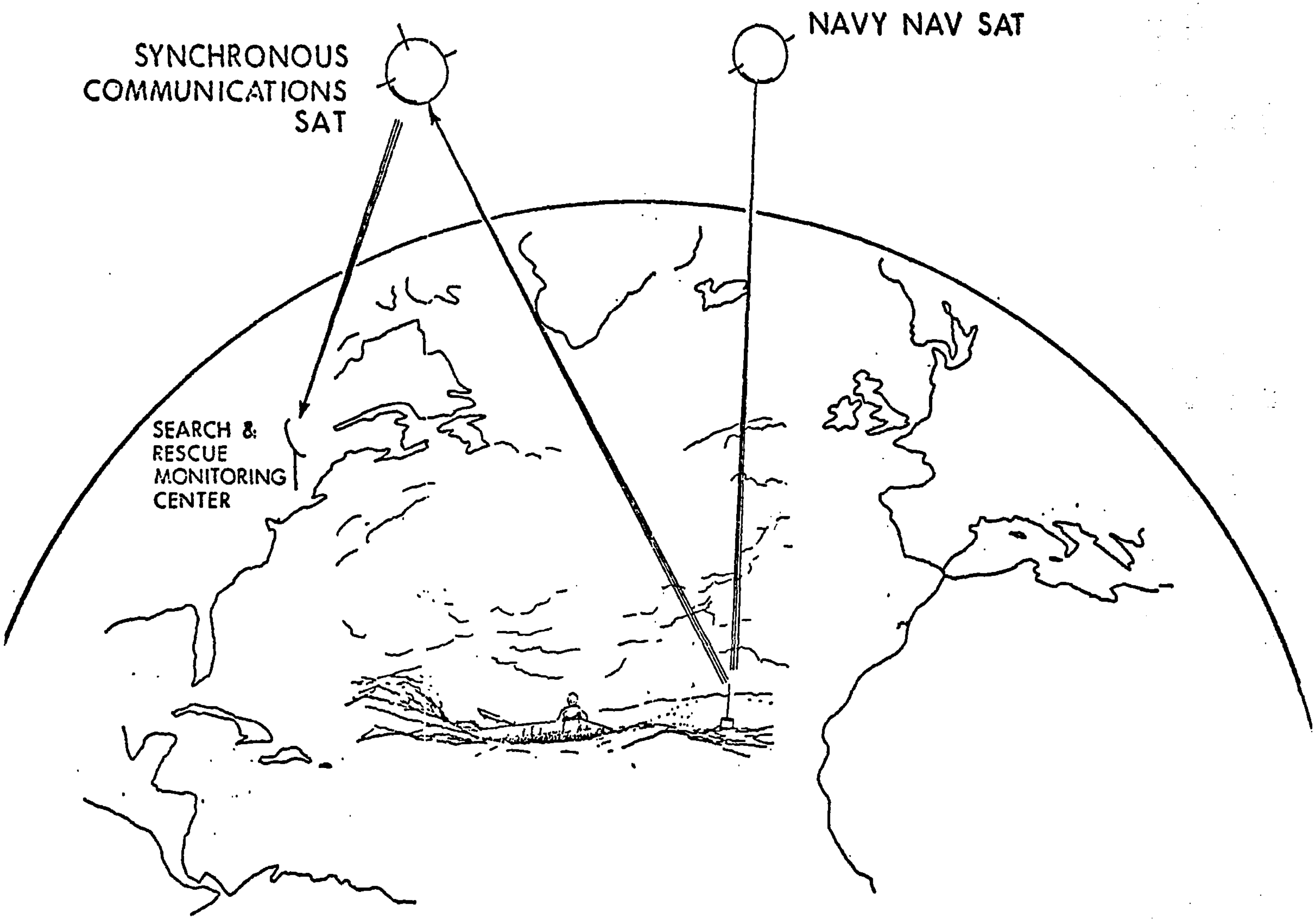


Figure 1. SIMPLIFIED DIAGRAM OF THE TURNER NAVIGATION SATELLITE RECEIVER SYSTEM

Figure 3. CONVENTIONAL SATELLITE NAVIGATION EQUIPMENT WITH SPLIT LOCATIONS

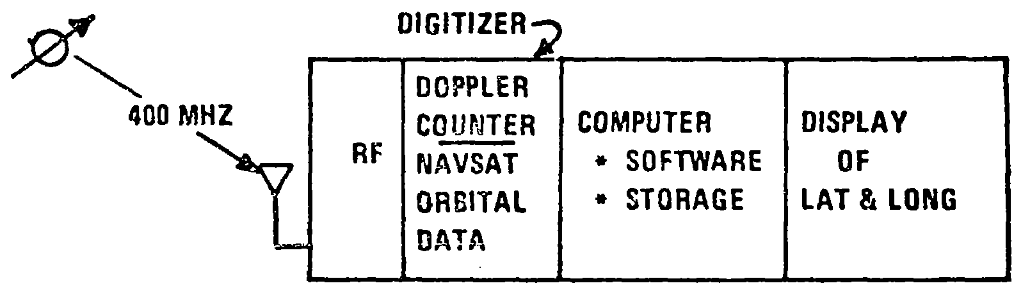
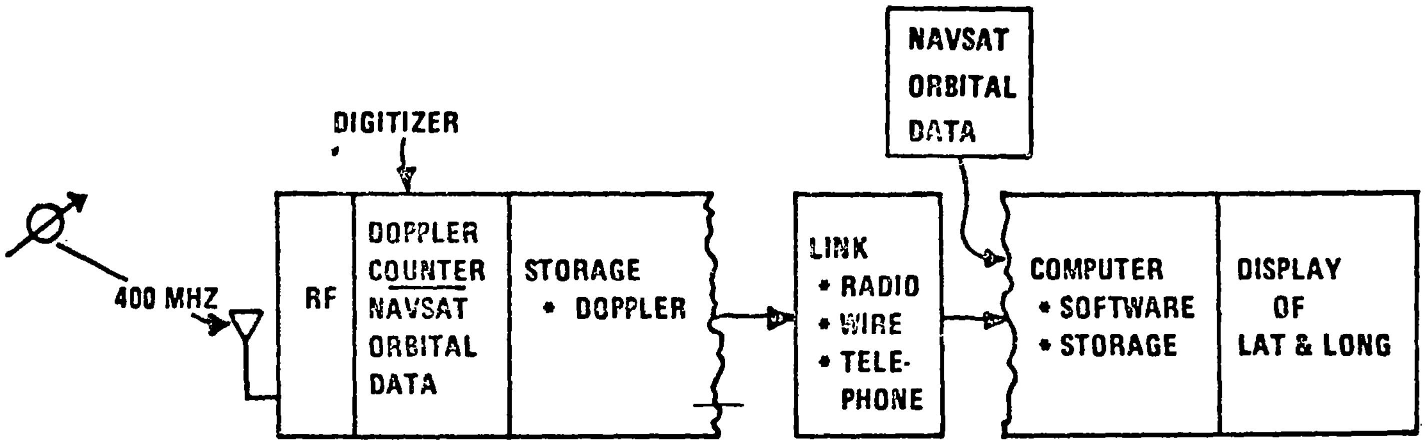


Figure 2. CONVENTIONAL SATELLITE NAVIGATION EQUIPMENT


DIGITIZER
400 MHZ
RF
DOPPLER COUNTER
NAVSAT ORBITAL DATA
STORAGE
• DOPPLER
LINK
• RADIO
• WIRE
• TELE-PHONE
NAVSAT ORBITAL DATA
COMPUTER
• SOFTWARE
• STORAGE
DISPLAY OF LAT & LONG

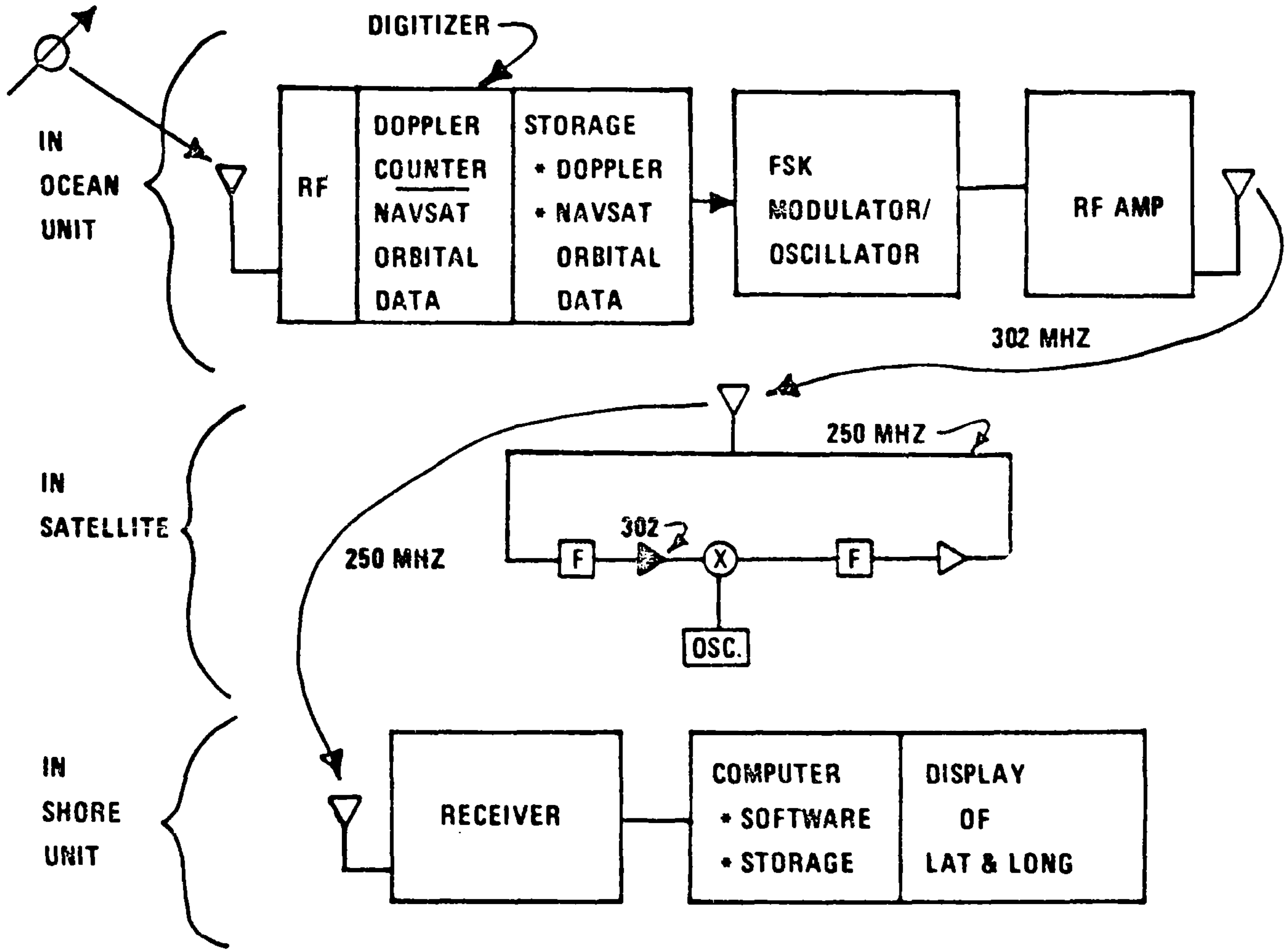


Figure 4. TURNER NAVIGATION SATELLITE RECEIVER SYSTEM EQUIPMENT FUNCTIONS WITH SPLIT LOCATIONS

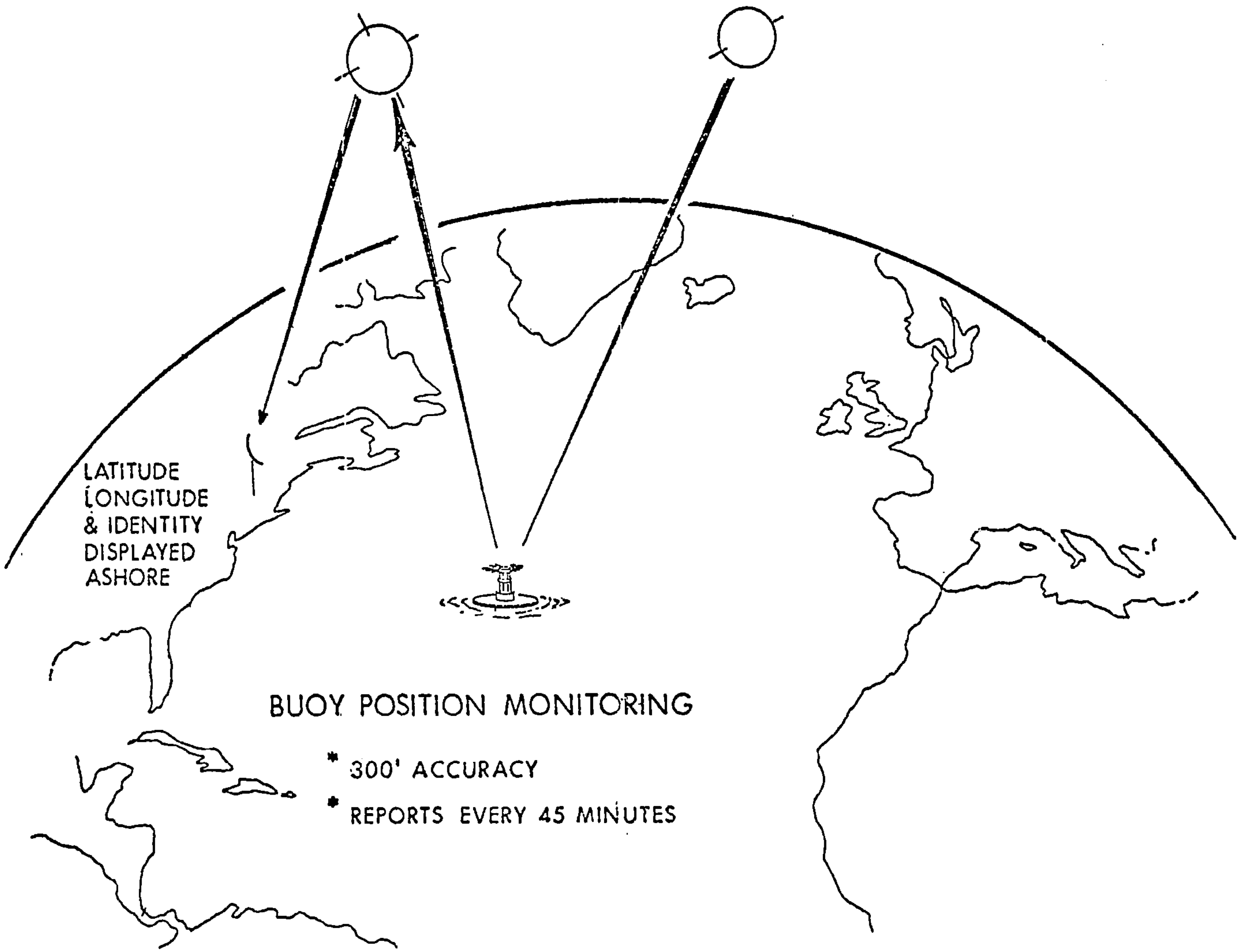

LATITUDE
LONGITUDE
& IDENTITY
DISPLAYED
ASHORE
BUOY POSITION MONITORING
* 300' ACCURACY
* REPORTS EVERY 45 MINUTES

Figure 5.

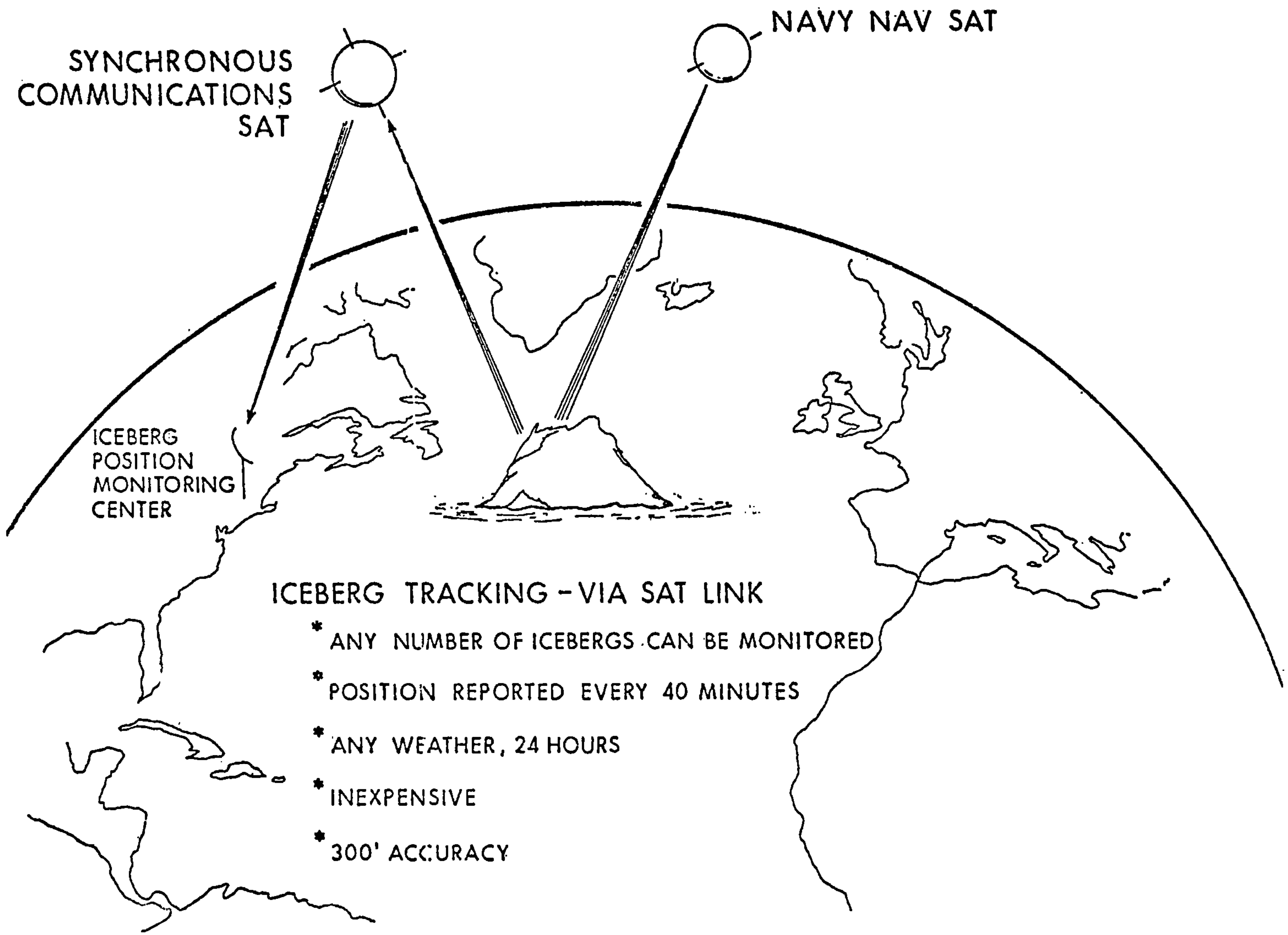

SYNCHRONOUS COMMUNICATIONS SAT
NAVY NAV SAT
ICEBERG POSITION MONITORING CENTER
ICEBERG TRACKING - VIA SAT LINK
* ANY NUMBER OF ICEBERGS CAN BE MONITORED
* POSITION REPORTED EVERY 40 MINUTES
* ANY WEATHER, 24 HOURS
* INEXPENSIVE
* 300' ACCURACY

Figure 6.

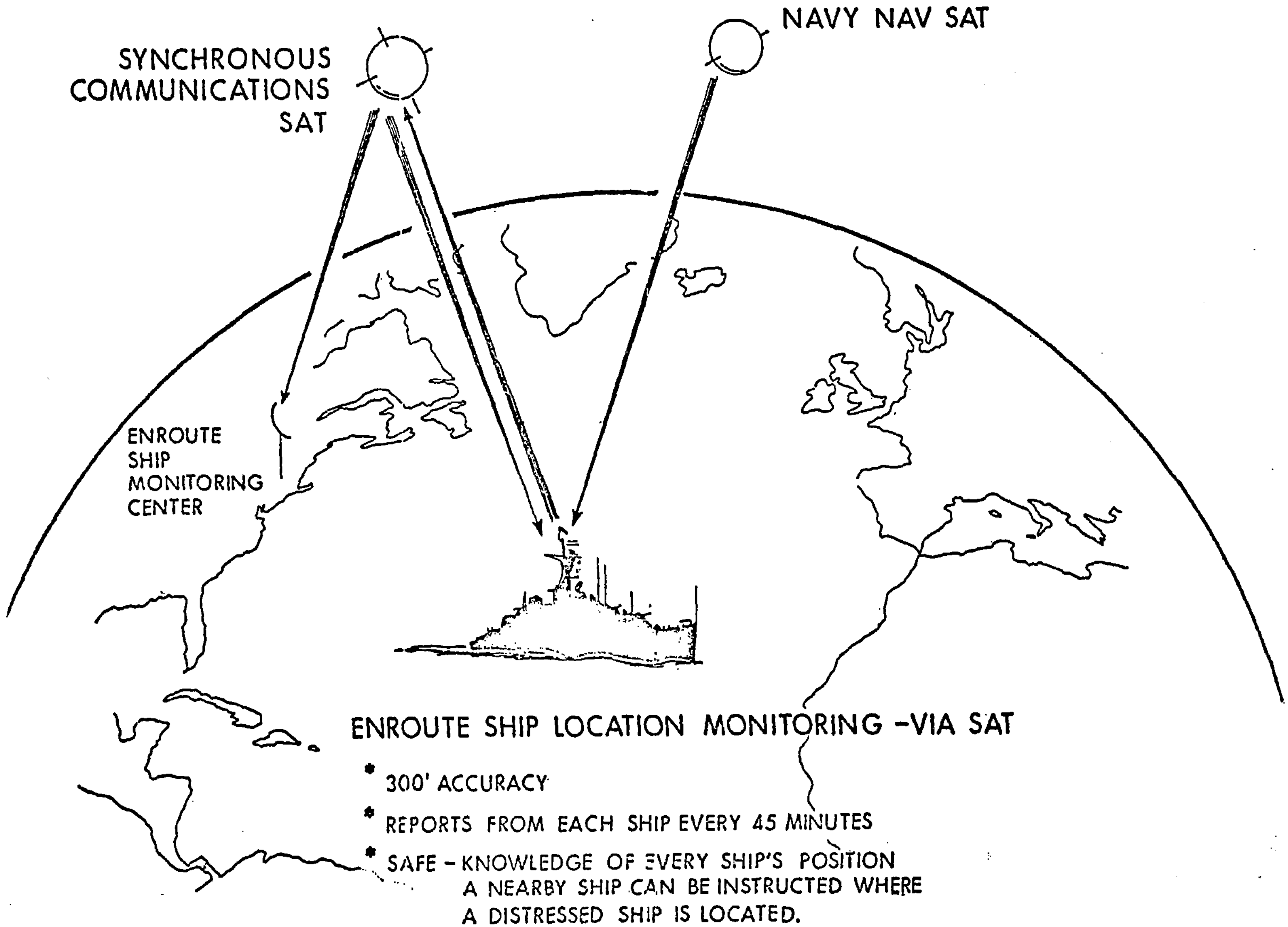

SYNCHRONOUS
COMMUNICATIONS
SAT
NAVY NAV SAT
ENROUTE
SHIP
MONITORING
CENTER
ENROUTE SHIP LOCATION MONITORING -VIA SAT
* 300' ACCURACY
* REPORTS FROM EACH SHIP EVERY 45 MINUTES
* SAFE - KNOWLEDGE OF EVERY SHIP'S POSITION
A NEARBY SHIP CAN BE INSTRUCTED WHERE
A DISTRESSED SHIP IS LOCATED.

Figure 7.

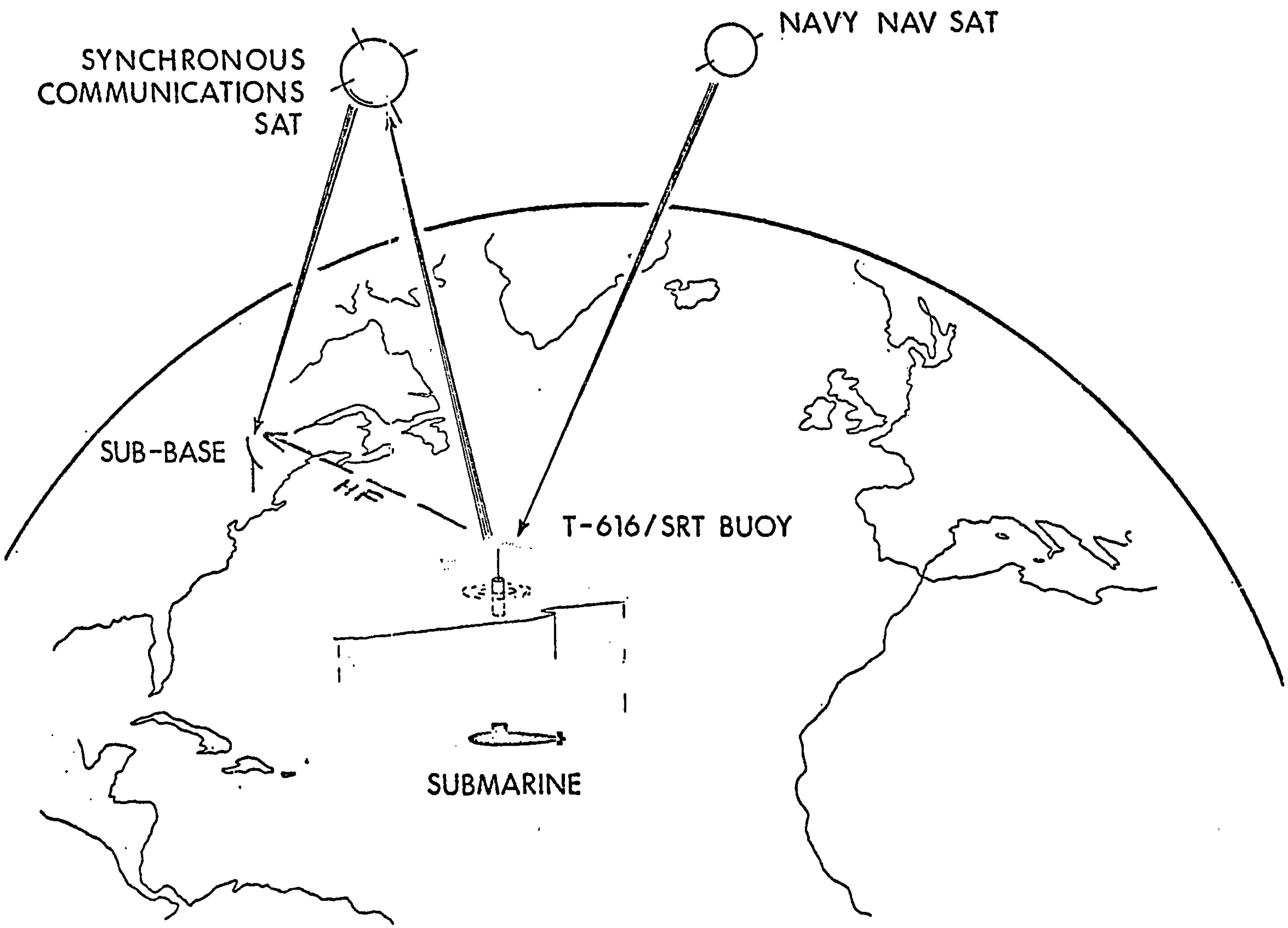


Figure 8. DISTRESSED SUBMARINE LOCATION

SESSION B

Deployment and Retrieval

Chairman: Evon Ruzecki, Virginia Institute of Marine Science

DEPLOYMENT AND RETRIEVAL OF TWO BUOY SYSTEMS

by

Charles Kearse

NOAA Engineering Development Laboratory
Ocean Engineering Branch

Deployment and retrieval of systems in the ocean, I think as all of us here who work in the ocean know, is something that is usually left until the very last in the consideration of a system design. Many dollars are spent in designing the device, producing it, and finally at the very end, when the system is proven and ready to go, people usually consider how am I going to put this in the ocean? Because the oceanographic community is small, we utilize the vessels that are available. Sometimes they aren't always practical, and I guess we have more equipment fail during the deployment and retrieval, than we do on the bench. So as you can see, it's a difficult task and it's something that I think we at EDL have tried to adjust in our latest effort of putting systems in the sea. This afternoon I'm going to talk about two of these systems that are directly related to free drifting buoy systems. This is the OPLE system and the EOLE which Don Hansen from AMOL will talk about later on tomorrow. I will cover the deployment of these buoys -- I can't say retrieval even though several of them were retrieved because they were designed to have a lifetime at sea and disappear and not be retrieved, due to the cost of ship operation.

Back in 1968 we were involved in a program with NASA; the Navigation and Communications Division of Goddard Space Flight Center. They had a requirement for the OPLE system to be tested from a buoy, and we entered into a cooperative program with them at that time to mount the system on a buoy and to test it. This took place in May of 1968, which was some time before the EOLE drift. The buoy had a large VHF turnstile antenna on top and also on top of that a VLF antenna. The canisters which contained the OPLE package, also contained some engineering sensors to look at the tilt of the buoy vs. dropout of signal from sea conditions. Also a beacon was included so in case the OPLE system failed we could go back and relocate the buoy. I only talk about this because it's a little bit of history, but also the fact that the free drift of the buoy was delayed due to a handling mishap during an actual retrieval. We performed three tests with the buoy -- one was a test in Biscayne Bay to determine the feasibility of the satellite link back in 1968 from a buoy this size. We had about a 60 percent data return which said that we should proceed. Next, we moored the buoy off of Florida and looked at the response of the buoy from a number of interrogations that Goddard attempted. In the first couple of weeks we had a very low response. We had something like less than 20 percent favorable response from the buoy. This was due to an interferring signal on the pass band. At which time we changed the frequency of the OPLE package and we went up to 90 percent return. With about 90 percent we couldn't very well correlate the engineering information of buoy tilt to signal dropout on interrogation or data relayed back from the buoy because the data return was very high. Upon completion of the mooring test, which was a period of 30 days the last portion was a free drift in the Gulf Stream from a NOAA vessel, in cooperation with an experiment with AMOL who were also putting out drogue buoys at the same time. During the deployment things went very smoothly with absolutely no problems until they got to the point of deploying the drogue chute, which was to be located at 30 meters. It was the last part of the buoy system to go overboard; the buoy first, the drogue chute last. The drogue chute got entangled in the stern or a clete on the stern and ripped. Well they wanted to do this experiment absolutely right so they proceeded to come about and recover the buoy system in total and replace the drogue chute. In recovering it unfortunately the skipper of the ship came upwind of the buoy, because that way he would give the buoy a lee. Unfortunately the buoy got in the shadow of the vessel, the lee was formed, the vessel continued to move and the buoy did not. So unfortunately the VLF antenna on the top was broken off when it collided with the side of the vessel before they could get the ship off the top of it. This delayed the free drift some 3 to 4 months, because of availability of a second vessel. Also replacing the VLF antenna delayed the drift because there were very few around at that time. So that mishap delayed the program, and we also had a loss of data and a loss of equipment. But in May of 1968, and I'm not trying to get into the scientific end of it because I'm really talking about deployment and retrieval, we did perform a free drift of the system from Miami up to West Palm Beach. It covered some 26 hours free drifting with the drogue chute again at the 30-meter level and it was tracked with a NOAA vessel using Loran A. The NOAA vessel, Gulf Stream, came out from Ft. Lauderdale and took a high fix position. The offset between the two tracks was about 1.2 miles to the northeast, and this essentially is the same error that we saw down at Fiery Rock on the mooring.

The second program that we've been involved in with free-drifting buoys is the EOLE buoy. EDL provided support for AMOL in developing the system to be deployed at sea. It consists of a 41 foot spar buoy which was originally designed by the Navy. This was a fast reaction type program so a lot of time was not available to design smaller lighterweight packages to be used in the ocean. The instrument cannister located in the center contained the EOLE package and electronics. I had a sensor package which extended off the bottom down to a depth of 100 meters. The buoy was constructed of aluminum and the EOLE antenna was located at the upper end. The vessel which was ultimately available to deploy these systems, was the R.V. Gillis from the University of Miami. To proceed in an orderly manner, as we try to do, we have pretty much insisted on dockside tests, sea-trial tests, sea tests with dummy sensors and then finally the full-scale deployment deploying five of these buoys north of Puerto Rico. We find that making our engineers write a test plan fully describing what is to be accomplished at sea, describing the deployment retrieval methods, alternate methods and then reviewing this test plan with the operating people on the vessel (the captain and the crew members) works well. Performing a dockside test before you go, and if the ship time is available, doing a sea test prior to actually installing equipment has also been very successful. Five of these 41 foot spar buoys were deployed without mishap, except one minor incident which was not a problem of the deployment system. There was almost no loss of ship time and all systems operated. Because the buoy was 41 foot long and we had to carry five of these buoys onboard the Gillis, deck space became of prime importance. It was difficult to allow sufficient room to work around the buoy system, to assemble them or disassemble them if needed. Also, it was necessary to store the antennas in the vertical position, so that we could get satellite fixes as the ship was proceeding to station. The buoys were essentially in two sections. The chore at sea was to bring the upper-middle section to horizontal on the deck then bring the lower spar section in, assemble the two sections, and then deploy the buoy over the side. The stations where the buoys were deployed were approximately 60 miles apart, our test plan calling to do two deployments per day.

Once the buoy was laying on the deck the lower spar section was brought in and the two were married up in a cradle, where things fit together very nicely. The system was then essentially checked out and ready to go. We used a lot of steady lines, which we believe in to maintain the lateral control necessary to keep this large object from starting to move. Because we used a fixed crane, we end up with a fairly large pendulum, so we have considerable weight that starts moving around and that's when things usually start happening. The buoy was deployed from a single point pickup , with the antenna near the top and the ballast plate at the bottom end. The ship was in a slow turn, proceeding very slowly ahead so that as the buoy is deployed it goes over the side and he's turning away from it so that we don't have the problem of the ship drifting on the buoy or the buoy drifting on the ship.

A salt lick which is located at the head of the drogue chute is used to get it to depth. It dissolves in a very short period of time. An additional floatation collar was located in the upper area in case a leak developed in the buoy. This would add additional floatation to help it survive in case someone happened to be near or actually during deployment.

As I said the system wasn't really designed to be retrieved. It was designed to be deployed, but we did go through the retrieval procedure so that in case problems did develop with the buoy the system could be retrieved and repaired on deck, as these buoys are expensive. The test plan that was written and the procedures that were altered after the test cruise, I think contributed to the fact that the operation went quite successfully with only minor problems during the actual deployment. One problem did develop during deployment. As they were picking the first buoy off the deck a weldjoint on the lower ballast plate parted on the first buoy over the side. It resulted in simply replacing that lower section with a spare section. The other buoys were subsequentially looked at very carefully and we could see no signs of any other type of weld problems. The buoys were all deployed two per day. The buoys were checked out as they were deployed, using the EOLE simulator onboard the Gillis, so that we knew they were operational prior to the vessel leaving station. The buoys were designed for a minimum of 3 months at sea. Some of them actually operated 9 months to a year at sea. One buoy from the original group of five has been recovered. That was recovered by the Virginia Key and is now back at AOML in their lobby as a showpiece. The other buoys I think will crop up somewhere in future years. Somewhere somebody will report them still floating around in that part of the ocean.

This was an interesting situation. The ship wasn't designed to handle this system, but with the correct handling procedures, was able to safely deploy the system. I think it just points up the fact that ocean engineers or buoy people have to start considering the shipboard handling problems when they start designing the system on paper, so that the systems all come together and we can safely deploy and retrieve these systems without losing them at the rail.

QUESTIONS

Speaker Unidentified:
I think your talk points out, maybe the need to consider the expendable buoy. I wonder if you agree with that. A low cost buoy that you release and you don't recover it. You just design it for that purpose.

Charles Kearse, NOAA
I think that that's very true. The cost of ships, at least our vessels are becoming prohibitive--in the neighborhood of four to five thousand dollars per day for our larger vessels at sea, and either a deployment mishap or handling problem or equipment problem and the ship time itself is added to the cost of the buoy. I think this all has to be considered in the total deployment picture. Not the fact that you may actually

get a free ride with some vessel that can put your system in, but the fact that it does cost money out of someone's pocket, sooner or later. Smaller buoys, lightweight buoys, expendable buoys that can be easily deployed at sea from whatever type of platform besides vessels. Using helicopters or aircraft, I think is certainly the way the community should be focusing their attention in the future.

TACTICAL AUTOMATIC WEATHER STATION (TAWS) BUOY DEPLOYMENT AND TESTING

by
James B. Russell
Naval Avionics Facility, Indianapolis

I would like to begin by giving you some background on NAFI. NAFI is an acronym for Naval Avionics Facility, Indianapolis. NAFI has been designing and/or fabricating buoys and buoy electronics almost continuously since 1956. At present, NAFI is developing a so-called TAWS (Tactical Automatic Weather Station) buoy. NAFI began working in 1966 on the TAWS buoy development. Naval Air System Command commenced funding of this program by requesting that NAFI study and report on the feasibility of such a system. The buoy was to be an air droppable expendable drifting buoy compatible with existing communications equipment aboard the P3 aircraft. Design considerations included meteorological parameters and sensors, deployment techniques, buoy configuration, transmission and modulation techniques, and compatibility with existing P3 equipment. This initial work resulted in a technical report entitled "Design Requirements for a Tactical Automatic Weather Station System." This was published in February 1967. Some of the

The Author: Mr. Russell received his Bachelors Degree in Electrical Engineering from the University of Louisville in June 1965. Since then, he has been employed by the Naval Avionics Facility, Indianapolis in the Meteorological Branch of the Functional Research Division. He obtained a Masters Degree in Oceanography from the University of Michigan in 1968, a Masters Degree in Aeronomy and Planetary Atmospheres in 1970 and has completed all course work in preliminary examinations for his Ph.D. He is presently Project Engineer of the Tactical Automatic Weather Station program at NAFI.

pertinent conclusions and recommendations made by NAFI in that report were as follows:

> The TAWS should be a spar type weighing from 200-400 pounds, ranging from 10-14 feet in length, and from 1.5-2 feet in diameter in its stored configuration. In its deployed configuration, it should extend to 30 feet or more in length, and have a reliable telemetry range of 200 miles from buoy to P3. One month buoy life was considered desirable.

Following this initial study, funding was dropped to a minimum, allowing only for development of a wind sensor suitable for the TAWS buoy, as it was originally envisioned. Early in 1971, NAVAIRSYSCOM decided that NAFI should try to configure the TAWS buoy in a much smaller package than had been proposed in the original study. It was to be compatible with an A-size sonobuoy configuration. A-size sonobuoys have storage dimensions of 4-7/8 inches diameter and 3-feet long. Because of this size restriction, some of NAFI's original conclusions and recommendations have been modified. Nevertheless, with an increased level of funding, NAFI has been developing hardware to satisfy this new requirement by taking advantage of existing sonobuoy and P3 technology (the P3 is an ASW aircraft from which sonobuoys are operationally launched).

To date, two field tests--one in 1972, and one in 1973--have been performed to evaluate the NAFI buoy design. These tests involved launch, interrogation, and reception by a P3-C aircraft, as well as the recovery of buoys and parts for post-analysis of drops. Launch was accomplished at aircraft speeds of 180 knots or below and from altitudes between 700 and 2000 feet. A one-stage parachute system is used for deceleration. The parachute is released on impact. The water entry velocity is approximately 85 feet per second. An inflation toroid filled by four 10-1/2 gram CO_2 cylinders on impact is used to provide the buoy additional buoyancy in the water. A mast extends upward to raise sensors and antenna approximately 2 meters above the surface, and a keel extends downward to provide hydrodynamic damping. A mast and keel extension will be demonstrated following this presentation. The mast and keel are patented items, called Stacers, built by Ametek/Hunter Spring in Hatfield, Pennsylvania. This technique of reducing storage space could be used very advantageously by some of the buoy systems previously discussed, especially those envisioned as air deployable. In addition to the demonstration, a brief film will be presented showing a complete deployment as it occurred in December 1973 off the coast of Key West, Florida. Photographic coverage was obtained from the aircraft as well as the surface observation vessel.

The TAWS buoy life of approximately 7 days is primarily determined by the loss of the CO_2 from the inflation toroid. In order to reduce size and weight, the buoy is now using new lithium cells--five D-cells in series providing energy for at least 10 interrogations per day over a 7-day period. The interrogation and reception range is now approximately 100 miles.

The buoy will sense the following meteorological and oceanographic parameters: barometric pressure, air and surface water temperature, relative humidity, wind speed, and ambient sea noise. There are nine telemetry channels unused or temporarily being used for "housekeeping" during the design stage which could be used for other sensors, e.g., a thermistor chain to obtain a water temperature profile.

At present, NAFI is finalizing the design of the TAWS buoy in its sonobuoy configuration. Thirteen buoys will be fabricated by NAFI by the end of FY 1975 with a technical evaluation (TECHEVAL) in June of 1975. In this TECHEVAL, we hope to use onboard P3-C systems so that data from the TAWS buoy will be read out in a number of ways. One of the systems will involve an analog, FM recording; one will involve a digital printout on a teleprinter; and one will involve a digital data link, which will send the data over an HF radio link back to a land station. In addition to this, meteorological observations and buoy data reception will be made from a ship for comparison purposes. Data from the buoy will be read out directly on a NAFI-built display.

The primary purpose of NAFI's development is to provide an air deployable buoy system capable of measuring, telemetering, and displaying certain meteorological variables. It is not intended as a drifting buoy whose primary mission is the gathering of ocean current information for ocean circulation studies. In fact, it does not presently have that capability. However, NAFI is studying the possibility of incorporating an Omega Navigation System in the buoy so that could be located precisely and thus, with time, could actually be tracked so that some indication of surface currents could be provided. In addition to this Omega system, there is a so-called Sonobuoy Locating System (SLS), which is being studied to improve the ability to locate sonobuoys. The present method of locating a sonobuoy is to use a signal strength meter in the aircraft to locate a null which thus pinpoints the location of the buoy as being directly below the aircraft. This technique can probably locate the buoy to within a mile or so. Also to make it more useful in the context of this conference, a small drogue chute might be used to present a drag surface at depth. To extend the buoy's life, a self-inflatable, foam-filled floatation collar might be considered along with an increase in battery size (probably with a loss of some other feature).

Since the TAWS buoy is eventually to be expendable, there will be no discussion on retrieval other than that concerning developmental testing. I say eventually because at present some refurbishing capability has been incorporated into the design of the buoy so that the cost of development will be less. The refurbishing capability follows closely the refurbishing kit concept used in sonobuoy development in which certain items such as battery, hydrophone assembly, etc., can easily be replaced on a buoy. The electronics, which may account for 2/3 of the total buoy cost, may be reused. The first year's design models cost $6,000 each for a quantity of 3. Of this total cost, $4,000 was needed for the electronics. This cost should come down appreciably in large quantities.

Before proceeding further, mention might be made of sonobuoy technology that might be useful to those interested in developing small, low-cost, air-deployable buoys. Studies involving both theoretical analysis and experimental testing associated with water entry have been performed by Naval Air Development Center in Warminster, Pennsylvania. A useful report resulting from this work is Sonobuoy Hydroballistic Testing, Report No. NADC 73248-30, 31 December 1973, by Edgar A. Reed. This report may provide guidance in determining the shock loading on a buoy entering the water at various angles and with various shapes. Work has been done at the Naval Ammunition Depot (NAD) at Crane, Indiana, both on theoretical (verified by experimental) and experimental analysis of antenna patterns and propagation from a simple omnidirectional antenna operating at approximately 160 MHz. Consideration has been given to the antenna's changing position as caused by ocean waves. Antenna ranges from determining propagation patterns are available at NAD, Crane, and the sonobuoy test range in the Virgin Islands.

Figure 1 illustrates the TAWS system operation. It illustrates the buoy coming out of the P3 aircraft launcher and being decelerated by a parachute. The sonobuoy launchers consist of more than 30 individual tubes from which a buoy can be launched automatically and remotely. The parachute is a 12-inch x 42-inch cross chute. The 24 transmissions per day for a 7 day life should read 10 per day. The interrogation link uses the command transmitter (AN/ASA-76) used by the Command Active Sonobuoy System (CASS) operating at approximately 290 MHz. The buoy information is transmitted at approximately 160 MHz and is received on the standard 31 channel sonobuoy receiver (AN/ARR-72).

Other P3-C equipment that may be used in the TAWS system is the onboard computer (CP-901) for data processing and formating, the teleprinter for onboard display, and an HF digital data link for transmitting the data to other aircraft or to a land-based Tactical Support Center (TSC).

Figure 2 shows one of the early TAWS models in its stored configuration and a dummy unit used in a deployment test at NAD, Crane. The extreme color contract (yellow/black/orange) used on the TAWS model was to aid the photographer in seeing and following the buoy in descent and to aid divers in recovering the buoy from the bottom of the very low visibility lake. A dummy unit deployment always preceeds the live drops to orientate the photographer, observers, divers, and pilot.

Figures 3 through 5 illustrate the launch sequence from the helicopter. Figure 3 allows you to see the door on the side of the helicopter through which the buoys were launched. The launch is accomplished by using a static line between the buoy parachute and helicopter. This line pulls the parachute out of its canister but is light enough to break easily without tearing the chute material once the chute is open. The launcher in this case is a man secured to the plane by a gunner's belt who steps up to the door and throws the buoy out. Figure 9 shows that the buoy has stabilized in the necessary vertical position before water entry (launch from less than 1000 feet). This parachute system has provided the buoy with very stable flight with virtually no spinning or swaying.

Figure 6 shows the diver's boat racing for the drop site to recover parts that are released from the buoy on impact and the buoy itself in case of a failure. This recovery, even of parts that are discarded on impact, serves to identify minor problems in deployment and saves development costs if the parts can be used again on other buoys.

Figures 7 through 9 are underwater photographs of the buoy. Figures 7 and 8 show the buoy near the surface from two different angles. They show the electronic section and the CO_2 floatation collar. Figure 9 shows the hydrophone used for making ambient sea noise measurements. It is designed to sit at 60 feet below the surface. Eventually, it is hoped that the correlation between ambient sea noise and conditions at the sea surface will yield a sensing technique for sea state.

Figure 10 shows the buoy in its deployed configuration after it was returned to a land site. Above the floatation collar is seen the buoy mast supporting the sensor platform and 1/4 wavelength (at approximately 160 MHz) monopole antenna with 4 ground radials of 1/4 wavelength. The mast is a product of AMETEK/Hunter Spring sold under the trade name Stacer. The mast and release mechanism is initially stored in a cylindrical housing approximately 6-1/2 inches long by 1-1/2 inches diameter. The release is accomplished by removing a clamping device used to hold the tip piece in position. This is done by firing a pyrotechnic device using a 1.5 volt seawater battery and time-delay electronics. The cable seen encircling the mast contains the sensor leads and antenna coax and provides a vent for the pressure transducer located within the buoy. Figure 11 shows a picture of a slightly later model of the TAWS buoy in its stored configuration.

Figures 12, 13, and 14 are pictures taken at a second facility located in Indiana that has been used for TAWS buoy tests. The site is Eagle Creek Reservoir in Indianapolis. Tests have been run there on buoy life, command link characteristics of the buoy and surface vessel as a function of distance and figures 13 and 14 show the buoy in slightly different surface wave conditions.

Figures 15 through 18 show another sequence of pictures depicting a launch from a helicopter at Key West, Florida. Figure 15 shows the launch helicopter furnished by VX-1 Squadron stationed at Boca Raton, Florida (now stationed at Patuxent River). Figure 18 shows the divers' boat moving in on a buoy just dropped. The divers here were furnished by the NADC field station in Key West, whereas for test in Indiana, NAFI furnishes its own divers. Figure 19 shows one of the smaller vessels (36 feet long) used to observe and coordinate buoy tests. These boats are operated by military Navy personnel.

Figure 20 shows three buoys after they were deployed and returned to shore in Key West. Note the cylindrical extension below the buoy shell. It serves as a keel for damping buoy motion. The large cylinder on the bottom serves as the housing for the keel, so that in its stored configuration the keel consumes only about 2.7 cubic inches but extends to a length greater than 3 feet. Figure 21 shows the latest design in hydrophone assembly. The yellow cup serves to store hydrophone, preamplifier, ballast, and 60 feet of cable and stretchable cord, and to damp the hydrophone motion caused by motion of the buoy at the surface. This keel is also a Stacer manufactured by AMETEK/Hunter Spring. It differs from the mast primarily in the way it is released. The keel is held into position by the bottom release plate, which falls away upon impact, thus allowing the spring force of the Stacer to extend the keel. Figure 22 shows a buoy that failed to have its parachute function properly. The dent is virtually all the damage incurred and the buoy functioned normally for a while, thus indicating the buoy's shock tolerance. In addition, the figure shows two seawater batteries (one used for the mast release and the other for the CO_2 inflation system). One of the CO_2 bottles can be identified, as can the bottom of the little electronic circuit used to delay mast erection after impact.

Data from the buoy will be read out directly on a NAFI-built display, figure 23. The display can accommodate five additional sensors without modification by rotating the Function Switch to Positions A, B, C, D, or E and reading the display above the error light.

Figure 24 shows two TAWS buoys that were dropped by a P3-C in Key West. The P3-C launch will be seen in the 12-minute movie that will be shown at the end of this presentation. In this case, the buoys are placed in a launch tube and kicked out of the launcher by an explosive charge or by a spring release (actually free-fall launch is also possible). A metal, spring-loaded flap which presents a drag surface to the wind is attached with a nylon cord to the top of the parachute. When the buoy (parachute end out first) exits the plane, the air in the slip stream moves the flap away from the buoy, thus pulling the chute from its canister. Both a nonbreakable and breakable cord have been used to keep the flap attached and not attached, respectively. The latter seems best because parachute fouling with the buoy upon surfacing after impact is less likely.

Figure 25 is a picture of the U.S.N.S. Hayes used in a recent marine fog expedition by Naval Research Labs. A TAWS buoy pictured in figure 25 was deployed over the side of the ship to gather data on modification of ambient air temperature by the ship itself. Temperature gradient with buoy separation was tabulated.

At present, NAFI is finalizing the design of the TAWS buoy in its sonobuoy configuration. In this configuration, onboard P3-C systems would be used to provide TAWS data in a number of ways. One of the systems involves an analog, FM recording; one involves a digital printout on a teleprinter; and one involves a digital data link, which sends the data over an HF radio link back to a land station. The final developmental testing of this design will occur this fall. In addition to the aircraft interrogation/reception/display facet of the tests, meteorological observations and buoy data reception will be made from a ship for comparison purposes. Figure 27 shows an illustration of this finally designed buoy in its stored configuration and figure 28 in its deployed configuration. Three of these buoys will be tested.

In addition to finalizing the present design, work has begun on developing a similar buoy with the following changes:

1. Satellite/buoy instead of aircraft/buoy telemetry link
2. Measurement of thermal structure below the ocean surface
3. Extension of buoy life from 7 to at least 30 days

The buoy will still be air deployable and may or may not be of sonobuoy size in its stored configuration. The addition of a long thermister chain to measure thermal structure may require the most additional space and may well determine how large the buoy will have to be in its stored configuration. At present, a chain of 10 thermistors to 1,000 feet is being envisioned for the thermal structure measurement. The satellite to be used for the buoy configuration has not been decided upon yet. Locating the buoy will now be a necessity because of the longer life of using retransmitted OMEGA navigation signals to locate the buoy. This technique or a satellite navigation technique will be used. The longer life requirements will probably necessitate using something besides a CO_2 inflatable system for floatation because of the leakage through the approach; however, the CO_2 system is probably the least space consuming. A cost/capability analysis of this new buoy system should be completed before May 1976.

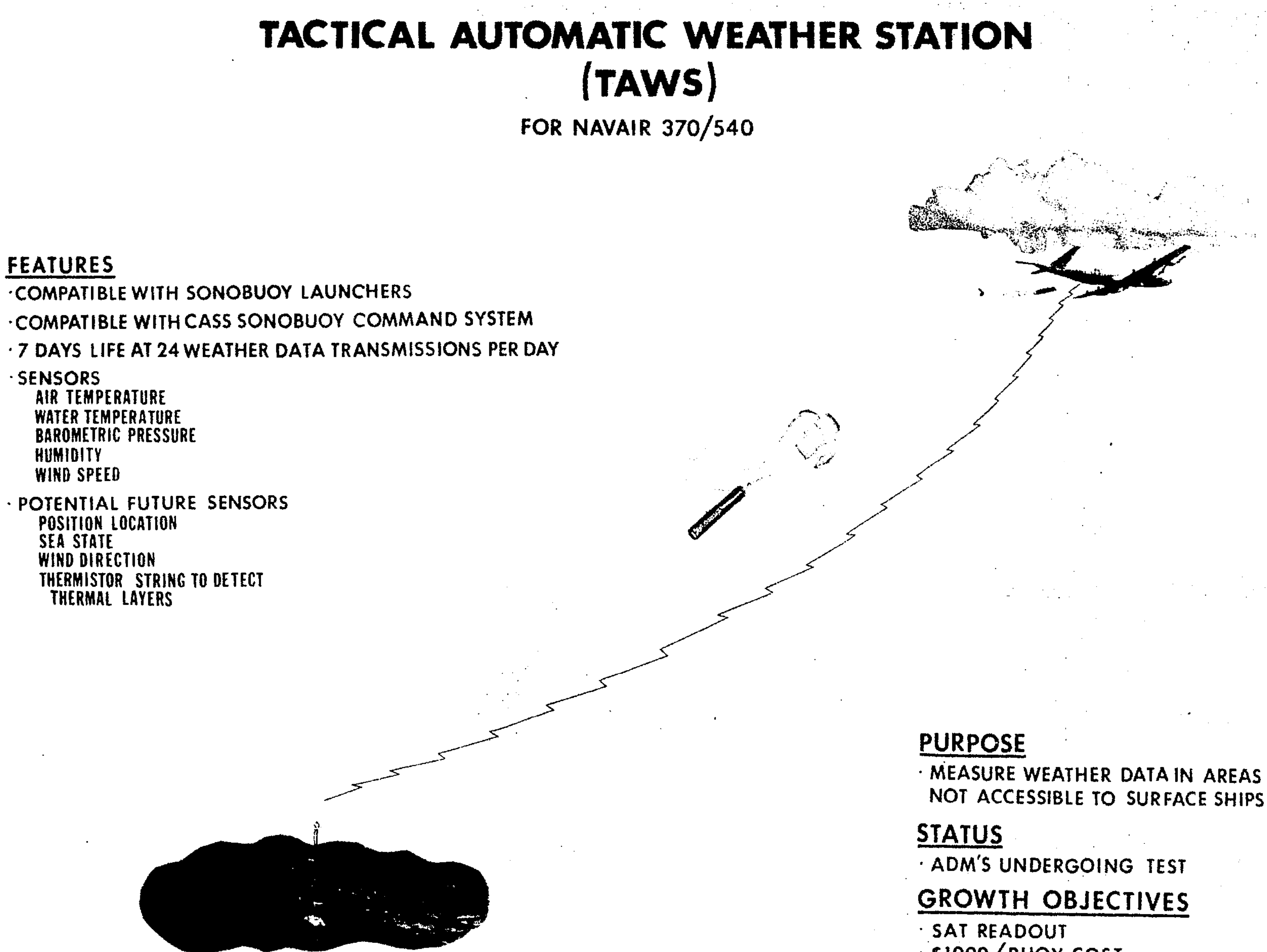


Figure 1. TAWS SYSTEM OPERATION

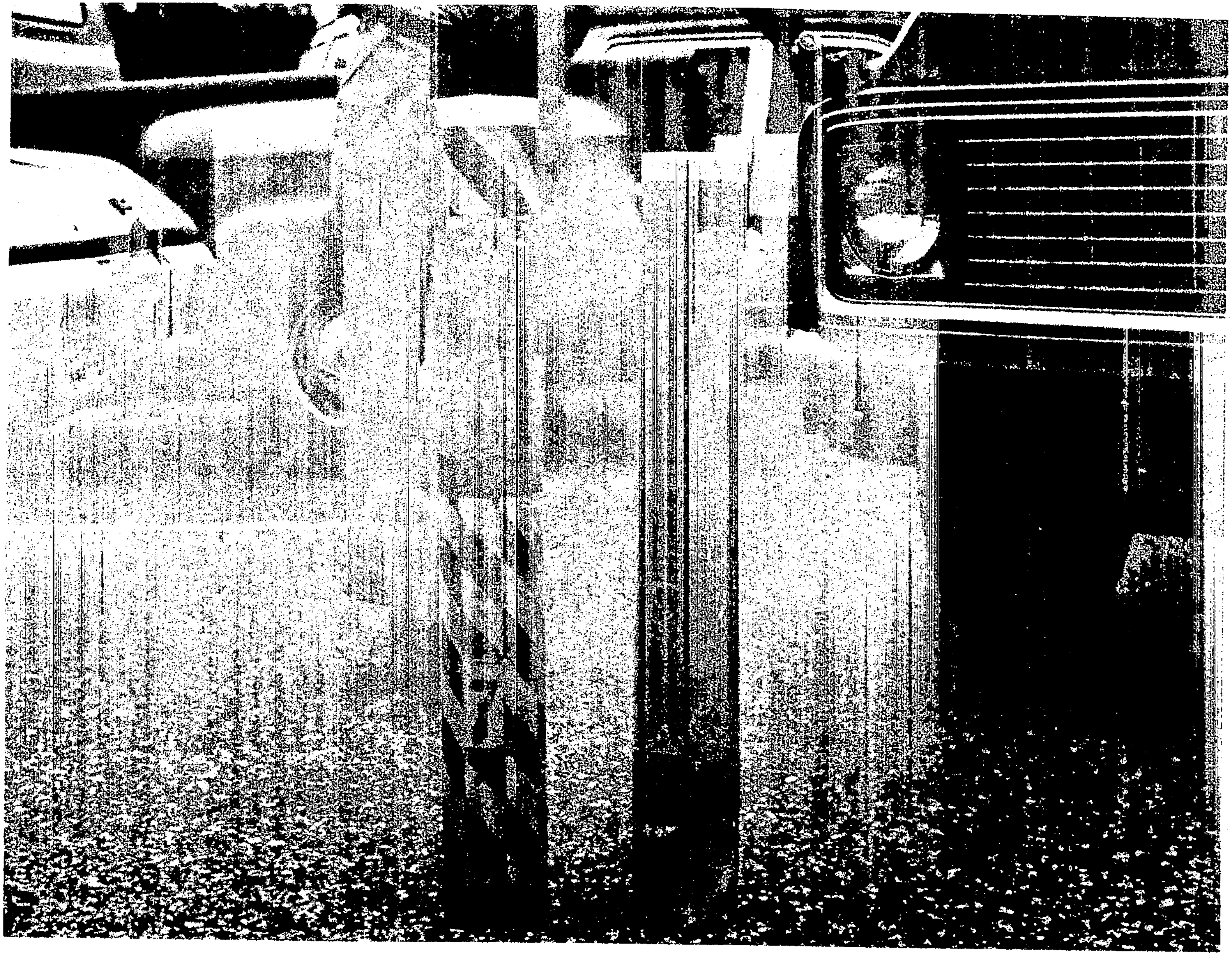


Figure 2. EARLY TAWS MODEL (left) DUMMY DROP MODEL (right)

Figure 3.   HELICOPTER JUST PRIOR TO BUOY LAUNCH

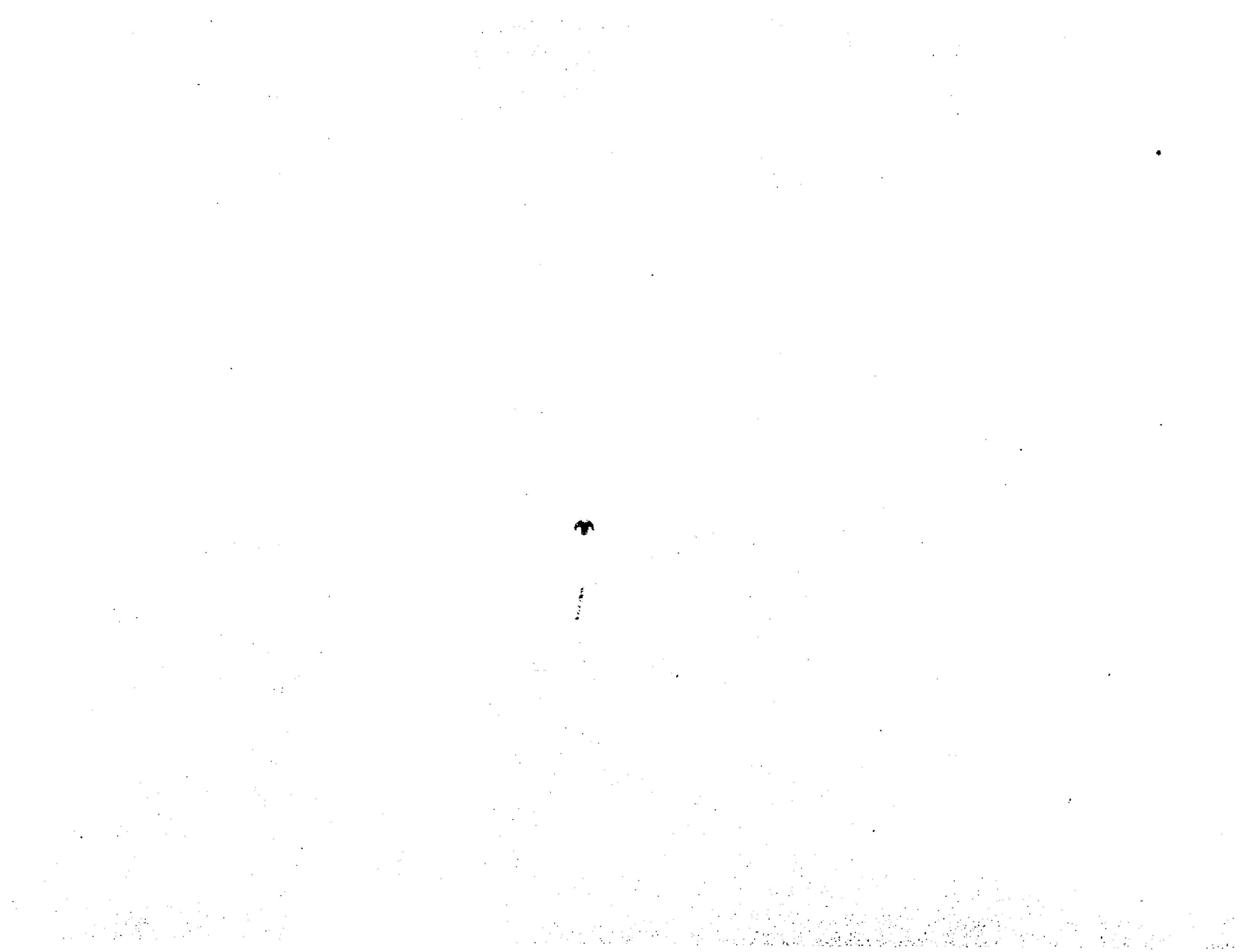


Figure 4. BUOY IN DESCENT

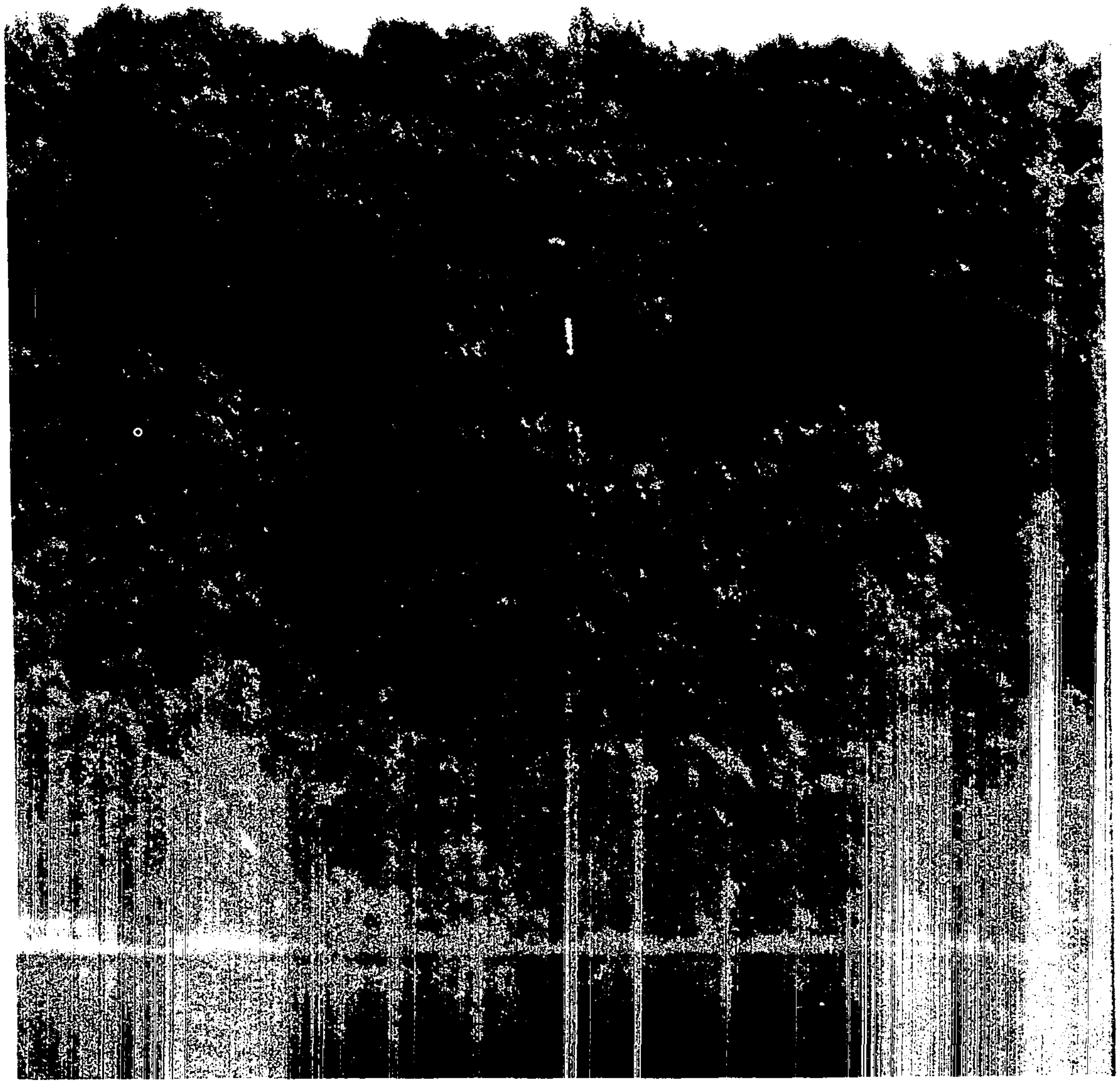


Figure 5.   BUOY JUST PRIOR TO WATER ENTRY

Figure 6. DIVER'S BOAT APPROACHING ENTRY LOCATION

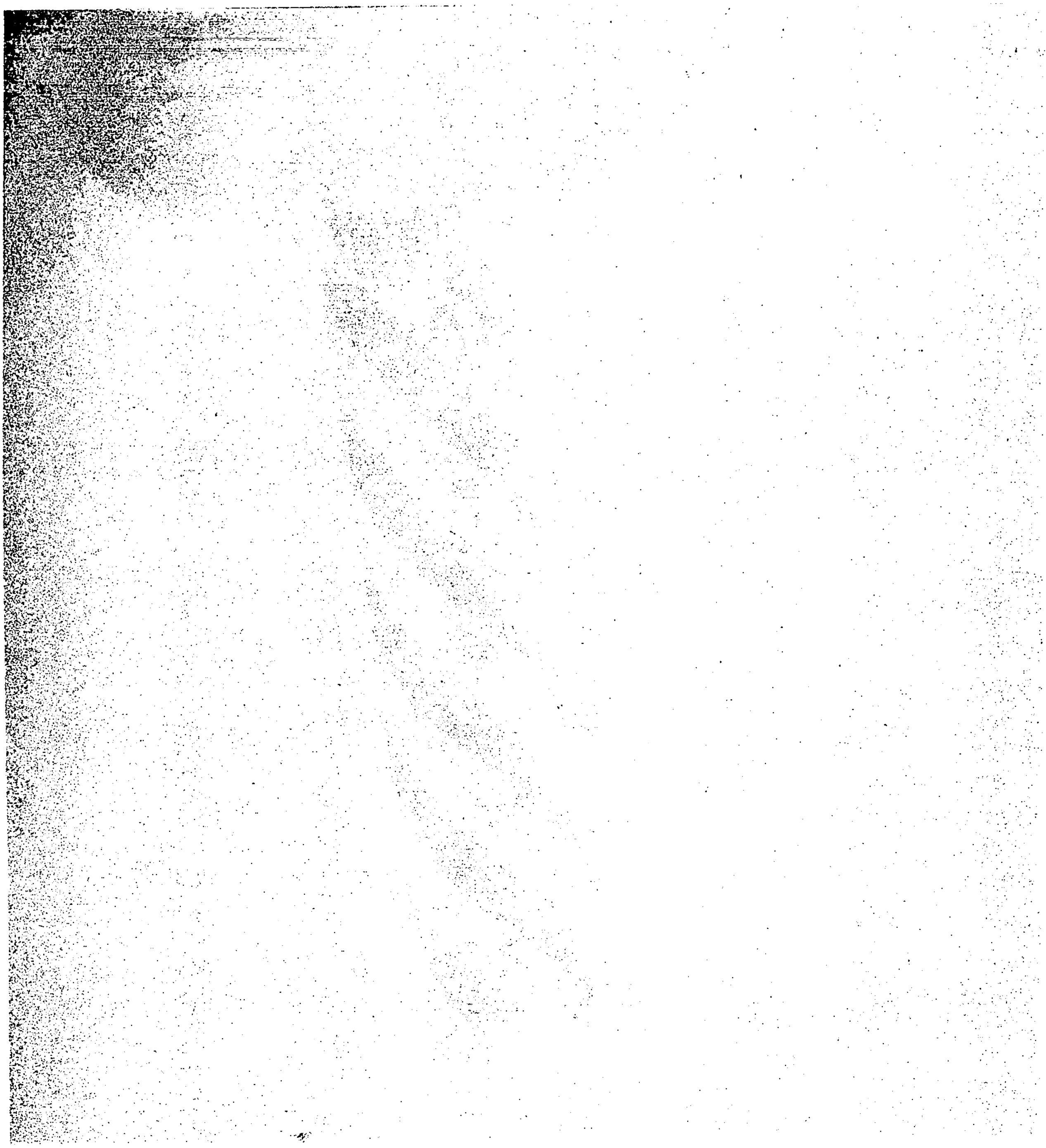


Figure 7. UNDERWATER VIEW OF BUOY

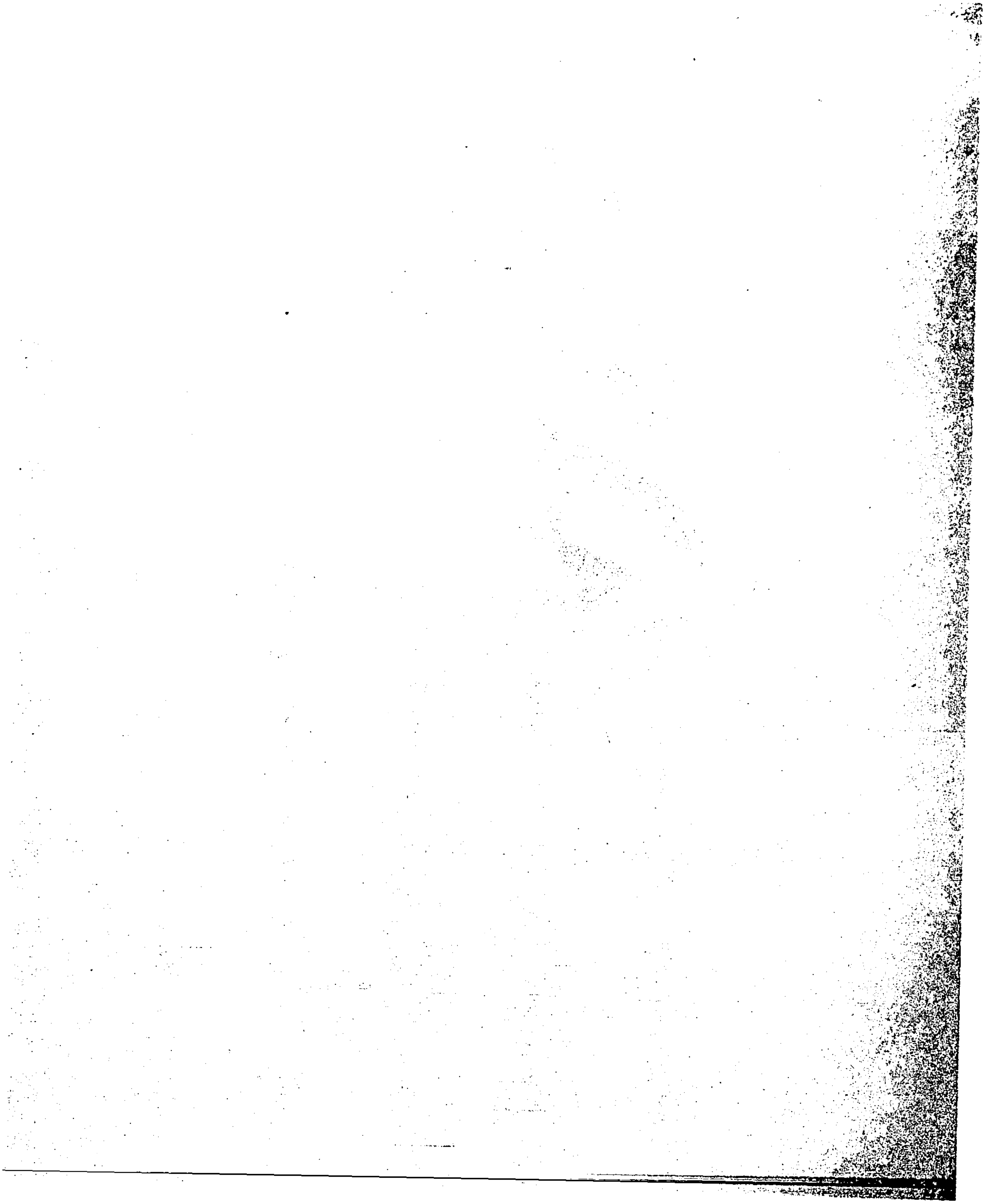


Figure 8. UNDERWATER VIEW OF BUOY AND STACER

Figure 9. DEPLOYED HYDROPHONE

Figure 10. BUOY IN DEPLOYED CONFIGURATION

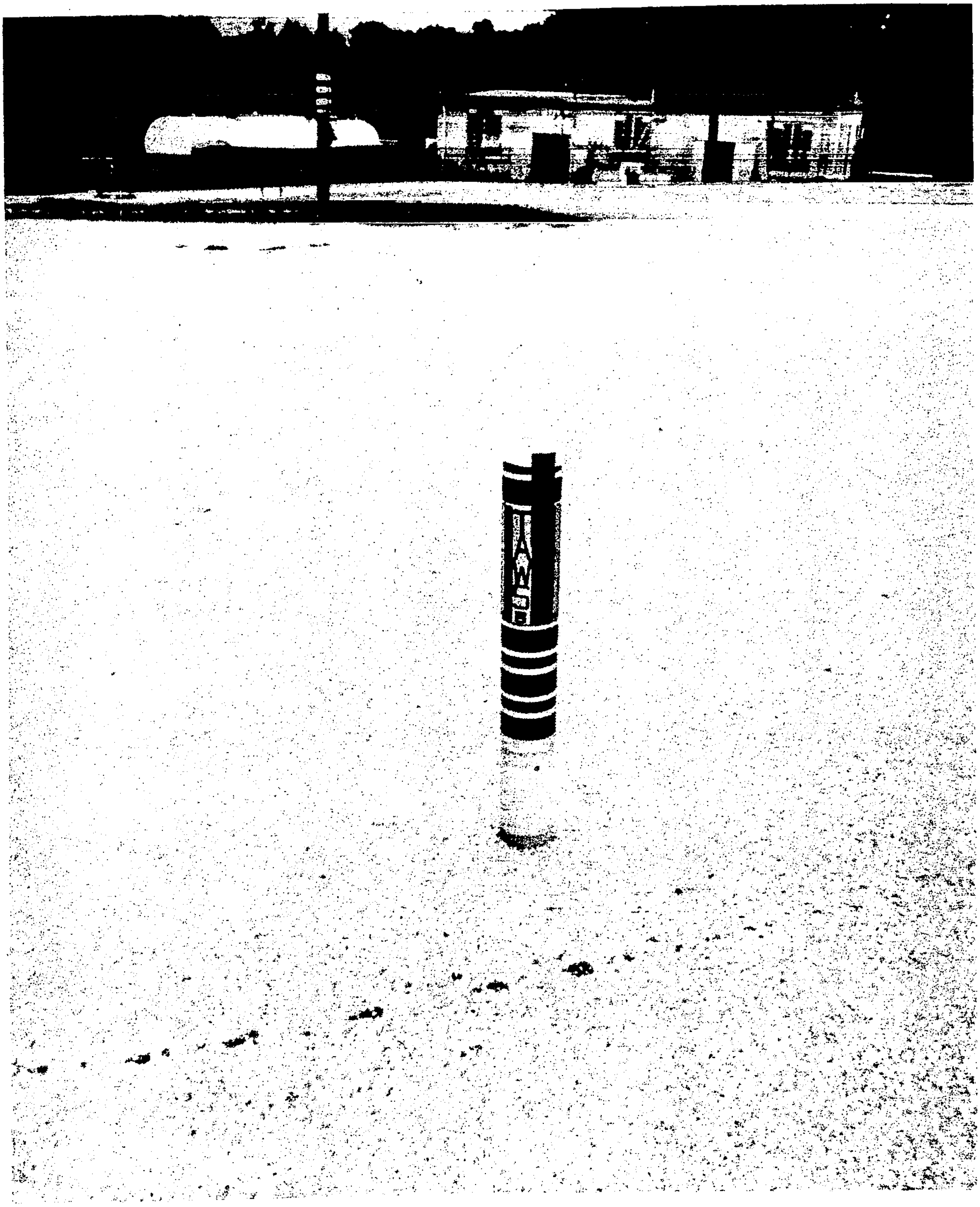


Figure 11. LATER MODEL BUOY IN STORED CONFIGURATION

Figure 12. NAFI BOAT CHECKING COMMUNICATIONS LINK

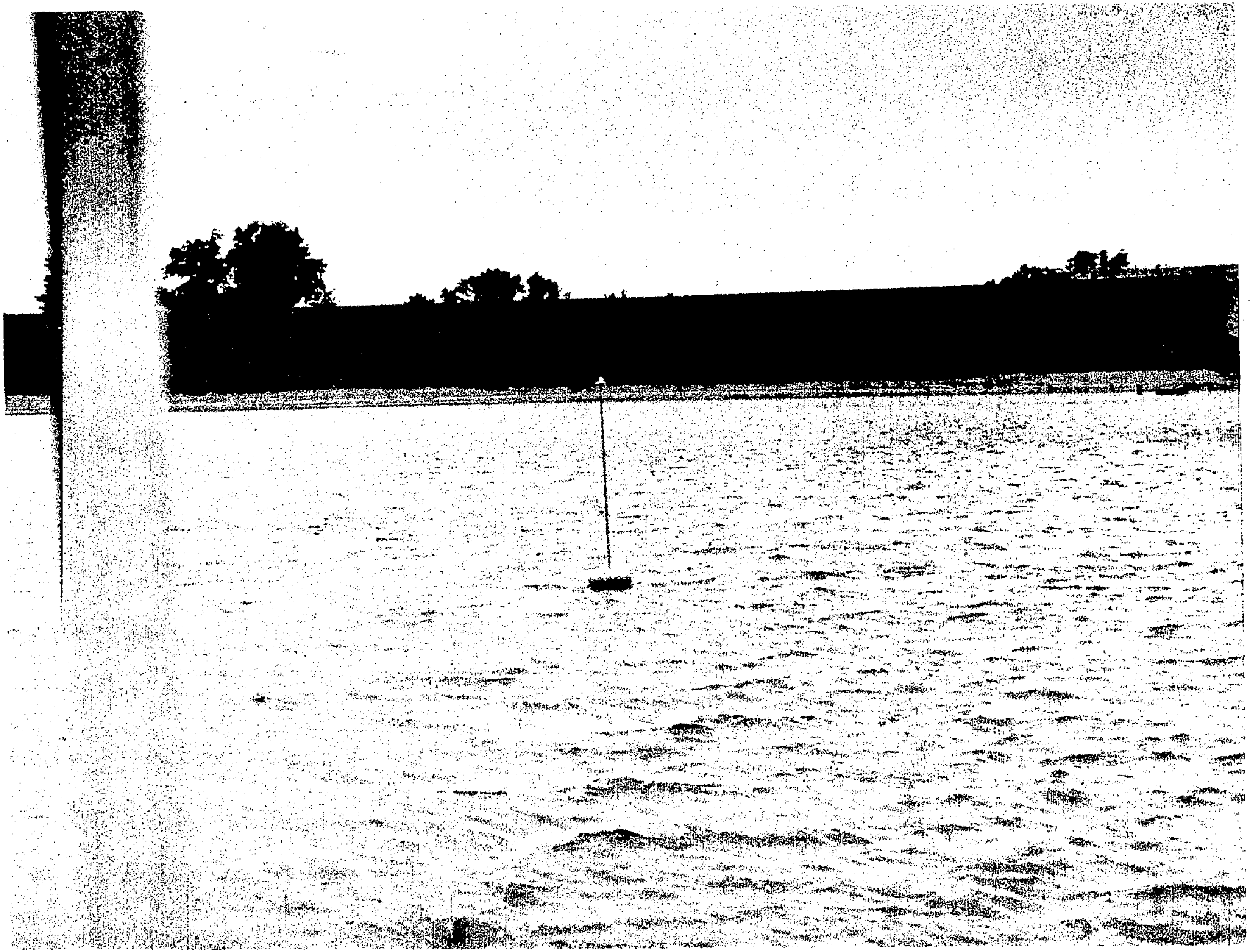


Figure 13. BUOY IN LIGHT SURFACE WAVES

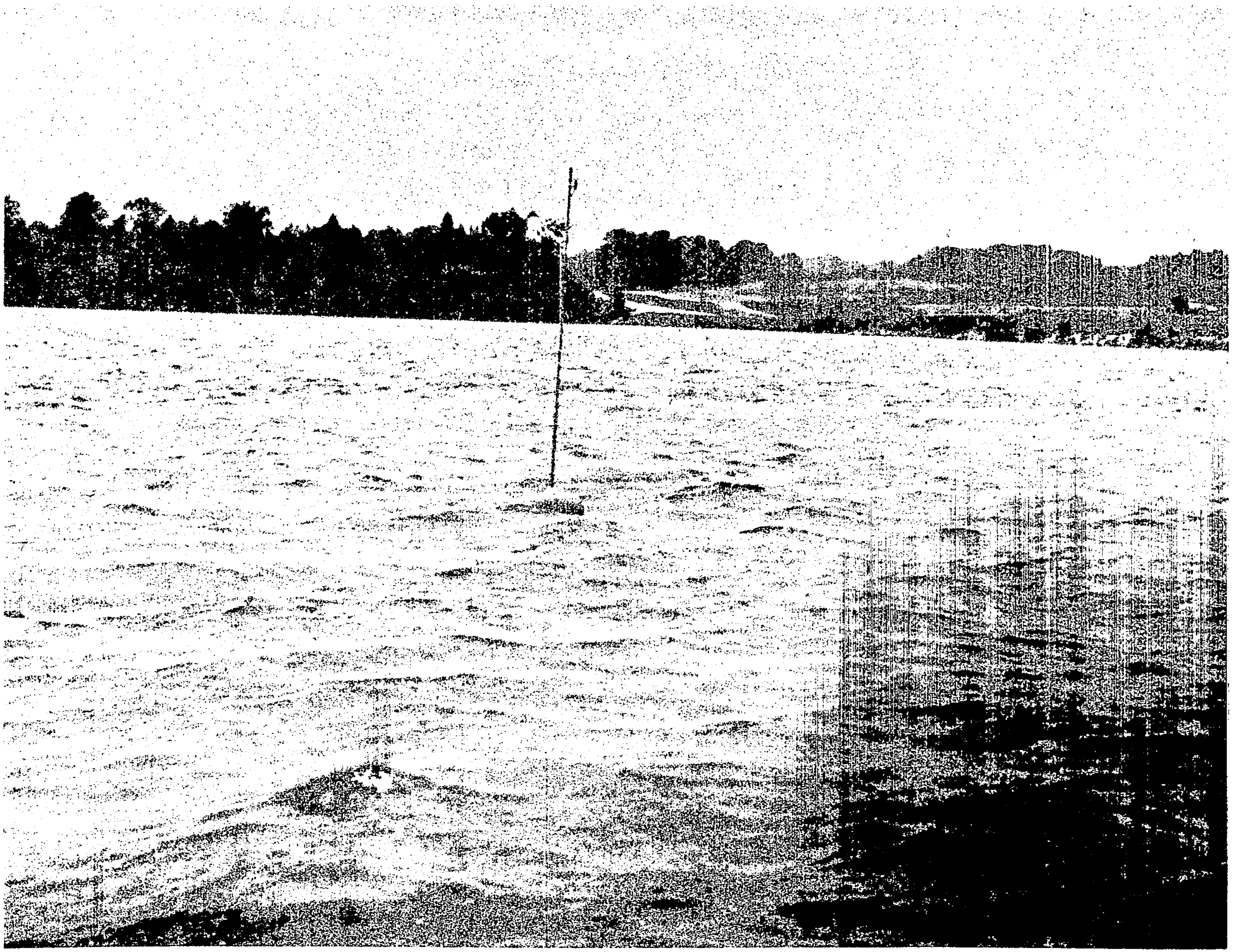


Figure 14. BUOY IN LARGER SURFACE WAVES

Figure 15. BUOY DEPLOYMENT FROM HELICOPTER

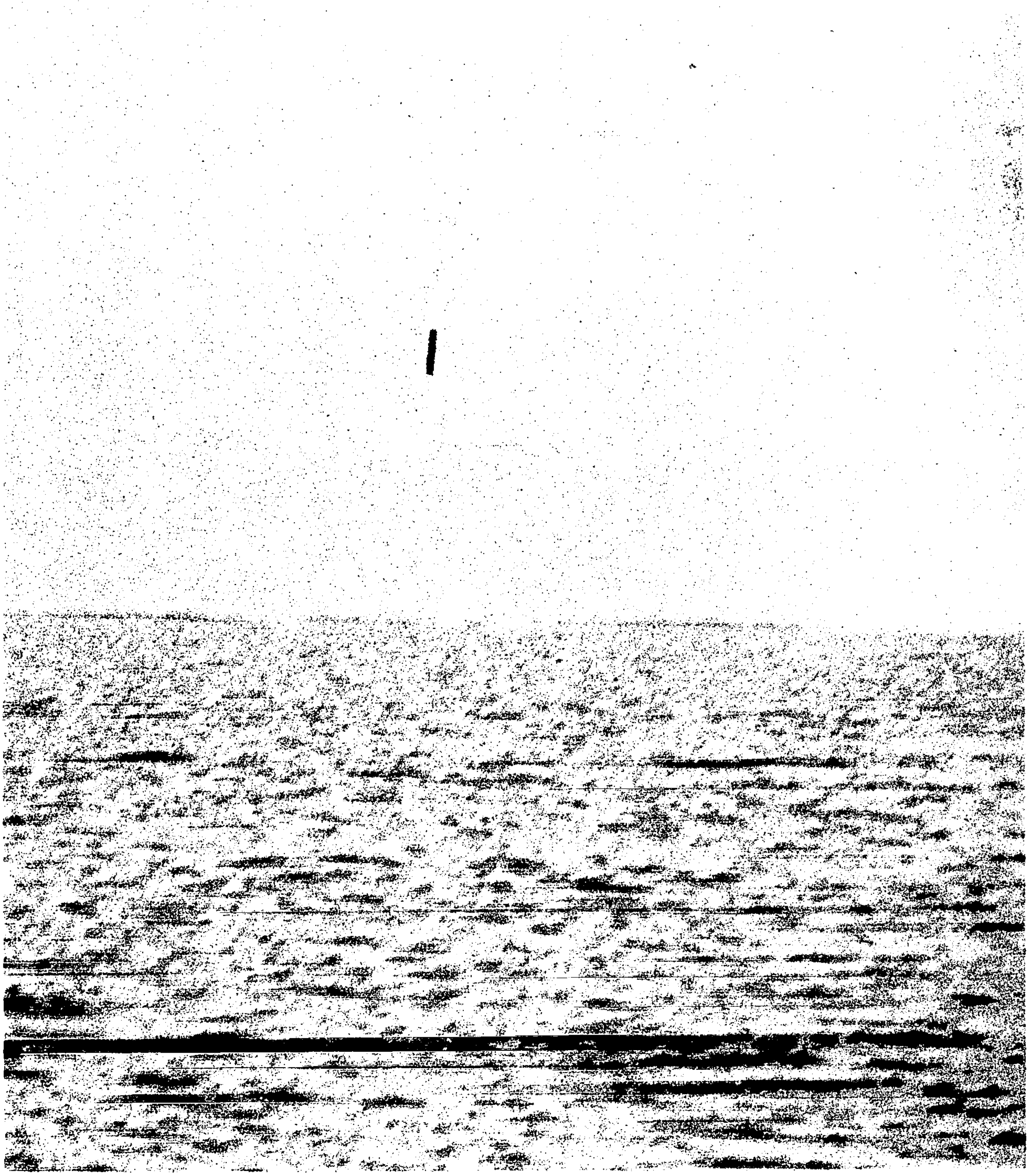


Figure 16. BUOY JUST PRIOR TO WATER ENTRY

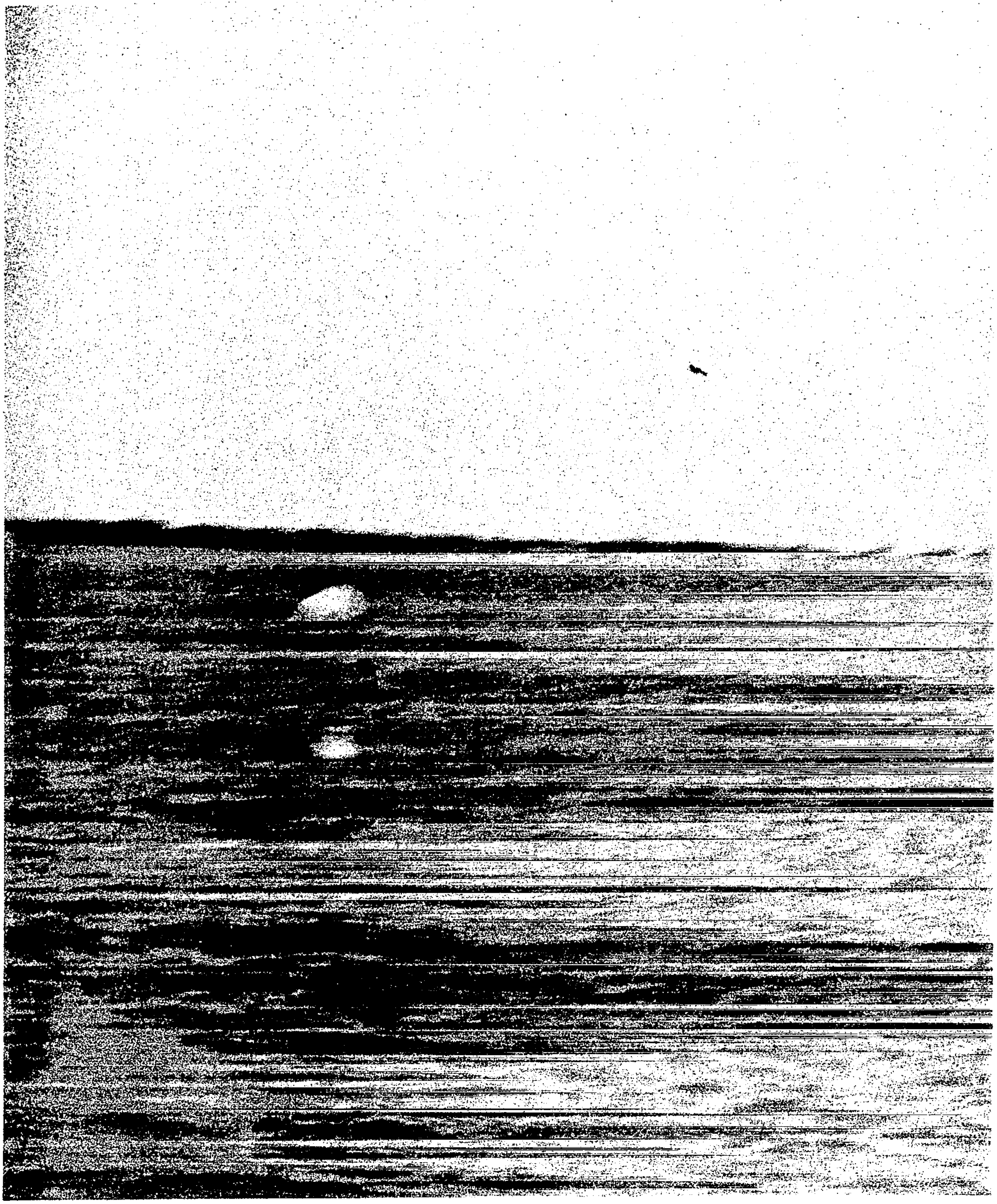


Figure 17.   BUOY AT TIME OF ENTRY

Figure 18. BOAT APPROACHING BUOY

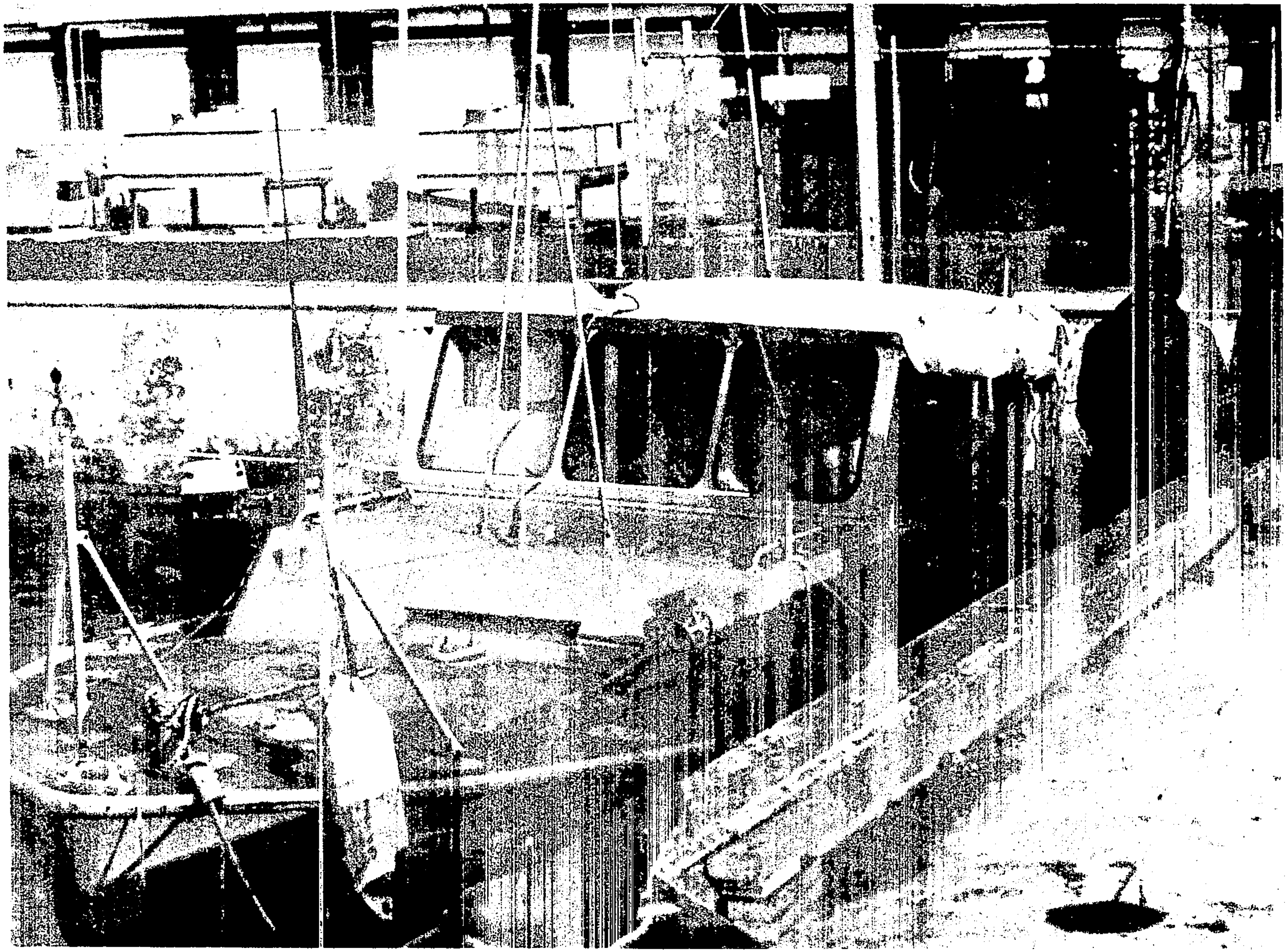


Figure 19. VESSEL USED TO OBSERVE AND COORDINATE TESTS

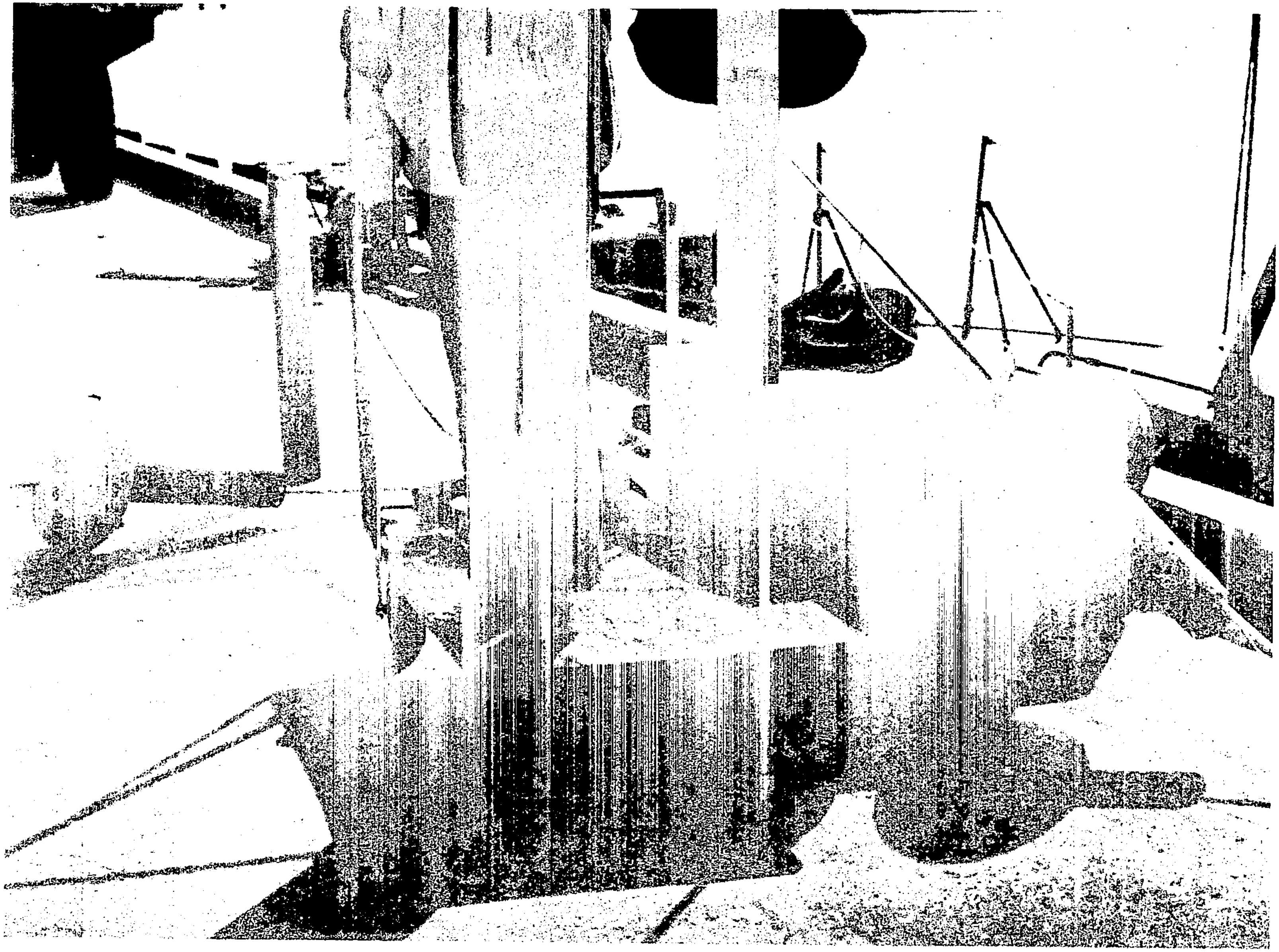


Figure 20.   3 BUOY AFTER BEING RETURNED TO SHORE

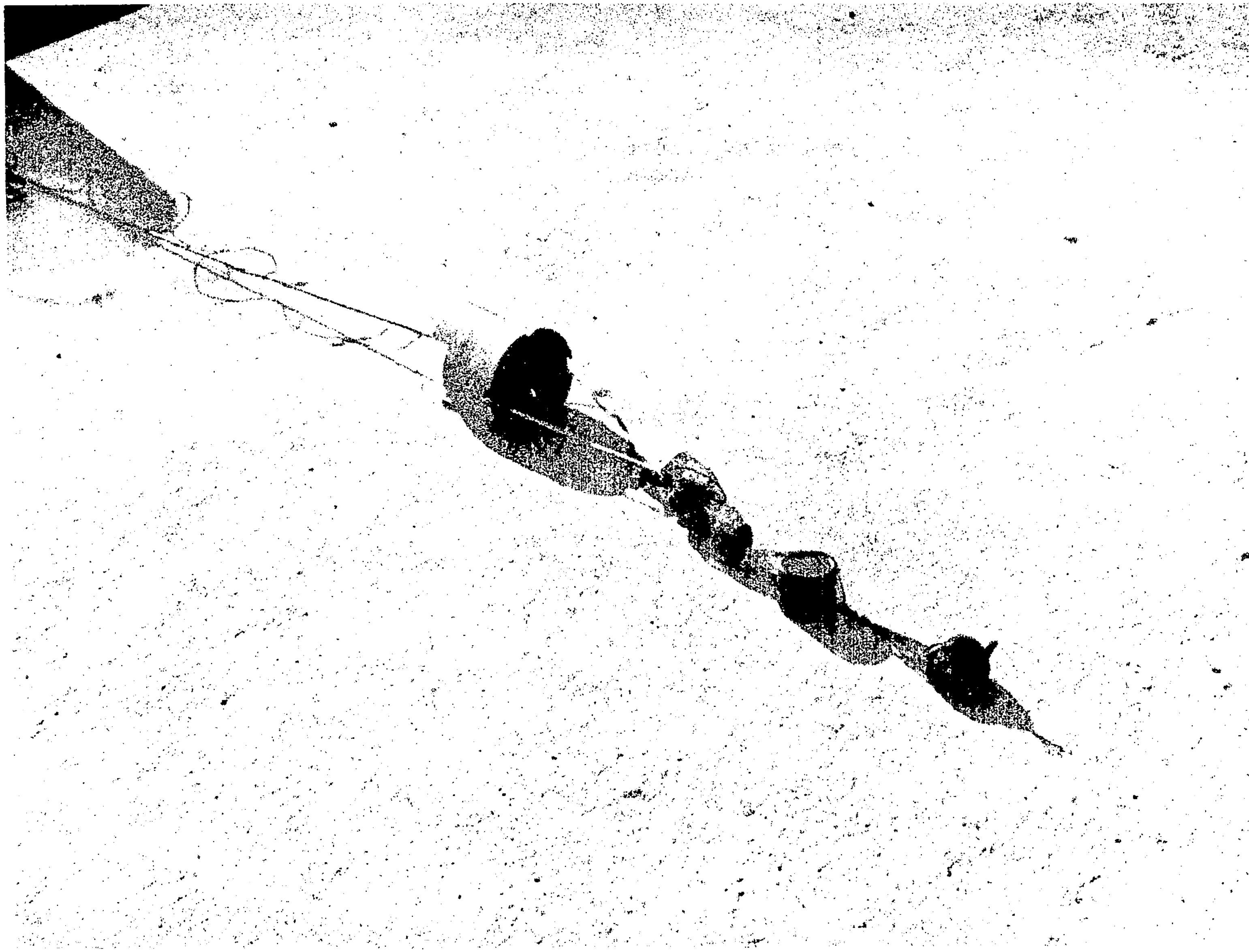


Figure 21.   BUOY HYDROPHONE ASSEMBLY

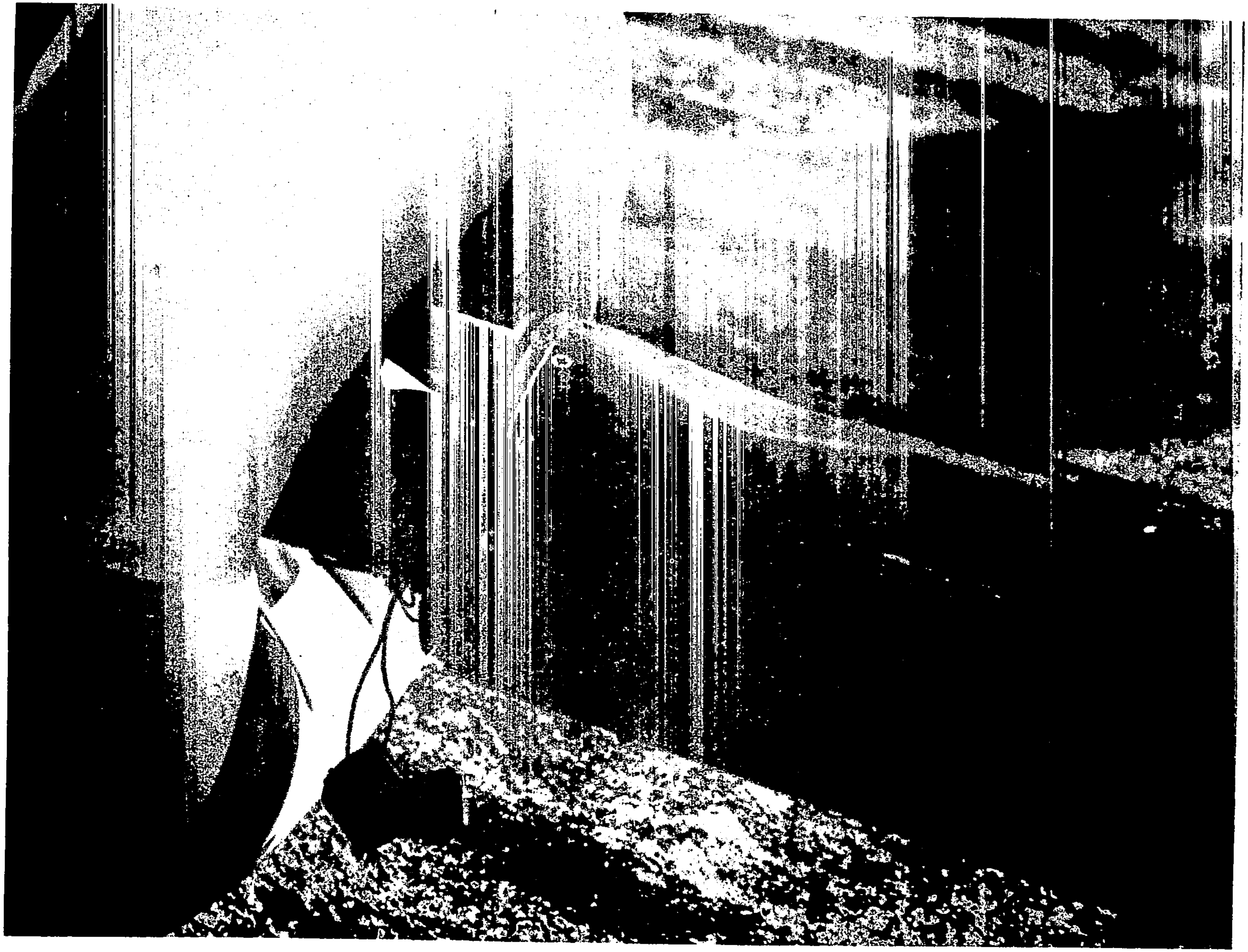


Figure 22. BUOY DAMAGE DUE TO PARACHUTE FAILURE

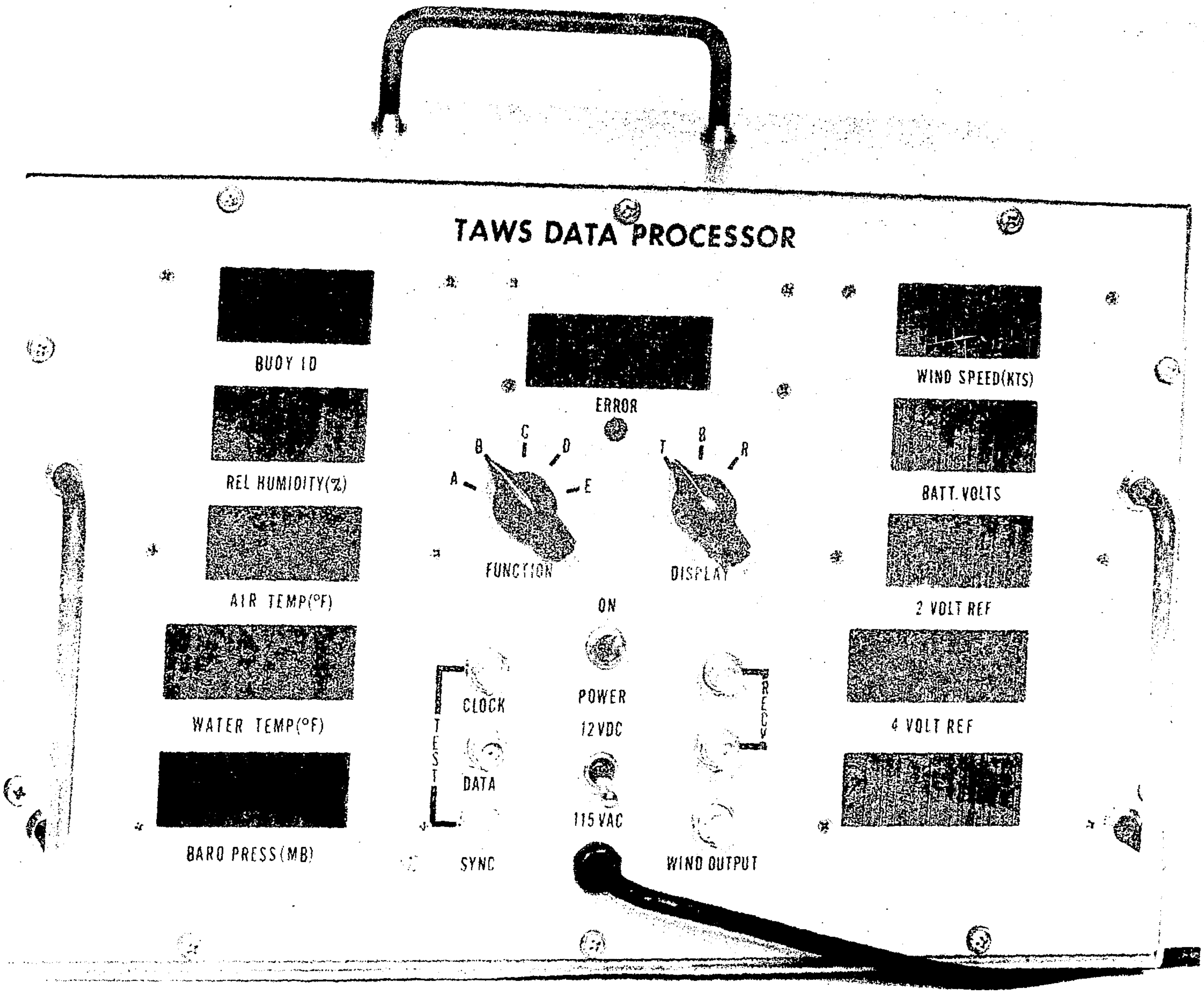


Figure 23. TAWS Data Processor

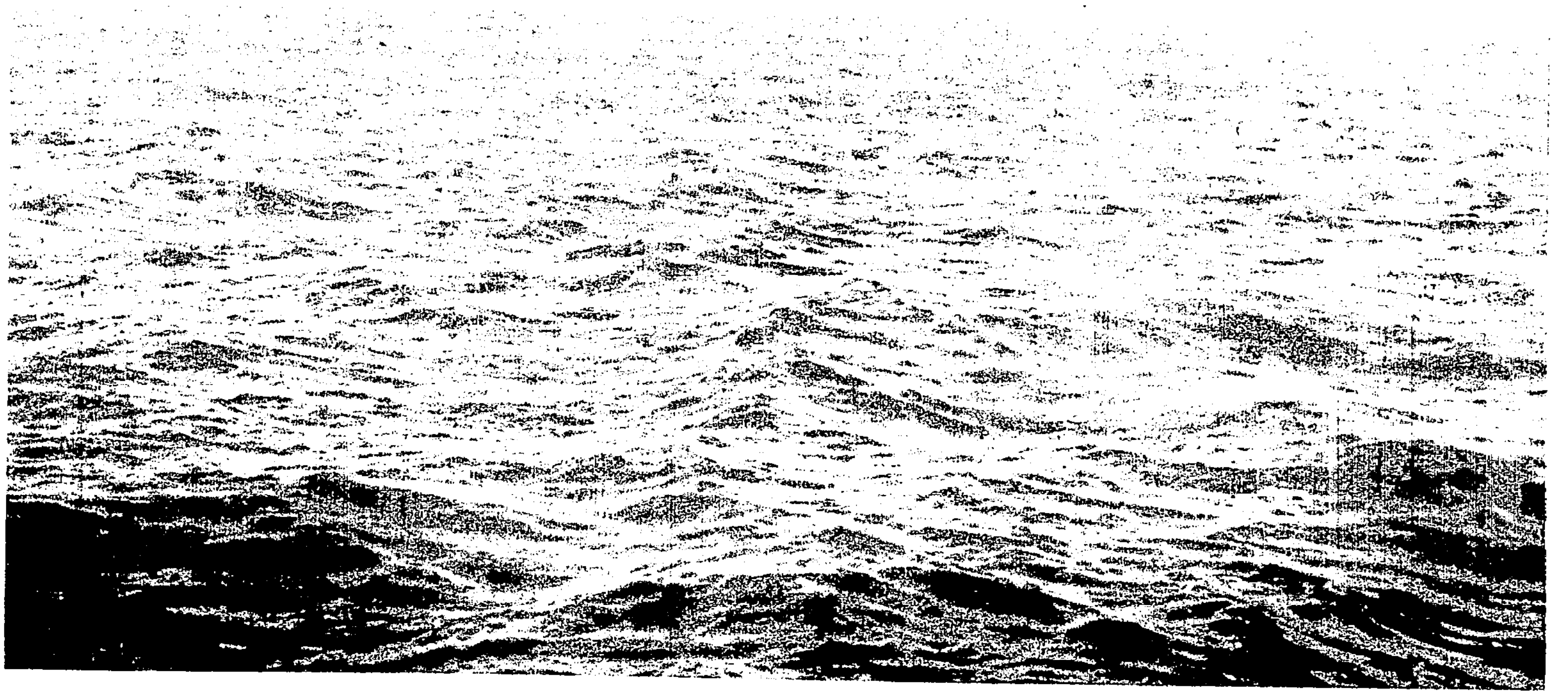


Figure 24. TWO BUOYS LAUNCHED BY P3-C AIRCRAFT

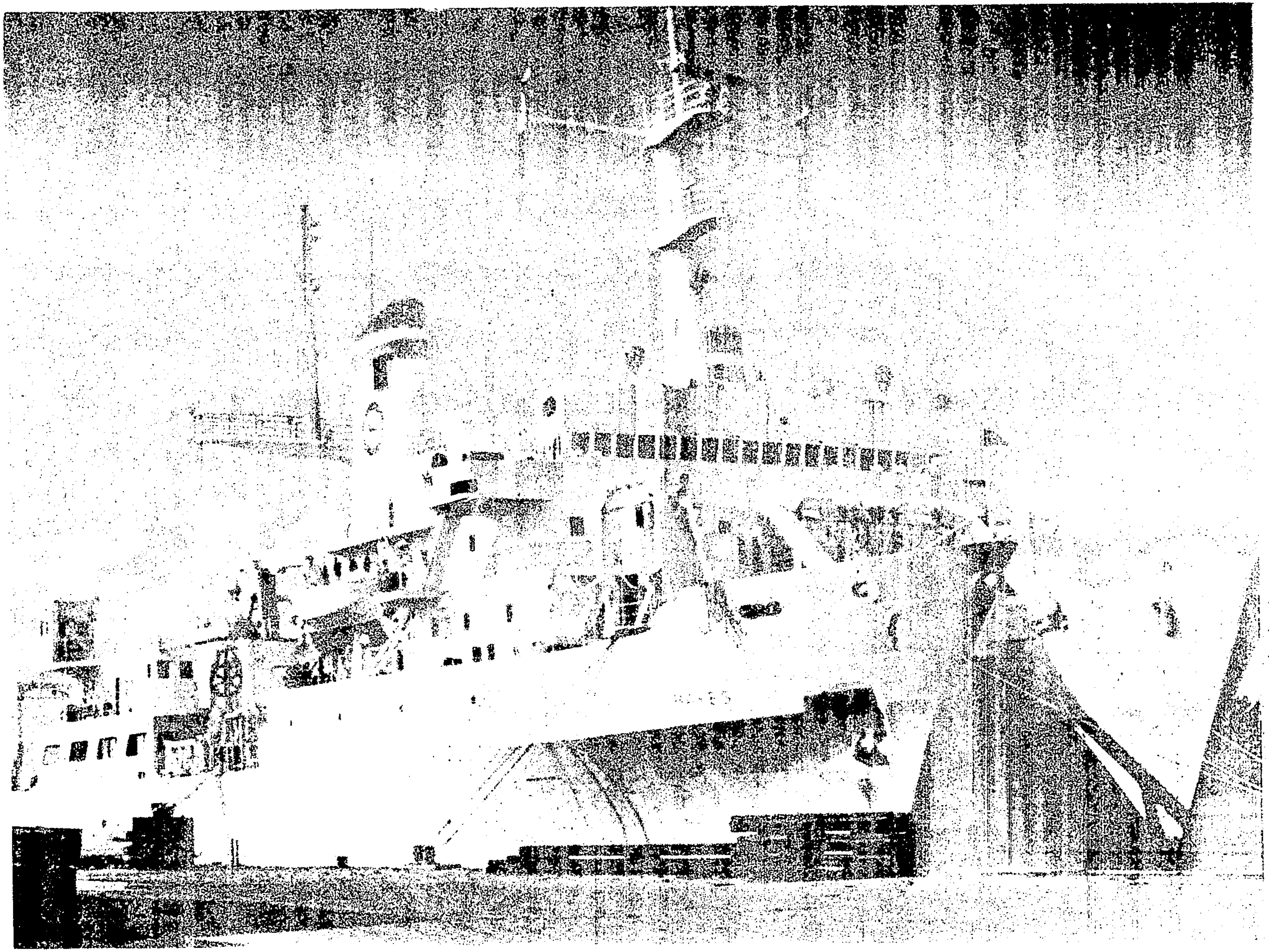


Figure 25. U.S.N.S. HAYES

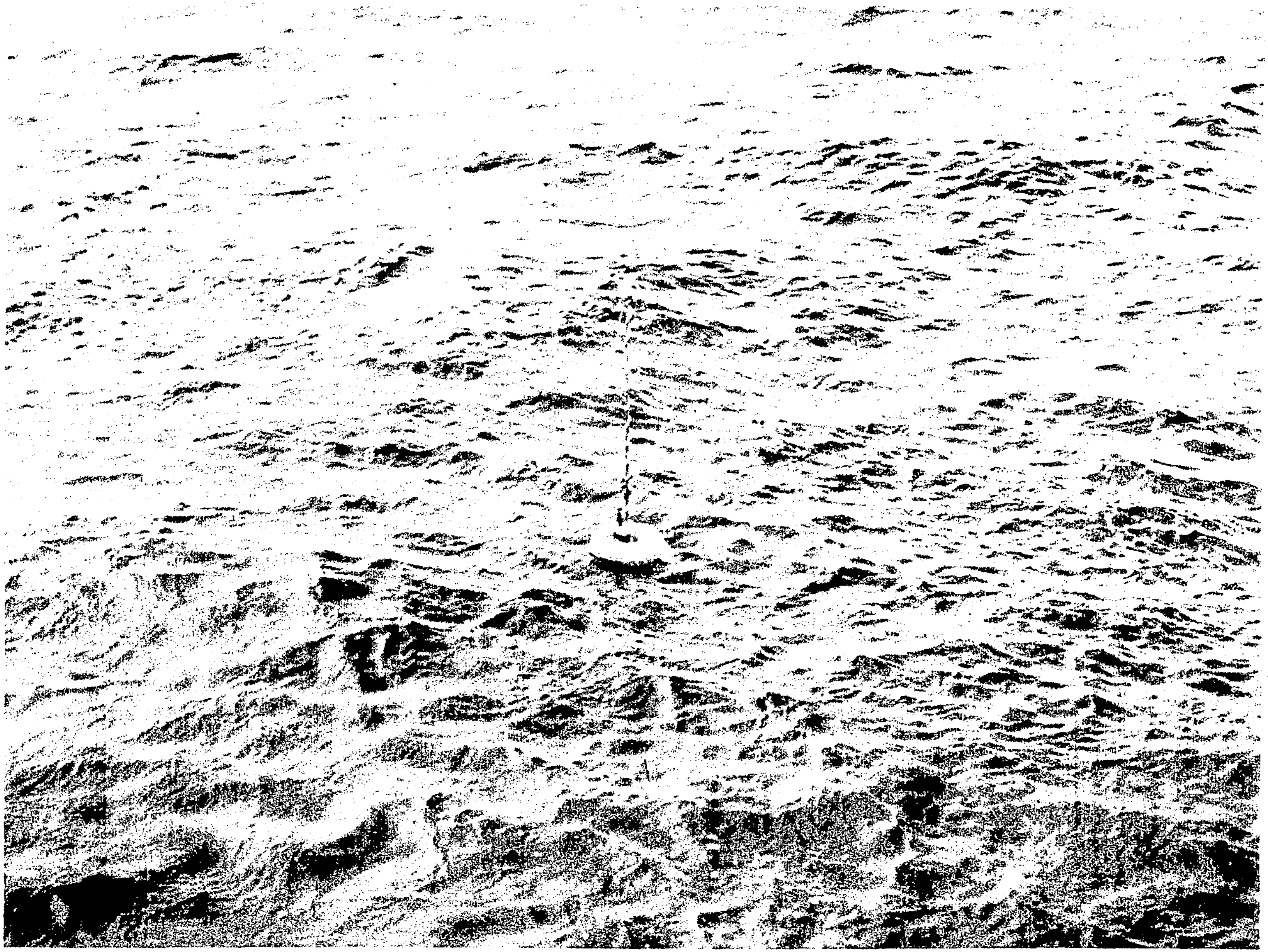


Figure 26. BUOY DEPLOYED FROM U.S.N.S. HAYES

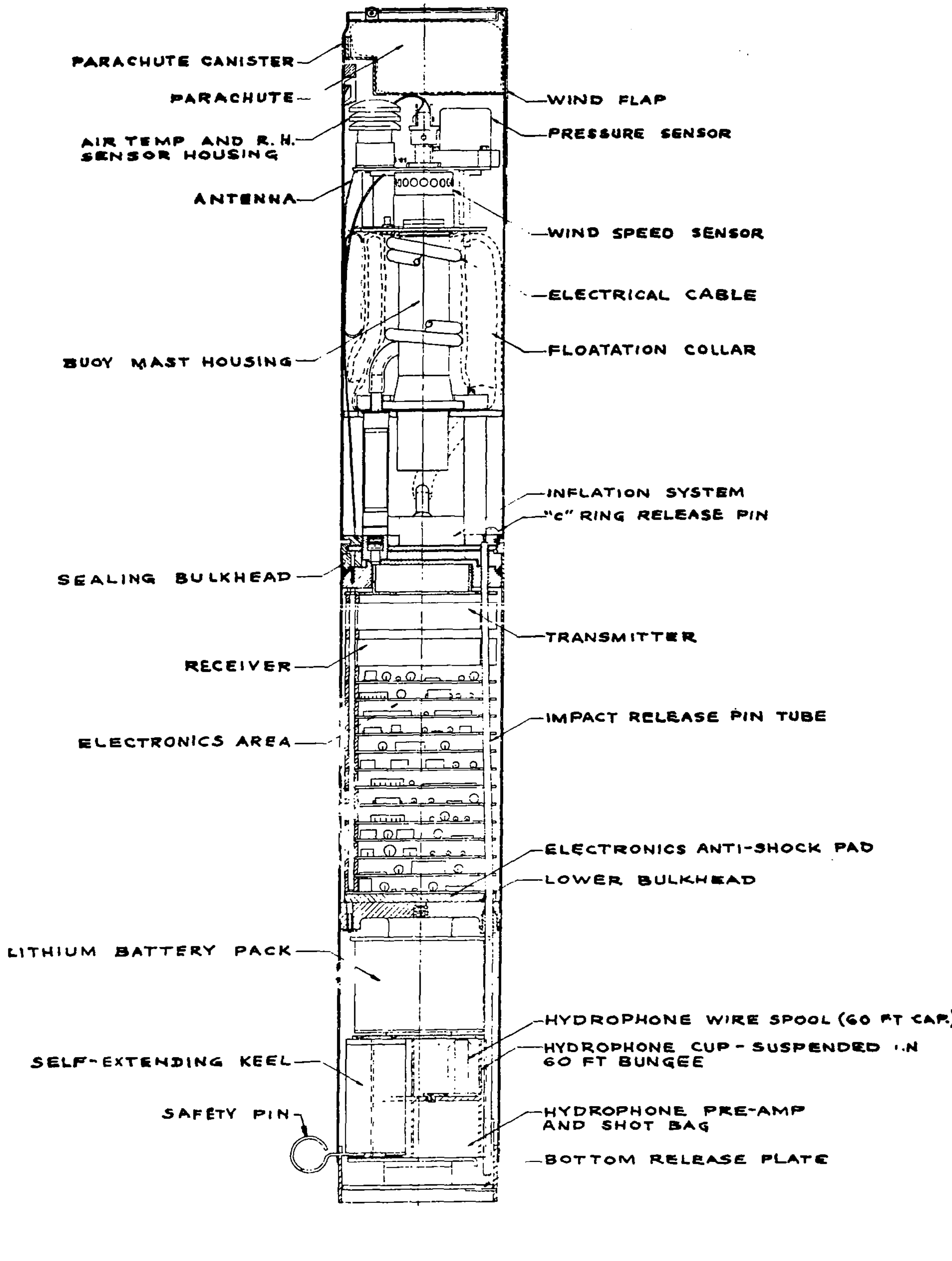


Figure 27. Pictorial Drawing of the TAWS Buoy
In its Stored Configuration

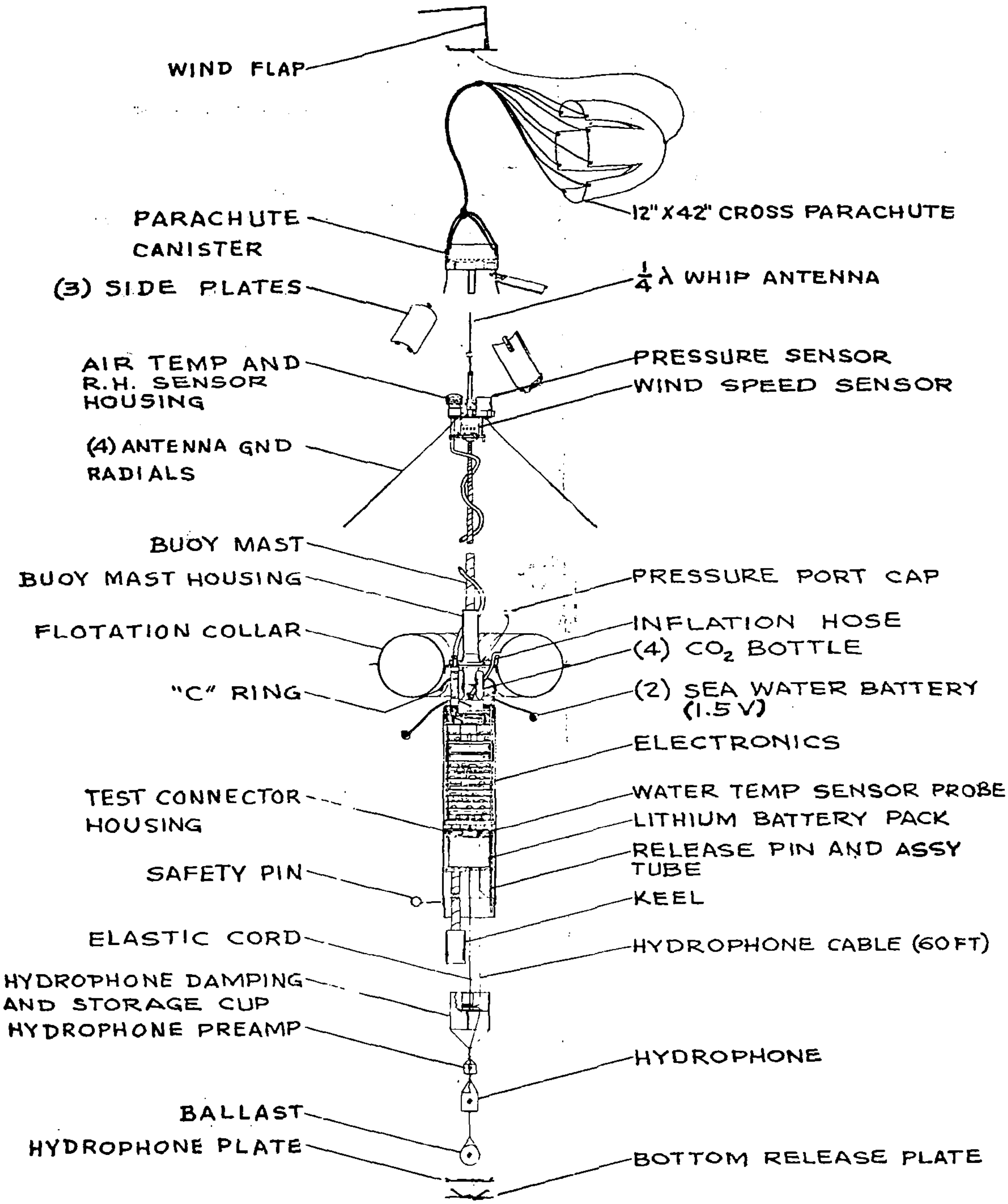


Figure 28. Pictorial Drawing of the TAWS Buoy In its Deployed Configuration

THE UNITED STATES COAST GUARD
TECHNIQUES FOR BUOY
DEPLOYMENT AND RETRIEVAL

by

Thomas J. McKerr
Lieutenant Commander, U. S. Coast Guard

First, I would like to point out that I represent the fraternity that gets you about six feet from your buoy and then we run it over. That's not strictly true--usually when we get that close and it looks like we might run it over I give the control to the exec and let him run it over.

I would also point out that my expertise in drift buoys is purely accidental. I've never worked a drift buoy that was suppose to be drifting, but they are somewhat easier to work than a moored buoy.

The Author: LCDR Tom McKerr is a graduate of Colgate University where he majored in Chemistry. Since joining the Coast Guard, he has served on the Coast Guard Cutter ACHUSHNET but before it was converted for its present oceanographic duties. He served 2-1/2 years as Executive Officer on a seagoing buoy tender and for the last year has been Commanding Officer of the Coast Guard Cutter CONIFER, another seagoing buoy tender working out of Portsmouth, Va.

Our primary goal on a buoy tender is safety--safety to the people that are down on deck doing the nitty gritty, watching antennas fly at them and all these little goodies; safety for the ship--we don't want any holes punched in the side, which can happen; and safety for the piece of equipment that we are working with. Perhaps the best place for me to begin is to describe the type of platform that the Coast Guard primarily uses for buoy deployment and retrieval, with the exception of the ACHUSHNET, which is an entirely different type of vessel.

Figure 1 is an A-class seagoing buoy tender. It is 180-feet long, weighs approximately 1025 tons with full load displacement, and is a good, solid, rugged sea boat. It's a lot tougher than any buoy that you might later put out--I say that for a reason. If you're going to put out a piece of light buoy that you want recovered and we run into it, we'll win every time. It has a 30,000 pound boom capacity. We can rig to five parts and work to 45,000 pound, so this obviously is quite a bit above most any type of oceanographic buoy that is in inventory, but we do have this capacity. We're a 1,000 horse single screw, but we have diesel electric and it surprises most mariners that even though we're 30 years old, we've always had pilot-house control so we're quite maneuverable for a single screw ship. We can put it within a few feet where you want to put it and hold it there. One other measurement that's of some concern is that it is 7 feet from the buoy deck to the waterline, so if you put something close to the water that we're suppose to hook into, somebody's got to try and hook into something with a heavy piece of gear that's 7 feet below them. Also, in the line of how much roll we can still work with, we can normally accept up to 10- or 15-degrees roll. This would be the effect of both the sea state and the weight of the load and how far out we have to boom in order to latch onto it.

A different class of vessel is shown in figure 2--a little more versatile. This has two working parts that can each carry 30,000 pounds and a total weight of 45,000 pounds. This does make it easier if you have a buoy that has to go in straight up and down, to use this type of vessel because when a single hook picks it up obviously the buoy is going to tip one way or the other. This tender can pick it up on opposite sides and get it into the water straight. This is an icebreaker, also built for icebreaking which does affect the roll we take. Icebreakers are round bottomed. They are not necessarily the most comfortable ship in the world and they do roll.

I would now like to describe some of our normal procedures. Our normal approach is to take the result and vector of both the wind and the sea current and drive right up it bringing the buoy or whatever is to be recovered along side the port or starboard (figure 3). It doesn't make any difference really which side. This is somewhat different for a drifting buoy, but it's a little easier really because you don't have to get alongside of something that's stopped and then try and hold your speed relative to something that is stopped over the ground. It eases your problem of solving the vectors. It also gets a little complicated if you have a sea or swell with the direction different than your vector of wind and current because you're starting to roll and these things are pretty roll happy. Ships never roll in the same direction as buoys and they usually meet right at the top and we win all those too. This means we may have to compromise somewhat to get in alongside a drifting buoy to balance the roll with the current wind vector. On deployment of the buoy, we do normally try to set it off the port side because a single-screw ship will back to the left and that will pull the ship's head away from your buoy and we can usually get them off without banging them up too badly.

On the approach, having decided on what heading we want to pick up the buoy, normally the first thing to do is try and get a line on the buoy. This is to keep it from rolling and whipping into the side of the ship, antennas from whacking people on the head, and this type of thing as we approach before we put anything else in (figure 4). The next step would be to stop the ship relative to the buoy and that is not always as easy as it seems. I've seen many buoys lined up past the small boat and heading aft and it's rather embarrassing, but it's not an uncommon mistake.

The next step is to reave, through one device or another, the main hook into the lifting eye of the buoy. This is where you get into a lot of problems. The lifting bails are often white metal which means you have to pick up on at least two points. Everytime you increase your points, you're about quadrupling the number of problems you're likely to have, as far as ship control and stability of the ship and safety of the people is concerned. So we try to work buoys or we hope for buoys that require only one lifting eye.

Next, you lift the buoy partially out of the water and you get another line that runs across the deck to the buoy port and this gives you a great deal of lateral stability (figure 5). Once the buoy does come all the way out of the water and on deck this should give you a means of positive control of the buoy to keep it from swinging around on deck and into people and bulkheads, etc., and damaging the electronics package. After bringing the buoy across the deck and securing it with various lines, you can disconnect the buoy from anything below it. In this way, you are only trying to handle one problem at a time. You don't want a buoy that's running around loose on deck and a long line or sensor package dangling below deck that can't be disconnected. When you are designing a buoy, if there is some sort of quick disconnecting device, that would really help a lot. Finally, the last step would be to pull in whatever is hanging over the side after the buoy is secured on deck. One thing should be pointed out on this or any type of vessel--if you have a long sensor package we're limited to about a 15-minute continuous run on our deck machinery before you have to shut it down for cooling. For instance, on the very deep moor buoys, it takes about a day to pull a deep moor because you can only run your gear for 15 minutes and then you have to shut down for about 10 or 12 minutes while it cools.

The buoy characteristics that we would look for as an operator from you people who are builders and designers and put a lot of expensive packages in and expect to get them back are: the sea can be pretty rugged so we look for a rugged buoy. Now this doesn't mean that you design a buoy to last 60 years like some of our buoys that are running around now, but they should be built to survive both the sea environment and handling. You don't always get pictures like the pictures at Key West; it gets a lot worse and we don't like to have an unsuccessful retrieval any more than you people like to see your electronics package at 5, 6, 10, or 20 thousand dollars get dropped on deck. The other thing I can't emphasize too strongly is try to avoid having to pick up a buoy by more than one point. It really creates a lot of problems if you have to use a bridle. It means you not only have to get two hooks reaved through but one usually tries to back out while the other one is going in and you can really create a lot of hassle.

Antennas-- that's the favorite bug-a-boo of a lot of people here, not only from getting information out of them but to keep people away from them. They tend to run into rigging and run into people (figure 6). It is quite effective though, we worked one that had one of these bird spike antennas on it and all of a sudden it was driving right up to the chief boatsman mate at eye level. I had been trying for a year to get everybody to wear safety goggles on the buoy deck and that finally did it, so I will say that much good for bad antennas. They do try to catch on in the gear if they brake, there is not much we can do about it except bring it back aboard if it's a matter of deploying it and try to fix it one way or the other. A buoy should be strong enough to really be secured on deck with steamboat jacks and quite a bit of compression forces (figure 7). There is nothing really more frightening than to be caught in a sudden storm and all of a sudden have your deckload loose. The only thing you can do at that point is to try and slide it over the side and let it go because there's very little else you can do and again it's a lot of money down the drain.

Figures 8 and 9 are our standard 8 foot buoy. We have a lot of them around. They're available for use--many of them are moored. Central packages can be deployed off them. They're also reasonably available through various storage yards where replacement equipment is kept. If we have a good year, we don't get rid of them, so they are available. They are a good stable buoy. They're a strong buoy and they take a lot of abuse. We also have other methods of deploying a buoy. This is NOAA's old friend EB01 being towed out to station. Of course this is not a drifter, but this buoy does bring up another point. This buoy has a flash tube; very brilliant--very short flash. The mariner cannot tell how far away he is from it and as a result this buoy has been hit at least three of four times and probably more, because people drive right up to it and all of a sudden they know they're getting close and go bang, bang, bang, down the side. EB01 is now moored at Portsmouth with a broken mast which is a lot of hours for an oversight.

QUESTIONS

Bob Leinmiller, Woods Hole Oceanographic Institution

I was on a couple class E buoy tenders a few years ago doing some oceanographic work and as I understand it your booms and your deck machinery were essentially designed to handle navaids, bell buoys and things like that. I feel that compared to the other stuff we use in oceanographic vessels it's relatively slow and awkward and I wonder if the Coast Guard has considered getting, at least for some of their buoy tenders that are going to be used for this kind of work, articulate cranes or deck wrenches that are a little more flexible?

Tom McKerr, USCG

I would like to know too and I'm not sure what the ACUSHNET has. She's our primary oceanographic vessel and works closely with NOAA and buoy retrieval and deployment. I'm not sure what type of boom she has now. As far as the other tenders go, it would be nice but I don't see any progress in that direction. It's expensive. The tenders are being rehabilitated. They are 30 years old now and they'll be around for another 30 years apparently. They're heavy--the Coast Guard is married right now cost-wise to a heavy buoy and heavy ship. Some day we would like to wind up with an all-plastic buoy system or basically plastic, and light hull ship, but it's a long ways down the road.

Bob Walden, Woods Hole Oceanographic Institution

I sympathize with you and couldn't agree more on the fragility of appendages on a buoy, particularly antennas, having had many of them broken off ourselves and the problem does not appear to go away. We put more and more gear atop these buoys but it's worth thinking about ways in which to make it more immune to banging against the side of a ship. In that respect, I'm a firm believer in adding large bumpers to most buoys. Normally it's impossible to prevent some kind of motion of the ship against the buoy because of the difference in periods and sizes. The last thing, in regards to a two point pickup on a buoy, we recently designed a spar buoy which because of its length requires a bridle. We got around this by making the bridle up in advance and attaching it to the buoy for the single point pickup and recovery line so that you only had to get down to the point of pickup with a single line. I think that if some thought is given to having to make a two point pickup as you would with most spar buoys because of their length, you can devise a way such as this for doing the job.

Tom McKer, USCG

This is true. I've seen this in some of our own experimental buoys. It does work--it's not really the best system, but again we're dealing with a spar where it would break if you tried to pick it up on a single-point moor. It is the best solution I've seen so far to the problem. The other problem is getting it aboard. You've got to have some sort of way of setting it down. Many of these buoys are fine, you can get them over without too much problem, but getting them back up on deck and trying to get them stopped sliding around is another problem. If you're going to have to require a special bridle, chafing blocks or anything like that, make sure the vessel that is going to work them realizes that he is not going to be able to set it down after he has already got it up there unless he has the equipment. I've had this happen--all of a sudden there's a buoy coming up with a great big keel on it and what are you going to do? You've already got the buoy, you've already disconnected it and you're stuck with it.

Mike Hall, NOAA Data Buoy Office

I thought I'd make the one observation that the ACUSHNET which is the only Coast Guard vessel dedicated to data buoy handling is being outfitted with a specialized crane in the next month or two. I know the gear she had originally was attended with Manilla bangs and all this stuff--strickly pre-World War II, and I understand that's what they put back on her when she went back into oceanographics. I would hope they would put an articulated type of crane on her.

Various people who we have talked to in the Coast Guard have mentioned the fact that some years ago they designed a line-threading device to put steady lines on buoys prior to pickup or to install the hook into the navigational aid buoy but we've never been able to find anybody that knew very much about them. Can you shed some light on this type of device and if they do exist in the Coast Guard right now.

Tom McKerr, USCG

Well, someone did design a line rigging hook which basically was a square with a snap-ring in it and you'd slam it on to the bale and the snap hook would shift sides and you just pull it back through on the other line. The other end of the snap hook would be on the line that would be into your main hook. We found that these lasted about three times and at $80 a throw it's expensive. So what we've come up with now is a 12¢ special--a long brass rod, you bend the tip of it, run a line to the top, bend it in a kind of half-arc and push that through and pick it up with the boat hook and pull it right through and you're in business. Save $79.88. It works forever.

Speaker Unidentified
Just an added bit of information-- not that we couldn't work this out with the Coast Guard, we went to NASA and NASA designed line threading hooks and devices to pick up their Mercury, Apollo and Gemini capsules and these devices worked. In fact, we tried one on our working buoys which are current measuring buoys. This year we got an urgent request saying they needed a back-up hook--not that the one they had was not working, but just the fact that they liked it so well they were afraid something was going to happen to it and their whole techniques for retrieving would be destroyed. They wouldn't know how to get the buoys out of the water.

They were free because they were surplus from NASA. I think they could be made very inexpensively because there are very few moving parts on them, but they have a 14 foot aluminum pole where you can use as a line-threading device and then you can pull in your hook to automatically snap on and retrieve.

Speaker Unidentified
The problem is not too uncommon as you develop a buoy; you build it--it's very expensive--you come back, and it's gone; a victim of vandalism. Hall has had some experience with that and I wonder if anybody could comment on that. How you can avoid this vandalism of an expensive buoy? Maybe Bob Heinmiller or Bob Walden can talk about

their history of a big expensive buoy that last year disappeared and--any ideas at all as to what you can do?

Speaker Unidentified

I can tell you something that was tried. We at NAFI had built some bigger buoys than you saw here--several thousand pound version torrids and I can remember seeing on some of the buoys--DANGER HIGH VOLTAGE, 20,000 VOLTS. I can't tell you how it works, but it was tried. If they can read English it might scare them away. My sister was mentioned a while ago. When she was working on a buoy program, they had a similar problem up in the Chesapeake Bay on vandalism so they got one of the test buoys and put on it DANGER 5000 OHMS and that apparently worked because nobody knew what an OHM was.

Speaker Unidentified

We've had two--one case well documented by a tension recorder and the fact that the thieves left behind (this is in open ocean down dear Bermuda) some of their own line that they tried to haul the mooring with and it parted. We got their line back which was traced to a European manufacturer but we couldn't go any further than that; and the other mooring--the ship had to sit there several miles away--she didn't sit there, she was trying to get over there but she was several miles away at night watching them on radar and through binoculars while they picked the darn thing up and walked away with it. They couldn't catch them and we've never tried anything special. We've figured anybody who's really serious enough to go out there and pick up a 5 foot or 10 foot diameter float that weighs a couple of tons is going to know enough not to be deterred by signs, but in shore people have a lot of trouble with vandalism--people shooting at them with rifles and things--near-shore data buoys.

One of the things I also think might help is if we would stop giving rewards for return of these things. I know the Navy in the past has rewarded whoever the fisherman might be some kind of a prize and once you do this then they become aware of the fact that if they do take something off the buoy they can say they found it and get the reward, (I'm not sure what it consisted of) but it was certainly bribery to a certain extent to get them to deface the buoy in some manner.

Bob Walden may be willing to tell you afterwards a story where he had a buoy stolen close in shore, picked up kind of adrift but the guy held it sort of for ransom. It was a 5 foot diameter mine case and the guy eventually buried it and was demanding money for it before he'd tell anybody where it was.

Tom McKerr, USCG

If a vandal's determined, there's no way you can stop them. I've gone out to buoys--some of our buoys are bell buoys--these are nice brass bells--you know, U. S. Light House Service, which was established in 1905 - there were torch marks all over the buoy where they cut them right off and sold them not for scrap so much but for some of these front yard decorations. You're not going to stop them, but you can try and discourage them or make them think twice. I think the 5000 OHM thing is pretty good. I would be hesitant to give false information such as 20,000 volts if there really isn't 20,000 volts there.

John McFall, NASA

I have one sea tale to tell on Scripps. They put a sign on their buoys, "Radioactivity - if you're this close to the buoy, see your doctor immediately," so apparently people leave those alone. The other sea tale I have--one thing that we have done is put our name and telephone number on the buoys and let people call you collect. I'm sure you've done that. Another way is to put "back-up" radio transmitters inside the buoy and we did locate one buoy in a station-wagon down at Nags Head.

Speaker Unidentified

That's dirty pool, a guy's at least got to have a sporting chance.

William Hakkarien, Naval Air Systems Command

I'd like to publically acknowledge the good work the Coast Guard did in handling the NOMAD buoy from 1958-70. It was a great piece of research and development seagoing operations and the Coast Guard was never found wanting and I say the greatest success came with strange crews and changeover personnel. We had a dockside test run before the seagoing operation and we had good success and I want to say thank you for the fine work they did.

Speaker Unidentified

Let me make a comment. I saw some movies that were probably shot in the early 1960's showing deployment of some of the NAFI buoys--the bigger ones and some of what you described went on, buoys flying all over the place, sailors dodging to keep from being hit by the buoy and apparently there was a captain out there who had some initiative and no money but he was able to get some scrap iron and he built a slide--I've never seen it used since, apparently it was taken off the ship after the test--it apparently got rid of almost all of the problems in other words, they'd set the buoy on the slide and slide it into the water and you had no contact between the buoy flying up in the air with lines on it and the like, it appeared to be much safer, much quicker, and I'm just surprised that after having seen that there wasn't some money put into actually developing it for that kind of launching sequence.

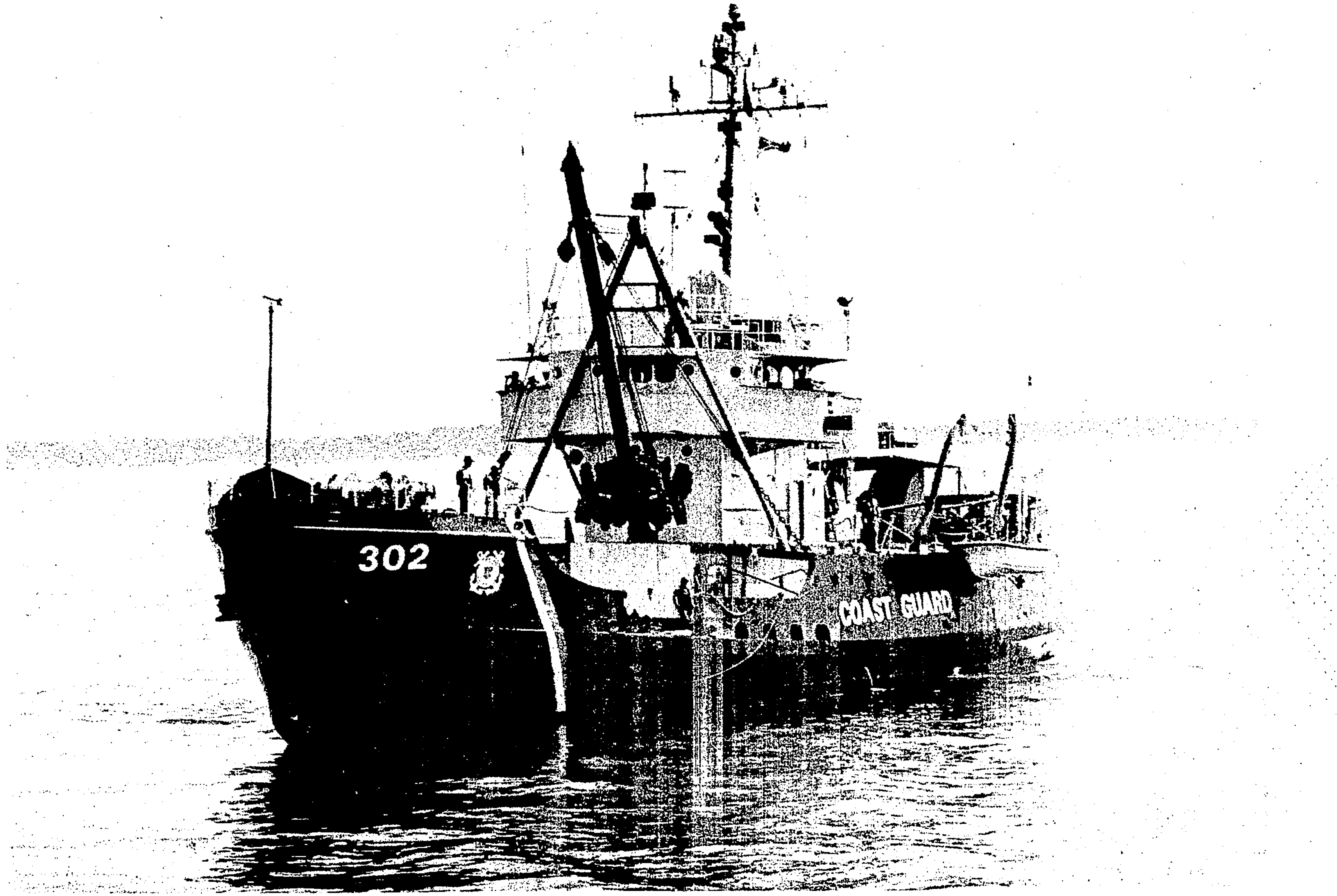


Figure 1. A-CLASS SEAGOING BUOY TENDER

Figure 2. ICE-BREAKING BUOY TENDER

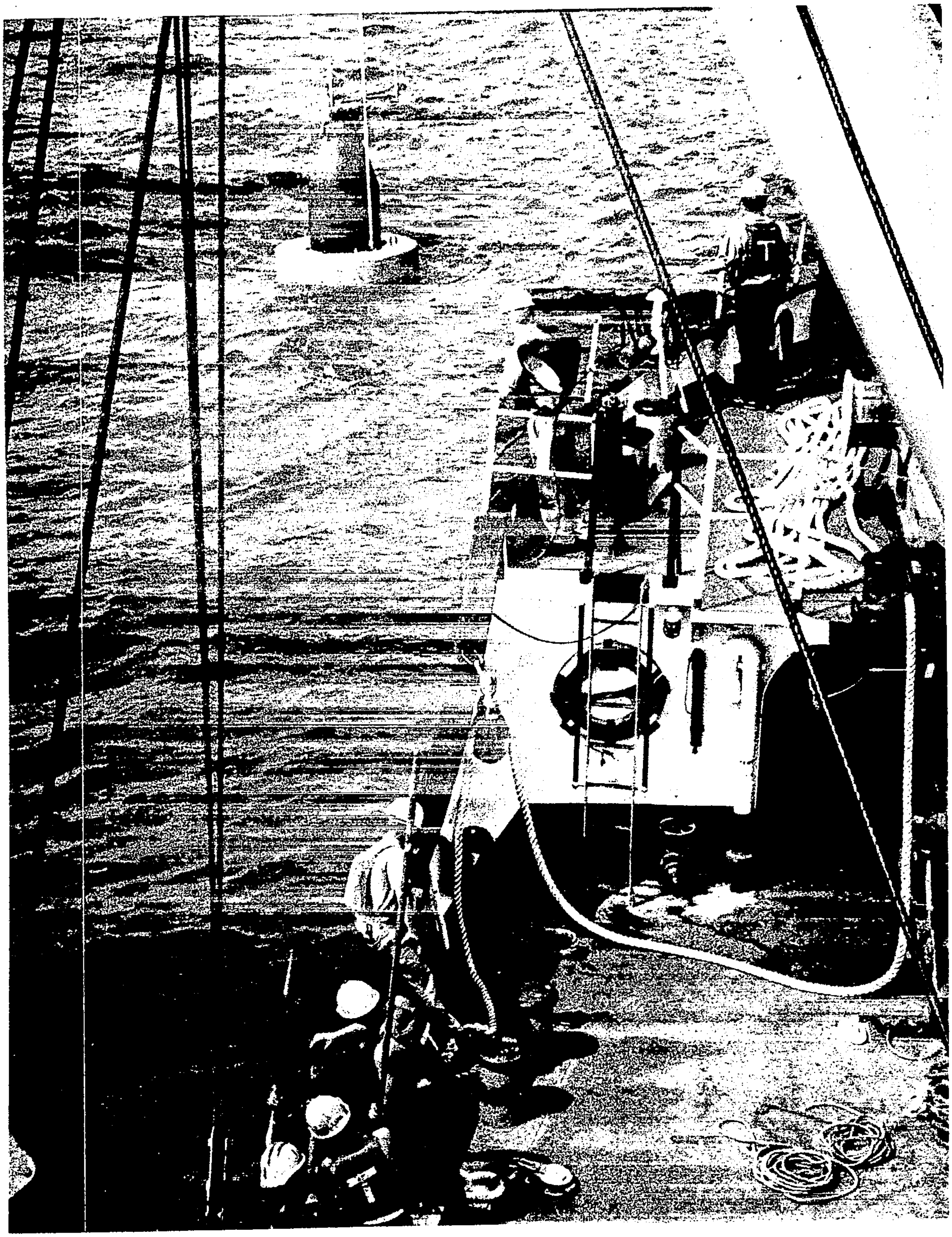


Figure 3. TENDER APPROACHING BUOY

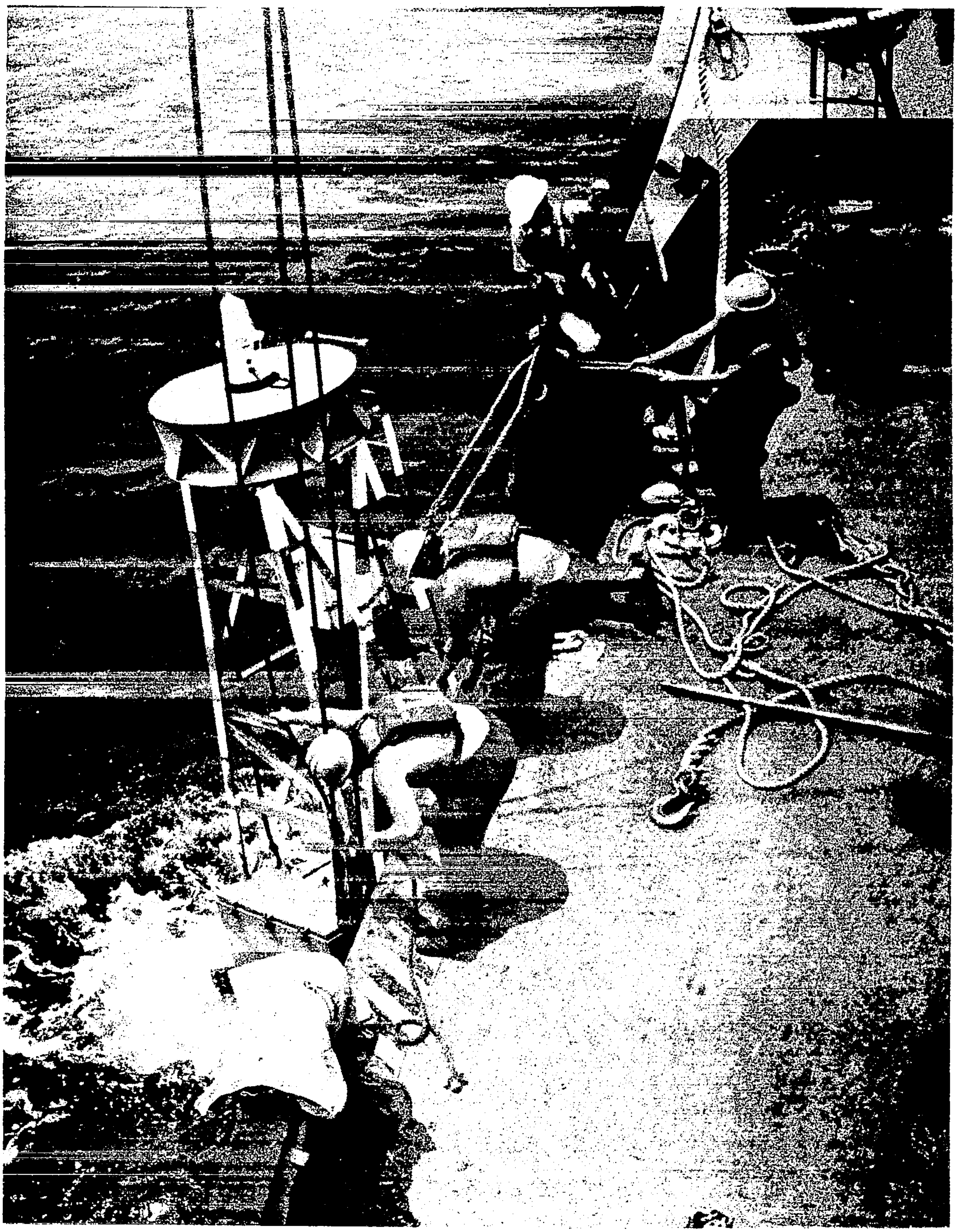


Figure 4. FIRST LINE SECURED TO BUOY

Figure 5. BUOY ON BOARD BEFORE SECURED TO DECK

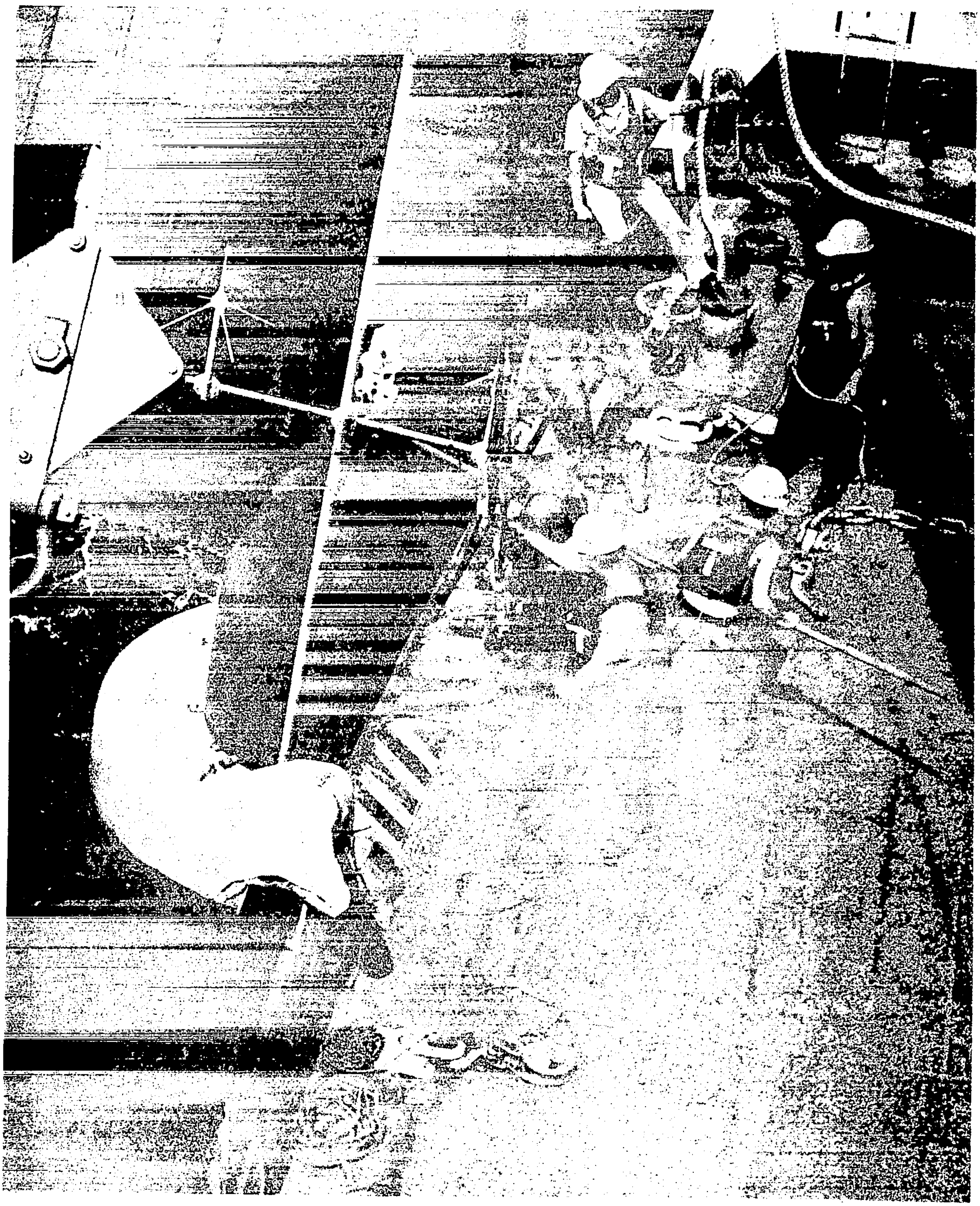


Figure 6. CAUTION BEING EXERCISED WITH ANTENNA ON BUOY

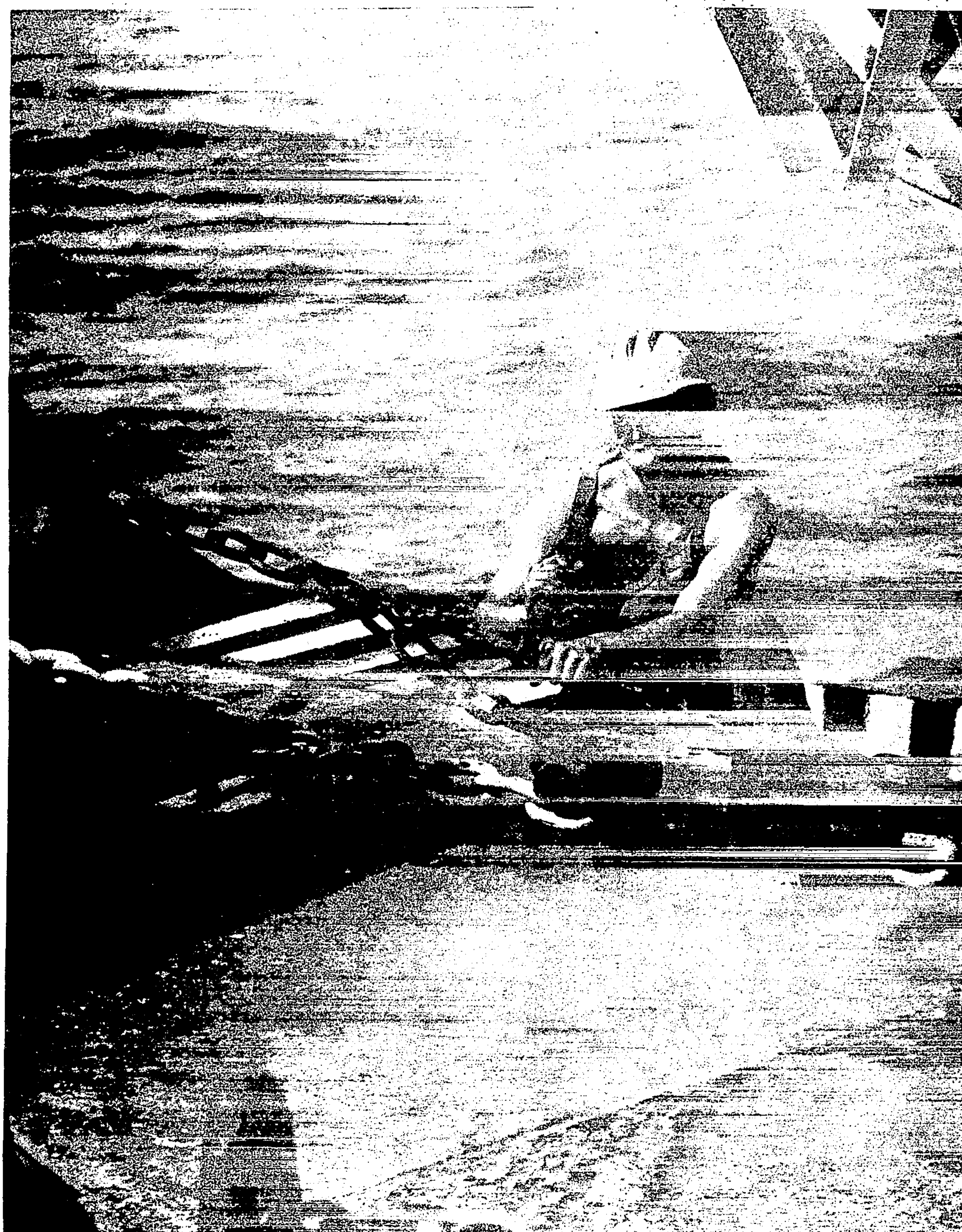


Figure 7. BUOY BEING SECURED TO SHIP DECK

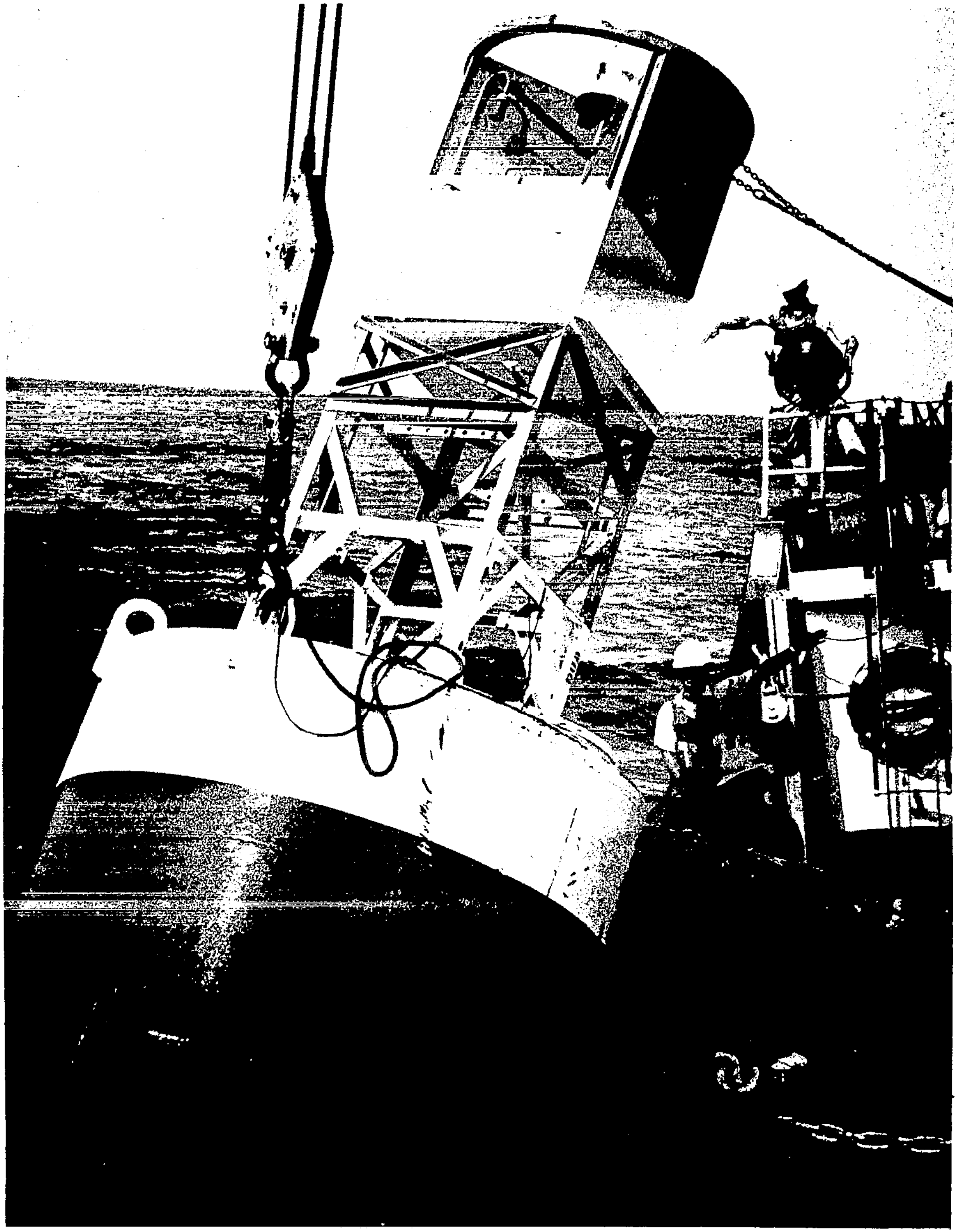


Figure 8. STANDARD 8-FOOT BUOY COMING ABOARD

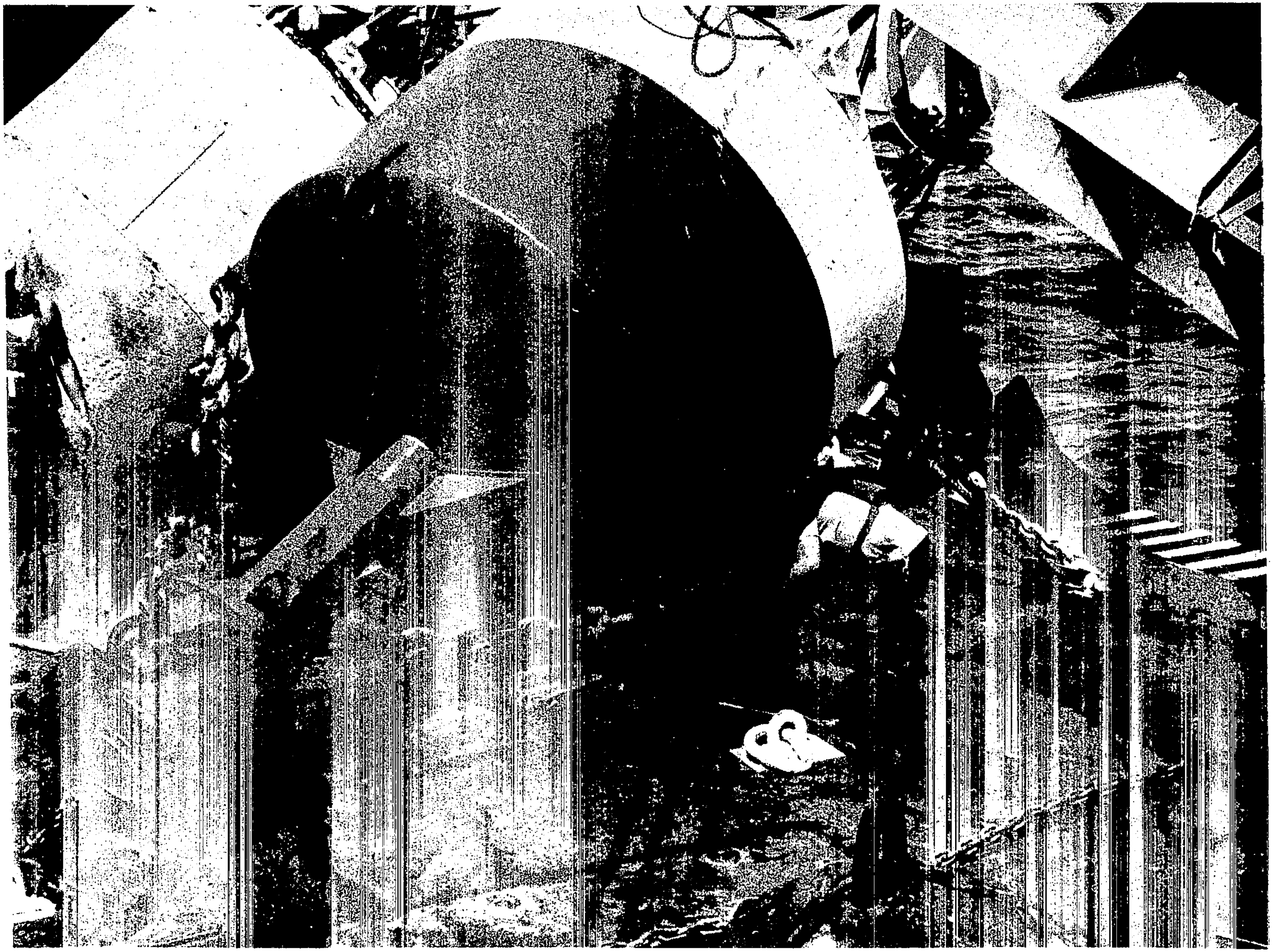


Figure 9. STANDARD 8-FOOT BUOY BEING SECURED TO DECK

METHODS FOR SUCCESSFUL DEPLOYMENT AND RECOVERY OF SMALL SCIENTIFIC BUOYS AND AN EXPENDABLE AIR DROPPED BUOY CONCEPT

by
Leon Williams
National Aeronautics and Space Administration
Langley Research Center

I would like to discuss several proven methods for the successful deployment and recovery of small scientific buoys. (For the purpose of this paper, small scientific buoys are classified as those buoys with a total dry weight of less than 600 pounds.)

Several different methods used in the deployment and recovery of this type buoy will be examined. They are:

1. Deployment/recovery by small boat
2. Deployment/recovery by ship
3. Deployment/recovery by helicopter
4. Deployment concept from fixed-wing aircraft

The Author: Mr. Williams is a Senior Project Engineer at the NASA Langley Research Center and is responsible for design and deployment of new research tools and techniques. His primary concern has been with preflight aerodynamic testing and recovery systems development. However, for the past 2 years he has been directly involved with the application of recovery systems technology in the marine field and particularly with free-drifting buoys.

First, let us examine proven methods of deployment and recovery from small boats: Storage space is of immediate concern here. Where multiple buoy deployments are required the systems must be:

1. Designed for "Breakdown" or "Quick Disconnect" to allow maximum utilization of cargo space.
2. Designed for quick and easy assembly upon arrival at the deployment site.
3. Designed to utilize existing davits and hand winches.

A typical mission where these restraints were satisfied was conducted by NASA and VIMS personnel teamed to obtain actual data on tidal flow in the James River, on the Hampton Flats, and around the Newport News Point, for the Virginia State Highway Department. The mission required the use of four drogued radar buoys which could be continuously tracked by land based radar. Figure 1 is a photograph of this type buoy. The surface float which houses the radar transponder and power pack is a 10 inch deep by 22 inch diameter disc. The drogue is a cross 3 feet deep by 5 feet wide. Mission requirements included varying the drogue depth from the surface to 15 feet below the surface. The completed buoy dry weight is approximately 145 pounds. Figure 2 is a close look at the drogue and reveals that two opposite panels are designed to be hinged and thus fold flat. Handles have been installed to aid in off and on loading both onto trucks and boats. A sonic pinger is also installed to allow recovery in the event of a failure in the connecting link between the surface float and the drogue, or, damage to the surface float resulting in loss of buoyancy. A bungee cord is used to open the drogue into a cross shape and hold the drogue in this configuration throughout the test program. Figure 3 shows a drogue in its folded position being readied for deployment. Note that the hinged panels are held in place utilizing two wooded "U" shaped restraining pins. These pins remain in position until the drogue is in the water and they are then retrieved to the boat by lanyard. Figure 4 shows the davit fixed to the starboard side and a hand winch which is used to lower the drogue into the water.

The surface float is being deployed into the water. This completes the buoy deployment and upon lock-on verification by radio from the land-based radar tracker, the buoy is released as a free drifter. To effect recovery after the mission, the procedure just described is completed in reverse order.

Figure 5 and 6 show a typical recovery procedure. Where the drogue is short-coupled to the surface float the buoy can be extracted from the water in a one-piece operation as in figure 5. When the buoy is drogued at 15 feet a two-step boarding procedure is recommended where the surface float is recovered first followed by recovery of the drogue. In figure 6, the buoy has been boarded and personnel are preparing the drogue for storage on the aft deck.

Next, let us look at deployment and recovery methods which have proven successful in ocean work. The design requirements are:

1. Designed for ease of on-off handling
2. Designed to withstand impact
3. Designed to reduce the effects of wind and wave forces

A typical buoy is shown in figure 7. This is a satellite interrogated buoy which has a total dry weight of approximately 560 pounds. The buoy has been drogued to depths of 30 meters; is equipped with sub-surface temperature sensors, radio beacons for location by aircraft or ship, flashing lights as aids to navigation, and solar switches for conservation of onboard power. A lifting hook for deployment and recovery is shown on the surface float. This type buoy is normally loaded aboard the delivery ship in a two-step operation. The surface float is loaded aboard first as shown in figure 8. The sub-surface drogue, as shown in figure 9, is then boarded and the buoy or buoys are secured to the deck for delivery to the test site. It is noted that simple hooks attached to a line guided through a pulley to a power takeoff winch is sufficient for loading purposes. The pulley can be attached to a pivoted overhead boom thus providing any horizontal movement which may be required. Upon arrival at the test site, the sub-surface drogue is readied and off loaded first (figure 10). A lanyard from the drogue to the ship's deck maintains control of the drogue after water entry and prior to deployment of the surface drogue. This also frees the overhead hoist and the surface float is then lifted for deployment as shown in figure 11. At this point the connecting link between the surface float and sub-surface drogue is payed out to verify no entanglement in the link which is comprised of chain, sensors, and wiring. The float is then lowered toward the water as shown in figure 12 and into the water as shown in figure 13. The lanyard to the sub-surface drogue is then retrieved and the buoy becomes a free drifter as shown in figure 14.

Now, let us investigate advantages of aerial deployment and recovery. The helicopter has proven a very useful machine in the deployment of buoys in bay and ocean side operations. It has also provided the answer to quick transplants where buoys have drifted out of the area of interest, or, are in eminent danger of being trapped in offshore fishing nets, on sandbars, etc. Figure 15 is a cross-section through the helicopter, and shows the makeup of the lifting system. Figure 16 shows the ground layout of a radar-tracked buoy and helicopter system during preparation for deployment. With all systems checked out on the ground, the Helo becomes airborne above the buoy as shown in figure 17. The crew chief observes the ground operation and is the eyes of the pilot through the use of the onboard intercom system. In some cases a ground-based observer has also provided accurate system description to the pilot during the initial lift-off phase and until the total system is airborne (figure 18). While delivery to the test site is usually accomplished in minutes, caution must be exercised to insure a stable towing configuration and establishing optimum air-tow speeds through an experimental test program. At the deployment site, the crew chief again uses his radio link to the pilot to request the desired deployment maneuvering and to inform him of the final release of the free-drifting buoy. Here again, the procedures are reversed to effect the recovery of the buoy. The helicopter can be directed to the recovery area by land-based radar or fixed-wing aircraft. While the figures identify a radar type buoy, radio type buoys have also been successfully deployed and recovered utilizing this same technique. Now, just a few comments are in order in terms of the recovery aids which are used with the buoy system just described. The four basic types used are:

1. fluorescent paint
2. flashing lights
3. recovery beacon
4. sonic pinger

Fluorescent paints are applied to the upper surface of the floats as shown in figure 19. Where multiple buoys are deployed these surfaces have been divided into quadrants with colors alternated. This scheme has allowed positive identification from aircraft of a particular buoy in several instances. Flashing lights are also installed on all of our free-drifting buoys (figure 20). These lights flash every 3 seconds and have a life of approximately 8 weeks and a visual range of approximately 0.6 mile. In addition, the buoys are each fitted with a recovery beacon which transmits on a frequency of 235 megacycles (figure 21). Receivers are installed on aircraft which receives this signal at ranges up to 70 nautical miles when the flight altitude is 10,000 feet. The range is shortened as flight altitude is reduced. This is a proven recovery system which has been successfully used on numerous occasions. The system is presently being upgraded to not only locate deployed buoys but to determine which buoy of a group is being interrogated. This is being accomplished by modulating the 235 carrier frequency with different tones (300 cycles, 400 cycles, etc).

A sonar system is also a proven recovery aid. Figure 22 shows a typical sonic pinger used here at Langley. The unit was developed in-house, utilizes a mercury 7.0 volt battery, transmits on a frequency of 37 Hz; has a range of approximately 2 miles, a life of 15 days and is salt-water activated. The unit is 1.5 inches in diameter, 3 inches long and weighs about 1/4 pound. An attachment eye is fixed on one end. These pingers are normally mounted on buoys which are deployed in waters where the water depth does not exceed safe working depths for skin divers and where the mission plan is not expected to exceed 15 days.

Finally, a few comments about an upcoming air-launch buoy concept underway at Langley. This will require that a non-retrievable type buoy be parachuted from a fixed-wing aircraft to the ocean surface where it will free drift, deploy a sub-surface drogue for coupling effect, measure atmospheric pressure and temperature at the ocean surface and sub-surface water temperature. These data as well as tracking data will be transmitted to satellite as required over a life period of approximately 6 months. A preliminary concept is shown in the following illustrations. Figure 23 shows the proposed geometry. The buoy would be about 10 feet long with a maximum body diameter of 6 inches. Air pressure and temperature sensors are located in the top section of the proposed design and about 4 feet above the still waterline. The antenna would be located just below the sensor compartment. The transmitter, power supply and required ballast would all be located inside the 6-inch diameter buoy and below the waterline. Figure 24 shows the deployment method from a C-130 aircraft. The buoy would be placed on the lowered door. A decelerator system could be suspended directly over the buoy which could be released by a manual or automatically operated bomb shackle release rack. Recent discussions with the military indicate that the possibility exists to hand-toss the decelerator from the aircraft allowing it to pull the buoy free of the doorway. A guideway running the full length of the lowered door would be helpful, as shown. Riser lines would be routed and dressed in a riser guide pack to allow a smooth and orderly deployment. Figure 25 shows the deployed air-launch buoy on its decent trajectory. After impact the decelerator could be used as an underwater drogue as shown here.

COMMENTS

John McFall, NASA/Langley Research Center

In regard to air-launched buoys, we have talked to a lot of people around the country. We have experienced a lot of the problems that the oceanographic people have experienced in deployment and retrieval. John Masterson has, of course, influenced our thinking as well as Scripps and Woods Hole and other people around the country. It appears that a small device that could be deployed from a fixed-wing aircraft is definitely in order. We don't intend to do this by ourselves by any means. We have found different people around the country exploring these techniques already. Some people have built and used air-launch devices. We're hoping, and I've been talking to Mike Hall and Ed Kerut and Mr. Winchester out at NOAA Data Buoy and Jack Townsend who are all quite enthusiastic about air-launched devices as well as some people in NASA Headquarters. I think that all together the time has come to work on a small expendable buoy and I'm hoping that a number of people who are here today are going to be able to get together toward some device like this. The focus of the Langley effort in this particular application field is going to be in this direction for the next year or so.

Ron Johnson, Old Dominion University

Your picture of the buoy configuration that you tested with the helicopter is exactly the same one I was using in the entrance to Chesapeake Bay for my work, except that it was launched by our 65-foot research vessel using the techniques you described.

Lt. Cmdr. T. McKerr, U. S. Coast Guard

The only caution I'd put out on expendable buoys comes from a legal one. Somebody someday is going to find one and punch a hole in his boat and the way we win our court cases, we don't, we wind up buying ourselves a boat and you might well find yourselves in the same situation. If you can get them to self-destruct, that'll be fine but if you can't somebody's going to find it and punch a hole in their boat and you've bought yourself a boat.

Bob Heinmiller, Woods Hole Oceanographic Institution

I just want to mention that in connection with all this, Bob Walden and I have a proposal into funding agency to develop systems for a 1600 pound package and we'd like to be able to deliver it from fixed-wing aircraft and recover it from fixed-wing aircraft and we hope also to be able to talk to it with underwater acoustics and from aircraft. We also hope to be able to get the parachute back immediately after launch and save the cost of the chute, because with a package that big the chute gets rather expensive and we're interested in anyone who has a potential use for this or want to collaborate on it. We believe that the systems exist separately and it's mainly a problem of putting them together. The retrieval has actually been done by a couple of different people and we believe it can be made operational.

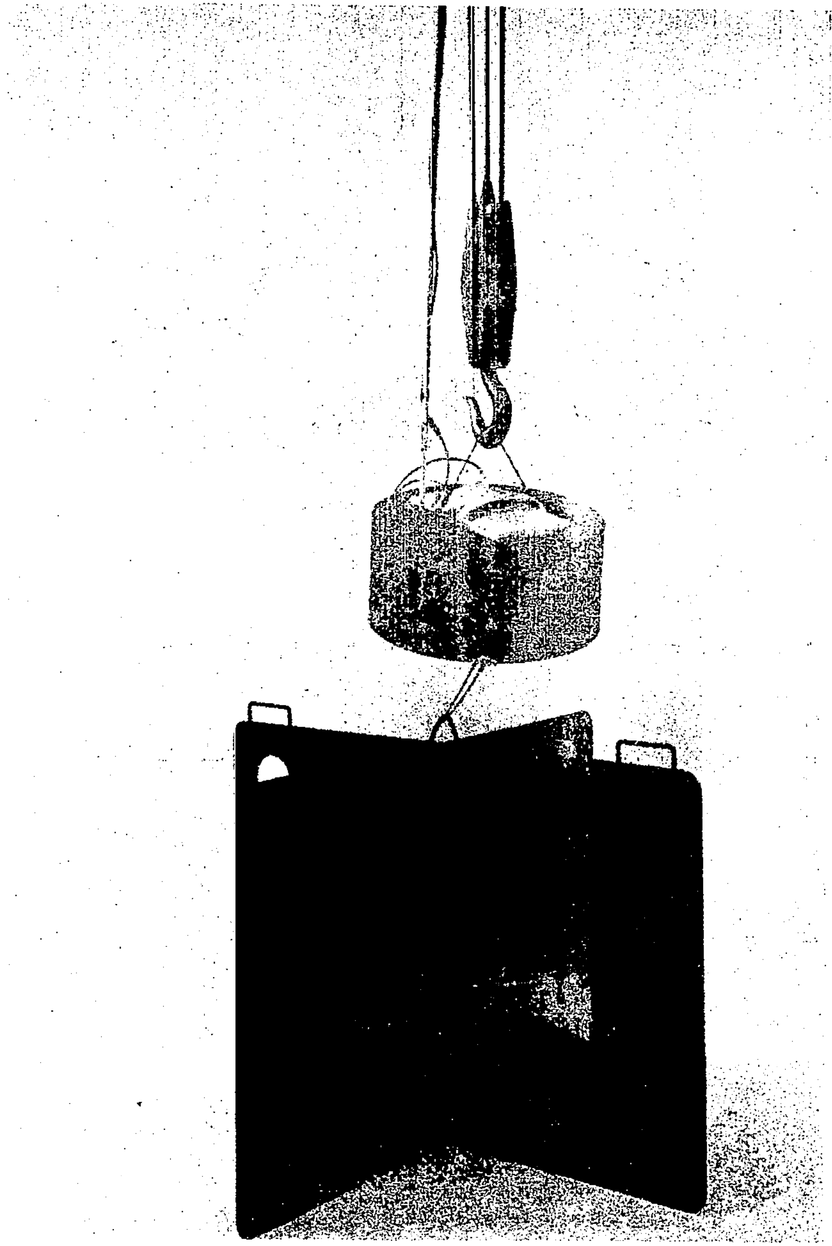


Figure 1. FREE-DRIFTING DROGUE RADAR BUOY

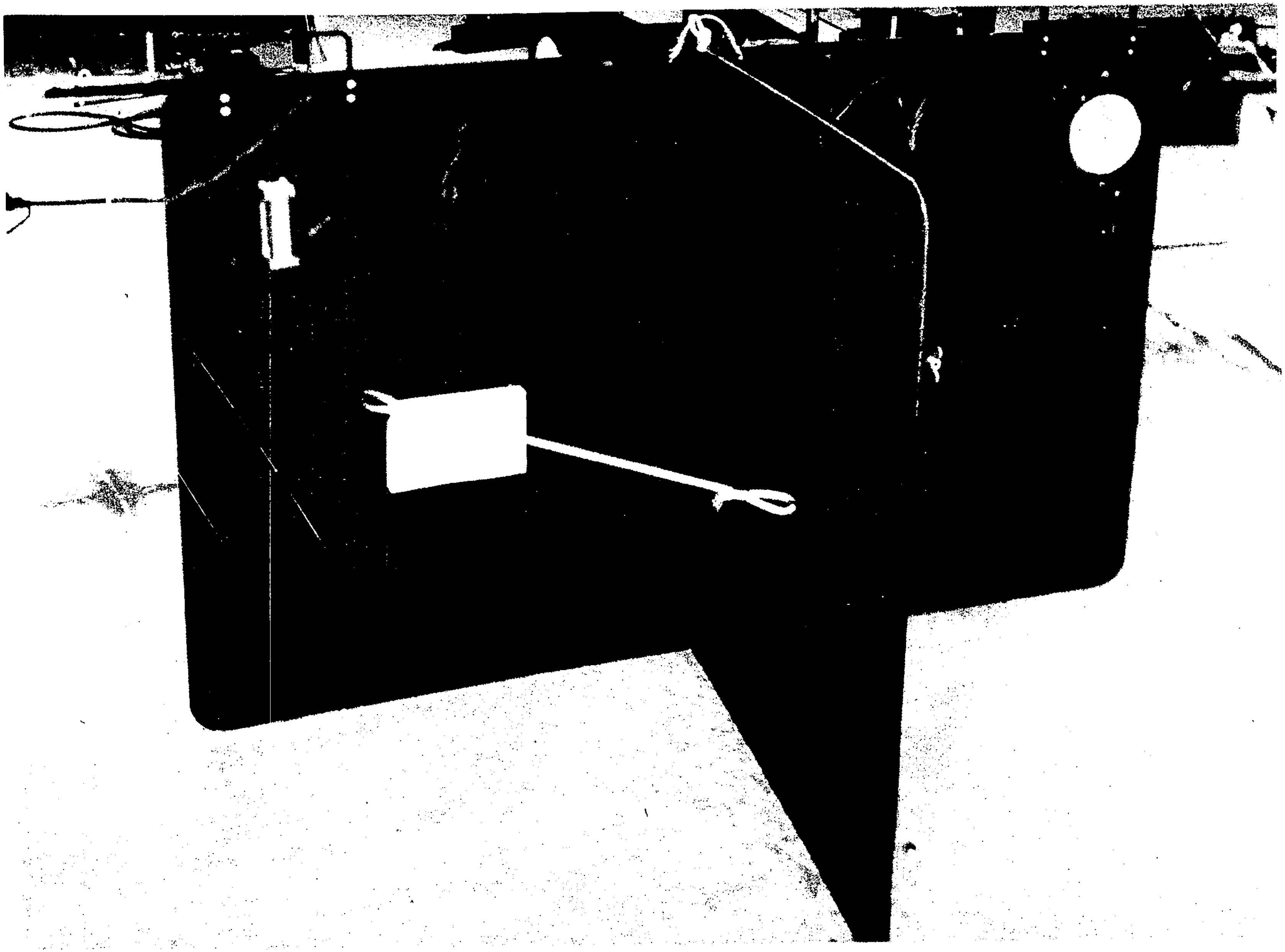


Figure 2. DROGUE PLATES

Figure 3.   FOLDED DROGUE PLATES READY FOR DEPLOYMENT

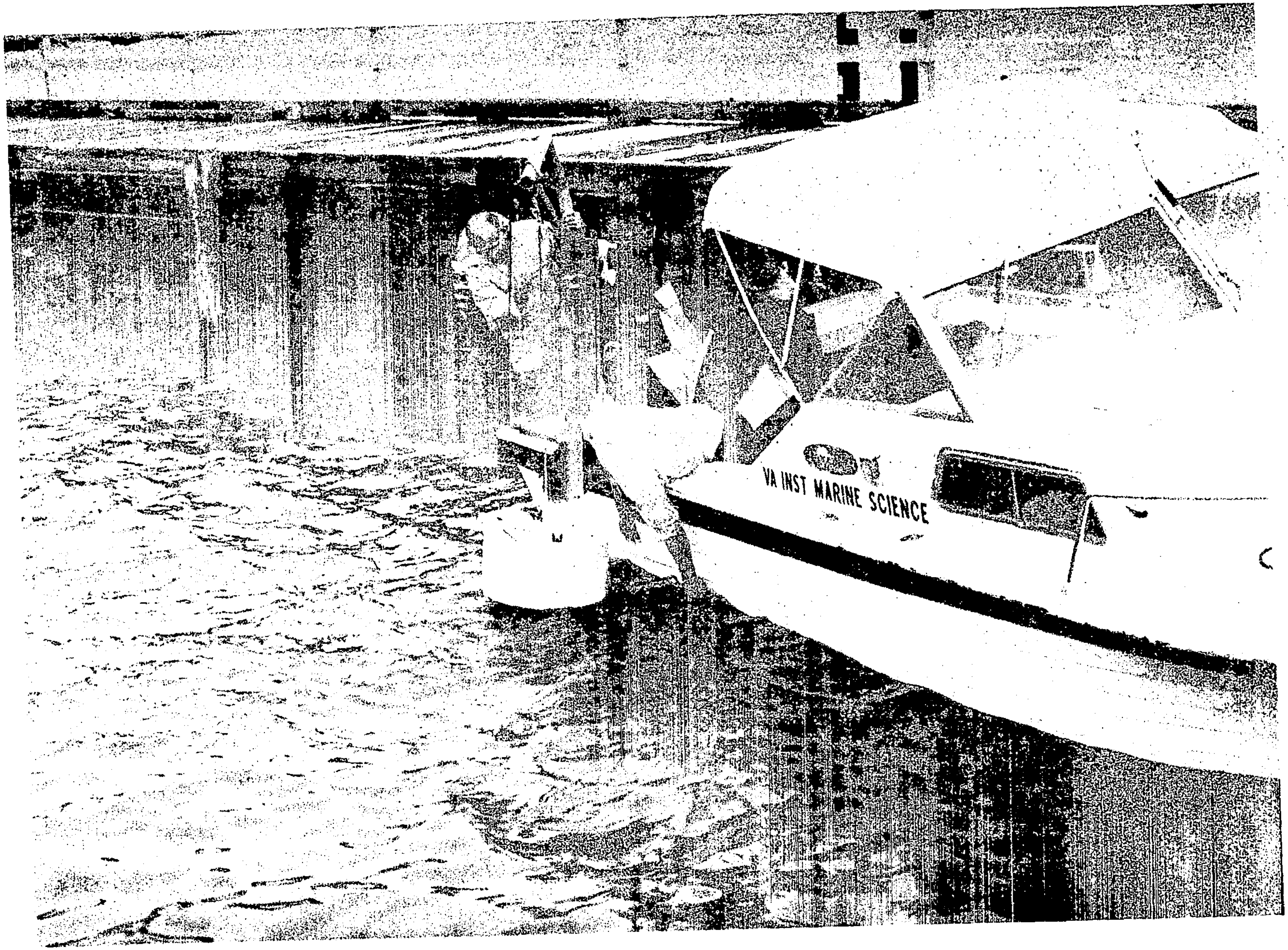


Figure 4. DAVIT WITH HAND WINCH LOWERING BUOY AND DROGUE

Figure 5.   RECOVERY OF BUOY AND DROGUE SIMULTANEOUSLY

Figure 6. BUOY AND DROGUE BOARDED

EOLE BUOY

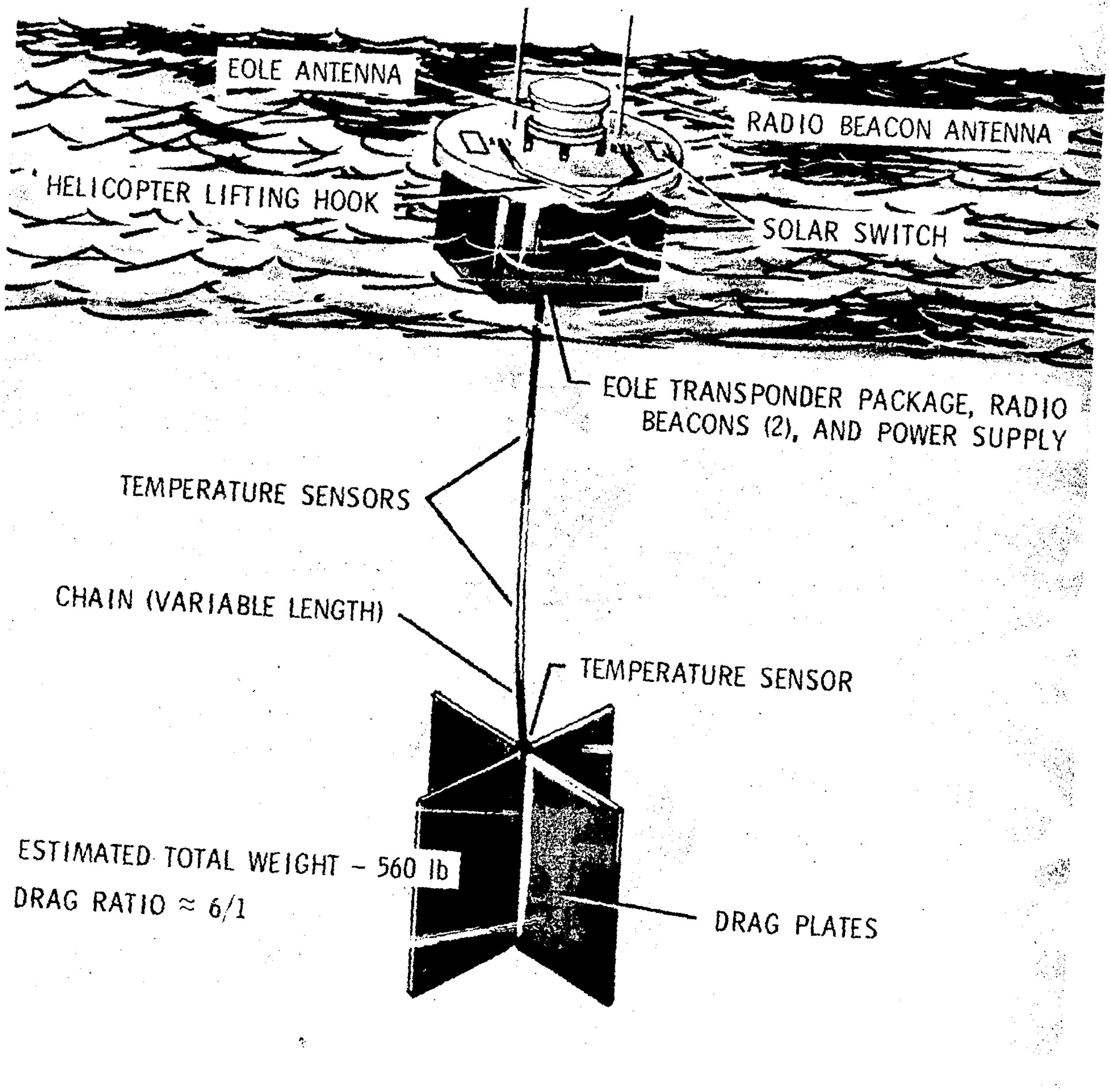


Figure 7. FREE-DRIFTING EOLE SATELLITE INTERROGATED BUOY

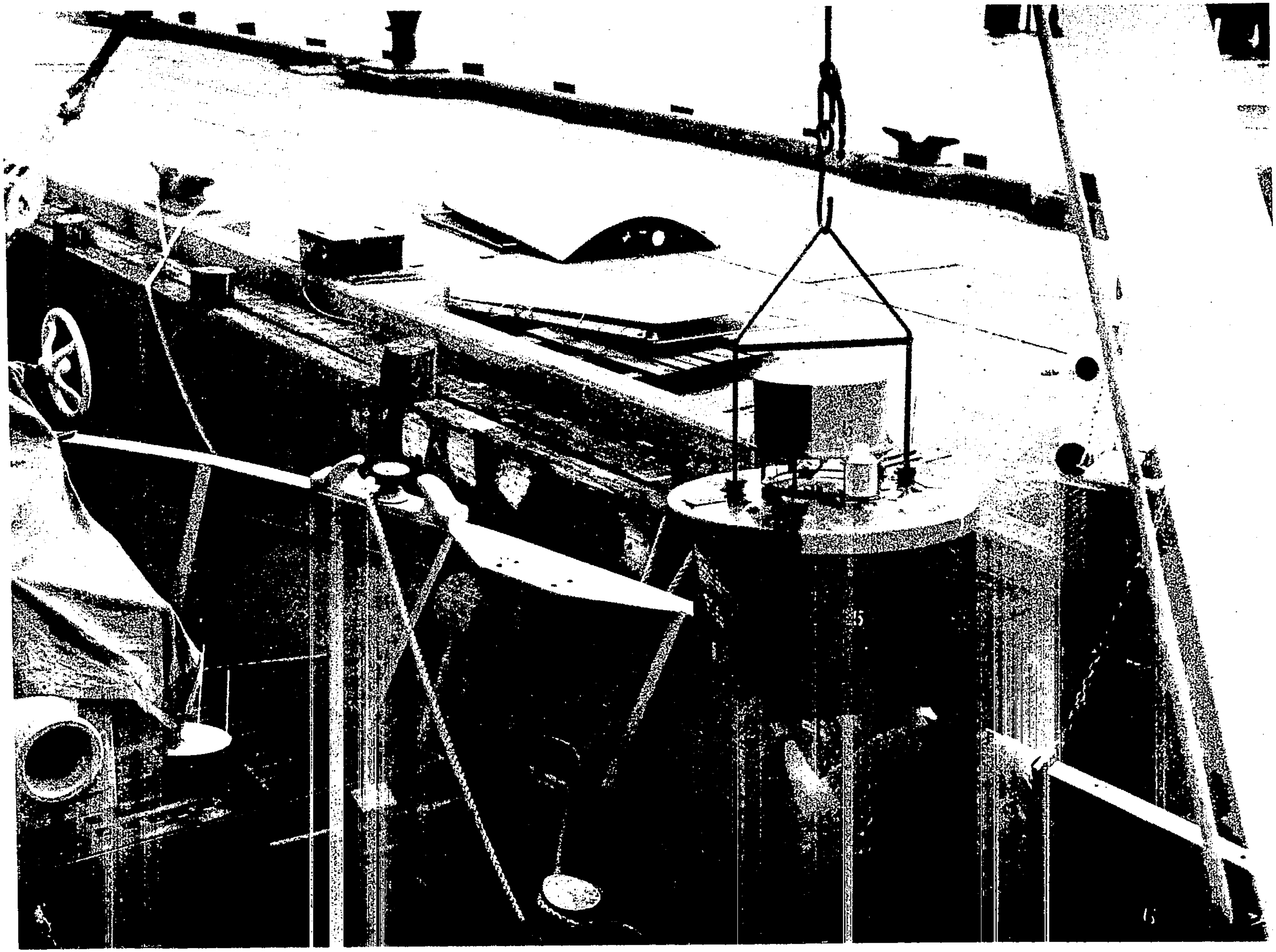


Figure 8. LOADING OF SURFACE FLOAT

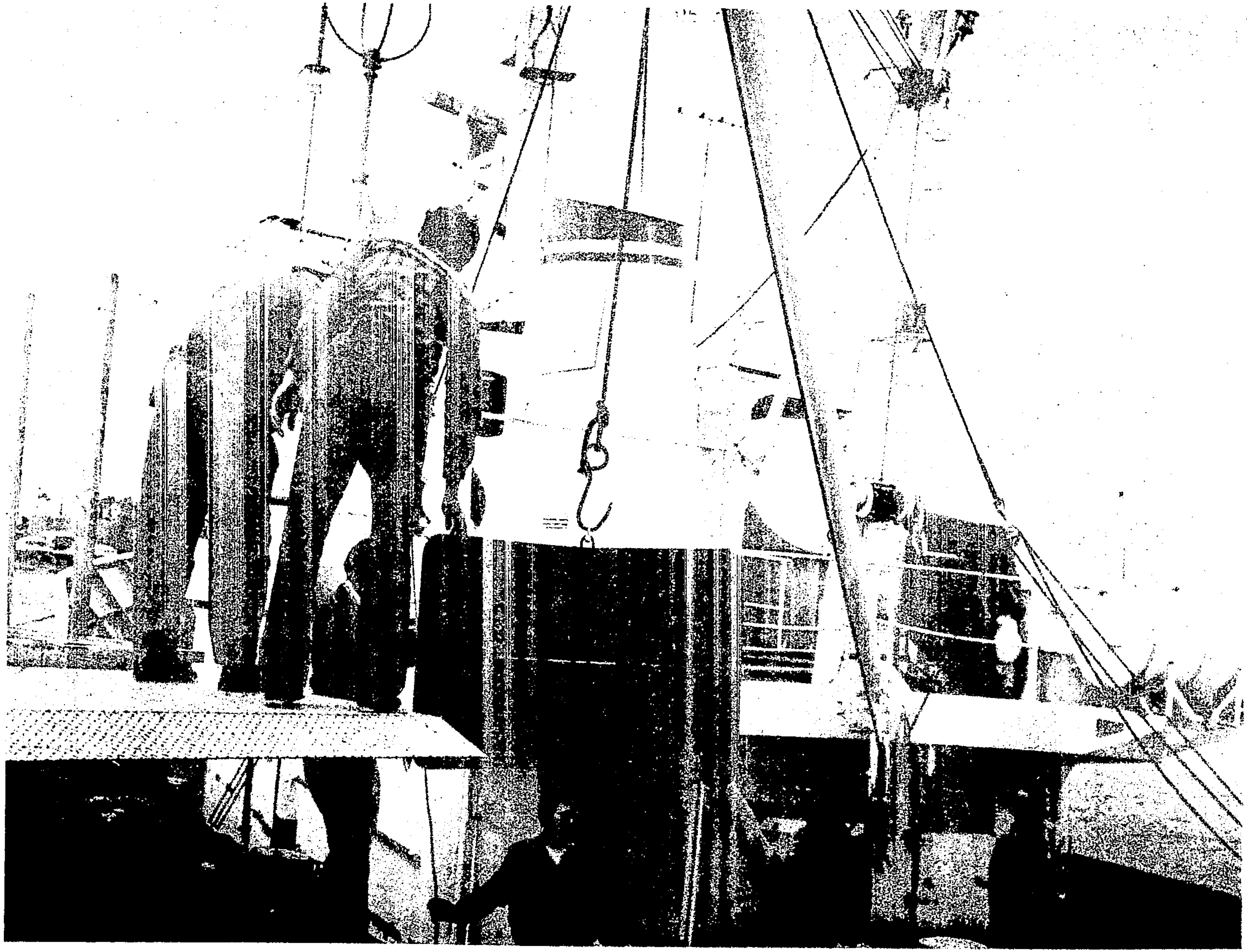


Figure 9. LOADING OF DROGUE

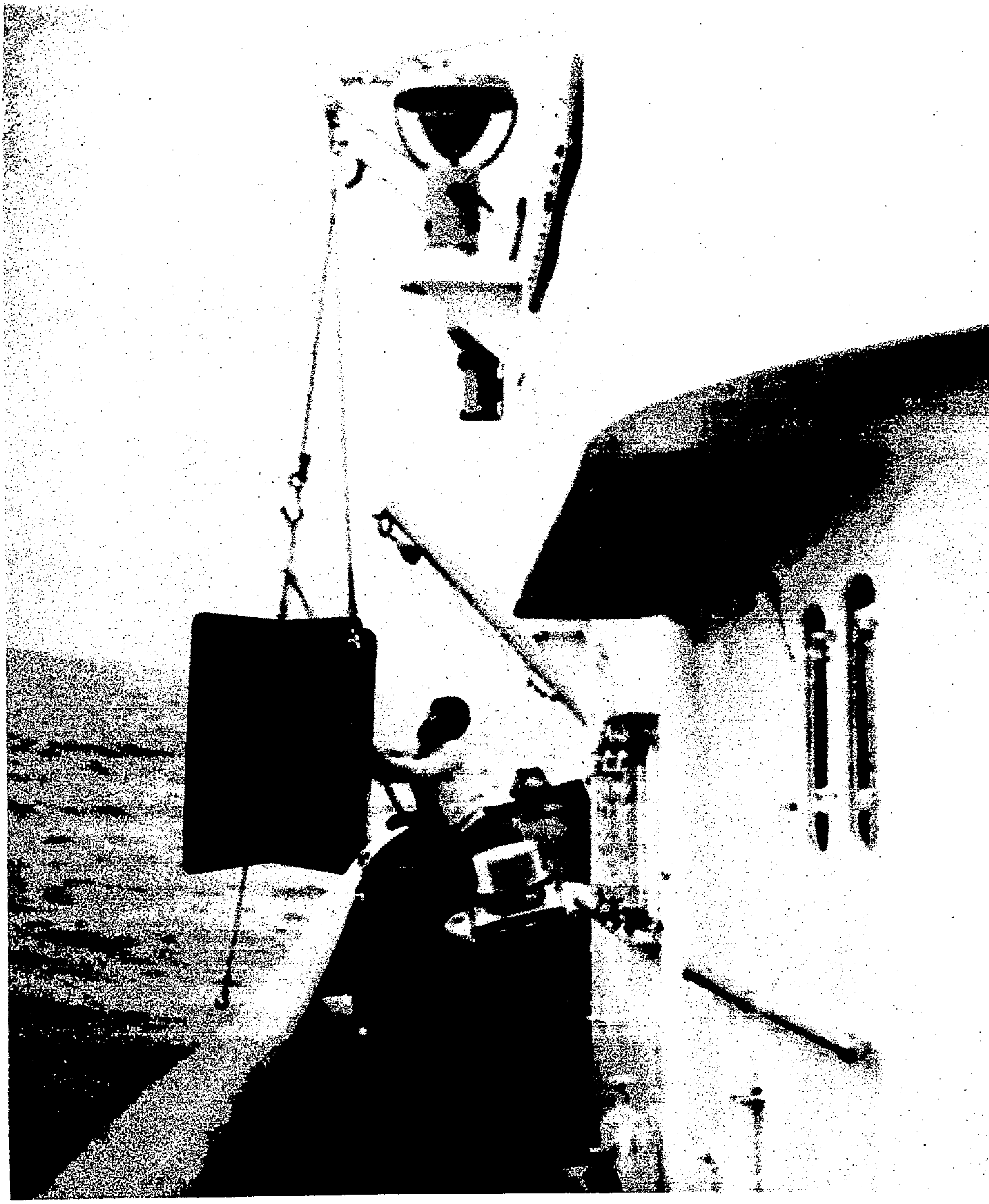


Figure 10. DROGUE READIED FOR OFF-LOADING

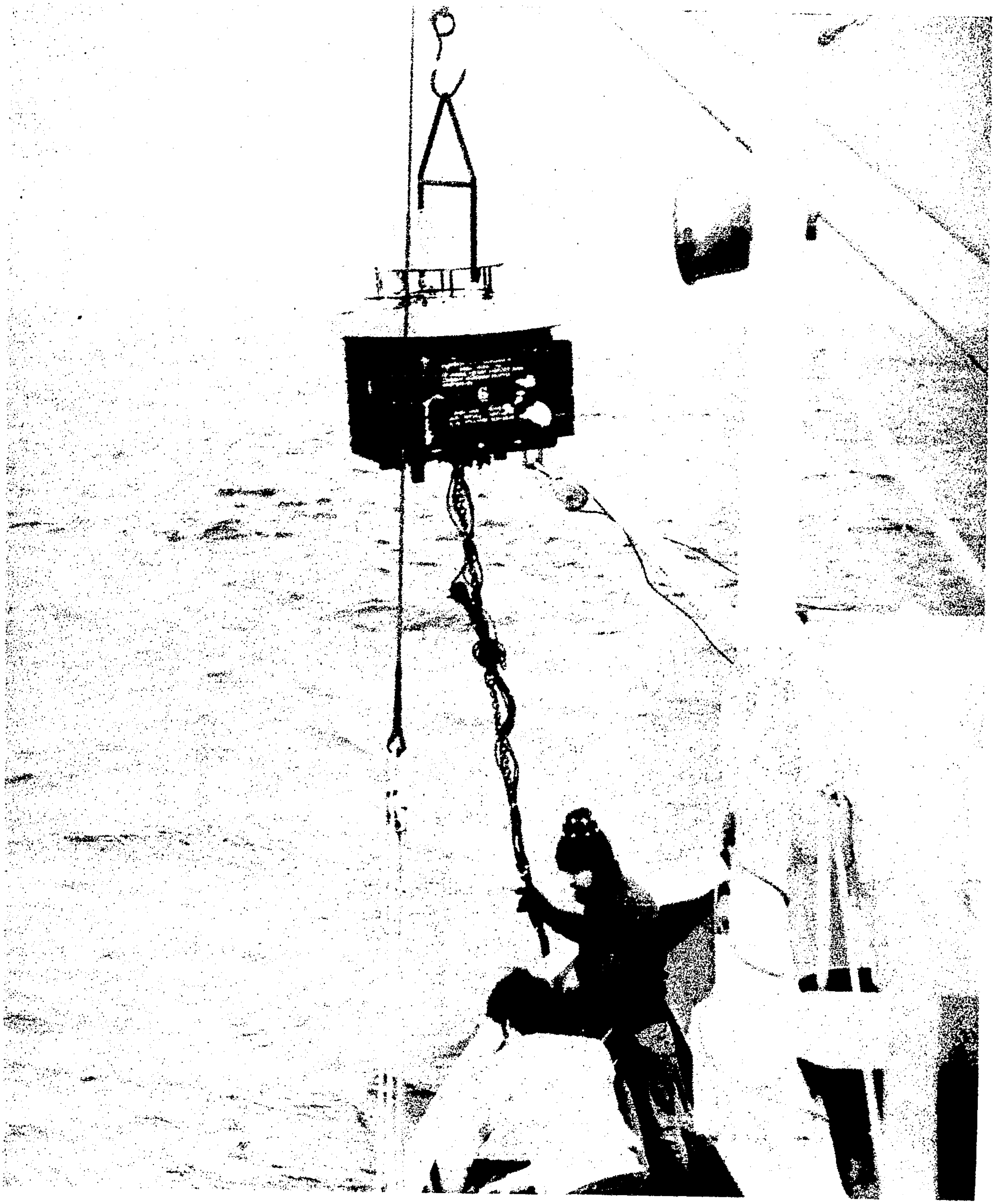


Figure 11. SURFACE FLOAT READIED FOR DEPLOYMENT

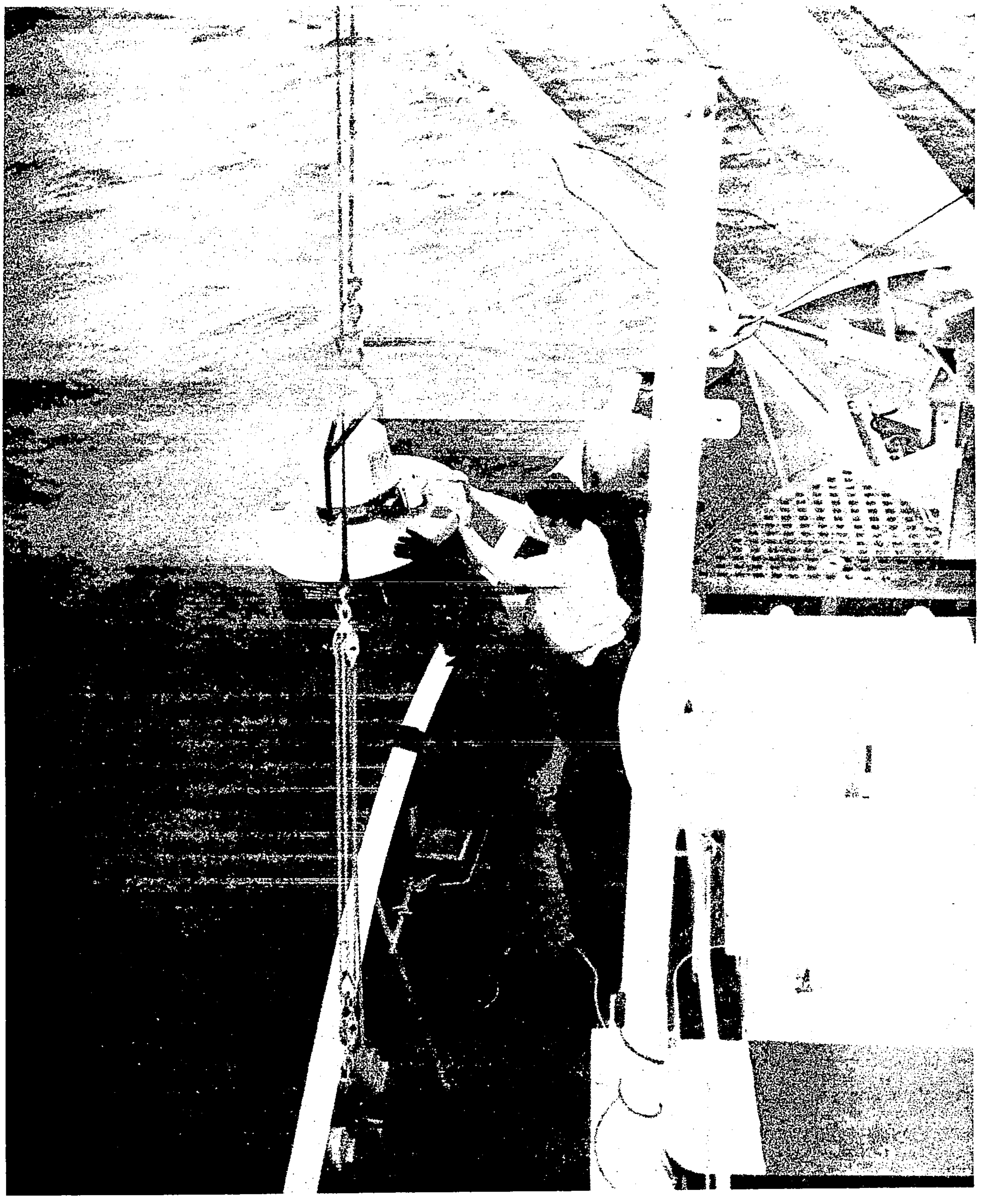


Figure 12. LOWERING OF SURFACE FLOAT

Figure 13. SURFACE FLOAT IN WATER

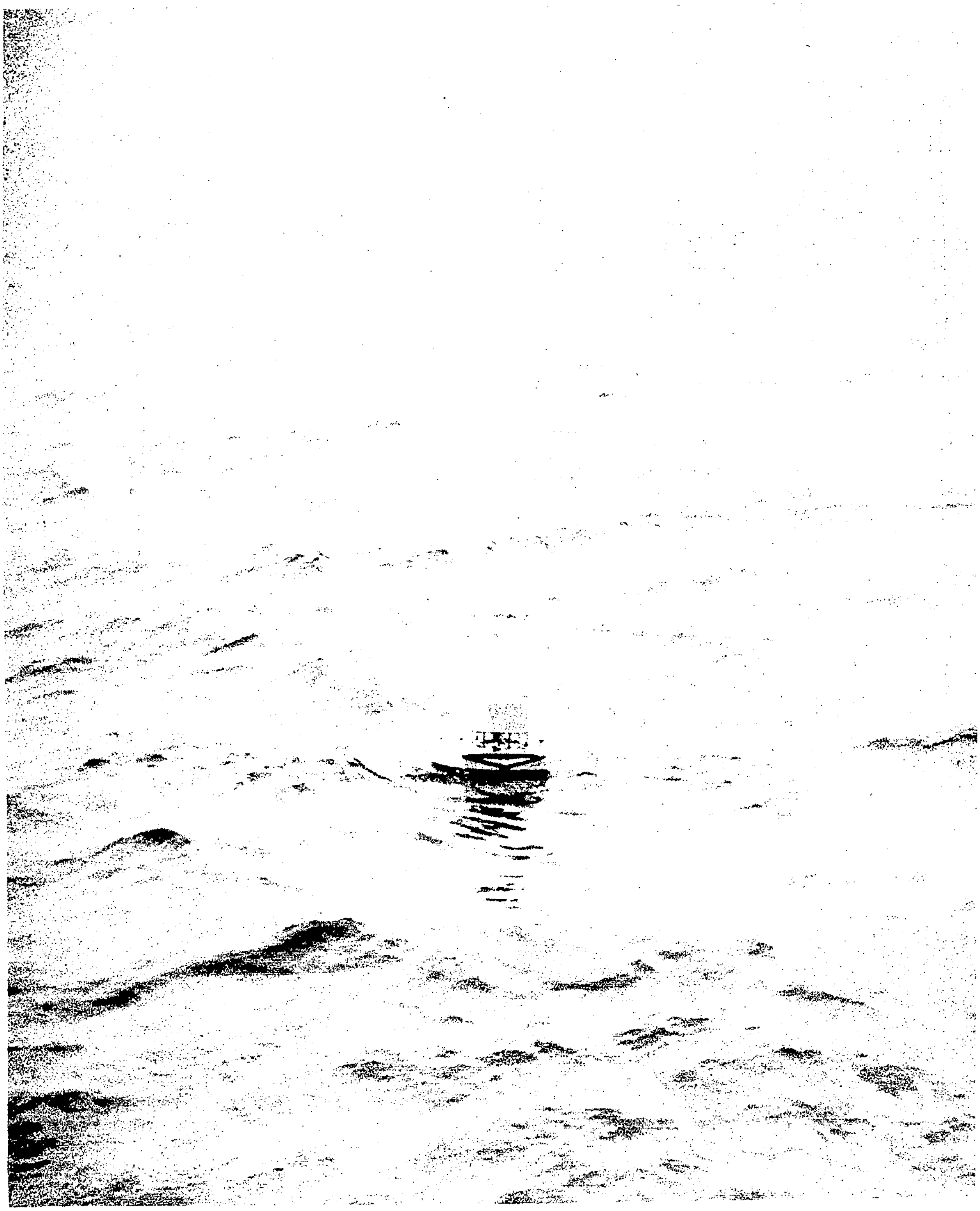
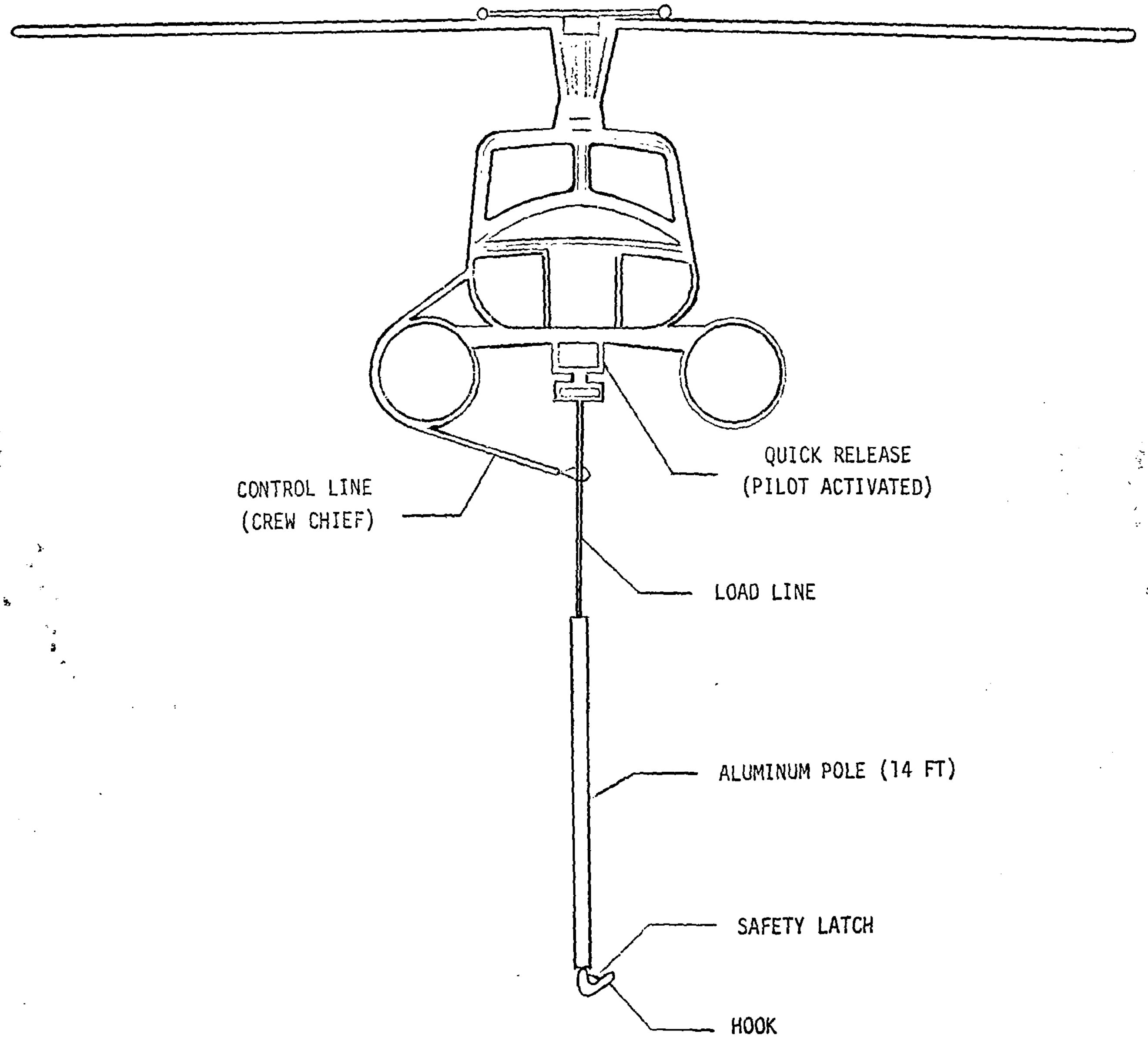


Figure 14. BUOY FREE-DRIFTING


QUICK RELEASE
(PILOT ACTIVATED)
CONTROL LINE
(CREW CHIEF)
LOAD LINE
ALUMINUM POLE (14 FT)
SAFETY LATCH
HOOK

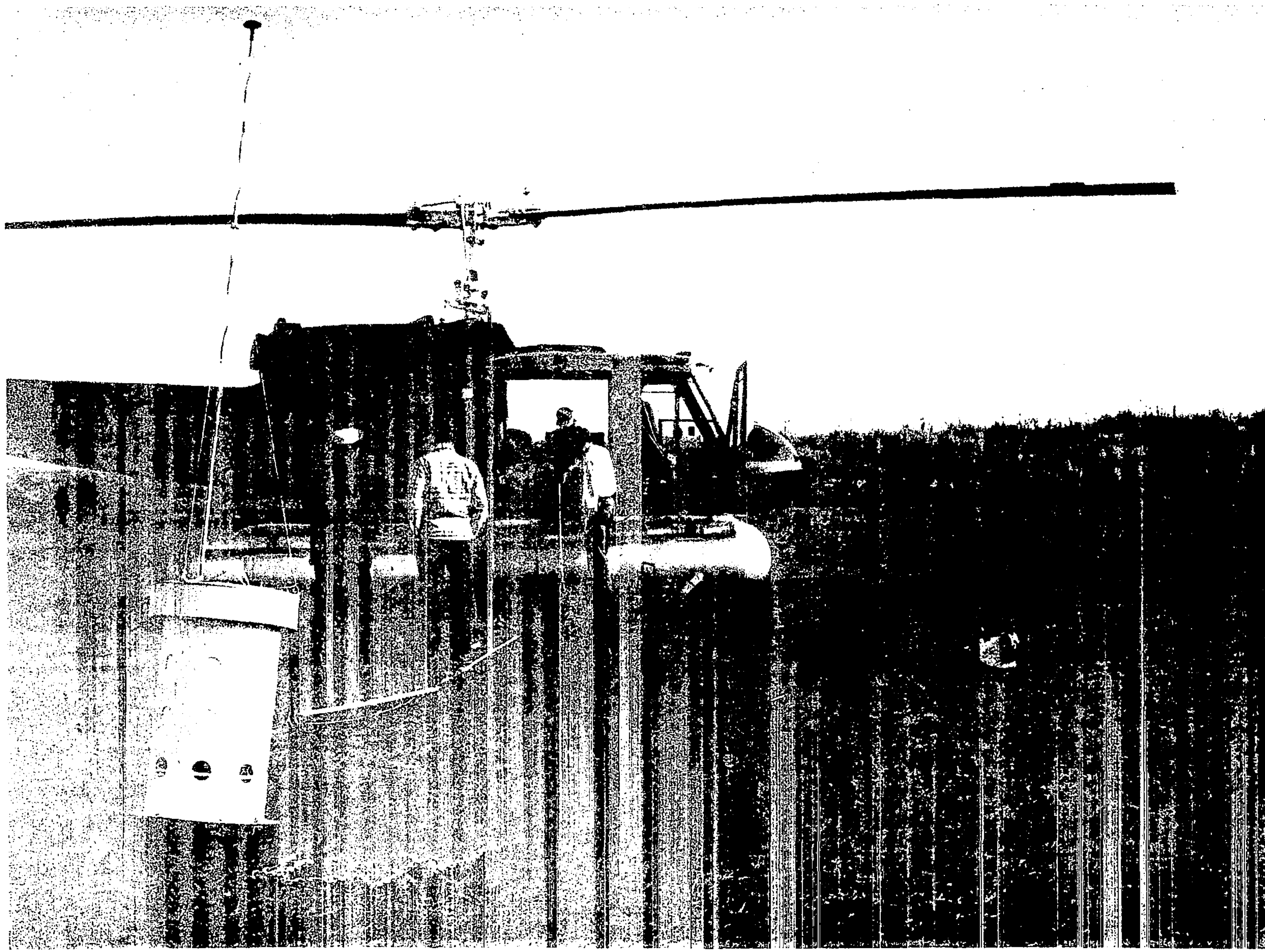


Figure 16. GROUND LAYOUT OF A RADAR TRACKED BUOY AND HELICOPTER

Figure 17. AIRBORNE HELICOPTER ABOVE B

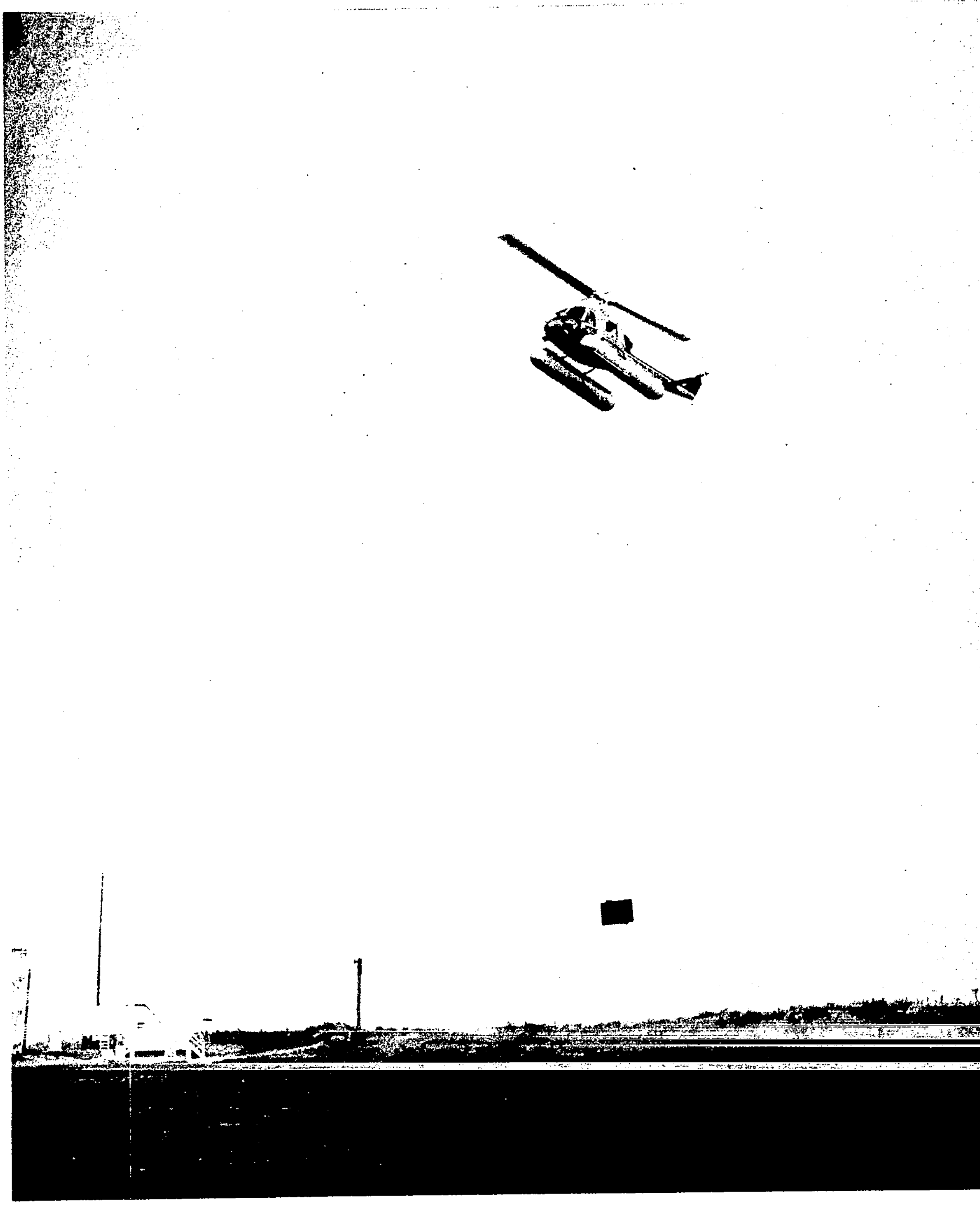


Figure 18.   HELICOPTER AND BUOY AIRBORNE

Figure 17. AIRBORNE HELICOPTER ABOVE BUOY

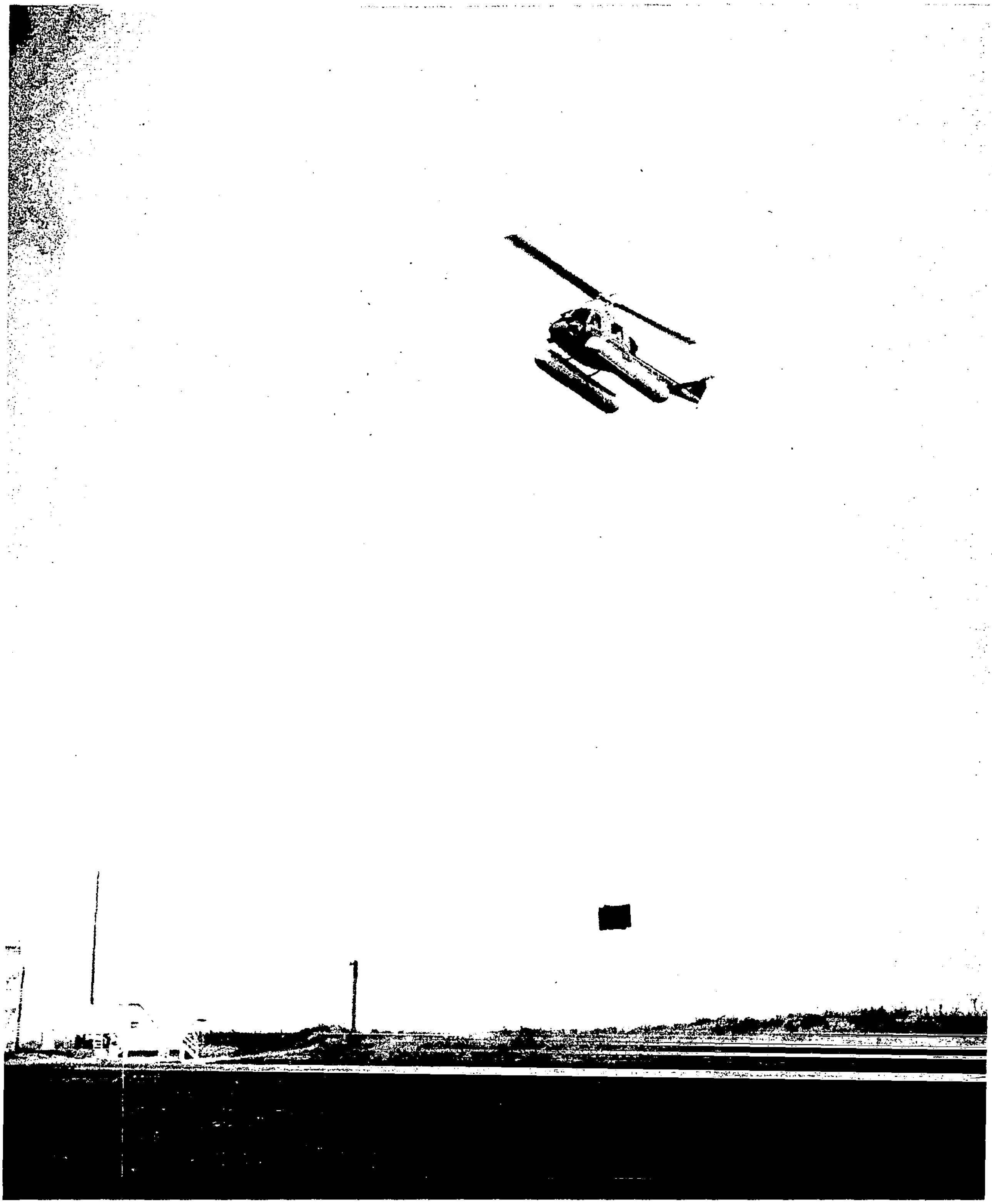


Figure 18.   HELICOPTER AND BUOY AIRBORNE

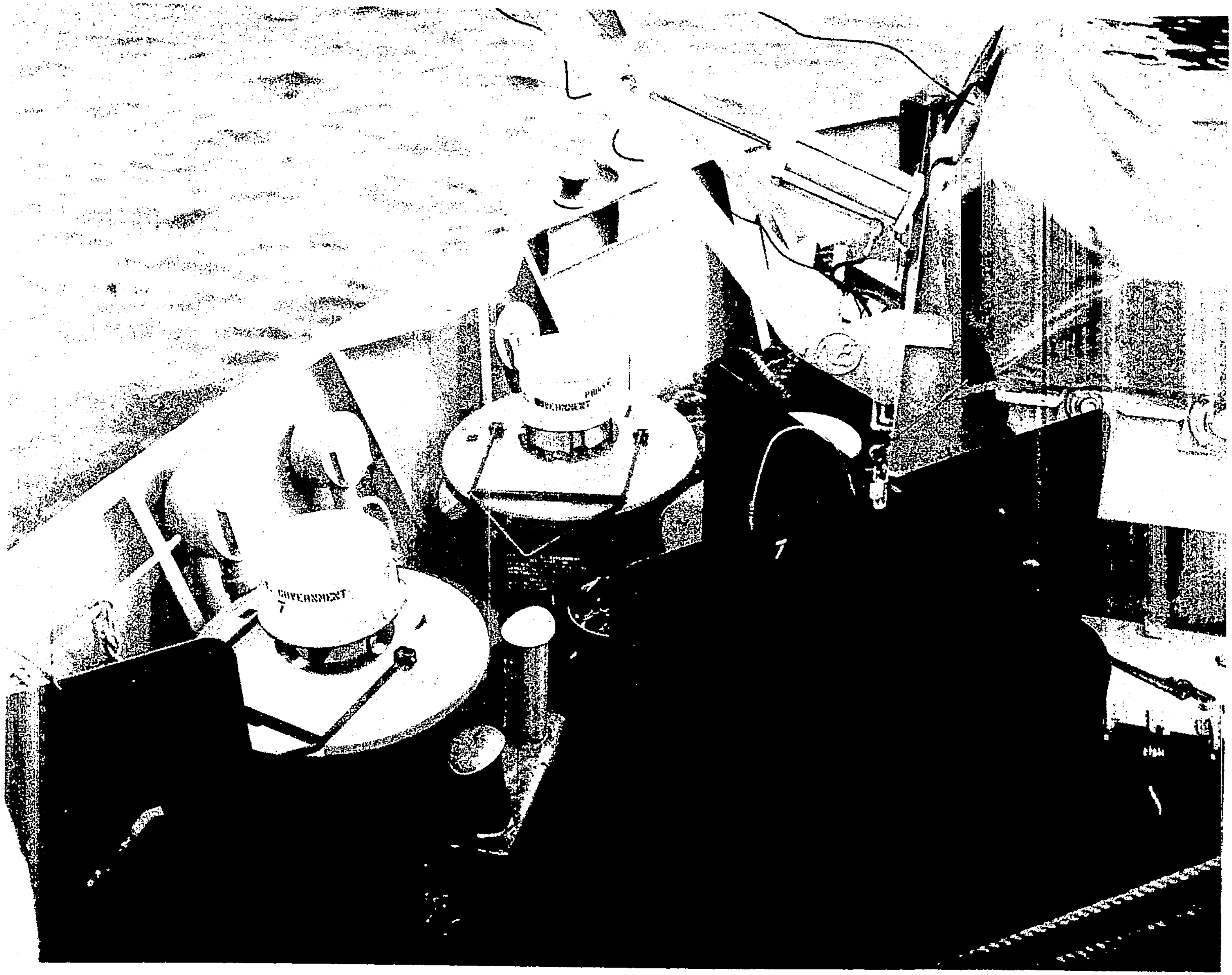


Figure 19.   FLUORESCENT PAINT PATTERNS ON SURFACE FLOATS

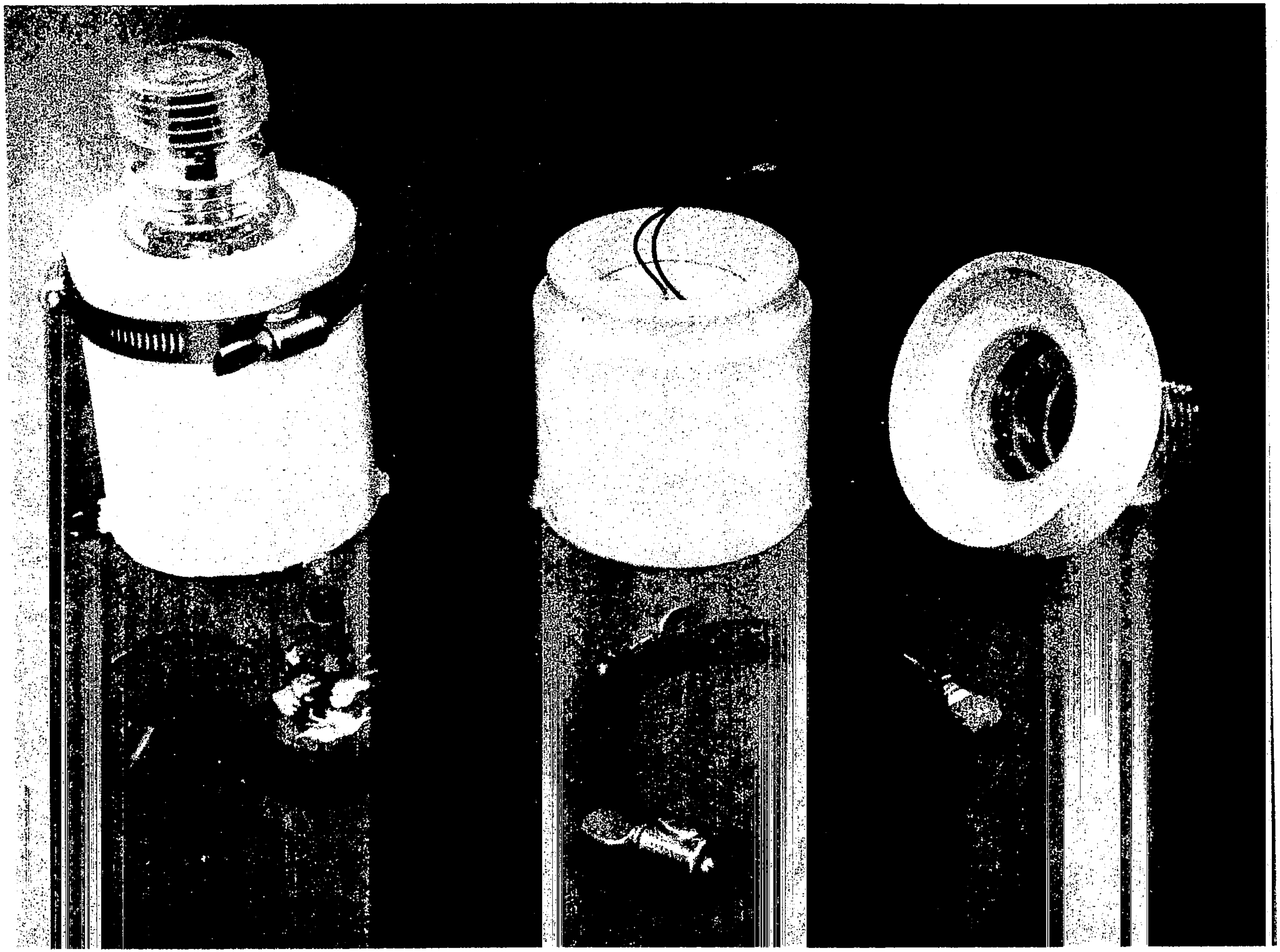


Figure 20.   FLASHING LIGHT FOR FREE-DRIFTING BUOY

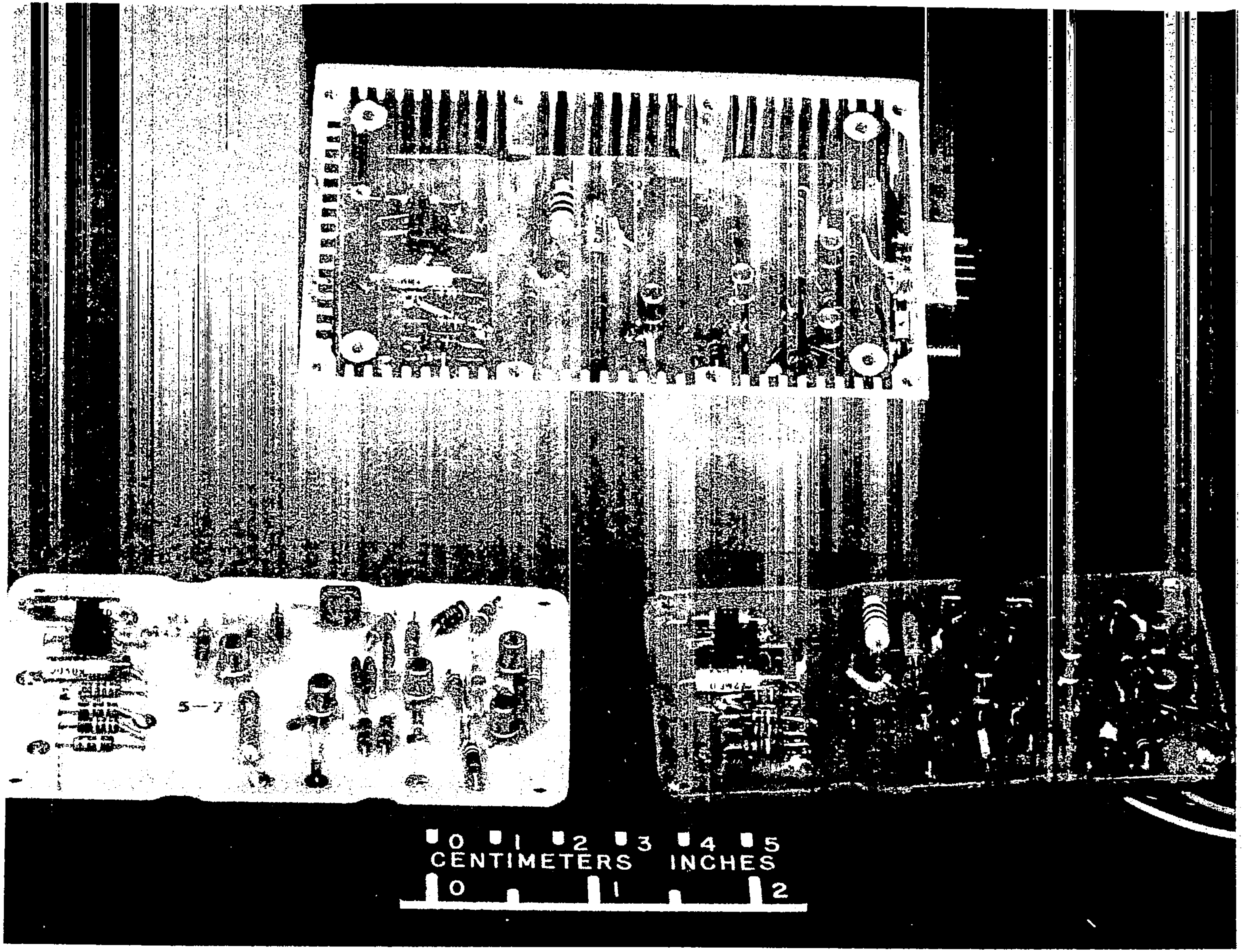


Figure 21. RECOVERY BEACON CIRCUITRY

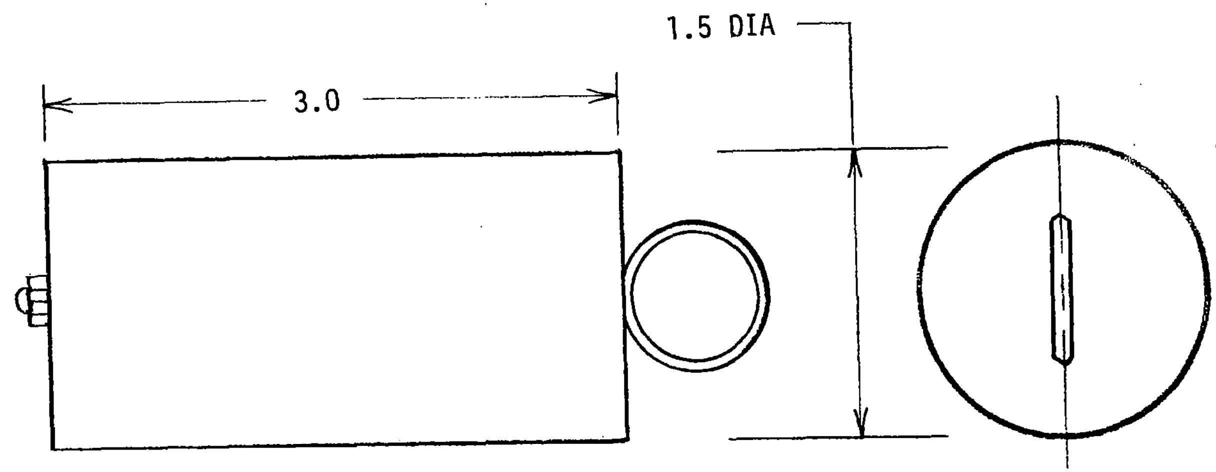


Figure 22. LANGLEY SONIC PINGER

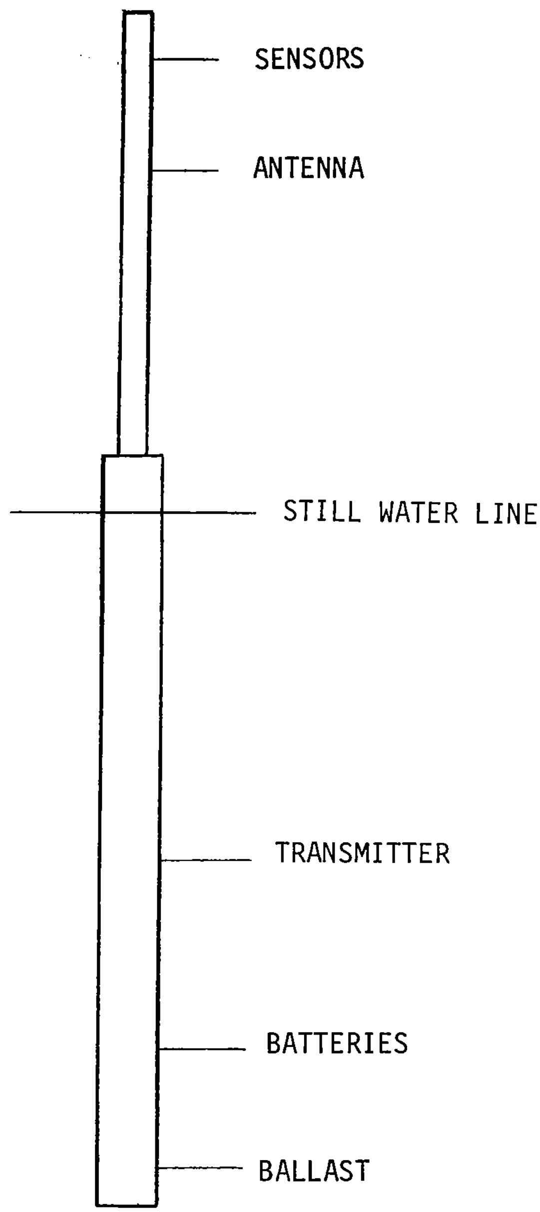


Figure 23. SMALL EXPENDABLE AIR-DROPPED REMOTE OCEAN PLATFORM (SEADROP)

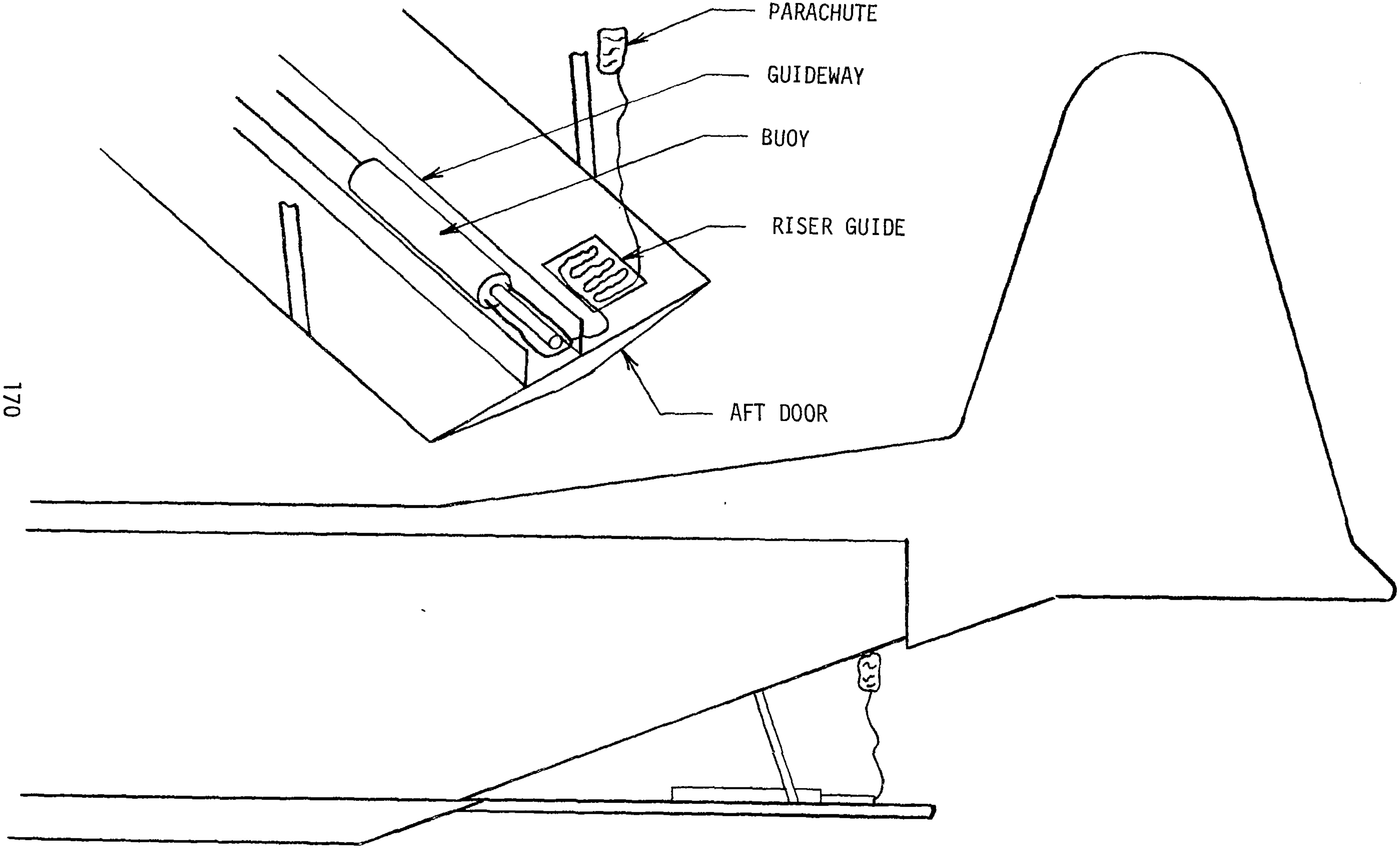


Figure 24. DEPLOYMENT METHOD FROM C-130

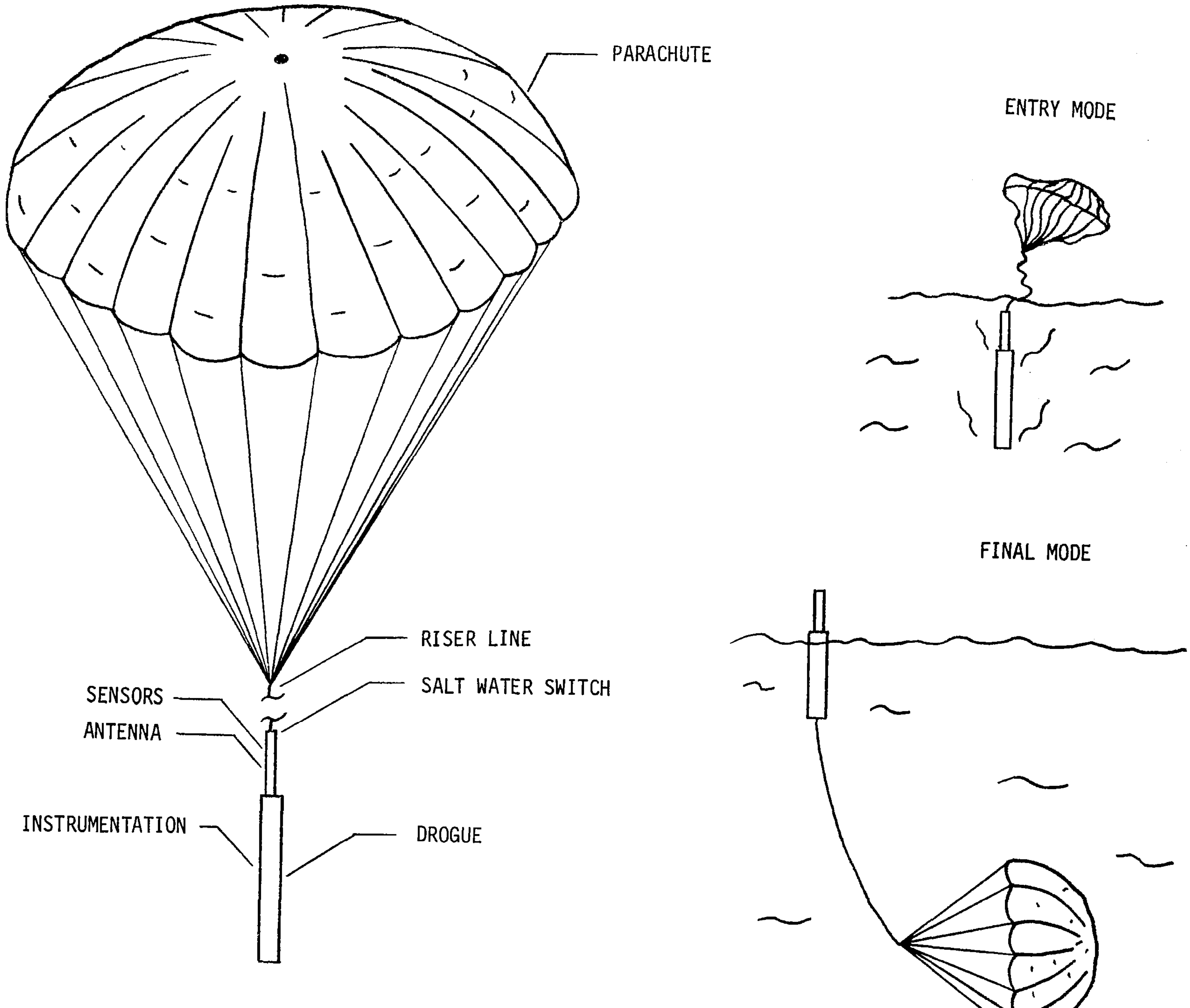


Figure 25. BUOY DEPLOYMENT SEQUENCE

SESSION C

Simulation, Sensors, and Data

Chairman: Dr. Donald Hansen,

National Oceanic and Atmospheric Administration,
Atlantic Oceanographic and Meteorological Laboratories

A LAGRANGIAN BUOY EXPERIMENT IN THE SARGASSO SEA

by

Dr. Donald V. Hansen
Atlantic Oceanographic and Meteorological Laboratories
Environmental Research Laboratories
National Oceanic and Atmospheric Administration
Miami, Florida

As indicated, we'll hear from a group of distinguished drifters this morning. In order to be sure we don't run out of time for me, I'll say my piece first. I can make mine a little bit shorter than I'd planned because a number of comments that have already been given set the stage for it. The genesis of my story begins back about 1970 when a number of people in the physical oceanographic community in this country and abroad began thinking and talking about a project to be called the Mid-Ocean Dynamics Experiment (MODE). It was referred to yesterday by Doug Webb and others as the MODE-1 Project. About that time, I began talking to Sam Stevens about the possibility of hitching a free ride, or at least an inexpensive ride, on the French EOLE satellite system, and through the very good offices of Sam and his crack team, we were indeed able to do that. The engineering for the project was done by the Miami Branch of the Engineering Development

The Author: Dr. Hansen received his Ph.D. in Oceanography from the University of Washington in 1964. He worked as a Research Assistant Professor at the University for 1 year before becoming a Research Oceanographer with the Department of Commerce in 1965. He is presently Director, Physical Oceanography Laboratory, Atlantic Oceanographic and Meteorological Laboratories, NOAA, Miami, Florida.

Laboratory of NOAA's National Ocean Survey in Miami. Charlie Kearse described yesterday some of the shipboard procedures and arrangements that were developed by them for us to get these buoys in the water, but what he did not mention was that they also were entirely in charge of the engineering and fitting out of these buoys, and in getting them into the water on what turned out to be extremely short notice. As the project developed, it really didn't go quite as we had planned to have it go, because, due to changes in the scheduling of the MODE Project and of the EOLE Satellite Project, it appeared at a critical time that the two after all were not going to be coincident in time. The EOLE Project was to terminate before the MODE Project went to sea. However, it seemed an interesting and important enough experiment to do in its own right, so we pressed on and did it anyway, almost totally independent of MODE. There was about a 1 month overlap between the termination of this project and the initiation of MODE and, in fact, the buoy that we initially had deployed farthest from the MODE area passed within 30 miles of the central mooring of MODE during the second month of that project.

I want to show you a few slides first to indicate some of the motivation for having done the experiment in the way we did it, and to set the stage to address the question of interpretation which Dean Bumpus raised yesterday with some vigor. If I can see the first slide now, please.

This is an example of a publication that is put out by the Navy. They're called Pilot Charts and show currents and wind to be expected in this region of the Sargasso Sea, what mariners and, in fact, what the rest of us know about surface currents in the Sargasso Sea. I might mention in passing, that all of the data that you can find anywhere on such atlases or charts are, in fact, derived by Lagrangian means. These currents summarized in atlases are about 99 44/100% pure ship drift calculation. They're currents inferred from the deviation of ships from their navigational calculations. The major feature I want to point out here is the fact that all of these current vectors show a very smooth steady flow to the west at speeds ranging from about a knot to speeds on the order of 1/2 a knot. The MODE Project which you saw illustrated in one of Doug Webb's slides, I believe, was conducted in a circle of about 200 kilometer radius. Figure 2 is a copy of a slide taken from some Soviet work in this region. The Soviets have an active interest in the oceanography of the low latitude Atlantic because they conduct vigorous fisheries activities out there and they have conducted intensive research cruises in this region in 1969 and again in 1971. Figure 2 shows their interpretation of those observations. They're a rather intensive set of observations. Soviet literature is a bit hard to interpret as many of you know, in that they don't document their conclusions by Western standards, but as best one can determine, the observations themselves are good. The interpretation is that the solid dark vectors represent the conventional wisdom about the Antilles Current - the northward and westward flow. Imbedded within them are open vectors which are directed to the southeast, which they interpret as a major countercurrent within the Antilles Current and flowing from someplace just off Florida, all the way down, as a continuous feature, joining the complicated equatorial current system and then flowing off to the east. The light lines you see are where they have intensive sets of observations. The observations consist of moored current meter measurements and shipboard measurements of temperature and salinity, from which are computed the velocity field by classical methods. This is the interpretation of what looks like a rather good set of conventional measurements in the region. When I first saw it, I was a little skeptical to say the least - if it's true, it certainly is rather exciting news to the oceanographic community in general and, in fact, rather embarrassing news to the American oceanographic community: that the Soviets should discover right on our doorstep a very major oceanographic feature about which we have no knowledge. This is a very major current. It is a surface current which, however, extends to about a kilometer deep in the ocean and it has a volume transport approximately equivalent to that of the Gulf Stream or Florida Current as it issues from the Florida Strait and heads up the east coast, which all of you are aware, I am sure, is the major oceanographic feature off the U.S. east coast.

So to try to serve two purposes here--one, we recognized before we went to sea that we would not be able to conduct an experiment in close coordination with the rest of the MODE operations; nontheless, it seemed worthwhile to try to obtain a direct measure of the near surface current structure and its variability in the MODE region. Hence we deployed our buoys along 67°W, immediately to the east of the MODE area, presuming that with the northward and westward drift they would sweep through the MODE area and probably be gone, along the lines of the rather imaginative sketch that Vukovich showed us yesterday, before MODE-I operations began. That was my preliminary guess as to what we might expect in the way of a trajectory development of these buoys when they were deployed, but as you will see, it didn't go quite that way. The idea then was to deploy the buoys so that they would sweep through the surface water in the MODE area before MODE ships came out for that project, except for the southernmost buoy. We learned of the Russian work fairly late in the game and modified the plan to some extent. The buoys were deployed 1° of latitude, 60 miles apart, between 28 north and 25 north. We placed the last one an additional 30 miles south, to place it in the middle of the region where the Soviets claimed to have discovered the countercurrent, to test that particular hypothesis.

Figure 3 shows one of our buoys in the water, using the EOLE satellite tracking system which is exhibited in the side room.

The next slide is of some interest because I think there probably will be additional discussion of this EOLE system today. Figure 4 shows the distribution of position fixes in time for the No. 5 buoy. It shows the hour of the day from midnight to midnight versus day of drift, so the points show the hour and day from time 0 that positions were obtained through the satellite system. They have a quasi-random pattern providing generally 2 - 5 fixes per day which round the clock slowly. The satellite "day" turns out to be something on the order of 23 1/2 hours. This is not a particularly good data distribution for most kinds of analysis we anticipated doing. Once we saw how the data were evolving, we did polynomial fitting to the X and Y coordinates of the position to provide some smoothing, then we interpolated positions on these polynomial fits at 1-day intervals so we could deal in terms of a fixed time interval. I have a film animation, which we will see, that is the same sort of thing that Doug Webb showed yesterday. It runs rather rapidly so I want to take just a moment to tell you what it contains.

EDITOR'S NOTE: At this point, an animated film sequence showing the drift history of all five buoys was shown.

Figure 5 shows the complete trajectory for the buoy, No. 4, that survived longest. It was retrieved and returned to the laboratory in April 1973.

To speak very breifly about interpretations now, I think that even from this fairly simple experiment, one must begin to make some interpretations and begin to think seriously about how to interpret such data. Some things come fairly immediately to mind--in particular the region where we were exploring the possibility of a major countercurrent. Three of the buoys moved into the region of the supposed countercurrent and pretty much negate the possibility of there being any such countercurrent, and in fact, identified the source of confusion about a countercurrent. It's a sampling problem. The Soviet observations I think are good observations. Their current observations are usually good ones and their shipboard observations are good also. However, they have sampled at fairly widely spaced sections, as one must by traditional methods, and in each of these sections they have found some sort of eddy motion. The error is not in the observation, but in interpretation, in assuming continuity between these various sections. A Lagrangian technique appears to offer much potential for exploring spatial structure in the flow, and offers a fairly economic means for exploring or answering questions about spatial distributions or the existence of particular phenomena.

Another thing that we are working on now--I just have a bare beginning of some things to say about it, another kind of application that has been of interest in oceanography for many decades now is an interest in trying to predict in a semiquantitative sense the transport or the distribution or dispersion of things dissolved or fine objects scattered in the sea, etc. As an example, the interest is in being able to predict the concentration or the probability of a particular object being in a particular place by an equation in the form:

$$\frac{\partial P}{\partial t} = K_{ij} \frac{\partial 2}{\partial_{x_i} \partial_{x_j}} P$$

In this formulation, K_{ij} is a dispersion coefficient relating the concentration change to the spatial gradient in two dimensions, essentially saying the time change is a diffusion type process related to the gradient, but with a tensor diffusion coefficient. In some classic work by G. I. Taylor dating back to 1921, it's shown that the kind of information that is needed to approach this kind of a problem is, in fact, the Lagrangian information--not Eulerian information, and is laboratory and wind tunnel dynamics, a lot of work over several decades has gone into the problem of trying to establish a relation between Lagrangian and Eulerian statistics. The Eulerian statistics are easier to measure, but for certain problems the Lagrangian statistics are the ones that you really want.

Given a particular particle or particular buoy that has a particular path, one can consider the mean path and the deviations from it, and compute the time lagged autocorrelation. That's what we have done but only for the diagonal components to date.

What we did was to take position data, differentiate it to obtain velocity data, and then compute a statistical function.

The autocorrelation, call it R, is the ensemble average $\overline{V_i(t)V_j(t+\tau)}$.

$V_i(t)$ denotes east or north component of velocity at some time t, and τ a time lag interval.

This is averaged over the ensemble or averaged over all time for the buoy motion. It's a measure of how rapidly the motion loses similarity with itself. At no lag at all the velocity looks exactly like itself so the autocorrelation is equal to the variance. The correlation decays with some structure as time runs on. Figure 6 shows the nature of the autocorrelation function for buoy 4.

The major features of the curve show that the correlation drops to zero on a time scale of about a week or 10 days. That is the Lagrangian time scale for motion in the Sargasso Sea. It also has an oscillatory structure that damps out with increasing lag. It really has validity only out to about 120 days. After that there are too few data points, to draw even tentative conclusions. There are roughly 200 observations going to make up each data point in the beginning of the curve. I don't believe anything out in the tail end of the curve, so basically what is revealed at least during this set of observations is a periodic variability in current having a time scale of about a month--peak to peak here is a lag time of about a month.

The next step to apply this to the dispersion problem is to relate the K_{ij} to the autocorrelation function using logic of G. I. Taylor and others. The result is that the K_{ij} is obtained from the integral of the autocorrelation over time. From a quick calculator integration of the function for buoy 4, I obtained a value 10^7 cm^2/s, which turns out to be a number popular among oceanographers. If one had to guess without knowing anything else it probably would be slightly higher than this, perhaps by a factor of 5 or so.

The other thing you can do--this is a thing that I think Lagrangian techniques are in fact more appropriate for, relative to other kinds of observation and fixed moorings, etc., is to explore not the time correlation behavior but the thing that's really hard to get from moored current meters, the space correlation behavior, because it's really an expensive undertaking to put down a lot of moorings with fixed current meters to explore how currents vary on a time scale of 1 mile - 10 miles - a hundred miles, etc.

By deploying an array of drifters such as we've been discussing here, one can set the initial scale, but as the pattern evolves, it covers quite efficiently a considerable band of space scale. We tried doing that with the buoy data we have here using the buoys in pairs, and in order to get the bulk of data up to some usable level it turns out we don't really have very much data at all yet. In order to try to get the statistics as well behaved as possible, we borrowed a ploy from the field of homogeneous turbulence and worked with buoys in pairs which are separated by a vector having some direction and some length L and decomposed them, presuming that the flow field is isotropic. I really don't have any very good argument to defend that except that the r.m.s. speed in the east directions are approximately the same at about 15 cm/sec, so with a little bit of hand waving we must pass over that question. Then we decomposed the velocity components at buoy pairs into components parallel to and orthogonal to the separation vector between them and computed spatial correlations at fixed times for the parallel and perpendicular components so defined. It turns out, however, that for the space scales covered by this data set, 100-400 km, the correlations are evidently so low that they cannot be distinguished from zero in the quantity of data available. Indications are that probably the spatial correlation is lowest someplace here in the first 100 kilometers or so which is essentially the same sort of thing that was found before and during the MODE experiment for the deep water circulation--deep currents in this same area. I think I've taken about as much time as I ought to. Thank you for your attention. If anyone has any comments, I'll try to respond.

QUESTIONS

JIM RUSSELL -- U. S. Naval Avionics Facility:
When you assume your isotrophy in your turbulence, what kind of scales are you really looking at in your measurements? Are they fairly large?

Answer -- DR. HANSEN:
Right, the scales we're looking at here are roughly in the 100-500 kilometer range for enough data to be of any significance at all.

JIM RUSSELL -- U. S. Naval Avionics Facility:
And it's also in the surface water rather in the deeper water that we're talking about?

Answer -- DR. HANSEN:
This is strictly the surface water. This was using the buoy that Charlie Kearse showed some slides of yesterday. We had a parachute drogue on them which was at 30 meters depth, so it's really very much in the upper layers of the ocean. The thermocline there is 800 meters deep or so.

JIM RUSSELL -- U. S. Naval Avionics Facility:
Something does bother me about assuming isotrophy there. Did the results you got indicate that assumption may have been o.k.?

Answer -- DR. HANSEN:
I really don't think I can address that. I haven't looked at it carefully. The only think I can say in justification is that the variance in the north-south and in the east-west direction is approximately equal, about 13 and 15 centimeters per second for the r.m.s. speed. There is some indication that in deeper water there probably is some anistrophy, higher energy levels in the north-south direction as compared to the east-west, but it does not show up in this surface data set.

BOB HEINMILLER -- Woods Hole:

There is a little event on your film that caught my eye. There were two buoys--looked like they were very close together--just estimating from the scale 5 and 10 miles--the tail of one about 10 times the tail of the other, both going in the same direction which implies that the speeds for one were considerably, an order of magnitude, higher than the other. I didn't notice that that occured any other time during the film. Have you seen any sort of that? That seems like an awfully high differential.

Answer -- DR. HANSEN:

It does happen other times. You have to see the film several times to detect more of these events, but when we first deployed the buoys I thought we had discovered the center of the ocean circulation because for a period of about 10 days the No. 4 didn't move within the resolution of the satellite, which is about a kilometer there, while buoys north and south of it, particularly one of them north of it, turned and moved toward it and came by at a good rate of speed within about 30 miles, yet the one that was initially deployed there hardly moved for about 10 days. After 10 days it suddenly took off and moved to the south as rapidly as any of them. I interpret that as being indicative of large lateral shears in the flow. In the movies that Doug showed yesterday, you see very much the same sort of thing in the SOFAR float measurements. It looks there as if there are jets imbedded in the flow. They seem to be north-south oriented there but didn't show up quite so much here perhaps because a lot of the statistics may be biased by the fact that the buoys spent a fraction of their time fairly near the Bahama Banks where presumably north-south motion is strongly inhibited and east-west motion parallel to the banks is favored.

CHRIS WELSH -- Virginia Institute of Marine Science(VIMS):

It occurs to me that if you were to put a current meter section out where the Russians did for a long length of time and average over the time to get a climatological circulation, you would still see the countercurrent structure that they apparently saw simply because when the currents going south, the western boundary, if you want to call it that, structure is apparently more intense from the little worms you have than when they go off to the north.

Answer -- DR HANSEN:

I think that's probably true - if you'd observe just those sections, you likely would see what you interpret as a countercurrent migrating onshore and offshore and north and south or something. I suspect different eddies or different waves or whatever they are occur there at various times. You're probably right. You'd really have to have a very dense set of current meter moorings to be able to resolve the spatial structure in the flow to disabuse yourself of that idea.

PETER HACKER -- JOHN HOPKINS:

I'm worried a little bit about the slippage of the drogues in regions where you do have high lateral shear from the currents you observed and from the winds that are typical in that area. Do you have any kind of a percentage estimate of slippage of the drogue with respect to a water mass?

Answer -- DR. HANSEN:

I haven't put a number on it. We're investigating. We just got all the tropical weather information. We will correlate the local winds with the buoy movements; however, I haven't put a number on it. Maybe Charlie has, I don't know. I think the wind drift for this particular buoy is probably negligable in terms of the currents and the things we see for two reasons: one, the dominant periodicities in the major flow features have a time scale of about a month and you just don't see things like that down there in the weather pattern. You don't expect major wind events in a time scale of a month. Strictly from the engineering point of view, this buoy was about 40-41' long with the major portion of the cross section submerged and, in addition, it has a parachute drogue on it. All indications are that the parachute droges did indeed survive for a time scale of 6 months or better. Bob Heinmiller was one of the last people, I think, to see buoy No. 5 and the reports I have from the appearance of the buoy in the water, the way accessory floats were arrayed and so on, indicate that the parachute drogue hardware apparently was still on at that time. We recovered one in December after 3 months at sea, and the whole subsurface hardware was essentially in perfect condition then. The one we recovered after 8 months outside the Bahamas had lost its parachute. I don't think it's a serious problem. Did you ever put a number on the windage Charlie?

CHARLIE KEARSE:

I guess I'm just worried. You know, even if it's just 5 or 10 percent--if a flow drifts 100 kilometers downstream or something, at the same time it can be going cross stream 5 of 10 kilometers in a region where you do have intense sheer, it may in fact drift from a countercurrent into the other part of the countercurrent if you do have closely spaced currents.

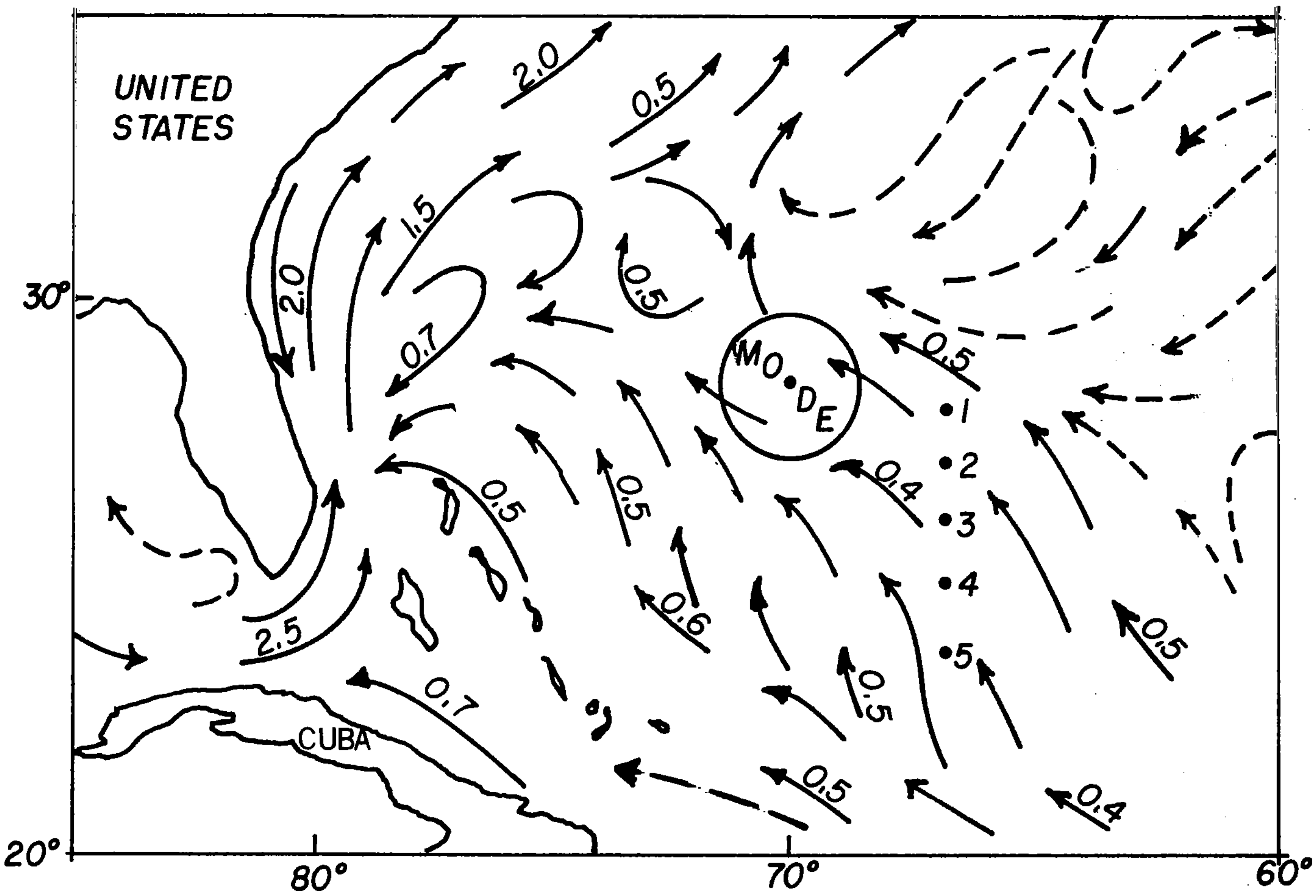


Figure 1. SURFACE CURRENTS IN THE SARGASSO SEA AS TYPICALLY SHOWN IN ATLASES (The MODE-1 region and deployment positions of 5 drifting buoys using the EOLE position locating system are also indicated.

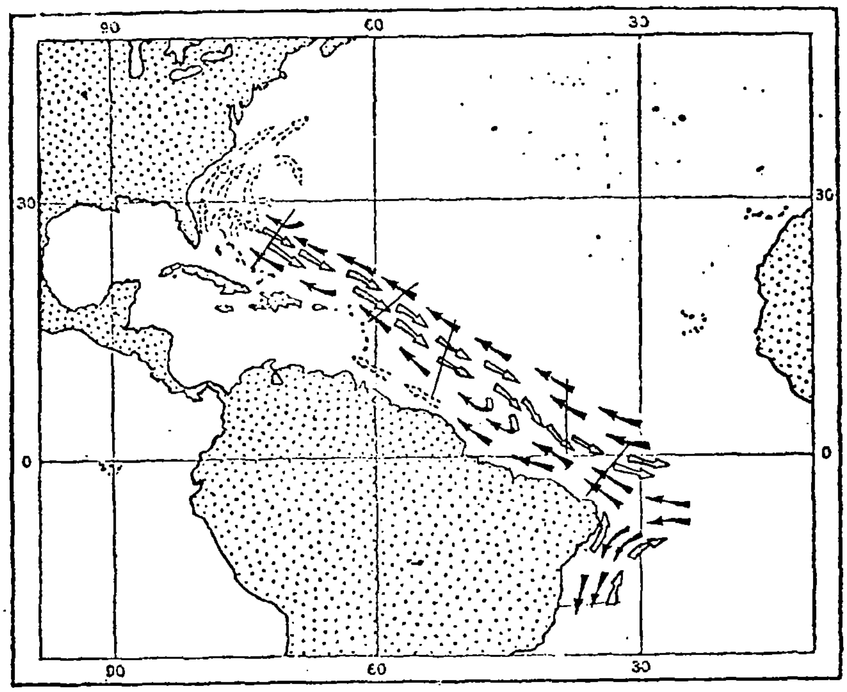


Figure 2 THE ANTILLES COUNTERCURRENT AS HYPOTHESIZED BY V.G. KORT

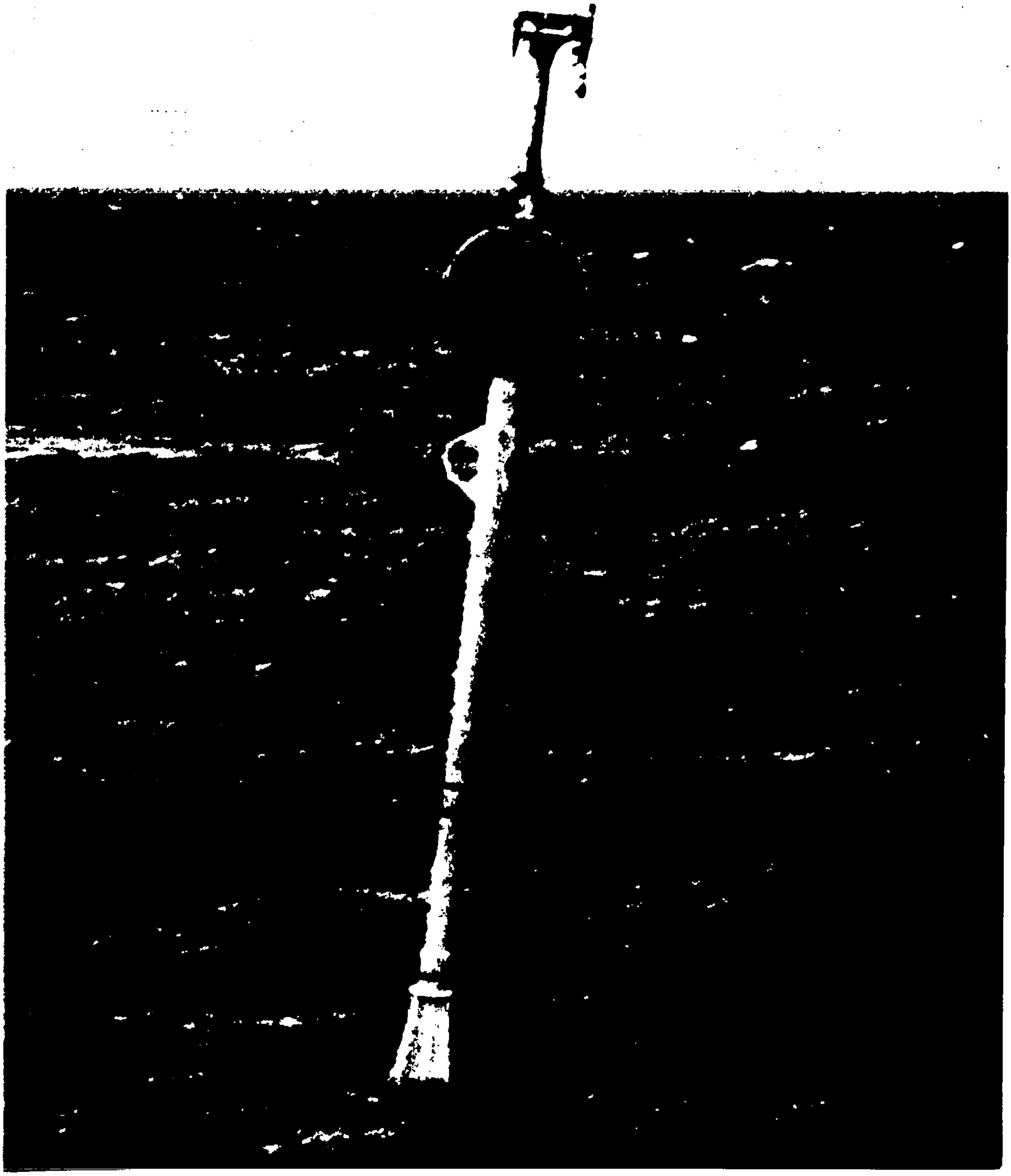


Figure 3 DRIFTING BUOY USING EOLE TRANSPONDER DEPLOYED AT SEA

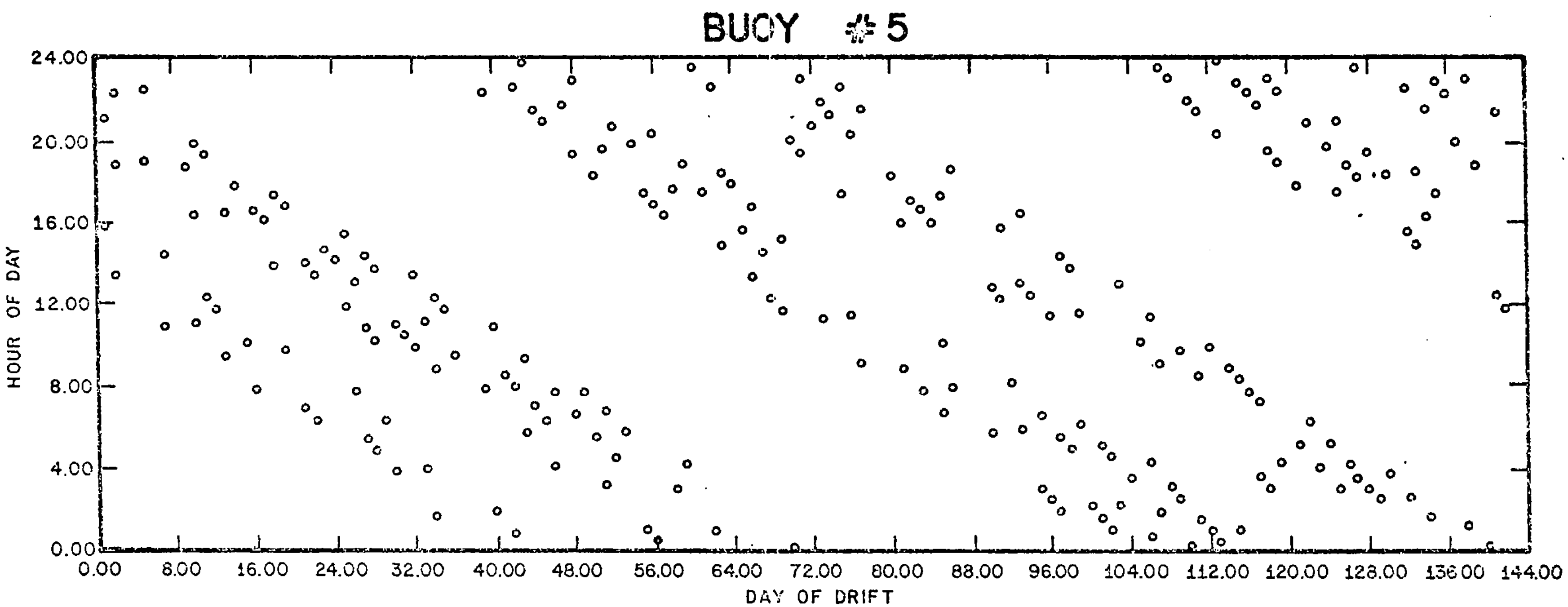


Figure 4 DISTRIBUTION OF EOLE POSITION FIXES IN TIME FOR A DRIFTING BUOY

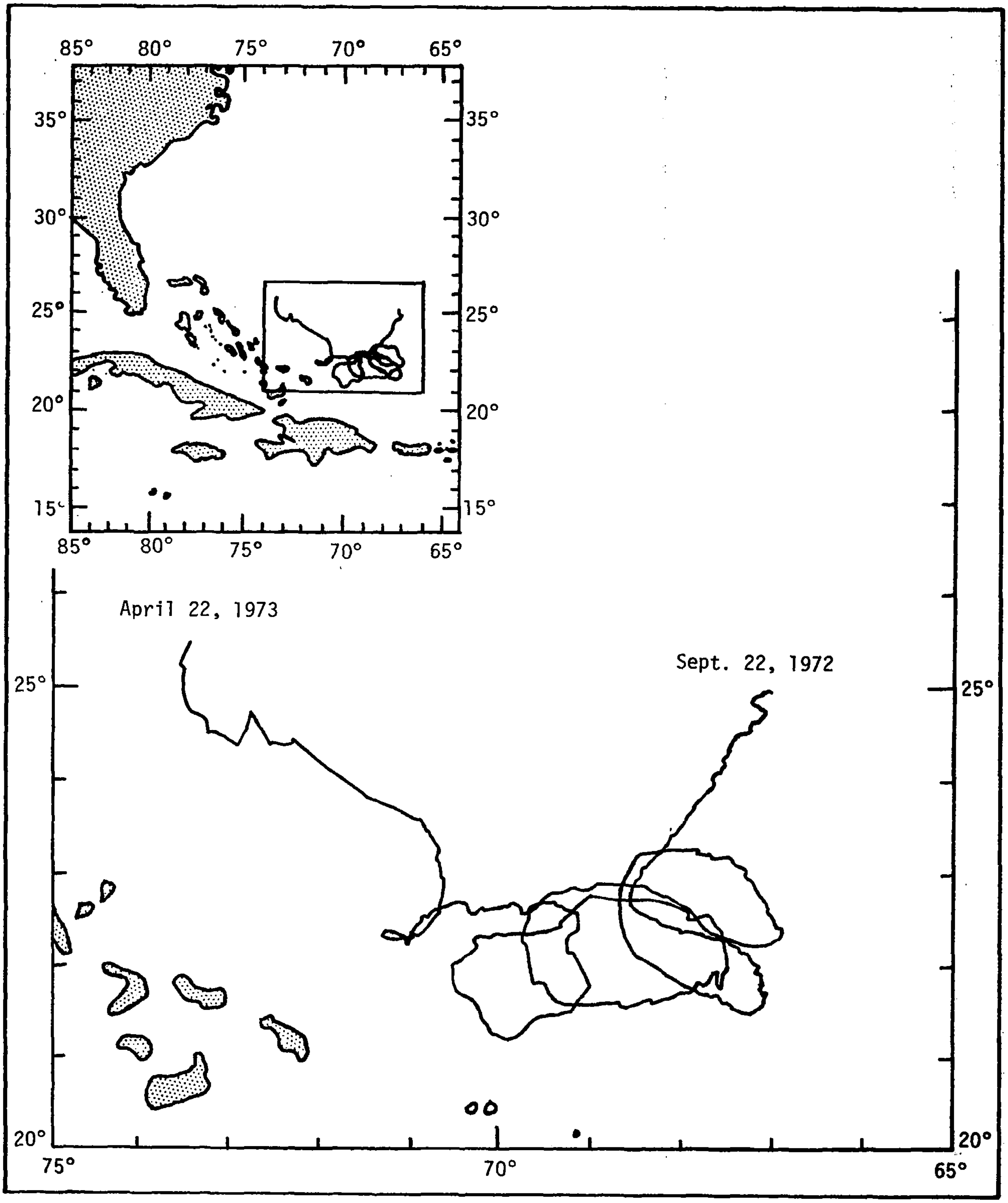


Figure 5 DRIFT TRAJECTORY FOR BUOY No. 4

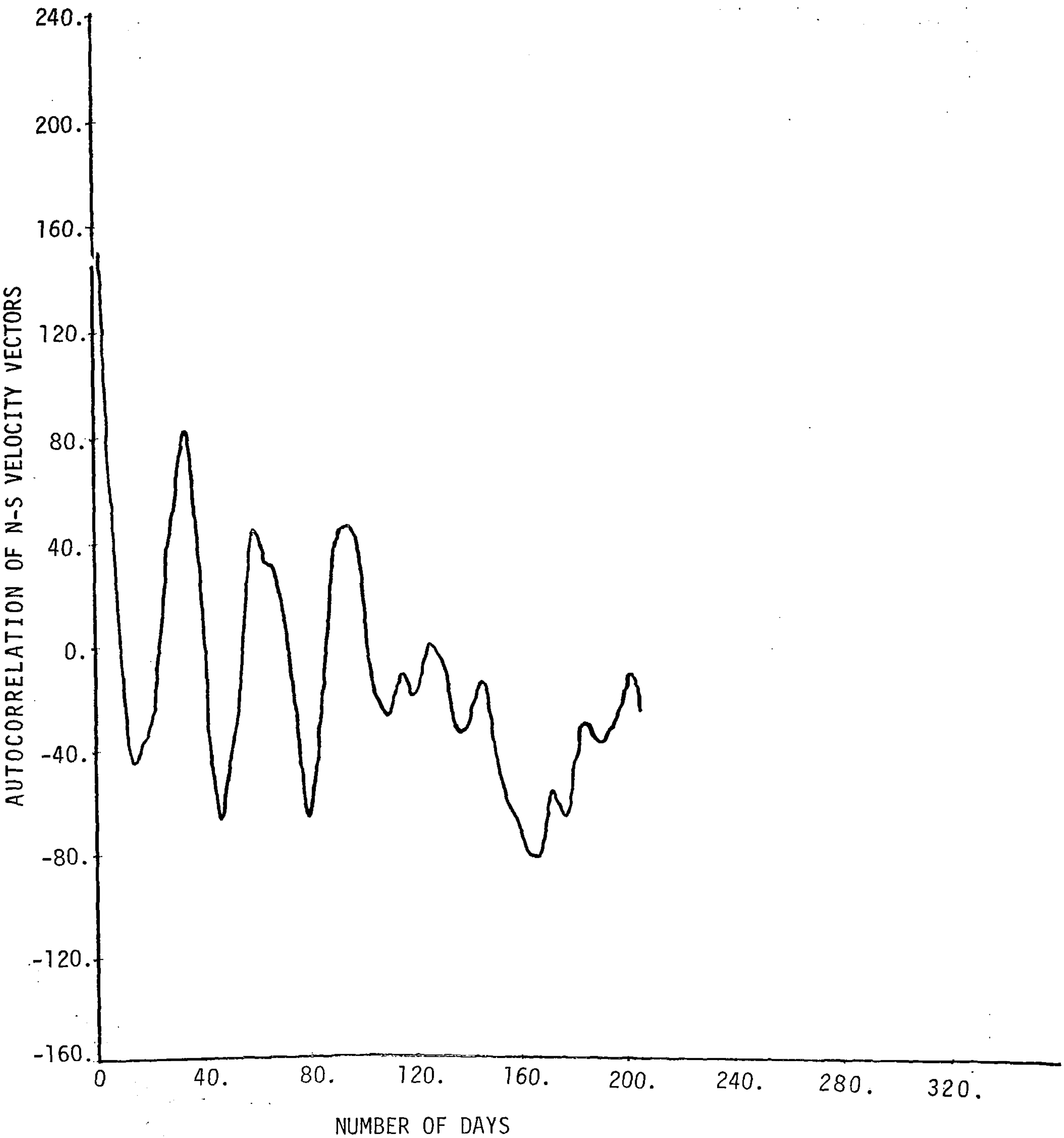


Figure 6 LAGRANGIAN TIME CORRELATION FUNCTION FOR BUOY No. 4

CALCULATIONS OF DIFFERENTIAL KINEMATIC PROPERTIES FROM LAGRANGIAN OBSERVATIONS

by

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Atlantic Oceanographic and Meteorogical Laboratories

Dr. A. D. Kirwan, Jr.
Texas A&M University, Department of Oceanography

INTRODUCTION

In the past oceanographers have used Lagrangian data, primarily to obtain elementary fluid properties such as trajectories, velocities, and accelarations. However, meteorologists have recognized the utility of Lagrangian data in determining estimates of the differential kinematic properties, divergence, vorticity, shearing deformation, and stretching deformation. These properties are important ingredients in any description and/or explanation of fluid motions. For instance, divergence is an important factor in determining vertical motion in the ocean, vorticity can be related to the field of force that drives ocean flows, and the two deformations are important in the formation and dissipation of fronts.

The Authors: Robert Molinari received his Ph.D. in Physical Oceanography from Texas A&M University in 1970. Since 1971 he has held the position of Research Oceanographer with NOAA/AOML. His work has centered on observational and theoretical study of the Cayman Sea and the Gulf of Mexico.

Dennis Kirwan, Jr. also received his Ph.D. from Texas A&M. He has been an associate Professor at New York University and worked as a Program Director with the Office of Naval Research. More recently, he has become Research Scientist at Texas A&M and has been involved in drift buoy studies.

Two methods are presented for the calculation of these properties. One method is more readily applicable to a large number of buoys. The other approach is given to provide estimates to verify the results of the more general technique. A short description of the experiment and data analysis is given.

DATA COLLECTION AND ANALYSIS

Figure 1 is a schematic diagram of the ship-tracked buoy used in the experiment. The buoy nominally was tied to a water parcel at 40 m by a 35-ft diameter parachute.

The experiment was conducted in the western Caribbean Sea in the summer of 1971, aboard the NOAA ship RESEARCHER. The prime navigational control was supplied by a satellite positioning unit. In that region, satellite fixes can be obtained on the average every 1.5 hours. The satellite positions were supplemented by Omega fixes collected every 15 minutes. The buoys were positioned relative to the ship at each fix.

Errors are introduced into the buoy positions by the imprecision of the satellite, Omega, and radar systems. Assuming the satellite system to be the more precise of the two positioning techniques, an estimate of the Omega errors was made. An individual Omega position is accurate approximately to ±2 km. Thus, there is a very small signal-to-noise ratio when considering the 15-minute fixes.

The following smoothing procedure was applied to the Omega fixes to eliminate some of the noise in the trajectory data. Hourly fixes were obtained by taking 5-point running averages of the 15-minute component coordinates. A second degree polynomial curve was then fitted to 13 consecutive hourly fixes to arrive at the data used in the analysis.

Kirwan, in a previous talk, indicated other possible sources of error when attempting to tag a particular water parcel. Using his analysis for the drifter configuration used in this experiment, it was found that a 10 m/sec wind could cause a 5 percent error in the estimate of the true current. This effect was not considered in reducing the data from this experiment.

A. Least Square Method

Consider a small, but finite, parcel of water, and assume that within this parcel the velocity at any point is adequately represented by the linear terms in a Taylor's expansion about the center of mass of the parcel. For a cluster of N drifters located within the parcel, the expansion yields for the velocity components of the i^{th} drifter.

$$\begin{aligned} U_i &= U + g_i + \{(D + N)\, X_i\}/2 + \{(S - \zeta) Y_i\}/2 \\ & \qquad\qquad\qquad\qquad\qquad\qquad i = 1, \ldots N \qquad (1) \\ V_i &= V + h_i + \{(S + \zeta)\, X_i\}/2 + \{(D - N) Y_i\}/2 \end{aligned}$$

The U and V are the components of the velocity of the center of mass of the parcel. The coordinates with respect to the cluster center of mass of drifter i are X_i and Y_i. The g_i and h_i represent the sum of the higher order non-linear terms in the expansion.

The differential kinematic properties are:

$$\begin{aligned} D &= \partial U/\partial X + \partial V/\partial Y && \text{(Divergence)} \\ \partial &= \partial V/\partial X - \partial U/\partial Y && \text{(Vorticity)} \\ S &= \partial V/\partial X + \partial U/\partial Y && \text{(Shearing deformation rate)} \qquad (2) \\ N &= \partial U/\partial X - \partial V/\partial Y && \text{(Stretching deformation rate)} \end{aligned}$$

The divergence, D, is a measure of the parcel volume change without change of orientation or shape. ζ, the vorticity, is a measure of the orientation change without volume or shape change of the parcel. Shape changes without change of volume or orientation are given by S and N respectively.

In equation (1), U_i, V_i, and U and V are computed from the buoy coordinates. The g and h functions, and D, N, S, and ζ can be computed by noting that at each time the total kinetic energy density of the cluster due to small-scale turbulence is:

$$KE = \sum_{i=1}^{N} g_i^2 + h_i^2 \ /2 \qquad (3)$$

Substituting (1) into (3) shows that the kinetic energy density depends on the kinematic properties. These four parameters can be estimated by selecting values which give a minimum for the kinetic energy density. The g and h functions can then be determined from (1).

The minimum number of drifters that can be used to determine D, S, N, and ζ is three. However, this approach is readily extended to consider larger numbers of drifters. In addition, the approach generates time series of the turbulent velocities, g_i and h_i, from which direct estimates of turbulent stresses can be made.

B. Area Method

Horizontal divergence can be expressed as the fractional time rate of change of the horizontal area, A, of a parcel:

$$\frac{\partial U}{\partial X} + \frac{\partial V}{\partial Y} = \frac{1}{A}\frac{DA}{Dt} \qquad (4)$$

For a triad of drifters, A is readily evaluated from the buoy positions. From the time series of A's an appropriate numerical technique is used to estimate the time rate of change.

Vorticity, shearing and stretching deformation can be evaluated by selected rotations of the velocity vectors of the three drifters. Saucier (1955) describes this technique.

RESULTS

Figure 2 is a schematic diagram of representative drogue trajectories and speeds. Four trajectories are shown. Beginning with the trajectory over the Cayman Ridge and preceeding counterclockwise around the basin, the trajectories will be numbered 1, 2, 3, and 4 for purposes of identification.

The area of figure 2 is the formation region of the Yucatan Current. The accelarations which occur during legs 2 and 3 are indicative of the formation processes occuring in the vicinity of the Yucatan Channel.

Figure 3 is a more detailed plot of the drogue trajectories of leg 1. A Universal Transverse Mercator projection is used, and the x and y coordinates are marked in kilometers. The apexes of the triangles represent drogue positions.

Also given on the figure are the velocity, accelaration, and radius of curvature of the triad center of mass. The last two curves indicate the difficulty of obtaining from these data smooth estimates for higher order derivative terms.

Figure 4 gives the divergence and vorticity as determined by the two methods. The solid lines connect the values computed by the least square approach, and the crosses represent the values computed by the area method. The triad areas (figure 4) are small and the estimates of the kinematic properties are very irregular with respect to time.

Figures 5 and 6 present the buoy trajectories and all the kinematic properties for leg 2. Again, the agreement between the two methods is good. The estimates of these parameters are smoother functions of time for this leg.

Figures 7 and 8, and 9 and 10 display the results for legs 3 and 4 respectively. The buoy speeds were lowest during leg 4, on the average 0.3 m/sec, and the triangle areas small. The kinematic property estimates given on figure 10 are very ragged, with frequent crossings of the axis. It is doubtful that these values are reliable estimates of the differential kinematic properties.

The value of the measurements is increased if the resulting data can be used to explain the dynamics of the circulation. An attempt to incorporate the data of leg 2 (figure 6) into a dynamic expression is made.

Figure 11 gives the conservation of potential vorticity relation, and the evaluation of the terms in this relation using the data of leg 2. This equation is derived by assuming no external forces (tides, winds) are acting on the flow. The terms in this expression are Z, the relative vorticity, f, the Coriolis parameter, and $\nabla \cdot \overline{V}$, the divergence. The qualitative balance of the terms for the first two days of the trajectories suggests a balance exists.

To summarize, it appears feasible to compute differential kinematic properties from drifting buoy data. In addition, if estimates are sufficiently well-behaved, some dynamical statements about the flow can be offered.

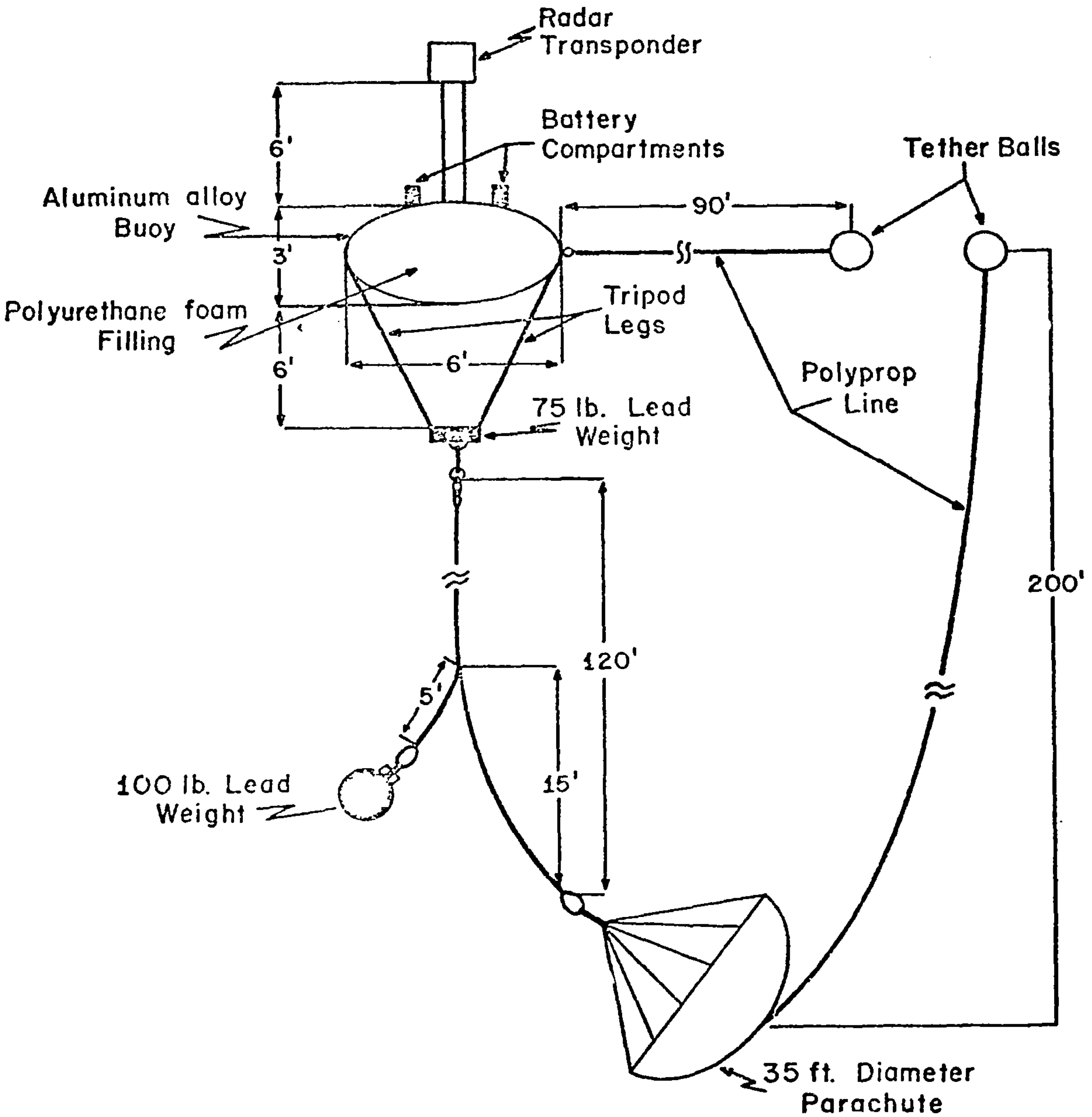
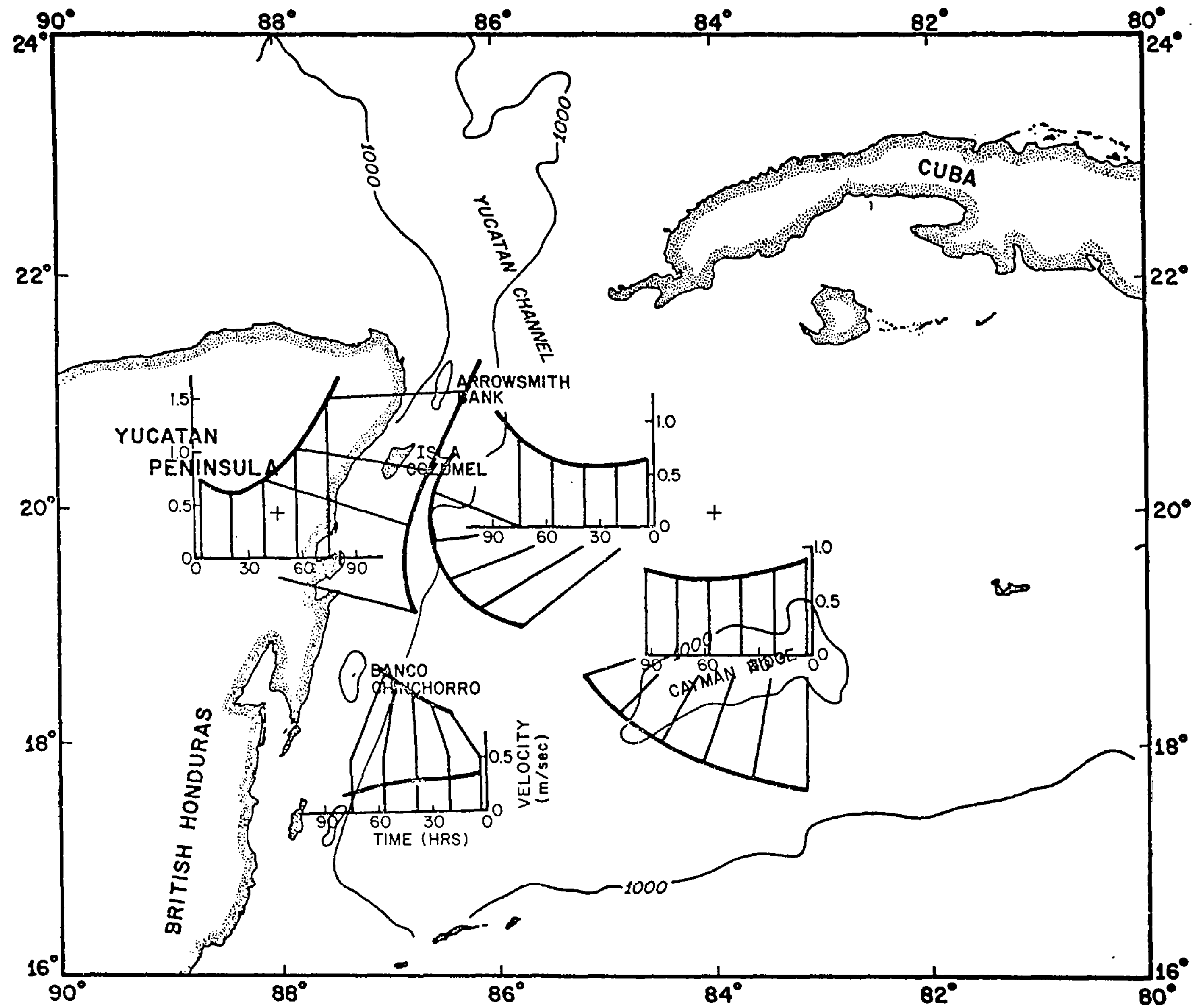

Radar Transponder
Battery Compartments
Tether Balls
Aluminum alloy Buoy
6'
3'
90'
Polyurethane foam Filling
Tripod Legs
6'
6'
Polyprop Line
75 lb. Lead Weight
200'
120'
5'
100 lb. Lead Weight
15'
35 ft. Diameter Parachute

Figure 1.



90°
88°
86°
84°
82°
80°
24°
22°
20°
18°
16°
1000
YUCATAN CHANNEL
CUBA
ARROWSMITH BANK
ISLA COZUMEL
YUCATAN PENINSULA
BANCO CHINCHORRO
CAYMAN RIDGE
BRITISH HONDURAS
VELOCITY (m/sec)
TIME (HRS)
1.5
1.0
0.5
0
30
60
90

Figure 2.

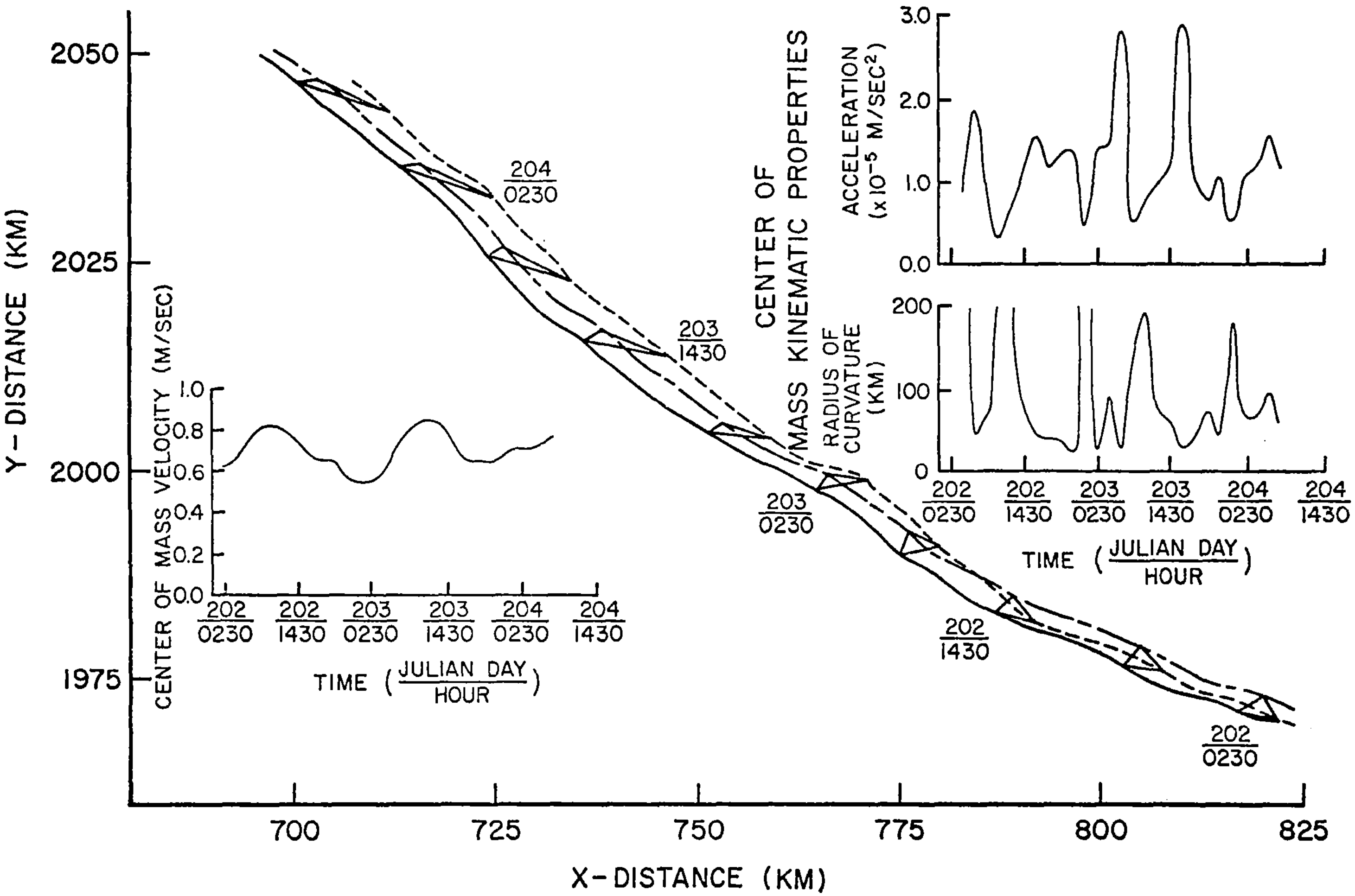

Y-DISTANCE (KM)
2050
2025
2000
1975
X-DISTANCE (KM)
700
725
750
775
800
825
204/0230
203/1430
203/0230
202/1430
202/0230
CENTER OF MASS KINEMATIC PROPERTIES
ACCELERATION (x10^-5 M/SEC^2)
3.0
2.0
1.0
0.0
RADIUS OF CURVATURE (KM)
200
100
0
202/0230
202/1430
203/0230
203/1430
204/0230
204/1430
TIME (JULIAN DAY/HOUR)
CENTER OF MASS VELOCITY (M/SEC)
1.0
0.8
0.6
0.4
0.2
0.0
202/0230
202/1430
203/0230
203/1430
204/0230
204/1430
TIME (JULIAN DAY/HOUR)

Figure 3.

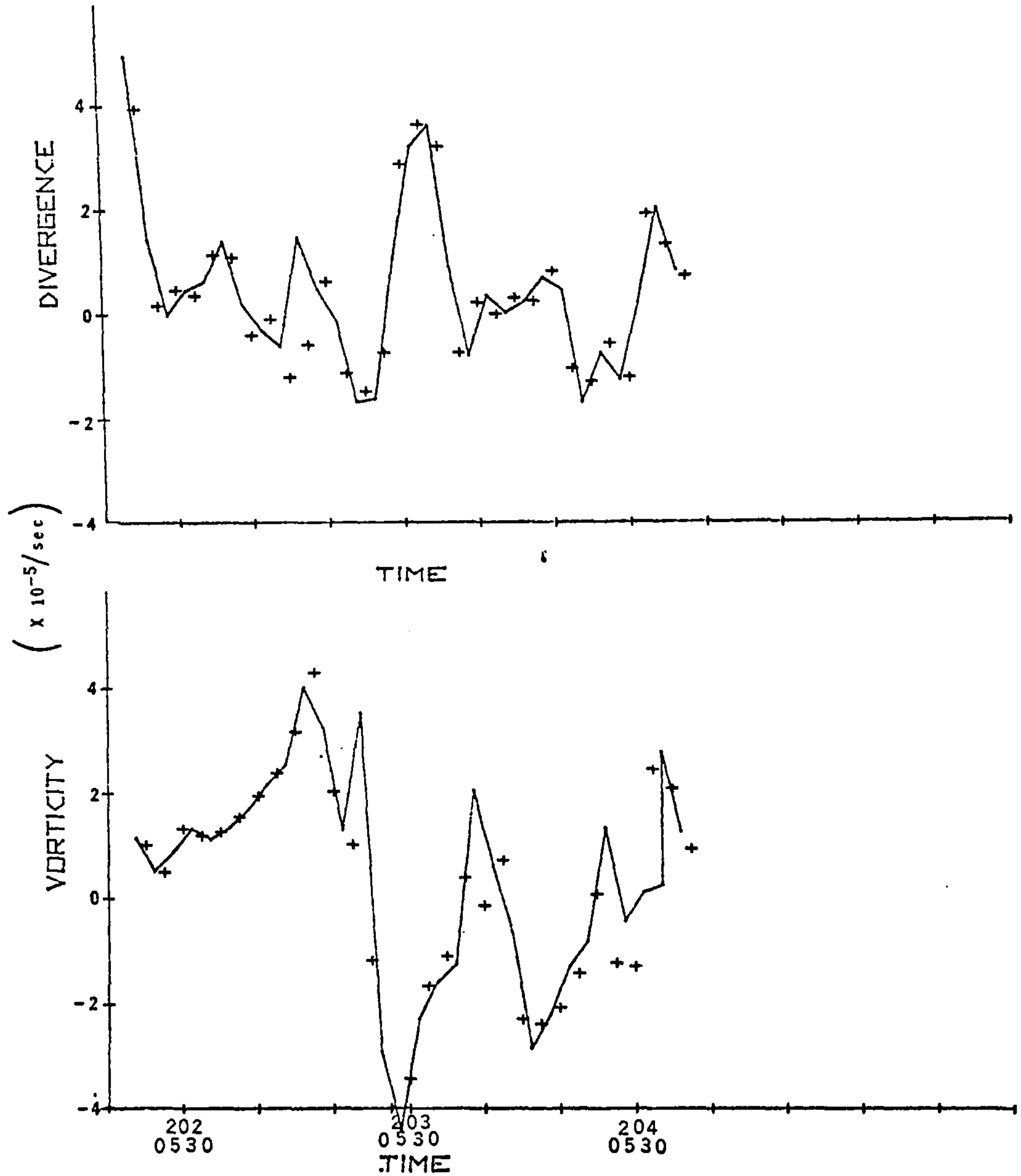
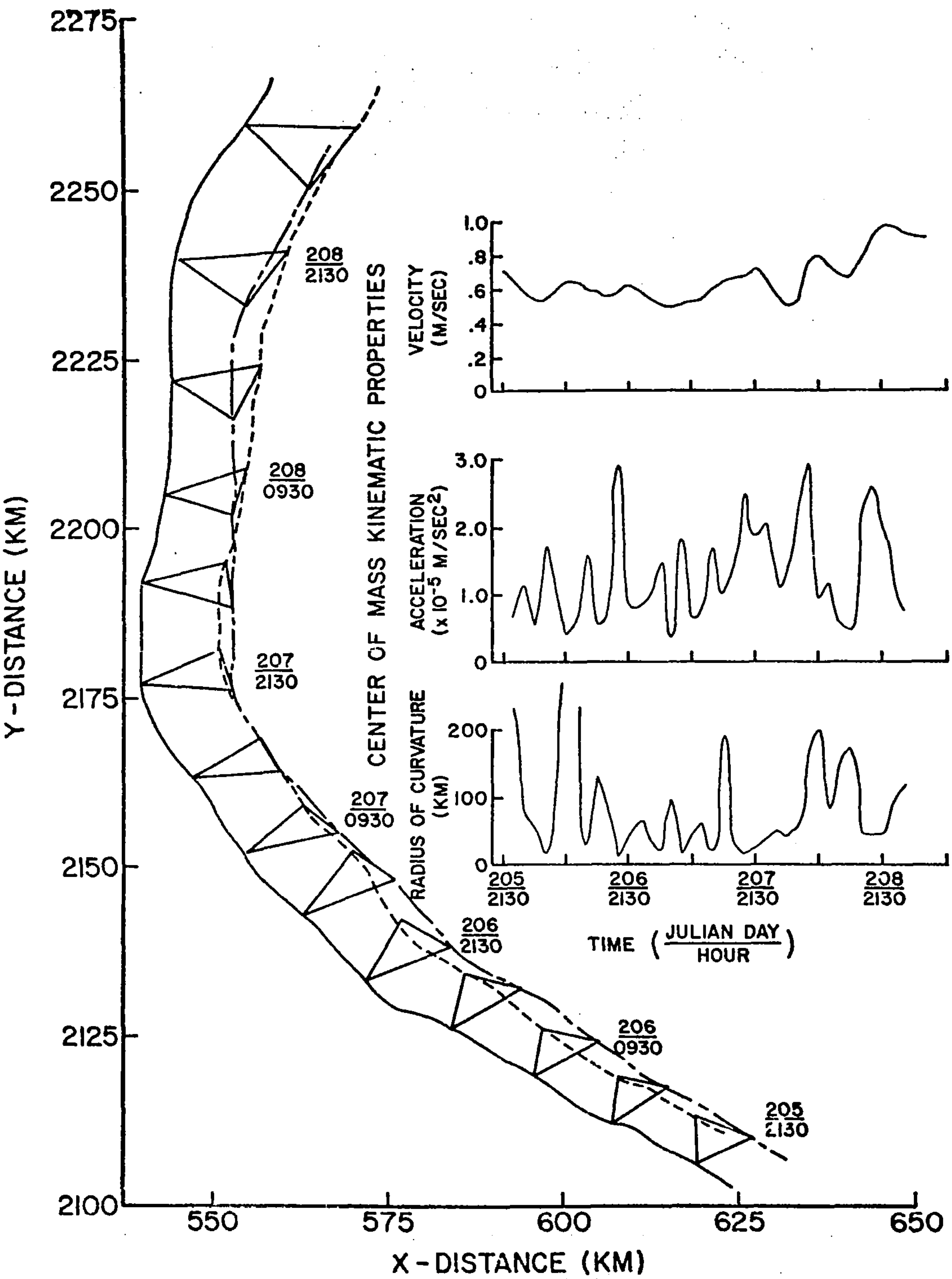
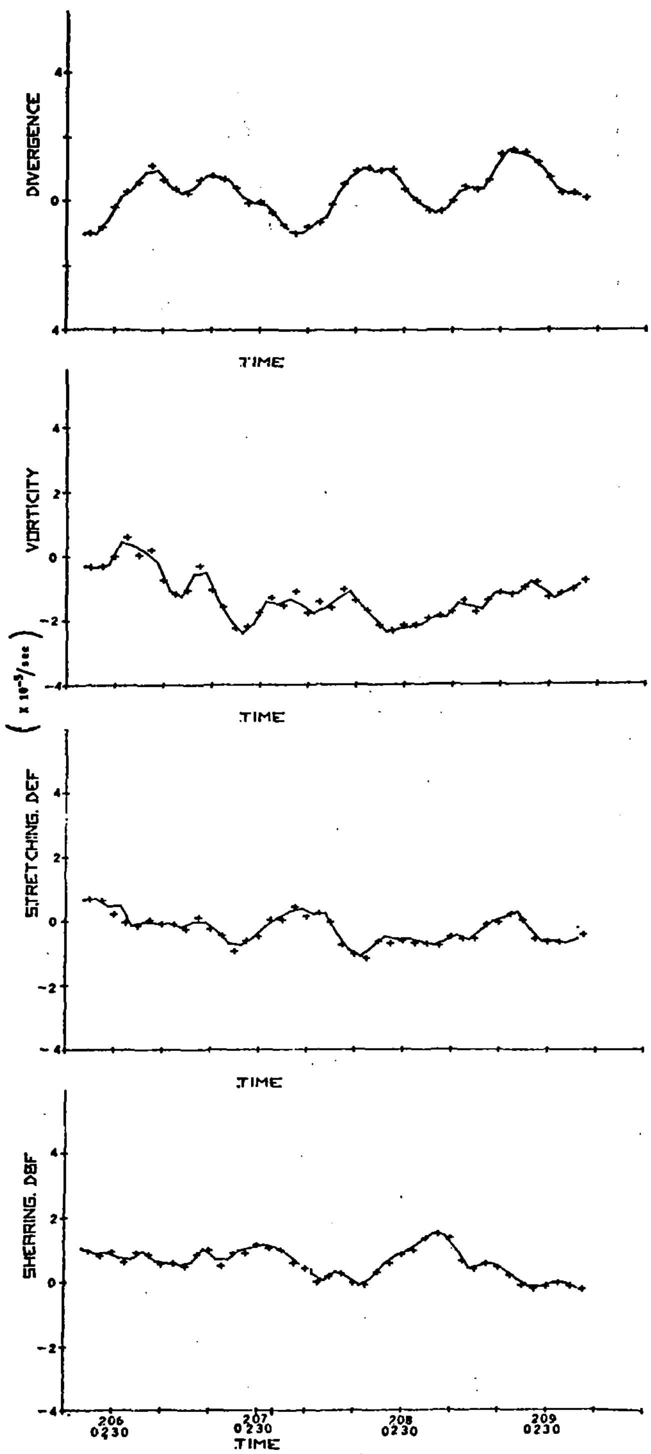

DIVERGENCE
4
2
0
-2
-4
TIME
(x 10^-5/sec)
VORTICITY
4
2
0
-2
-4
202
0530
203
0530
204
0530
TIME

Figure 4.



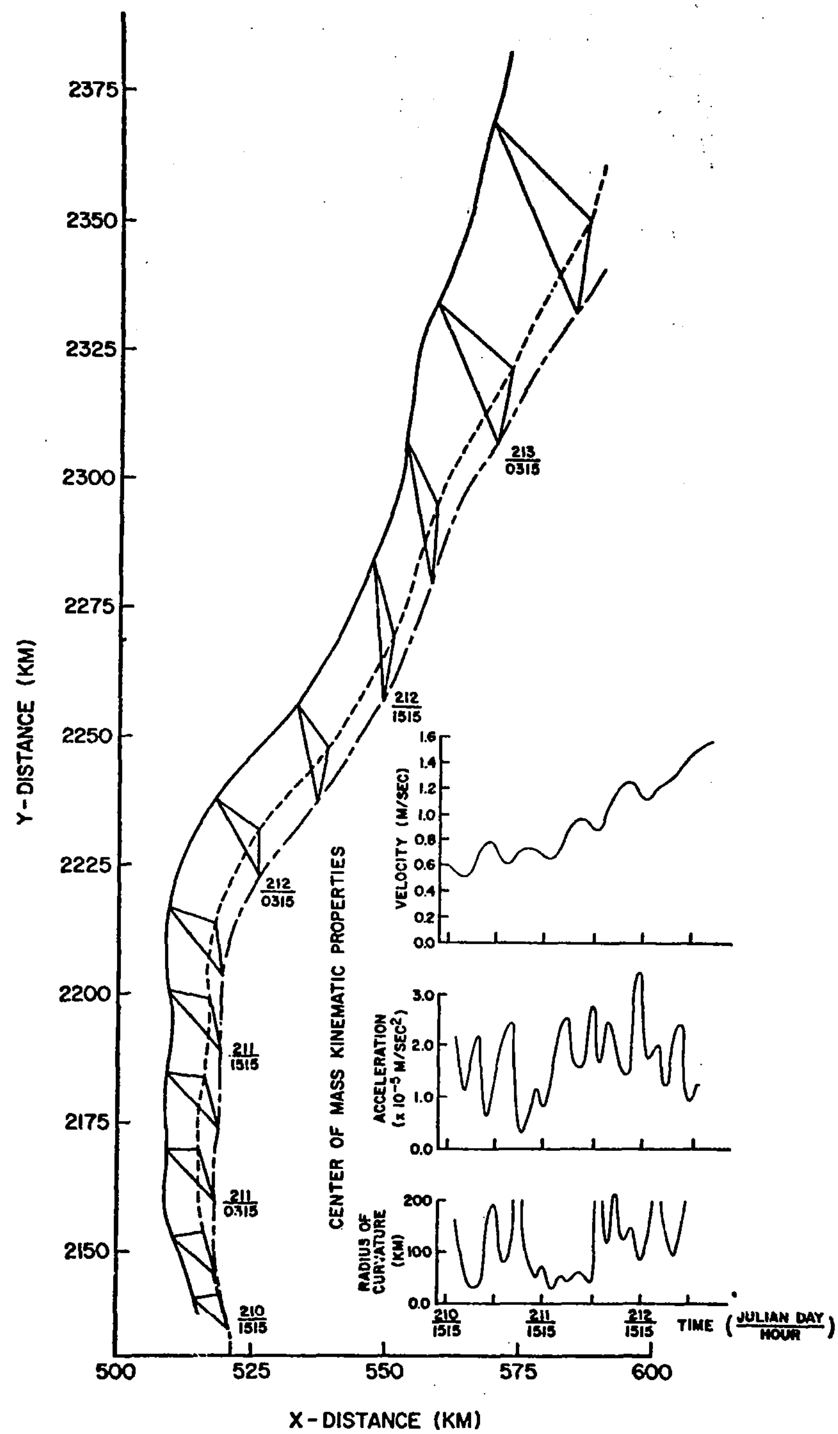
2275
2250
2225
2200
2175
2150
2125
2100
Y-DISTANCE (KM)
550
575
600
625
650
X-DISTANCE (KM)
208/2130
208/0930
207/2130
207/0930
206/2130
206/0930
205/2130
CENTER OF MASS KINEMATIC PROPERTIES
VELOCITY (M/SEC)
1.0
.8
.6
.4
.2
0
ACCELERATION (x 10^-5 M/SEC^2)
3.0
2.0
1.0
0
RADIUS OF CURVATURE (KM)
200
100
0
205/2130
206/2130
207/2130
208/2130
TIME (JULIAN DAY/HOUR)

Figure 5.



DIVERGENCE
TIME
VORTICITY
TIME
(X 10⁻⁵/sec)
STRETCHING. DEF
TIME
SHEARING. DEF
206 0230
207 0230
208 0230
209 0230
TIME

Figure 6.



Y-DISTANCE (KM)
X-DISTANCE (KM)
2375
2350
2325
2300
2275
2250
2225
2200
2175
2150
500
525
550
575
600
213/0315
212/1515
212/0315
211/1515
211/0315
210/1515
CENTER OF MASS KINEMATIC PROPERTIES
VELOCITY (M/SEC)
ACCELERATION (x 10^-5 M/SEC^2)
RADIUS OF CURVATURE (KM)
TIME (JULIAN DAY / HOUR)

Figure 7.

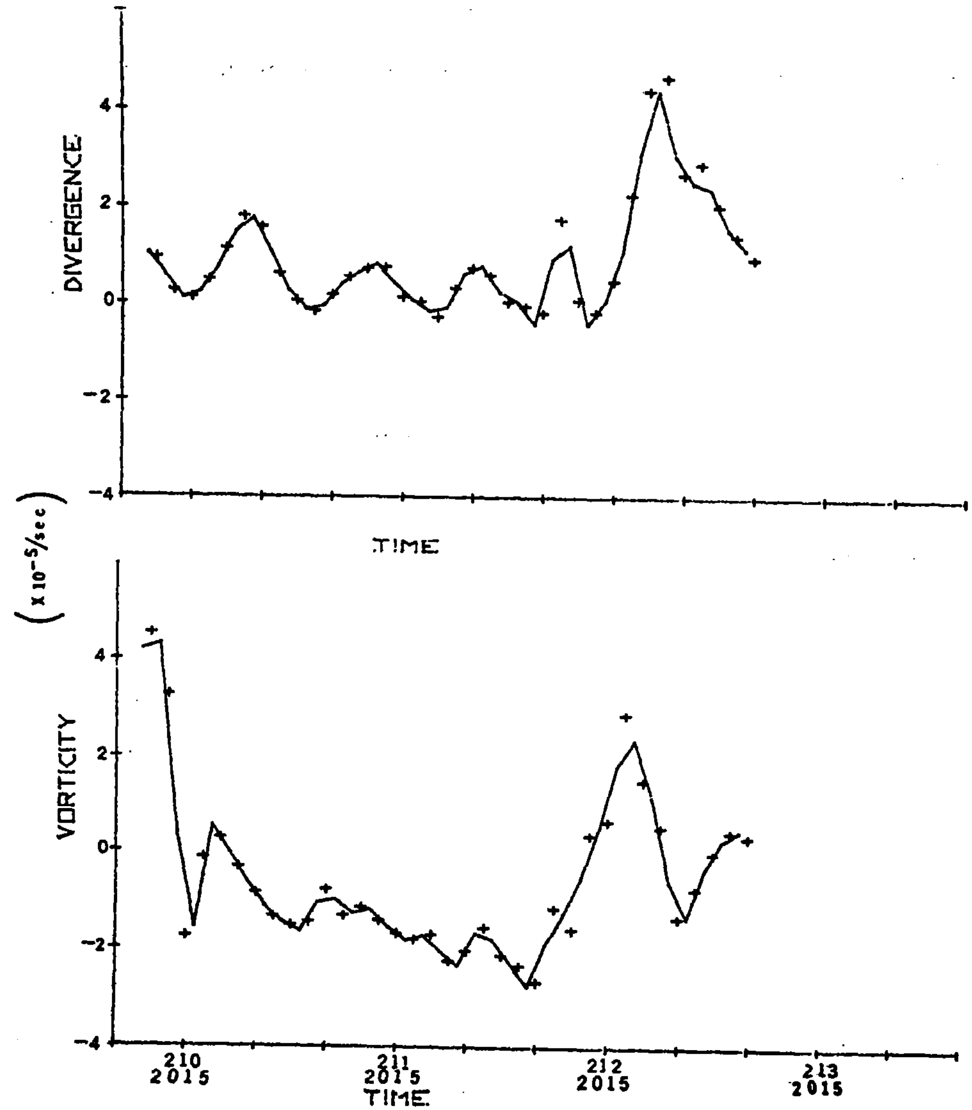
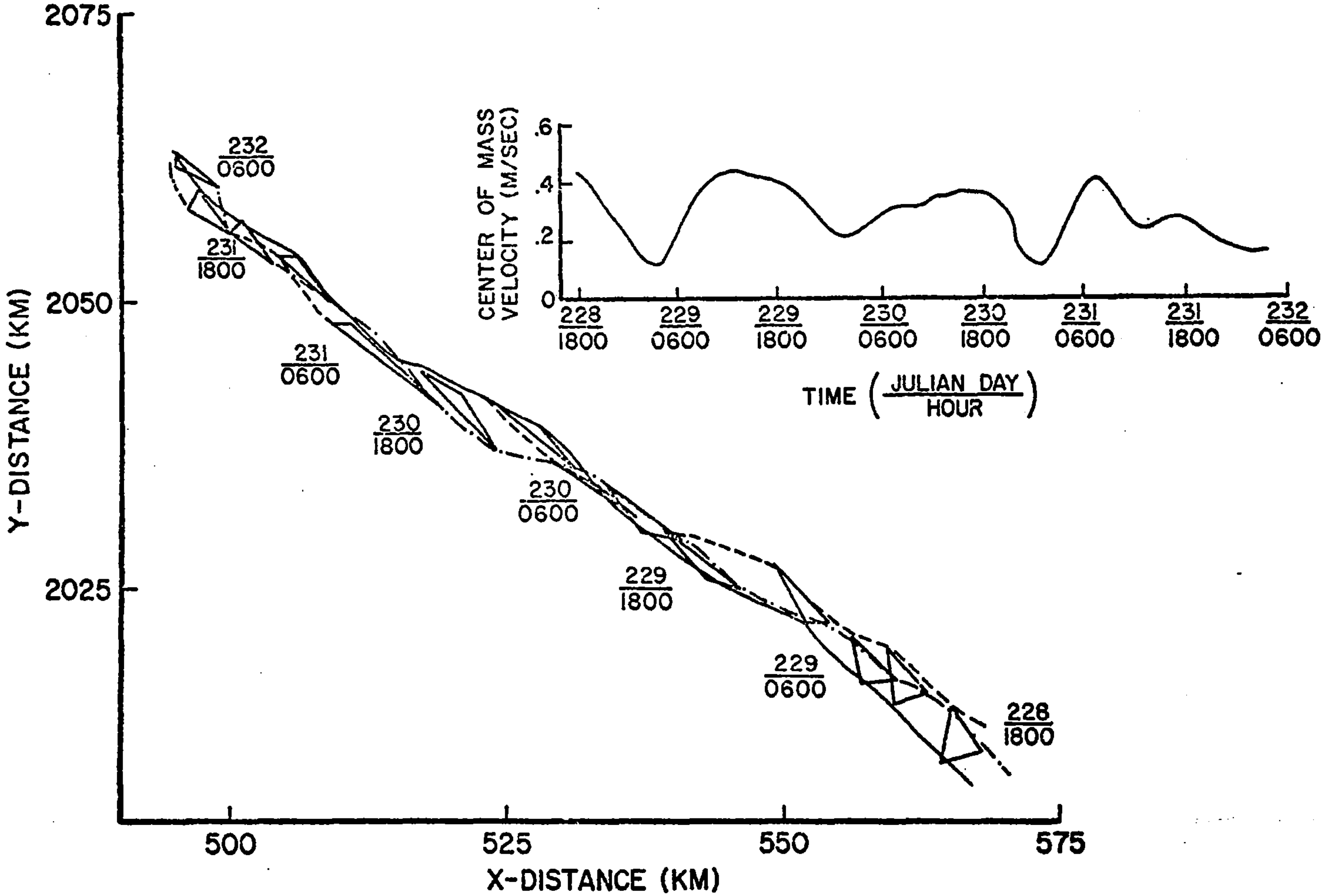

DIVERGENCE
4
2
0
-2
-4
TIME
(X 10^-5/sec)
VORTICITY
4
2
0
-2
-4
210
2015
211
2015
212
2015
213
2015
TIME

Figure 8.



2075
2050
2025
Y-DISTANCE (KM)
500
525
550
575
X-DISTANCE (KM)
232/0600
231/1800
231/0600
230/1800
230/0600
229/1800
229/0600
228/1800
CENTER OF MASS VELOCITY (M/SEC)
.6
.4
.2
0
228/1800
229/0600
229/1800
230/0600
230/1800
231/0600
231/1800
232/0600
TIME (JULIAN DAY / HOUR)

Figure 9.

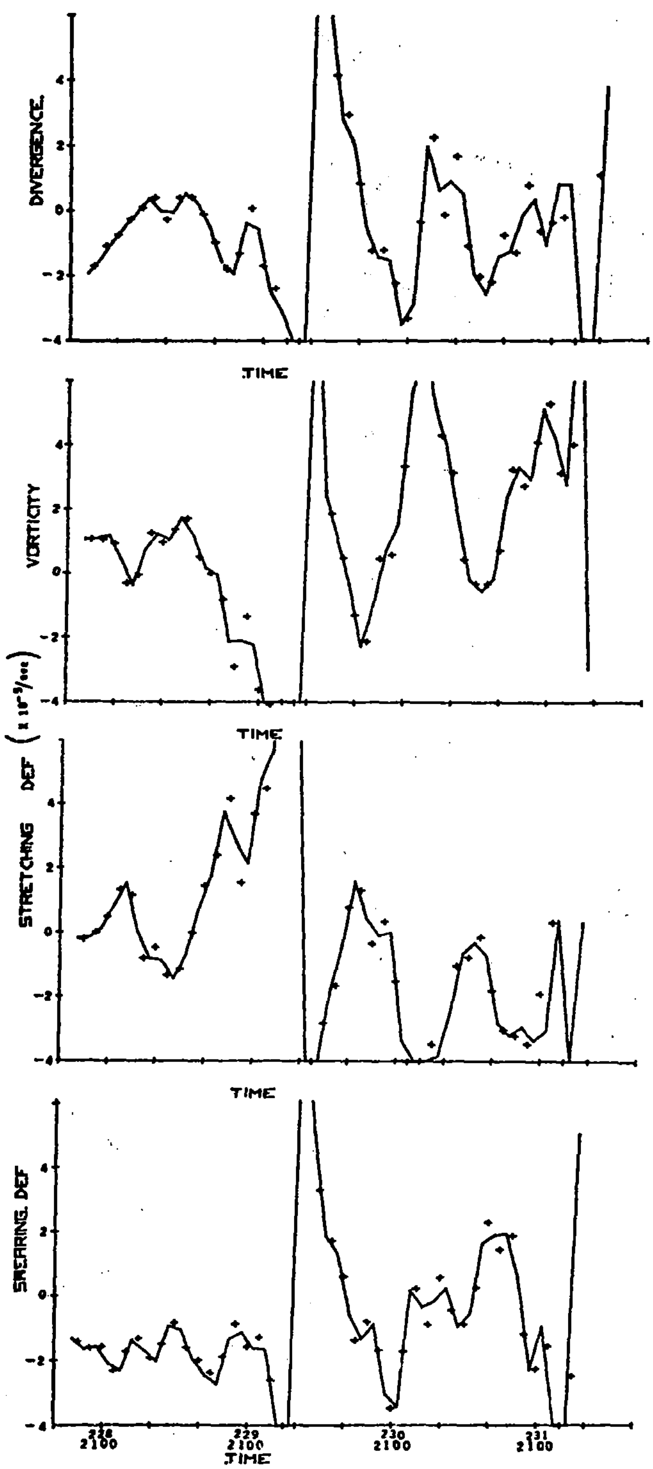
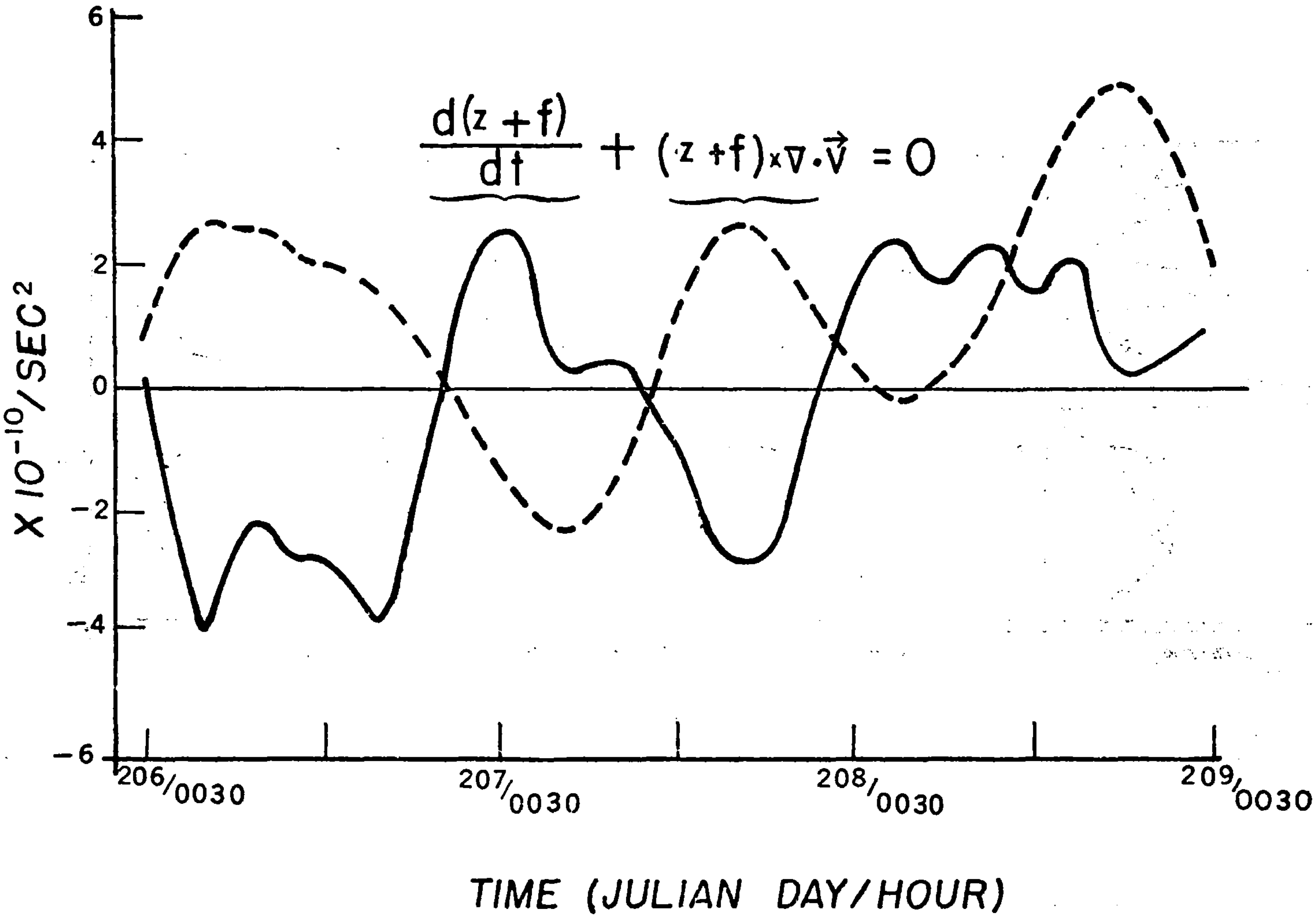

DIVERGENCE
TIME
VORTICITY
TIME
STRETCHING DEF
TIME
SHEARING DEF
228
2100
229
2100
230
2100
231
2100
TIME

Figure 10.



$\underbrace{\frac{d(z+f)}{dt}} + \underbrace{(z+f)\times\nabla\cdot\vec{V}} = 0$
6
4
2
0
-2
-4
-6
X 10^{-10}/SEC^2
206/0030
207/0030
208/0030
209/0030
TIME (JULIAN DAY/HOUR)

Figure 11.

A STUDY OF A COLD EDDY NEAR THE GULF STREAM USING SATELLITE DATA, SHIP DATA, AND BUOY DATA

by
Fred M. Vukovich
Research Triangle Institute

The Research Triangle Institute (RTI) has been involved with demonstrating the feasibility of using satellite data in oceanography since 1967, and has been primarily involved in demonstrating the usefulness of passive IR systems and active microwave systems. Our initial work using IR data in oceanography began with NIMBUS I data which had a great deal of low frequency, coherent noise and high-frequency, random noise problems. This was also true of NIMBUS II data.

The Author: Dr.Vukovich received his Bachelor of Science degree in Aeronautics from Parks Air College in 1960. He received his Masters and PH.D in Meteorology from St. Louis University in 1963 and 1966 respectively. He is presently a Senior Meteorologist at Research Triangle Institute and an adjunct Associate Professor at Duke University. He has been principal investigator on projects in satellite meteorology and oceanography, global and synoptic scale modeling, urban meteorology, radiational processes in the atmosphere, ozone behavior, and laser meterology.

Most of our early effort was to develop techniques to remove the noise which included development of Kalman filters.

In 1971 NOAA-I was launched which possessed a direct readout system: that is, they did not interface the IR data with the satellite tape recorders, which was a major source of the noise in the data, but directly transmitted it to the surface. It became evident that these data were relatively free of noise and could be used significantly in oceanography.

About a year after NOAA-I was launched, RTI set up a direct readout station on its campus. The system used is an EMR Model-111 AD, direct readout system. The system consists of an FM receiver and an oscilloscope device which gives a line-by-line display of the data. A polaroid time lapse photograph using a backpack yields the required display. A linearizing control is an integral part of the system which essentially takes the data for a curved earth and flattens it out so all distances are linear in the photographic display. A tape recorder is used to record the data and essentially compressing the gray scale to a smaller range than originally given and, therefore, bringing out features in the data that would not be quite as distinct as the original transmission might give them. The system can only receive direct readout data, and the satellite has to be within a thousand miles of our station.

In May and June 1973, and March and April 1974, a surveillance of the southeast coast of the United States using the NOAA IR data was made. In the 1973 study period, we were interested in eddies along the western boundary of the Gulf Stream south of Cape Hatteras. In the 1974 case study, a cold eddy was located on the east side of the Gulf Stream. Its approximate position was 74°W, 33°N. A ship from the Cape Fear Technical Institute was dispatched into each area to collect surface and subsurface data.

Figure 1 is a photographic display of data from the NOAA-III Very High Resolution Radiometer (VHRR) for March 7, 1974. There are two radiometers aboard NOAA-III: a medium resolution radiometer used for direct read-out purposes and the VHRR experimental radiometer which is being used for special studies. In the photograph, the Delaware Peninsula, Chesapeake Bay, the Outer Banks of North Carolina, Cape Hatteras, and Cape Lookout are quite evident. The warm Gulf Stream off the coast is evident (dark region). The white regions are generally clouds. The cold eddy is centered at about 74°W, 33°N. Note the warm ring around the eddy. On 1 April 1974, the cold eddy was still located at 74°W and 33°N (Figure 2). The warm ring is still evident and the area of the eddy appears to have become smaller.

On March 26, the Cape Fear Technical Institute's R/V ADVANCE II was sent into the region to collect surface and subsurface temperature and salinity data along three separate transects. At the third station, which was approximately 33°N, 74.7°W, the warm ring was encountered. At this point, an EOLE buoy was launched in order to obtain data on the circulation properties of the eddy. The buoy was launched March 27, and picked up on April 23, essentially 28 days later. Unfortunately, the EOLE data has not been received yet. However, estimates made from aircraft locations of the buoy indicate that the surface currents on the eastern side of the eddy were greater than those on the western side.

Figure 3 is a subsurface temperature analysis along the first transect of the ship. The cold eddy was centered at 33°N and 74.08°W, according to these data. The coldest temperature at the 500 m level was 11.4°C. Both the eastern and western side of the warm ring is evident.

Figure 4 is the analysis of the subsurface salinity data along the same transect. The cold eddy is also a region of minimum salinity. The lowest value was 35.7 percent at 500 meters. The salinity in the warm ring is about 37.0 percent. However, a region of 37.0 percent was found in the center of the cold eddy at the surface.

Figure 1. NOAA-III VHRR PHOTOGRAPH OF THE SOUTHEAST COAST OF THE UNITED STATES ON MARCH 7,1974.

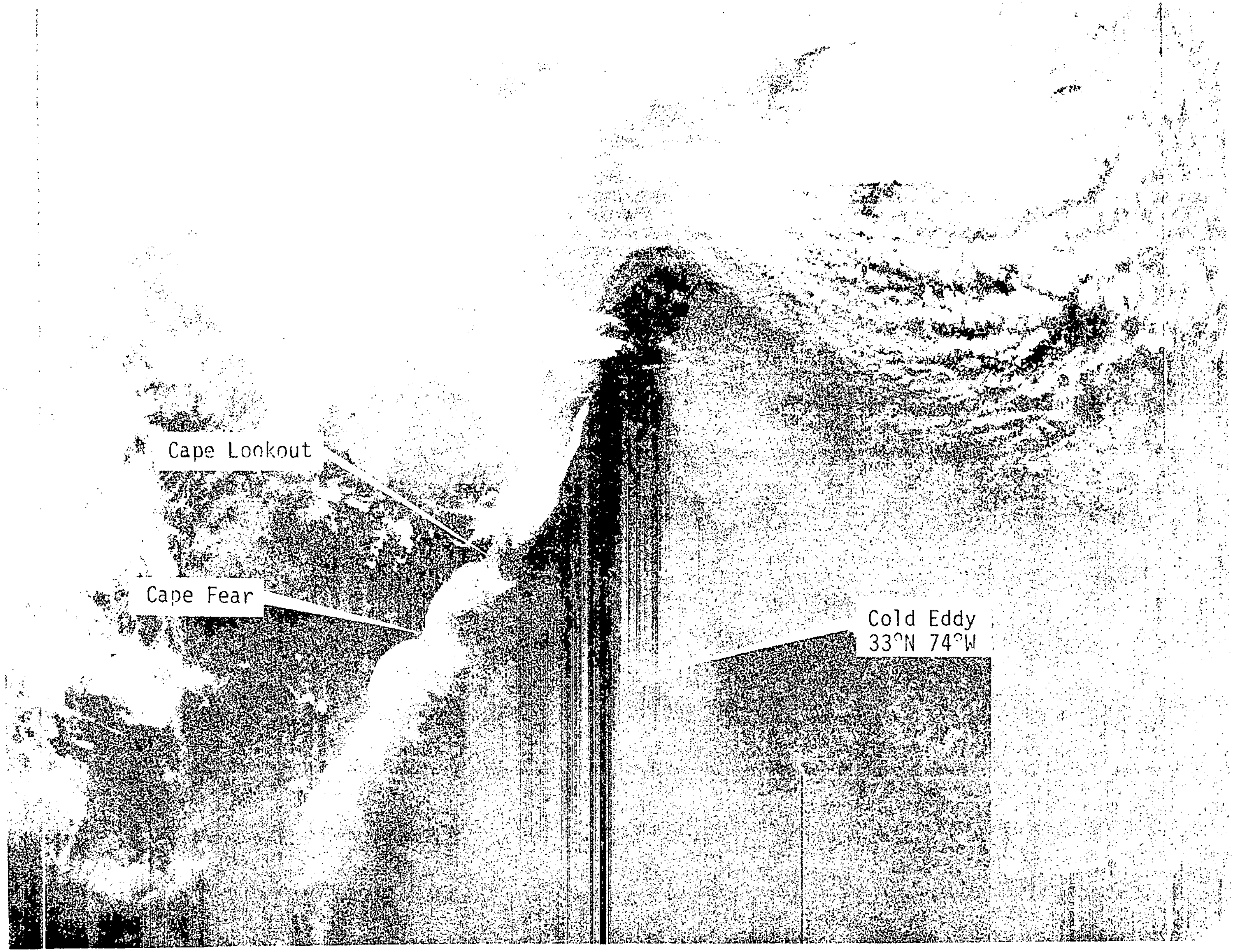


Figure 2. NOAA-III VHRR PHOTOGRAPH OF THE SOUTHEAST COAST OF THE UNITES STATES ON APRIL 1,1974.

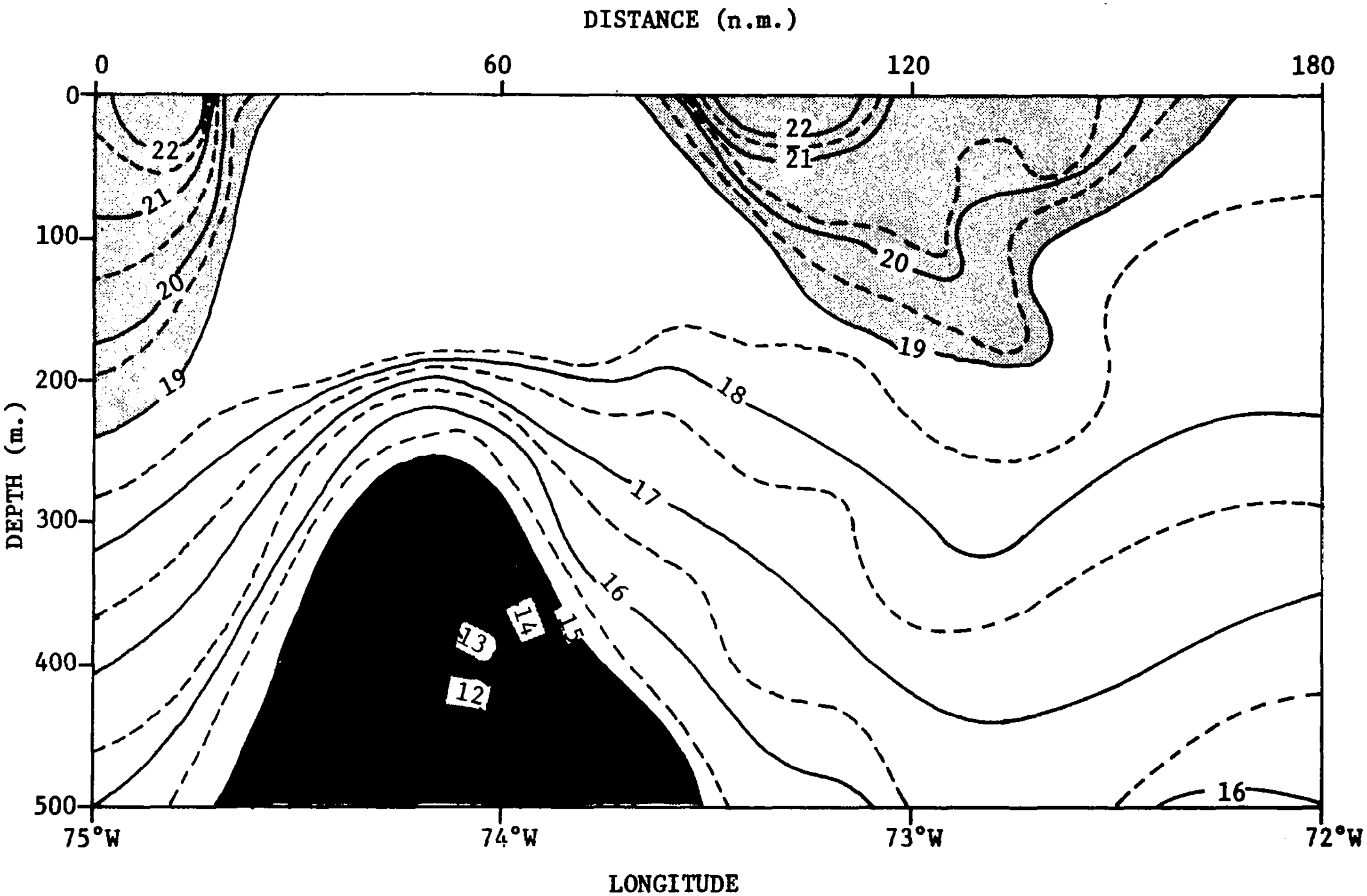


Figure 3. TEMPERATURE ANALYSIS IN THE FIRST 500 METERS OF WATER USING DATA COLLECTED ALONG LINE COINCIDENT TO THE 33°N LATITUDE LINE FROM 75°W TO 72°W LONGITUDE. THE DATA WERE COLLECTED ON MARCH 27,1974.

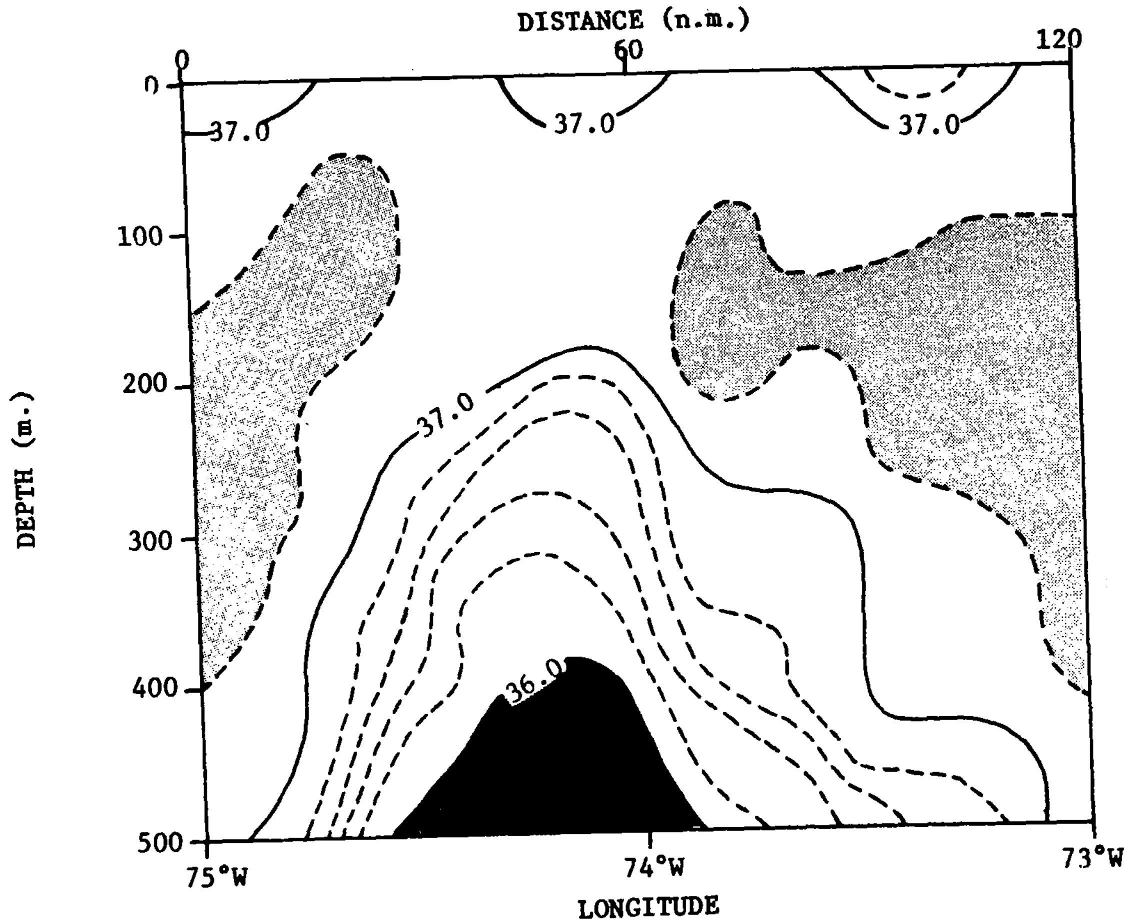


Figure 4. SALINITY ANALYSIS IN THE FIRST 500 METERS OF WATER USING DATA COLLECTED ALONG LINE COINCIDENT TO THE 33°N LATITUDE LINE FROM 75°W TO 73°W LONGITUDE. THE DATA WERE COLLECTED ON MARCH 27,1974.

PERFORMANCE TESTS AND ANALYSIS OF SCALE MODEL DROGUES FOR FREE DRIFTING BUOYS

by

Bill Vachon

Charles Stark Draper Laboratory

Introduction: The NOAA Data Buoy Office was farsighted enough to recognize that even though they're building drifting buoys for the oceanographic community, they still have to understand how well these devices tie themselves to the water current that they're trying to measure. So the Charles Stark Draper Lab embarked on a narrow study to understand what is the best drogue for measuring currents in the Lagrangian sense. It was roughly 2 years ago that we embarked on the study which I will discuss.

Figure 1 is basically the configuration in which drogues might be manually used. The buoy shown in figure 1 is basically the buoy developed at Nova University for drifting applications. A parachute drogue is shown as an example. One might put a sensor package in conjunction with a ballast weight at the bottom if one wanted to measure a particular parameter.

The Author: W. A. Vachon has been involved in the field of ocean technology and ocean instrumentation for the past 7 years. His areas of interest include mooring design and analysis, and the measurement of ocean currents. He has been employed by the C. S. Draper Laboratory since 1963. He was graduated from M.I.T. in 1963 with a B.S. in mechanical engineering. He received his M.S. and M.E. degrees from M.I.T. in 1965 and 1969. Mr. Vachon is a member of the American Society of Mechanical Engineers.

Models: Many different scale model test shapes were used in our investigation into which drogue was the best from the point of view of locking to the water mass element, minimizing expense, and shipboard storage. There are many desirable features in a drogue, but most importantly you'd like to think it locked to the water mass as well as possible.

Figure 2 is a crossed vane that a number of people have used, including work at the Langley Research Center and VIMS. We made both aluminum and plastic models of the version shown in figure 2. The plastic model could be collapsed for easy storage. The size of the models was 10 inch by 10 inch. End plates were put on it, in addition to those without end plates as shown on the left, in order to see if there was any increase in drag. A similar device to that shown in figure 2 was made with three axes. It was desirable to see if there was a better independence of the drag coefficient with azimuth orientation. We found in that, that there was an independence. For the two axis crossed vane, if the flow is going into the pocket at 45° angle to the plate, it displays a slightly different drag coefficient than perpendicular to the plate. If you take the product of drag coefficient and area, there was still a variation in drag area (C_DA) with flow orientation for the two axis versions. In the three axis version there was relatively little variation in drag area with relative flow orientation.

Other shapes were tested in a scale model fashion in order to ascertain something about their drag coefficient and their deployment characteristics. The shapes shown in figure 3 (parachutes, sea-bucket, sea anchor, etc.) all require a certain level of velocity going by the drag element in order to develop the drag coefficient. This is true unless you take quite a few pains to make sure that the drogue stays open in zero current. You could possibly do that with weights and floatation at the entrance, or spreader bars, but these means become a bit cumbersome and in cases weight the drogue down. It is, however, desirable to keep these drogues neutrally buoyant. Some of the measured drag coefficients are also shown in the figures. These coefficients are averaged over a Reynolds number region of about 10^4 to 10^5. An interesting point can be seen in figure 3. The downstream area was varied in relation to the inlet area on the sea anchor. The ratio was varied from 3 percent to

10 percent. It can be seen that the drag coefficient decreases as the downstream area is increased in relation to the upstream area.

Other shapes are shown in figure 4 on which tests were made. The drag coefficient parallel to the surface of a fish net was measured. In this case it's surface is parallel to the direction of the current. This is the type of drogue that was used back in the "50's" by Montgomery and Stroup in measuring the Pacific equatorial countercurrent. It seemed to be a decent device, but you have to have an awful lot of area in order to build up the type of drag force desired for a near surface drag element. The appealing factor is that maybe there are a lot of old fish nets hanging around that people might feel are too weak to support the fish load. Maybe they could be used in a pinch.

It is known that cylinders have a relatively high drag coefficient. Normally it is about 1.2 or so. We actually measured approximately 1.3 in the Reynolds number region of 10^4 to 10^5. The idea of the cylinder that was intriguing was that if a design is implemented by taking a disc on the top and a disk on the bottom, it is possible to make the intervening cylindrical wall area out of plastic and then just squash the two disks together for storage. It is possible to make the length of this cylinder as long as you'd like, depending on how much of the water column you'd like to sample. In this way it is possible to increase the area quite nicely, by just making it longer and it will still store fairly readily, while retaining a decent drag coefficient.

A sphere is an appealing shape (fig. 4c), even though it's got a low drag coefficient (approx. 0.5), it is possible to take a very large thin walled plastic sphere and inflate it with ambient water. It can be made as large as desired and develop a very reasonable product of drag coefficient and area. The window shade drogue, shown in figure 4d, has a very high drag coefficient when it's streaming perpendicular to the flow. In a high relative velocity, drag coefficient decreases. The window shade drogue is, however, the most desirable from the point of view of our original goals.

We had to narrow our studies a little bit at this point and go into a little more depth. Figure 5 is a picture of the towing device that we used to dynamically test some drogues as well as tow them. Most shapes were supported from the end of the vertical strut. Some of them were suspended by a string. Others were hard-clamped in order to get rid of pendulous effects. We clamped it to strictly look at how the Reynolds drag or the drag of a simple object through pressure forces would behave. A fairly complicated drive device is included in this design in order to make the strut go up and down in order to simulate buoy heave. At the same time that the drogues were heaving up and down, we measured the drag force, by a device attached to the strut. We were seeking answers to the question of how heaving it up and down, as a buoy, would alter the drag coefficient. This is a very difficult measurement to make, because you're in a dynamic environment trying to make a precise measurement. We encountered the standard problems.

<u>Test Results:</u> The parachute, if it isn't perfectly neutrally buoyant, will take a certain velocity to open. Figure 6 displays the results for three different models. The ordinate is the measured drag areas while the abscissa is a plot of the model relative velocity. The best parachute opened at about 0.02 knots. This chute was called neutrally buoyant. The other one which is almost as neutrally buoyant opened at a slightly higher velocity. Before it opened up, it hung down vertically, and as the velocity was increased, it started to open. For chute number 2, the drag coefficient seems to increase at the very low velocities. It is felt that at these speeds the drag force is dominated by viscus forces rather than pressure drag. Parachute number 3, illustrates the key design factor in a parachute drogue. That is, if it isn't neutrally buoyant, it takes a lot to get it open. This one had a steel ring at the apex used for retrieval as shown in the sketch in figure 6. If a line is put on the ring it is possible to retrieve the parachute very easily by pulling that line, and spilling all of the drag. However, that ring, if it's not neutrally buoyant, causes the parachute to hang down vertically and just won't open unless you get a large relative velocity.

Figure 7 is a plot which shows how a parachute performs. If a drag force is applied, as shown along the abscissa, the relative velocities that would be measured are plotted on the ordinate. These curves are plotted as a function of the drogue deployed (open) area. As an example, for a 50 pound force one would expect to have a 7 or 8 centimeter per second relative velocity employing an average drag coefficient of approximately 1.35.

Figure 8 shows the performance curves of a two-axis crossed vane, based on the drag tests. The crossed vane does develop lift at higher relative velocities. Therefore, the curves, as they get to higher forces, do not continue to display a force proportional to velocity squared. They start to fall off and eventually less drag force results.

Figure 9 displays performance curves for a three axis cross vane. It displays the same problems of lift as a two-axis vane, such that at high forces it's effectiveness as a drag device is diminished.

The window shade drogue had the most appeal of all the shapes tested. One factor is its high drag coefficient. Other factors are its simplicity and easy storage and deployment. Figure 10 outlines some of the forces that you get on a window shade drogue. It has a lift force, which a parachute or a sea anchor don't really have because they are symmetric. Figure 10 defines some angles and forces in order to analyze the drogue. A mathematical model of a window shade drogue was derived and checked against some of the data which is shown in figures 11 and 12. In these figures it can be seen that we tested many different shapes. We tested triangles, rectangles, squares, and diamond shapes. We employed both Froude and Reynolds scaling laws also. We got some scatter in the data, especially when we ran Reynolds tests. The Froude testing produced relatively good drag data. In Reynolds testing other problems of vortex shedding oscillation developed which made our data acquisition difficult. The major result of the data shown in figures 11 and 12 is that a rectangle, in which the material fills in the triangular apex area, is about as good as you can get. If a triangle or especially a diamond design is tried, there appears to be a problem of poor orientation perpendicular to the flow. That is, it may not orient itself as well to the flow as a shape that has flat sides. That is a feeling derived from the tests. The orientation factor can be viewed as arising from differences of drag forces between the upstream and the downstream edge of the drogue. When the drogue is drifting perpendicular to the flow, there's a balance of forces on either end. However, as it becomes at an acute angle the upstream drag coefficient can be thought of as being higher than the downstream; tending to right the drogue normal to the flow again. Therefore the diamond might not be as good because end effects may be less. A lot of testing was not done in order to verify this fact.

Figure 13 is a plot of some performance curves for a window shade drogue. It is derived using a mathematical model that we derived. From these results it is clear that you cannot develop any more drag force than the ballast weight that you put on the bottom of the drogue. That is, if you put a 50 pound ballast weight, the maximum drag force for a given area becomes asympotic to the 50 pound drag force. This is purely a mathematical derivation using measured drag values. The assumption that is important and could contribute to slightly lower forces at high relative velocities is that tangential drag forces are neglected, that is, the drag parallel to the surface. In reality the curves in figure 13 would probably go a bit above the value of drag equal to that of the ballast weight. The values might be increased by a few percent, near the value of the ballast weight, but this has not been verified. A desirable portion of the curve in which to work is where the ratio of the drag force to that of the weight is one half or less. Figure 14, a dimensionless plot of drogue performance, shows that in that region of the curve the dimensionless drag force is a linear function of the dimensionless relative velocity. Once the drive force becomes an appreciable percentage of your weight (50 percent) you start to lose drag and your drag force peaks out at about the weight. It should be reiterated that these curves are just mathematical and unverified by data at present.

It is, however, felt to be good for drag force values as a small part of the weight which is the desired situation in order to derive the most from a drogue and be well-locked to the water mass.

Figure 15 is a plot of some curves depicting the depth stability of a window shade drogue in the water column as a function of the drag force applied at the surface. The problem with the window shade drogue is that it develops lift forces as you start to pull on it, so it does come to the surface. This is, of course, true for crossed vanes also. It can be seen in figure 15 that as the drogue develops a drag force approaching that of the weight, it comes up towards the surface.

<u>Drogue Dynamics:</u> Once one has the basic performance curves of a window shade drogue under controlled conditions, other questions arise. What really happens in the ocean? Does the window shade drogue impart dynamic loads to the buoy? Does it submerge the buoy? Does it submerge your antenna which will be used to transmit to the satellite? Does it cause shock loads in the tether line which might eventually cause it to part? It is known that dynamic loads will result. Simple dynamic analyses were conducted in order to motivate some thinking on the idea of survivability, shock loading, and buoy submergence. The particular set of curves shown in figure 16 is derived using a few assumptions. The drogue area in square feet is twice the weight of the ballast in pounds. These assumptions are just a handle on the problem. Other assumptions can be made based on other ratios. The solid curves shown in figure 16 are for the drag parallel to the surface of the window shade drogue. They are plotted as a function of the drogue area for given sea heights. The sea heights are the peak of the sea height curves from the Pierson-Moskowitz curves of sea conditions versus wind speed. The inertial loads are also plotted for 40-foot seas based on the inertia of the drogue and ballast weight. It can be seen that, for the assumptions made, the drag forces of the drogue slipping vertically through the water exceed the inertia forces by a large margin. The drag coefficient of a drogue parallel to its surface was assumed to be approximately 0.05 based on the full area. This number was measured by John Garrett at Marine

Sciences Pacific in Voctoria, British Columbia.

As an aid in interpreting figure 16, it can be seen that a 100 square foot drogue in a 40-foot sea might develop a peak vertical force of roughly 300 pounds. This then indicates how much reserve buoyancy is needed in the buoy in order to keep it from submerging on the wave peaks.

There is the question of tether line shock loading from a window shade drogue, which impinges severely on buoy system survivability. Figure 17 is a plot which contains a number of interesting functions. On the top is plotted the position of a buoy going up and down in a wave. Below that we have the vertical drag force which is represented by the function "Y dot magnitude of Y dot". If the buoy was a surface following buoy, such that it did not submerge on the leading edge of a wave, as the buoy was going up it would pull the drogue up with it, then when the buoy started to go down into the trough, the drogue would start to descent; pulled down by the ballast weight. The buoy might be coming down quite rapidly, as shown by the top curve. However, the drag force parallel to the surface of the drogue might cause it to reach a downward terminal velocity which is shown by the dotted portion of the drogue position curve. Such a condition might persist until the buoy, rising on the leading edge of the next wave, takes up the slack in the tether line. At this time the tension in the tether line might look like the lower curve in which shock loading is evident. This problem points out that the proper design is to size the ballast weight such that it allows the drogue to follow the wave sinusoidal input. In other words make the weight heavy enough so that this shock loading condition is avoided. This problem could be one of the major reasons for buoy system failure. Based on that type of thinking the curves in Figure 18 have been developed in order to portray a locus of possible shock loading conditions.

For a given drogue area, there is the maximum permissible sea height that can be withstood by a given drogue for a surface following buoy. This is a function of the ballast weight. It can be seen that as you increase the weight that is at the bottom of the drogue, the system can withstand higher and higher seas without shock loads in the tether line. It is true that the system is quite often limited by the buoy reserve buoyancy because the buoy is small. With increased buoyancy, greater weights could be placed at the bottom of the drogue, minimizing the chances of shock loads.

Recent Finding: The material presented up to now is a summary of the work that appears in our lab report R-769. In recent weeks, however, a full scale window shade drogue has been built. Attempts have been made to minimize the drag parallel to the window shade drogue surface, by using faired struts that are often found on a sailboat. Instead of round pipes at the top and bottom of the window shade drogue we've used these aerodynamically low drag shapes to try to help the dynamics problem which I outlined. Two series of tests have been conducted. We went into Walden Pond, in Condord, MA., which Thoreau made famous. It's deep, but it's not quite deep enough. We found a pot-hole there, by sounding, and as it turned out the horizontal extent of the pot-hole was not enough for the drogue to reach steady state drag condition when it was being dragged through the water with a known force. The next test found us in a quarry with our window shade drogue. It should be mentioned that the drogue measures 26' vertically by 12 feet wide plus an apex and floats above that. We needed 37 or 38 ft. of water. We found that it was difficult to get a still body of water where there were no currents and little in the way of wind influence, so we ended up in the quarry. The quarry was 100 ft. deep and about 400 ft. across. We mounted a winch on the side of the quarry as shown in figure 19. The winch was used to pull a boat, and in the boat a man measured the horizontal drag force on the drogue. The float on the surface had very little wind drag. It did, however, have to have enough volume to support our window shade drogue, which weighed 80 lbs. in water. It was necessary to let the end pipes fill up with water and reach a stable configuration, before weighing. If care was not taken it would start out lighter, and change with time as trapped air escaped. We measured the force on the boat and the tow velocity on the dock. A polypropolene line which is neutrally buoyant was used as the tow line in order to minimize catenary forces which can act like a spring.

Figure 20 shows the drag test results from the quarry test. The measured drag coefficients are even higher than was found in the scale model tests. These data are tabulated after having subtracted out the drag of the float. The fifth column of figure 20 is measured drag coefficient at the given relative velocity. After correcting for non-zero angles at the drogue the drag coefficient, $(C_D)_o$, is tabulated for the drogue hanging straight down. It should be noted that the measurements were made over fairly lengthy runs. The average force and the average velocity were measured. The velocities on the left are given in ft/sec and in knots.

The next figure is a plot of the measured drag coefficient as a function of relative velocity. An attempt was made to see if the data could be made to fit into our mathematical model of the window shade drogue performance as a function of relative velocity. The data were projected corrected back to a zero relative velocity by calculating the angle at which it drifted with respect to vertical. Then a simple analytical relationship between the drag force, weight, and angle was used. That is: $F_D = W \sin \theta$. The angle θ is defined in figure 10.

The data seem to follow the curve. However, the maximum drag coefficient; $(C_D)_o$, calculated by a least squares fit to the data points, is 2.65 compared with 1.93 for the scale model results. It is not known how that big difference arises. It is fortuitious, if it can be believed. In general, you would like to have a very high drag coefficient. On the other hand, you would like your data to agree between scale model and full scale tests.

A rational explanation for the difference has not been found. If you read how parachutes behave it is seen that some give a very high drag coefficient that is actually higher than 2.65 if you take into account the vertical velocity of a parachutist descending. The explanation of this is in a drag book by Hoerner. This in one of the best books put out on the subject of drag. Hoerner has been around since the '30's, I guess, doing drag on every conceivable device, including human beings falling out of planes, nacelles, and wings. He has a lot of information on parachutes. Parachutes, in fact, will stream sideways as they're descending. A standard cup parachute will stream at an angle somewhere around 40 or 50 degrees to vertical. And it will drift sideways through the air unless you take special precautions to install openings and flaps. In this case you decrease your drag coefficient but increase the stability. In many cases the descent velocity is 70 percent of the streaming velocity. If this value is substituted in the calculation of C_D the drag coefficient will be increased by a factor of 2 or more. A drag coefficient of 1.35 was measured in the tow tank employing a scale model chute. However, when pulled, the parachute did stream off to the side by up to 30 or 45 degrees. So when you resolve a component of velocity perpendicular to the entrance of the parachute, you resolve it down by roughly the square root of two. In this way a smaller number is put in the denominator of the calculation of C_D and you're squaring it. An apparent doubling of the drag coefficient perpendicular to the entry results.

The reason why the window shade drogue gives such a high value of drag coefficient and why it's different from the models is elusive. At this point in time, I tend to think that the parachute permits the attachment of a vortex on the side of a trailing edge so that it will slip to the side more readily than a window shade drogue. We did not observe that in the window shade drogue. The window shade drogue, when properly balanced, would stream straight at the flow.

The weight balance in a window shade drogue is very, very critical. If the bottom weight is a little bit unbalanced to one side, the drogue will then stream sideways in the direction of the weight. Due to the weight unbalance the drogue will incline itself at an angle to the flow, such that it develops a lift force towards the more heavily weighted side. The drogue will then kite off in that direction.

Figure 22 is a summary of some pros and cons of a window shade drogue and a three-axis crossed vane. A review of the positive aspects of the window shade reveal it to be quite elegant. Other questions arise though. How does the thing respond angular-wise in low currents? We pulled the full scale model at fairly high currents, compared to what might exist as relative velocities in the ocean. We found good response. There was one situation, where we left a buoyed window shade drogue to drift only by wind effects on a small float. On this day there was a 15 to 20 knot wind. The float was sticking about 8 inches above the surface, so there was very little wind force acting on it. When the test started the drogue was pointing directly at the wind as seen from the surface. The drogue was white and only about 8 feet beneath the surface, permitting easy view. After about 45 minutes or an hour the drogue was pointing at about 45° to the wind and had moved about 75 ft. We continued to observe it and very gradually it drifted in the direction of the wind. When we finally quit the test after about an hour and a half the drogue was about 20 degrees off of the proper direction perpendicular to the wind. We concluded that it was coming around nicely at an estimated relative velocity of about .01 knots. Inherent in this test is the assumption that there was still water in the bottom of the quarry, which should be true.

In figure 22 there is always the question of dynamic behavior. Then the question of the weight balance is critical. This fact is not necessarily a con. One should just take precautions to make sure your weights don't shift, or that they are secured so that they won't shift.

Another appealing drogue is a three axis cross vane. Its product of drag coefficient and area (C_DA) is just about independent of orientation. Its performance is simple; possibly a little less complicated than the window shade drogue. It has negative features though. It is more complicated to build, and has a lower drag coefficient. Again the question of dynamic behavior applies to a crossed vane as well.

In summary, the window shade drogue looks like the best drogue to pursue. It is, however, felt that many more ocean tests on its performance are required before its performance is adequately understood.

COMMENTS/QUESTIONS

Bob Waldon, Woods Hole

One of the pros, that you missed Bill, but I think pretty important, that's the ease of retrieval, also. If anybody here has tried to pull back a parachute they know.

Bill Vachon

Yes, I tend to think of it as expendable. That's very, very true, though. It's also fast to launch, if you're working in deep water. Sometimes the parachute might take as much as a half-hour to get down to working depth, because it pulls against the cup shape as it's going down. This problem could be avoided by using a salt-lick as a weight on the drogue canopy. This will weight the drogue down during descent and after an hour or so will dissolve away, leaving the parachute to function normally.

Dave Leiter, AMETEK, Hunter Spring

I think there was one of the drogue configurations, that you might have passed over a little too quickly. That was the sphere. If I might, I'd like to come up and throw something on the board that I've been thinking about.

Bill Vachon

Sure. Go ahead. If that's okay with you, Don.

Dave Leiter, AMETEK, Hunter Spring

It seems that in all of the discussions so far, the idea is to lock the buoy as firmly as you can to the current. My thought is, why not make the entire buoy a part of the current? What I'm saying here is this: Take a package, air drop it or however onto the surface. This package basically consists of two things; a power source, a pump, and two big bags. One of the bags is any plastic film that's suitable. You inflate this thing and make it big; maybe 40 or 50 feet in diameter. Inflate the lower bag with water. Inflate the surface bag with air. It will always stay up on top. The lower bag is then inertially and drive-wise a piece of the current.

Bill Vachon

I think that is a good idea. In fact, in working near the surface, that might be very good. You'd need a pump, as you say to inflate it. It has drag independent of direction, so the alignment problem does not exist. You have one problem, though. That is the dynamic response of the lower ball. If you don't have a hefty attachment between your surface and the sphere, you may have trouble. There is also high drag to vertical motion also, which induces high loads. So over comes a big wave and the lower ball is essentially an inertial mass. It doesn't want to go anywhere. If you attach it with a line down to about 30, 40, or 50 meters to the sphere, you've got potentially, pretty severe dynamic problems as the upper air-filled ball follows the waves. There's nothing that says that you have to inflate the lower ball completely. It can be a floppy mass and have enough slack in it so that it can move with the surface.

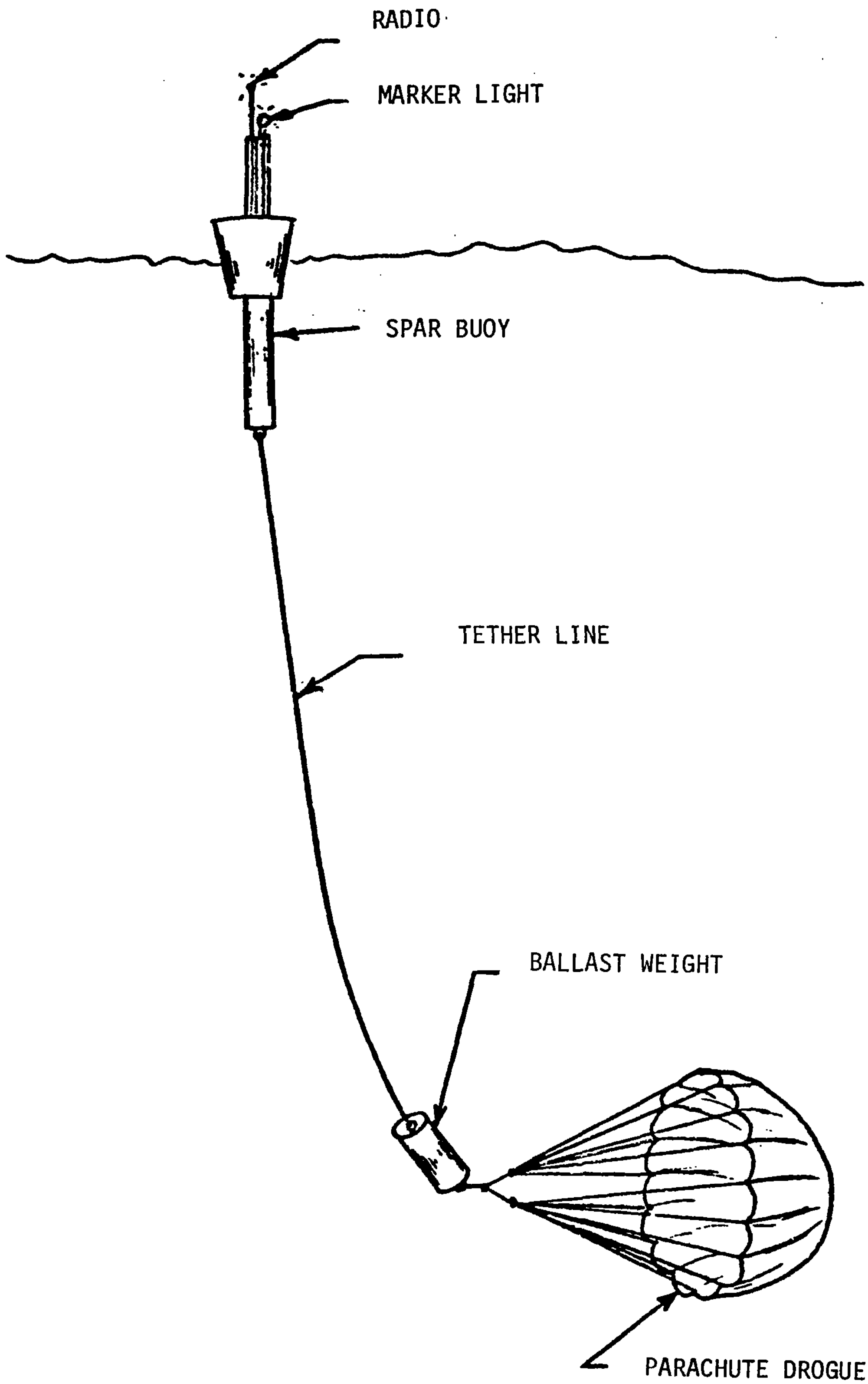


Figure 1 STANDARD DRIFTING BUOY CONFIGURATION

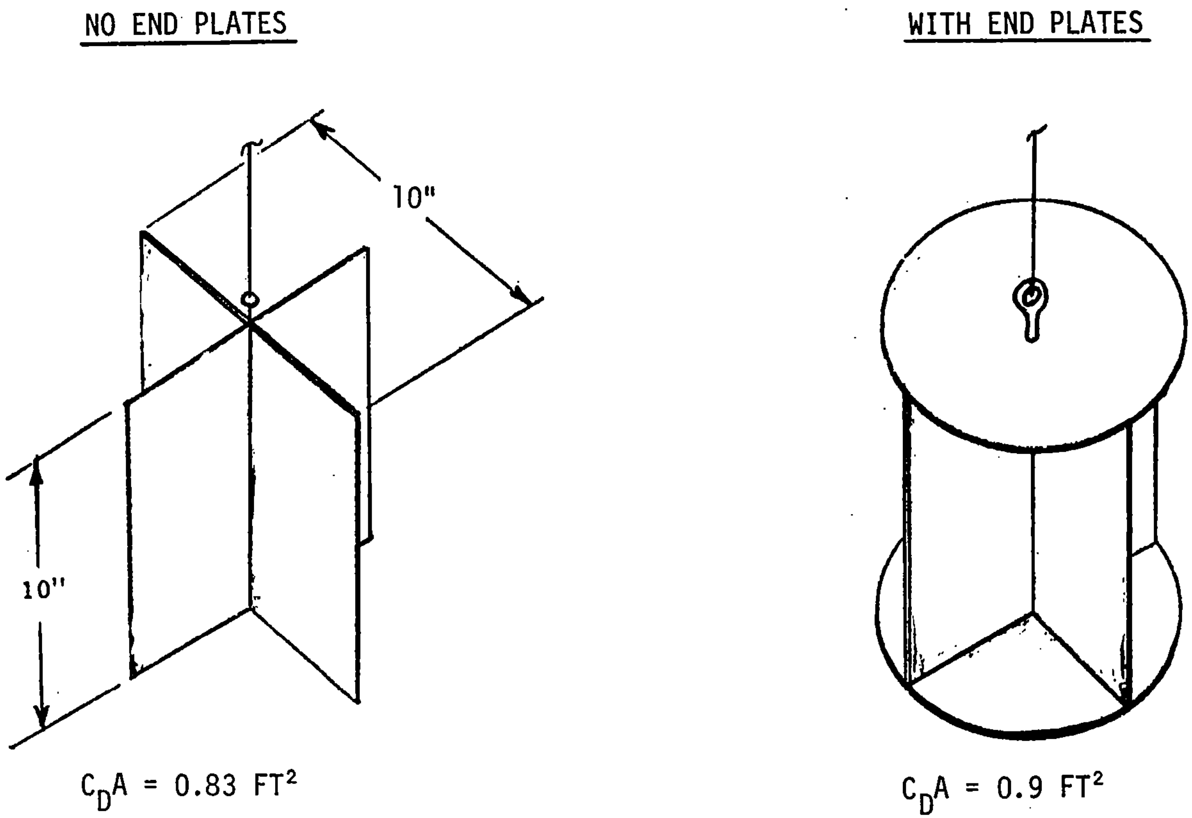


Figure 2 TWO-AXIS CROSSED VANES MODELS

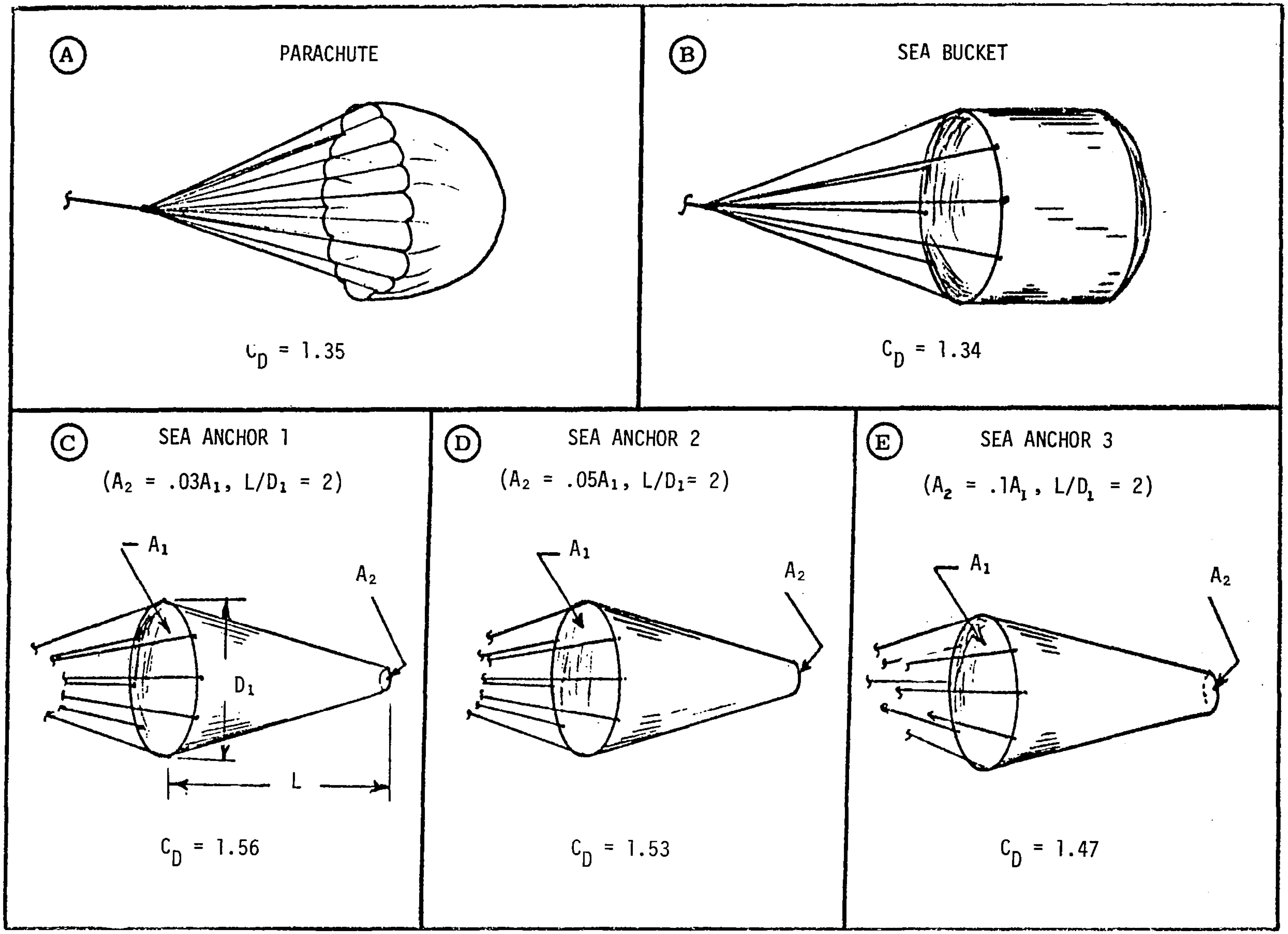


Figure 3 MODEL SEA DROGUES

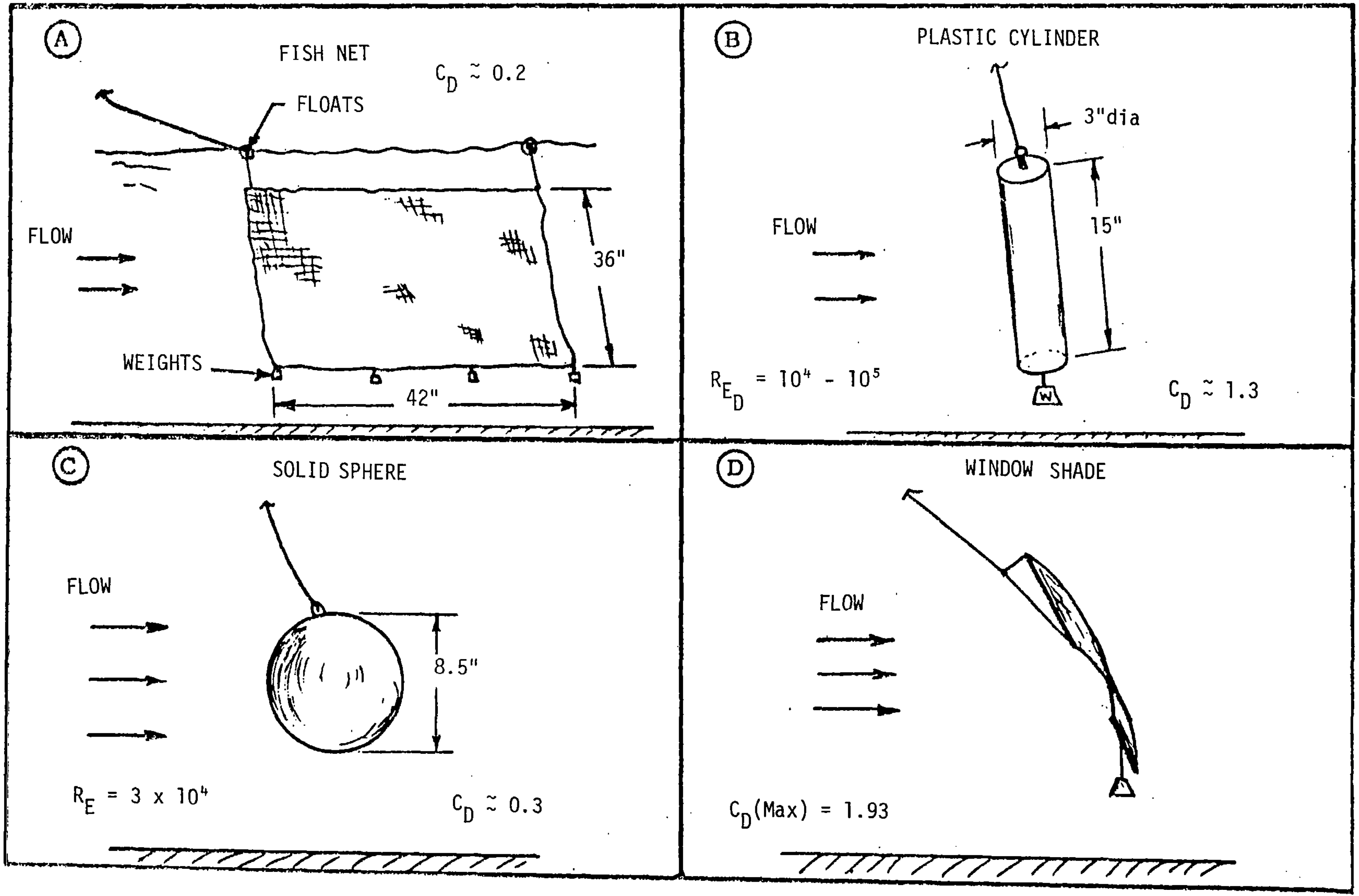


Figure 4 TEST MODEL DROGUES

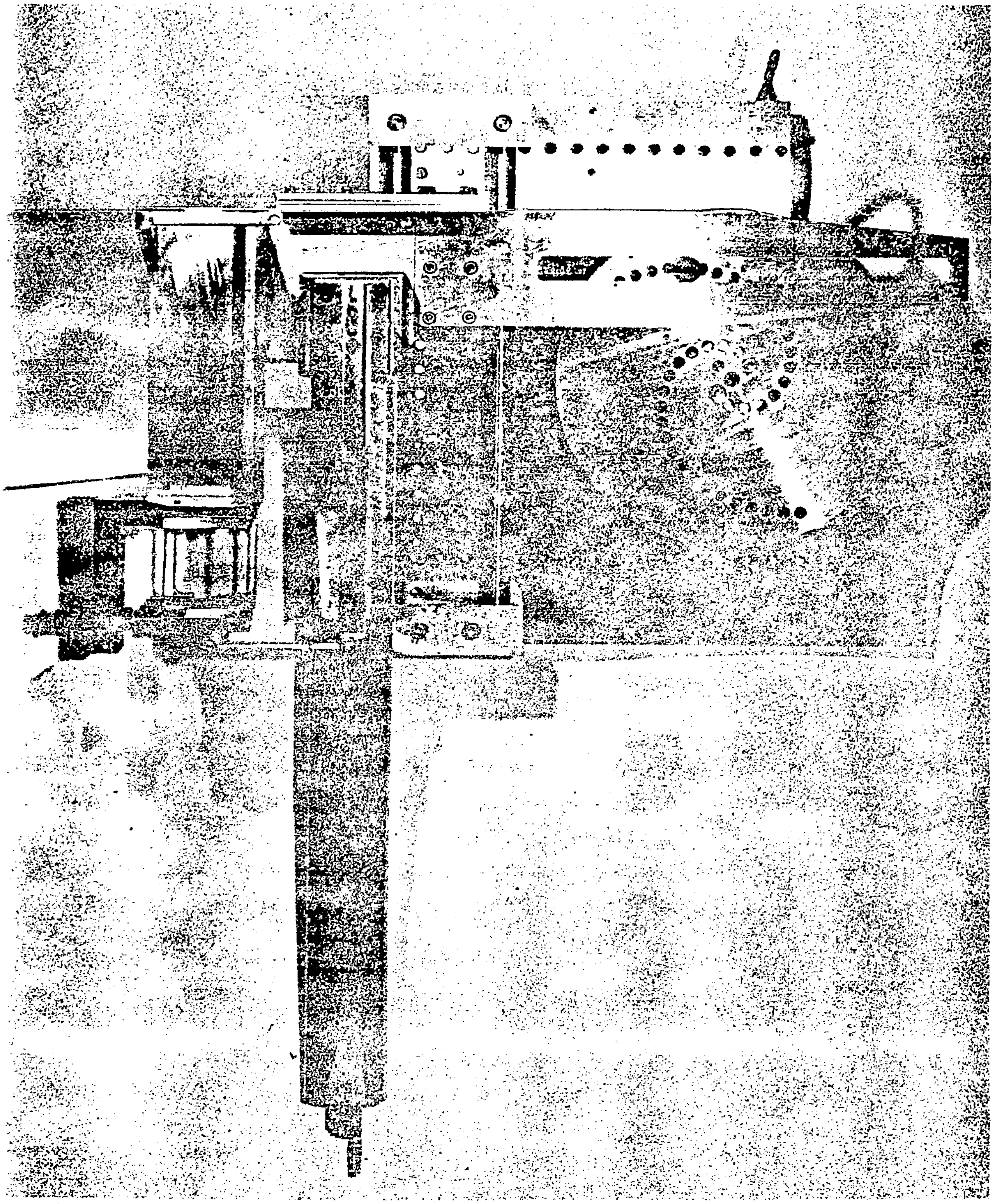


Figure 5

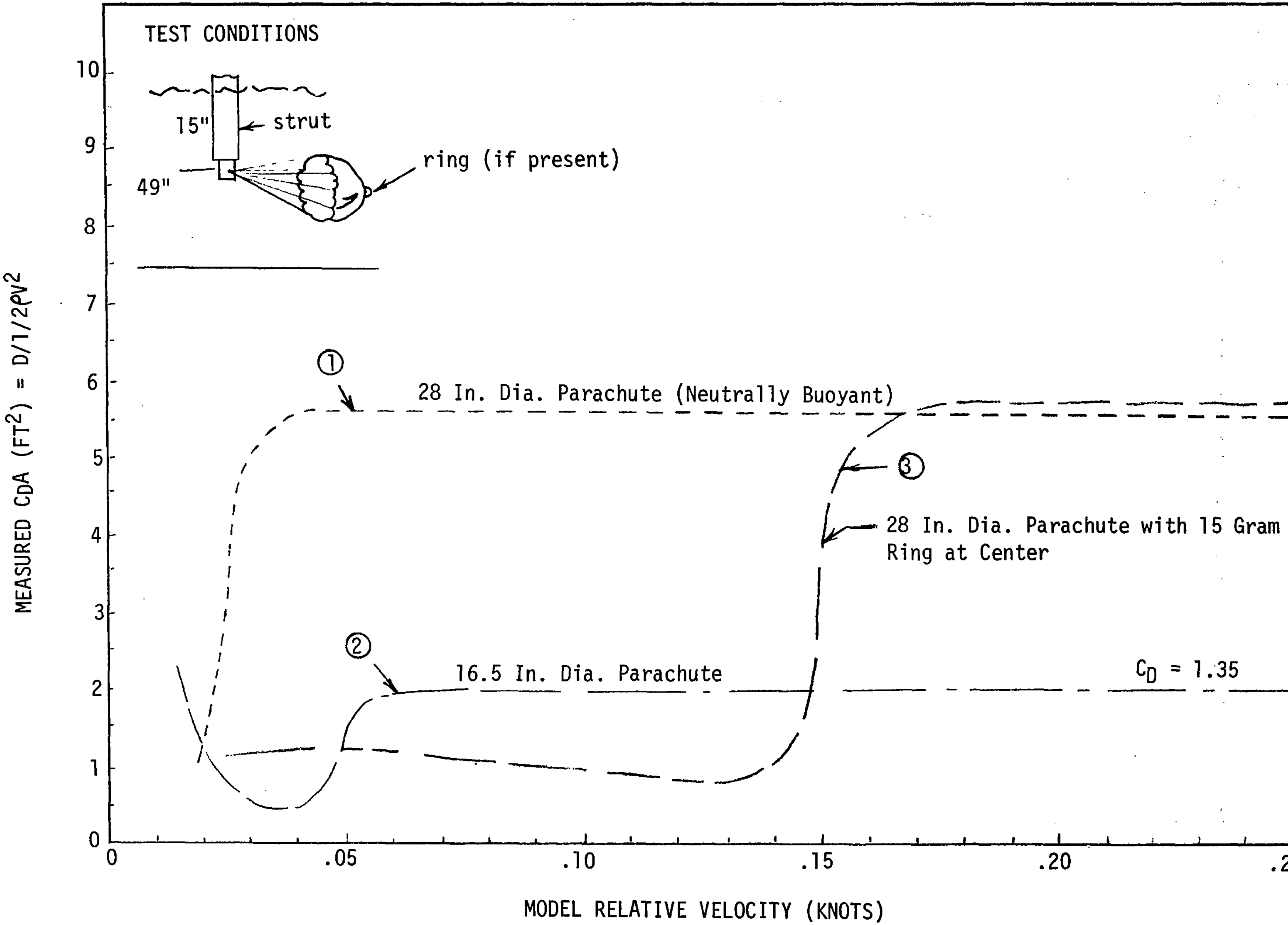


Figure 6 VELOCITY vs. DRAG

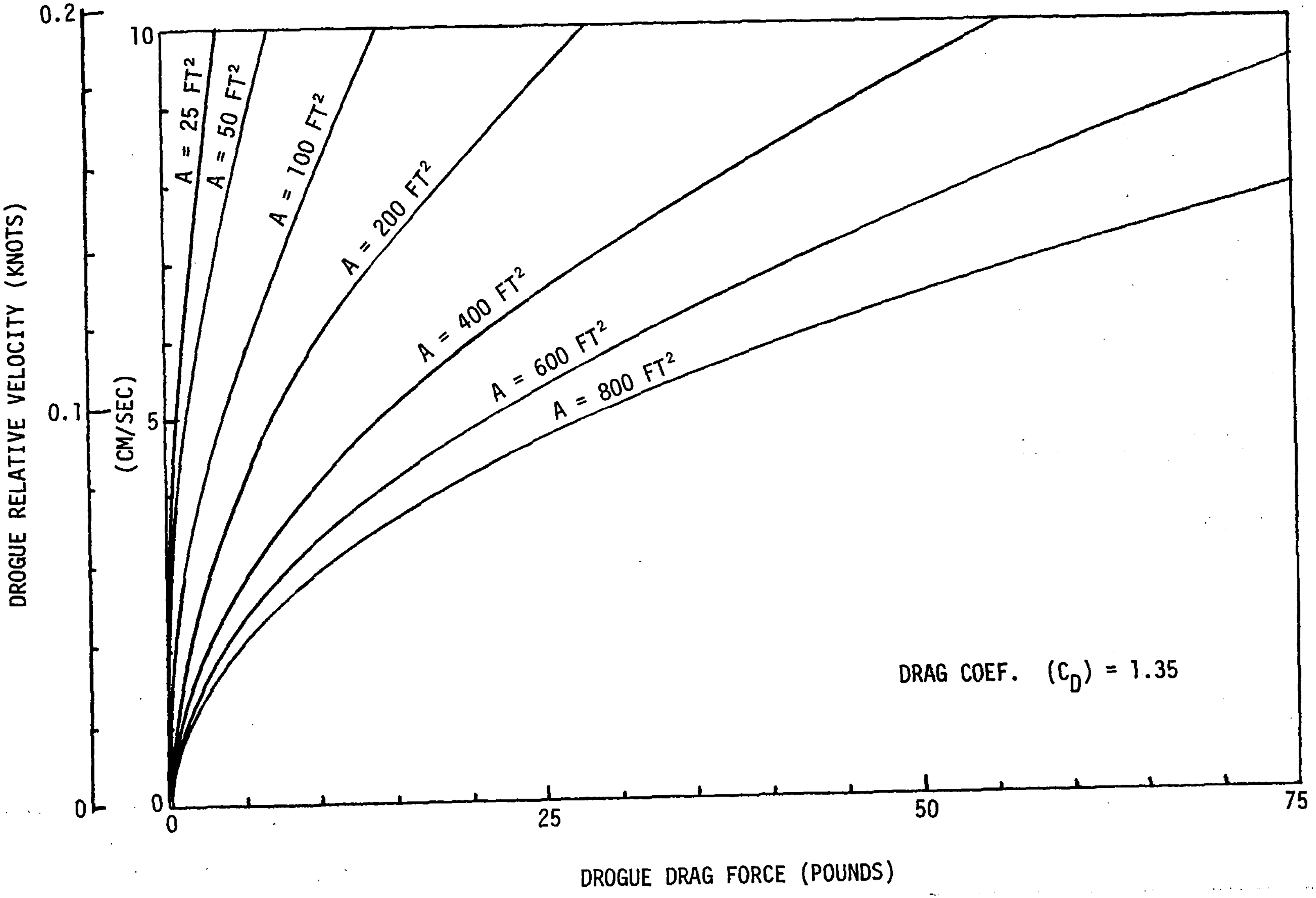


Figure 7 PARACHUTE DROGUE PERFORMANCE

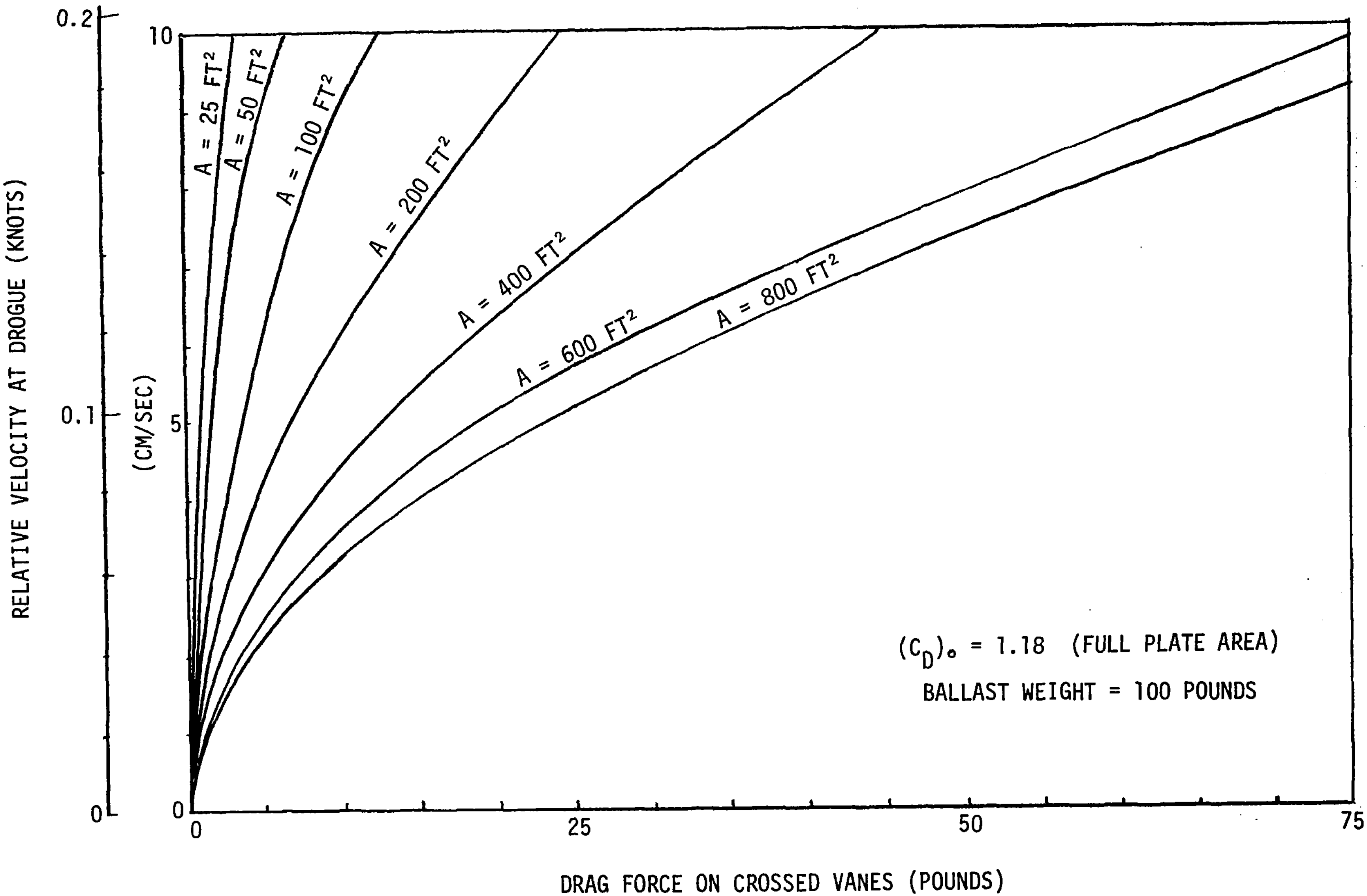


Figure 8 TWO-AXIS CROSSED VANE PERFORMANCE

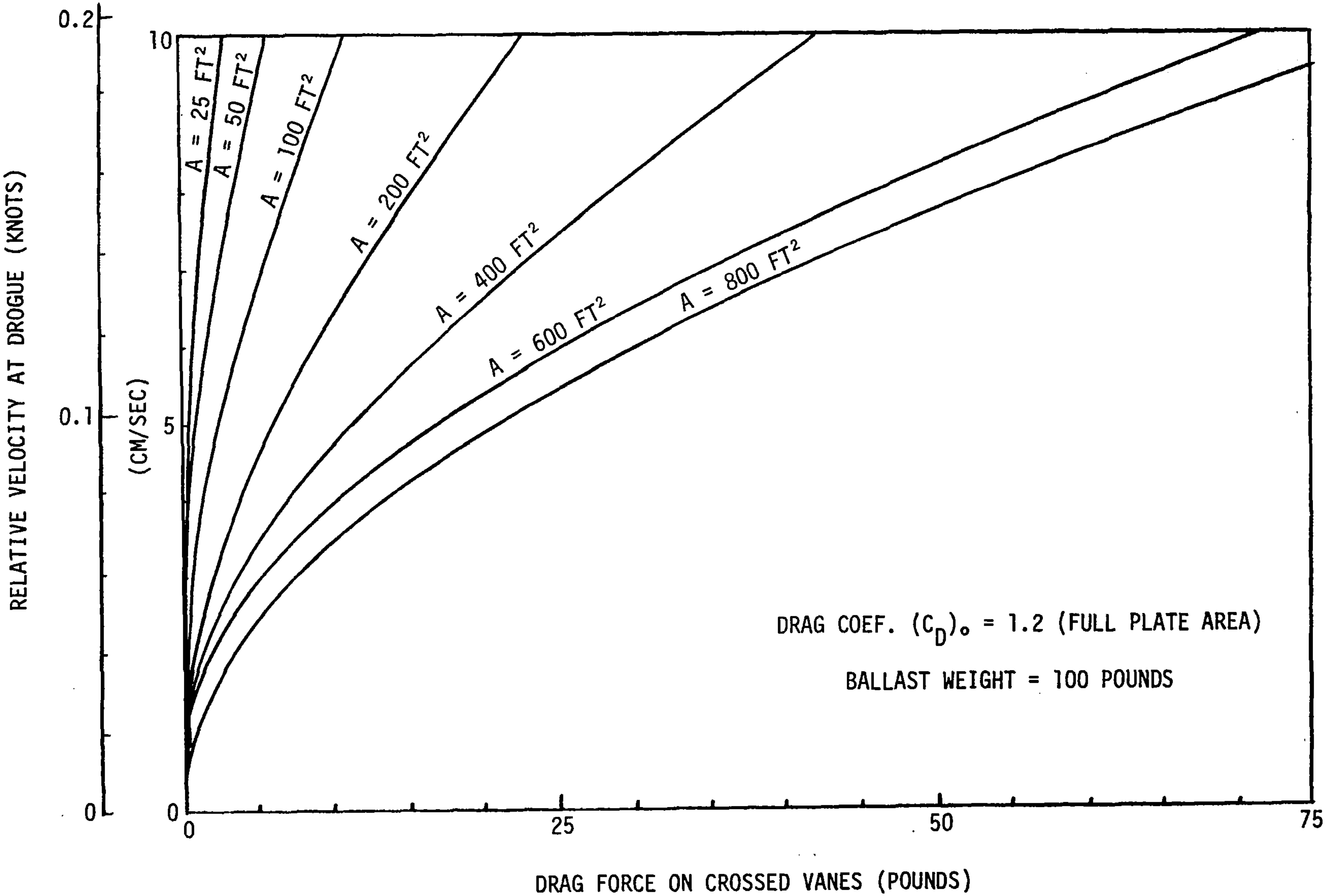


Figure 9 THREE-AXIS CROSSED VANE PERFORMANCE

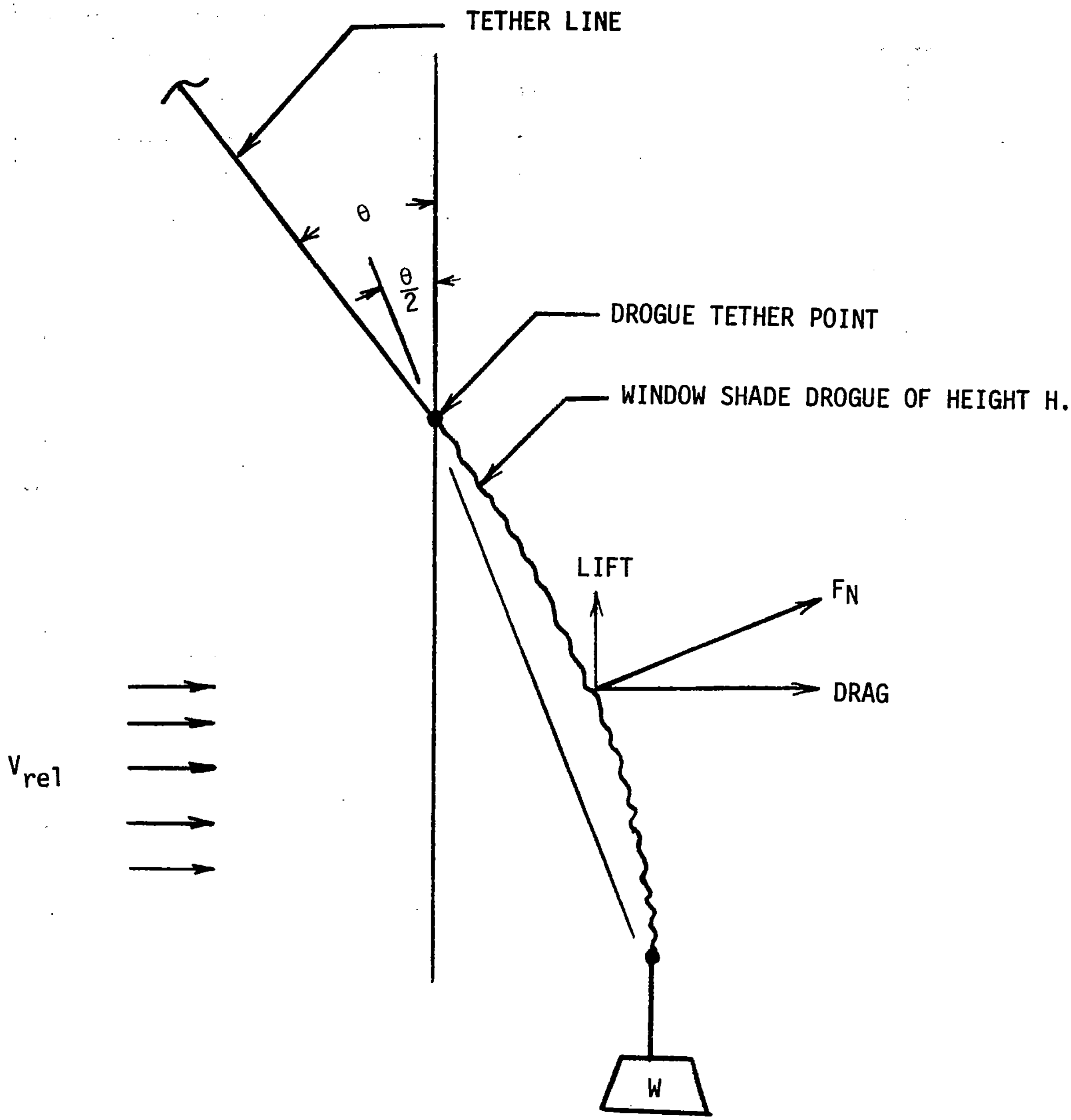


Figure 10 HYDRODYNAMIC FORCES ON WINDOW SHADE DROGUE

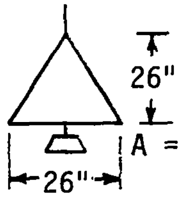
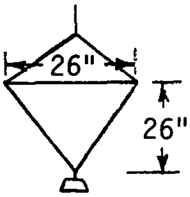
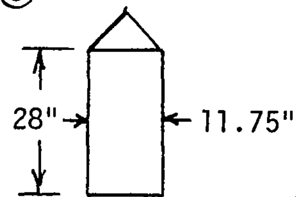
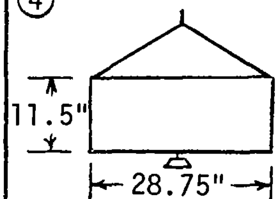
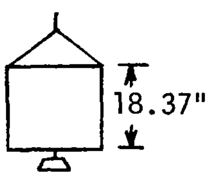
SHAPE DESCRIPTION	FROUDE SCALING RESULTS		REYNOLDS SCALING RESULTS	
	SPEED	C_D	SPEED	C_D
① TRIANGLE  A = 2.35 Ft2	0.039 KN.	1.53	0.31 KN.	1.63
	0.099 KN.	1.22	0.61 KN.	1.36
	0.2 KN.	1.73	0.813 KN.	1.37
② TRIANGLE  A = 2.35 Ft2	0.039 KN.	1.95	——	——
	0.099 KN.	1.72	0.31 KN.	1.46
	0.2 KN.	1.53	0.407 KN.	1.45
③ RECTANGLE  A = 2.29 Ft2	0.039 KN.	1.54	0.31 KN.	1.81
	0.099 KN.	2.13	0.61 KN.	2.03
	0.2 KN.	1.77	0.813 KN.	1.85
④ RECTANGLE  A = 2.30 Ft2	0.039 KN.	2.29	0.31 KN.	1.0
	0.099 KN.	1.77	0.61 KN.	1.92
	0.2 KN.	1.56	0.813 KN.	1.89
⑤ SQUARE  A = 2.35 Ft2	0.039 KN.	1.97	0.61 KN.	1.85
	0.099 KN.	1.83	0.813 KN.	1.89
	0.2 KN.	1.63	1.0 KN.	1.75

Figure 11 WINDOW SHADE DROGUE TEST SUMMARY

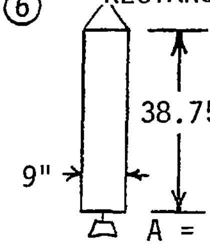
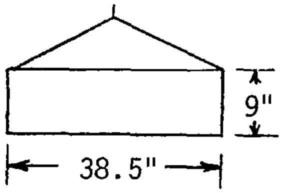
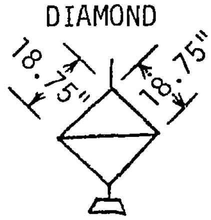
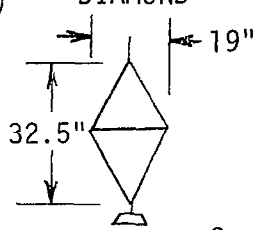
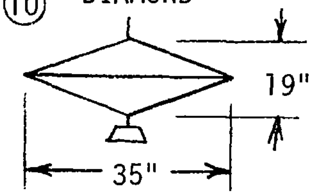
SHAPE DESCRIPTION	FROUDE SCALING RESULTS		REYNOLDS SCALING RESULTS	
	SPEED	C_D	SPEED	C_D
⑥ RECTANGLE  A = 2.42 Ft^2	.039 KN.	1.92	—	—
	.099 KN.	1.88	—	—
	0.2 KN.	1.53	—	—
⑦ RECTANGLE  A = 2.41 Ft^2	.039 KN.	2.03	—	—
	.099 KN.	2.16	—	—
	0.2 KN.	1.44	—	—
⑧ DIAMOND  A = 2.35 Ft^2	0.02 KN.	1.72	0.31 KN.	1.83
	0.099 KN.	1.58	0.61 KN.	1.87
	0.2 KN.	1.53	0.813 KN.	1.92
⑨ DIAMOND  A = 2.34 Ft^2	.039 KN.	1.40	—	—
	.099 KN.	2.30	—	—
	0.2 KN.	1.76	—	—
⑩ DIAMOND  A = 2.31 Ft^2	.039 KN.	1.92	—	—
	.099 KN.	1.84	—	—
	0.2 KN.	1.68	—	—

Figure 12 WINDOW SHADE DROGUE TEST SUMMARY

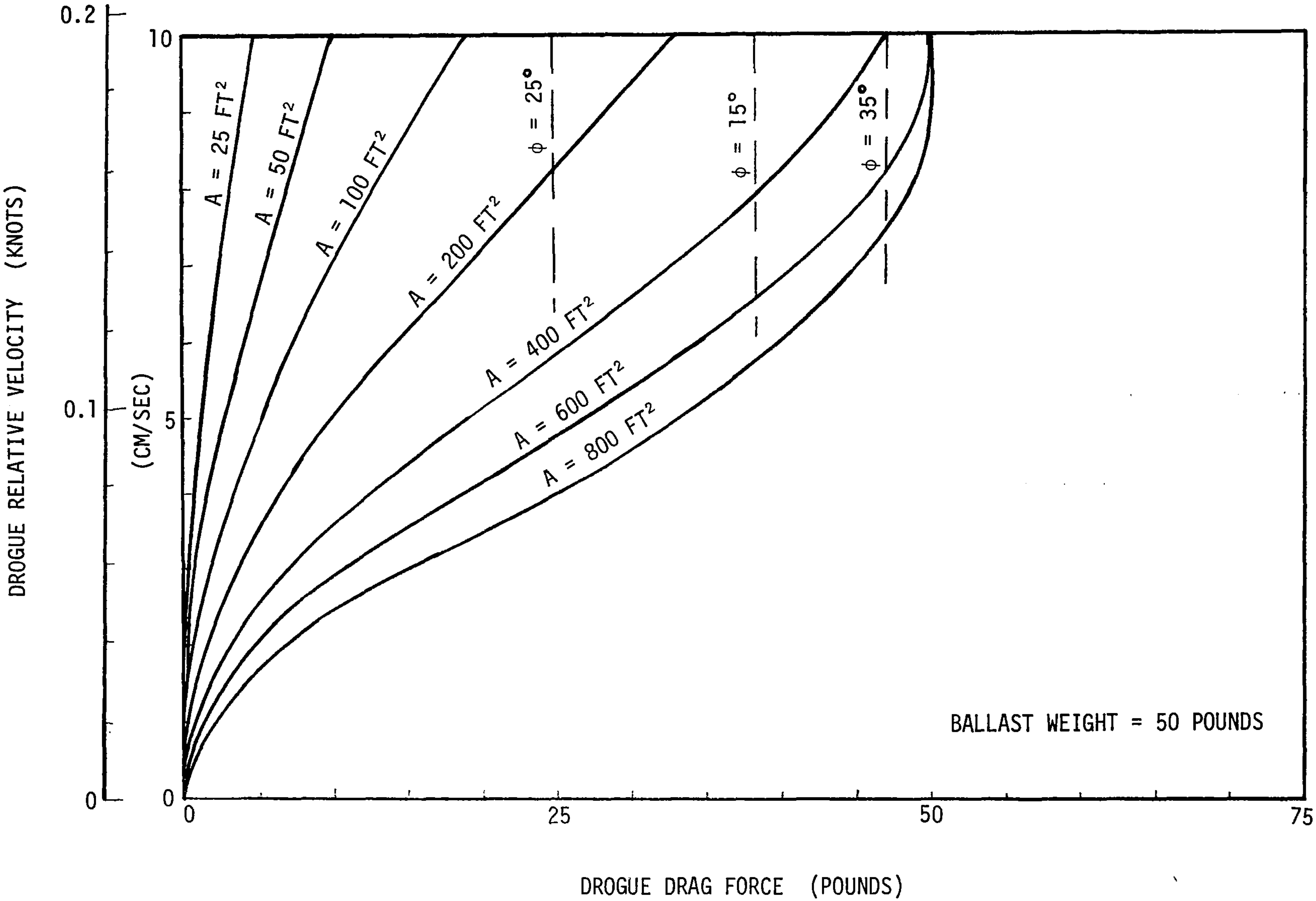


Figure 13 WINDOW SHADE DROGUE PERFORMANCE

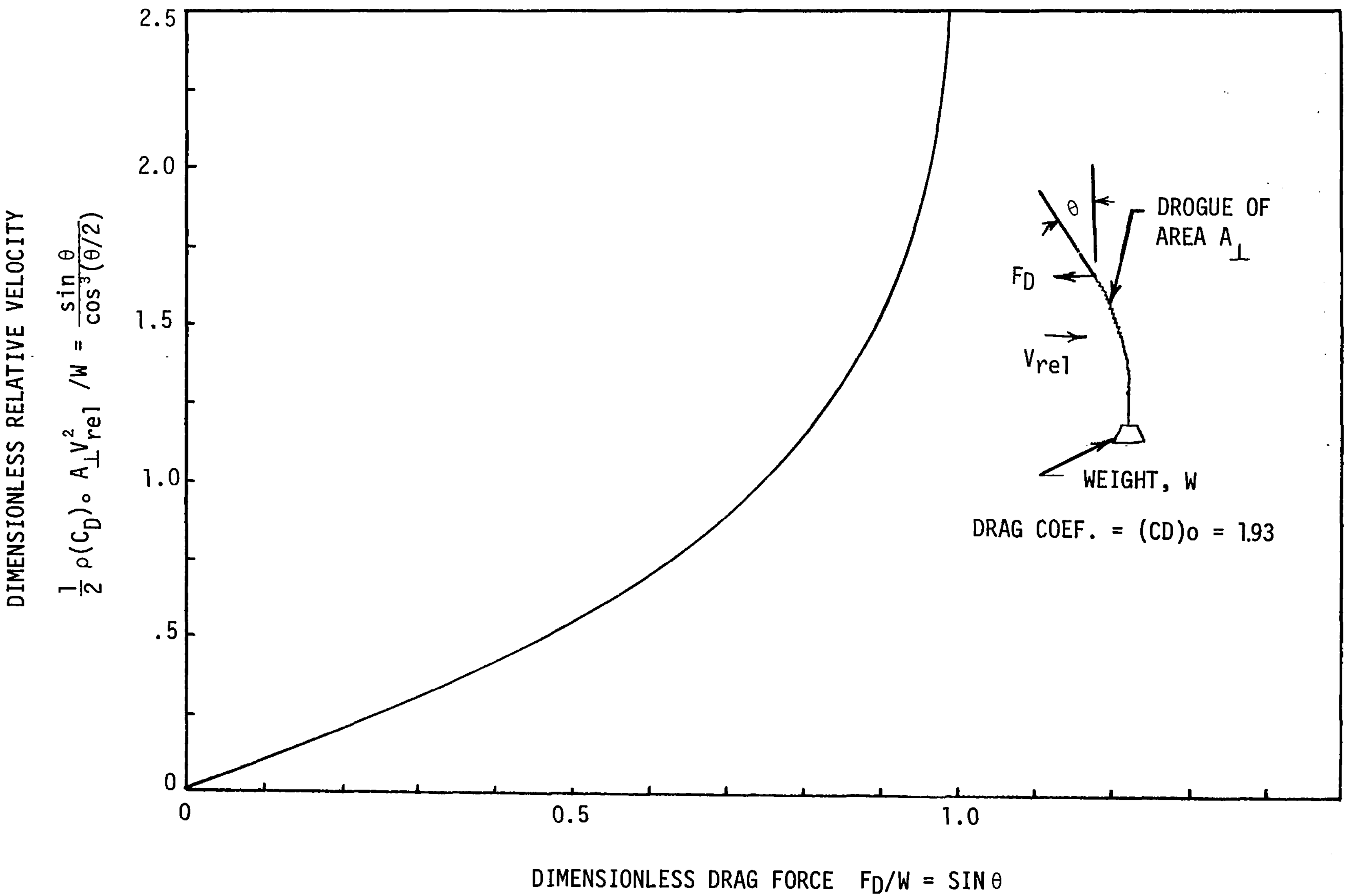


Figure 14 DIMENSIONLESS WINDOW SHADE DROGUE PERFORMANCE (THEORETICAL)

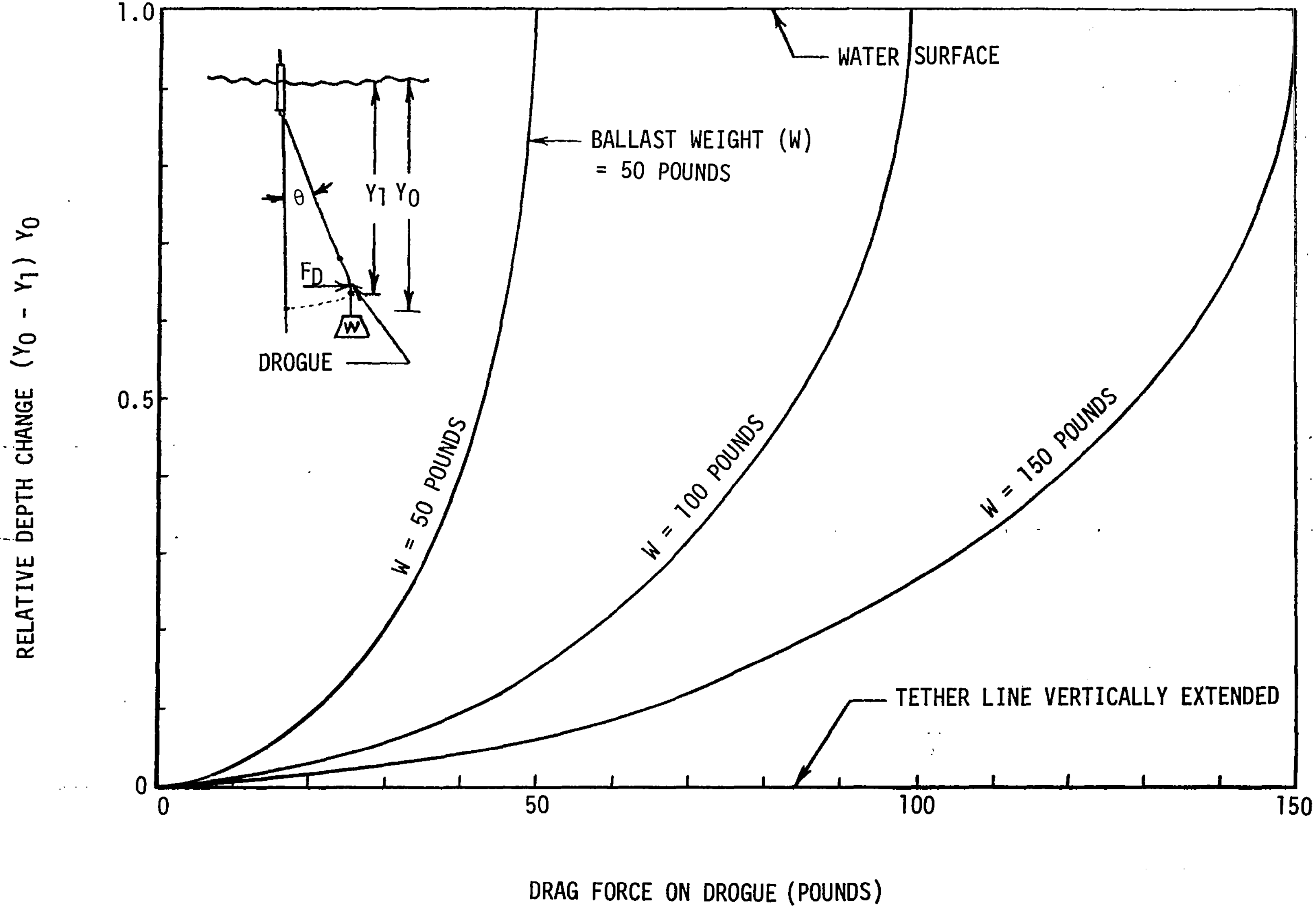


Figure 15 DROGUE DEPTH ACCURACY

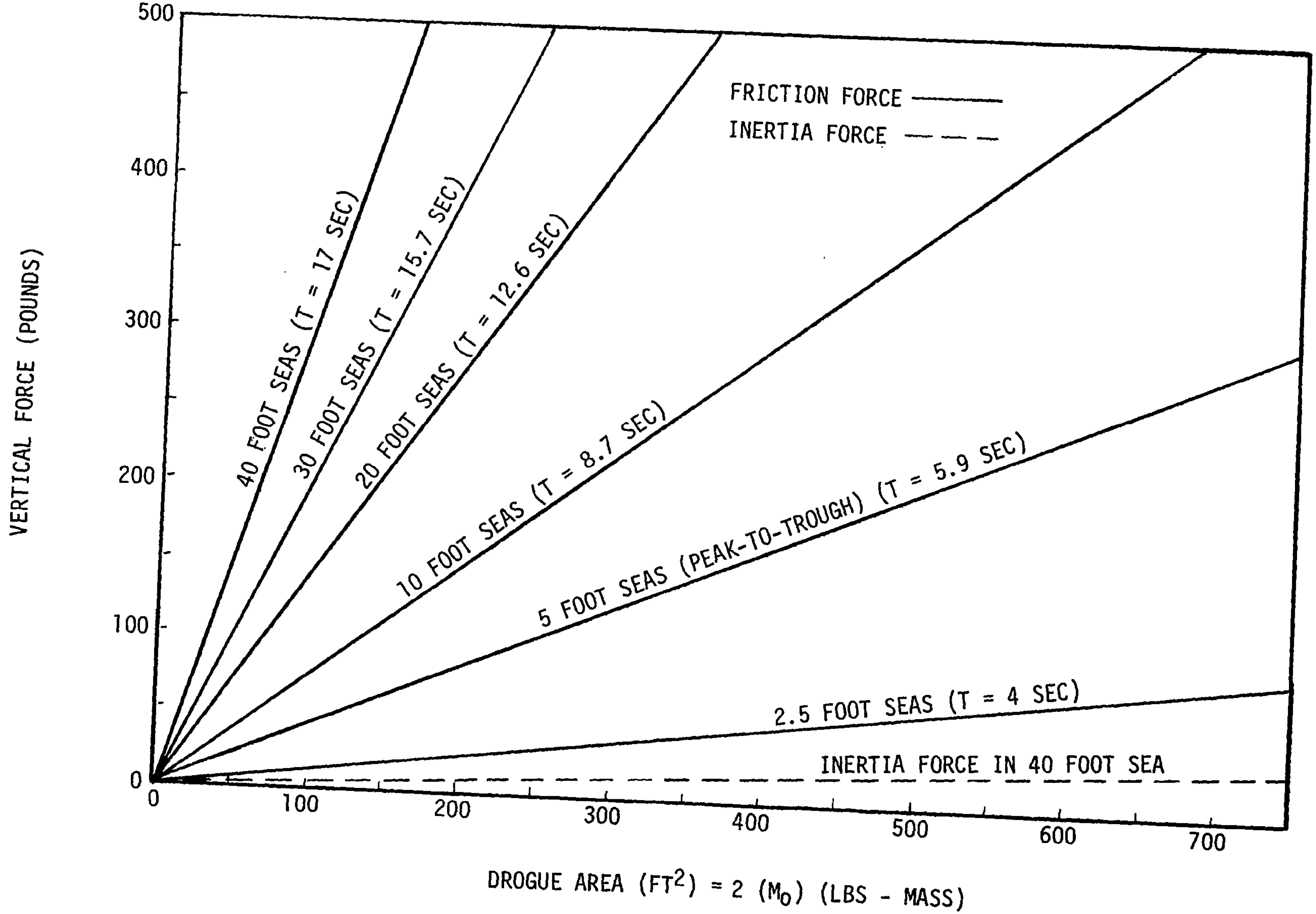


Figure 16 DYNAMIC FORCES FROM NEAR-SURFACE WINDOW SHADE DROGUES ON NON-COMPLAINT BUOYS (THEORETICAL CURVES)

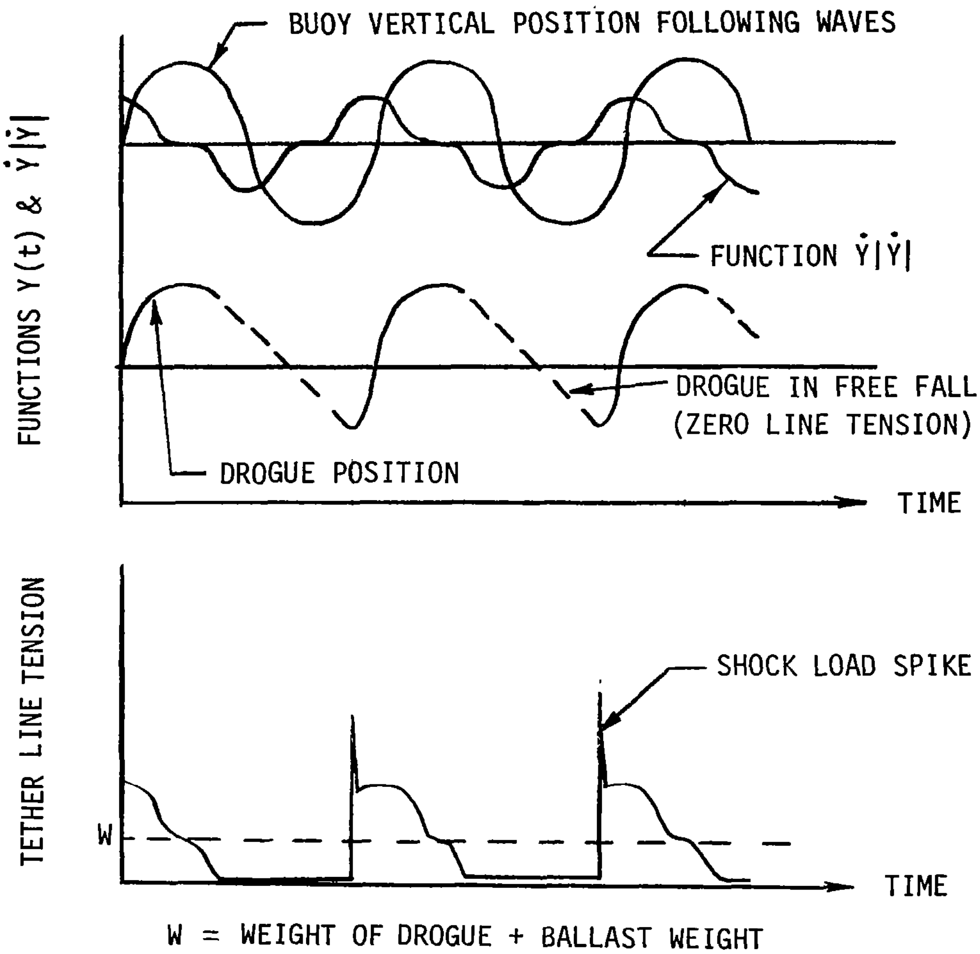


Figure 17 DROGUE DYNAMIC LOADS

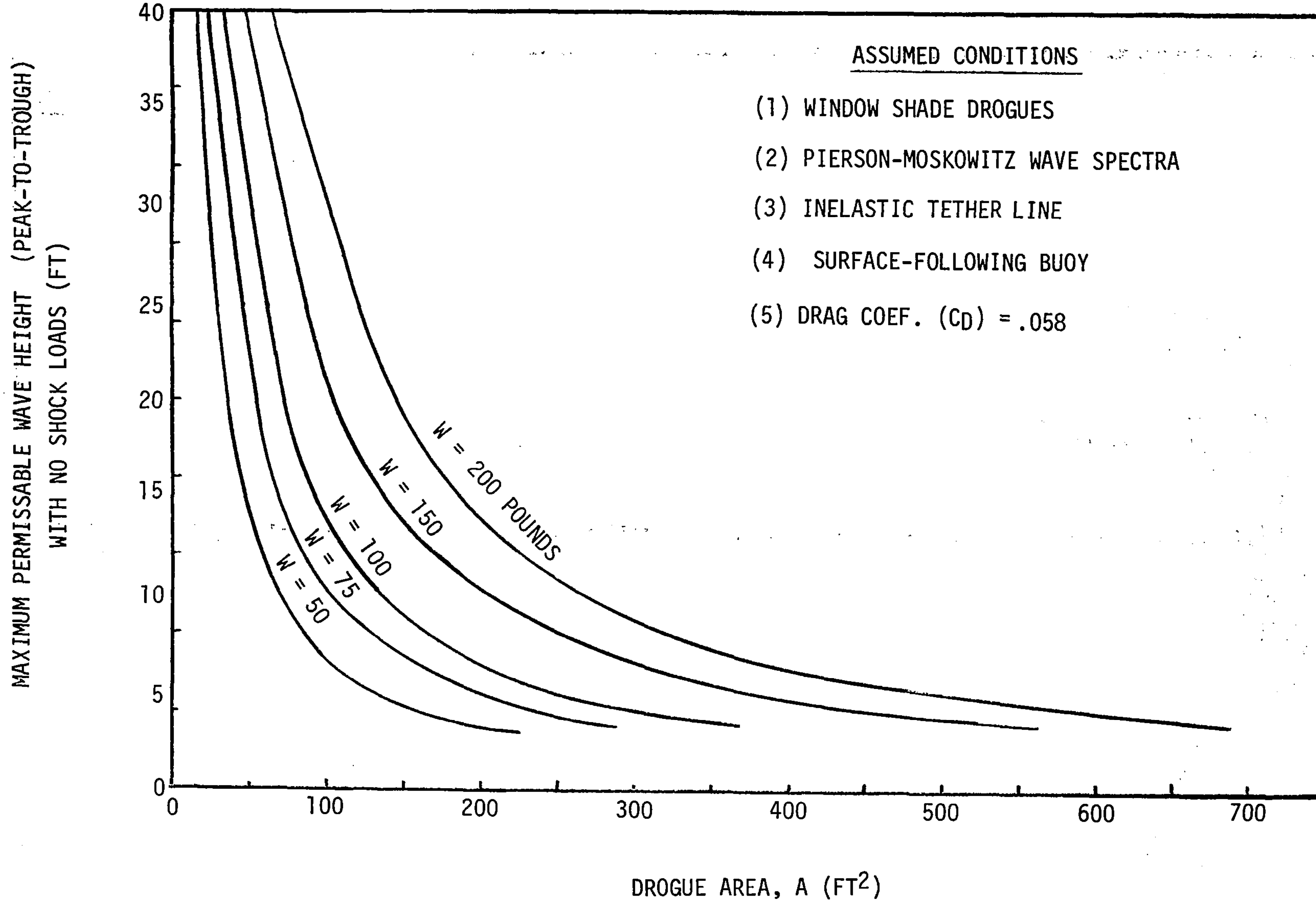


Figure 18 WINDOW SHADE DROGUE DYNAMIC SHOCK LOAD CONDITIONS

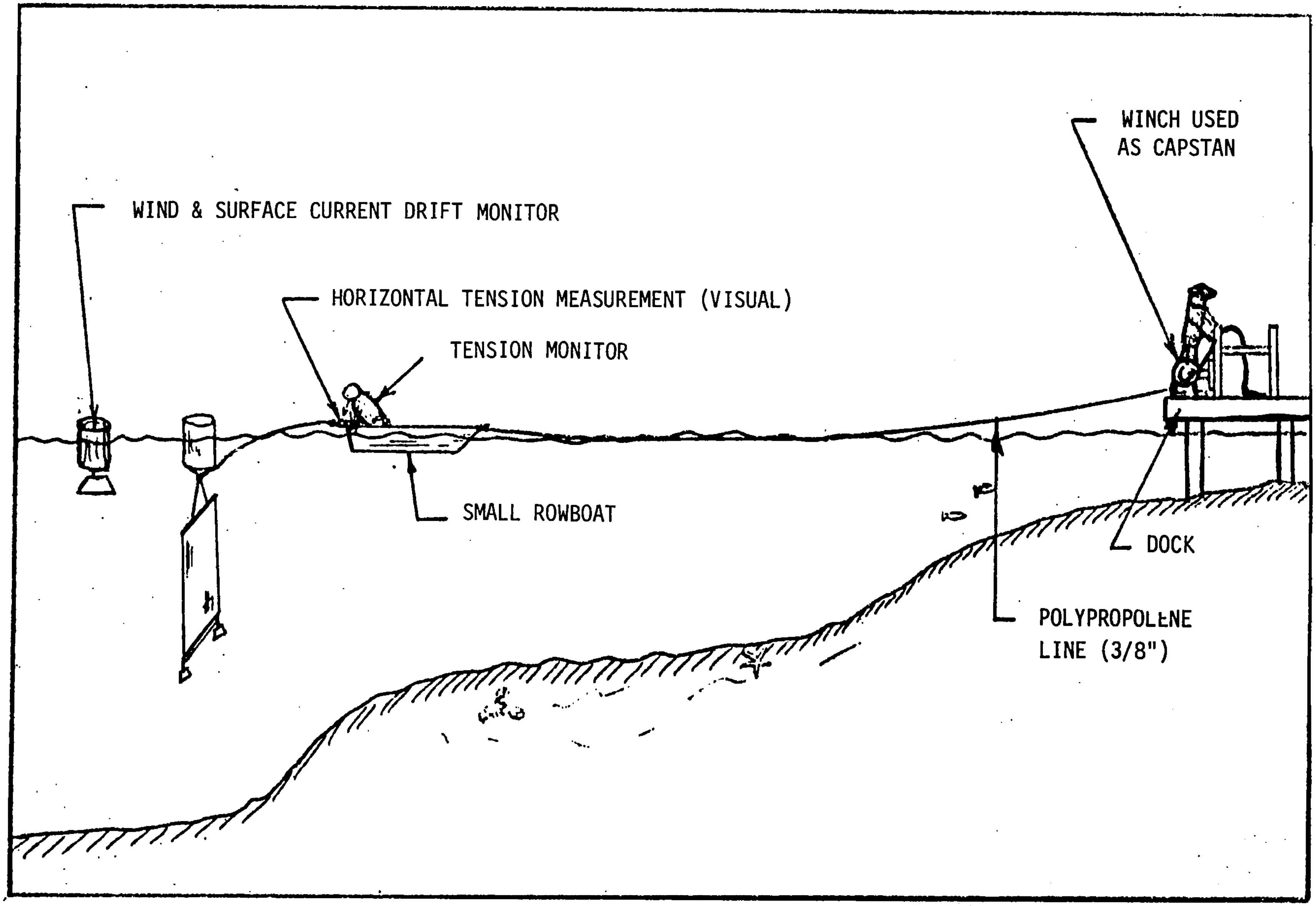


Figure 19 SHALLOW WATER DROGUE TEST CONFIGURATION

SHALLOW WATER WINDOW SHADE DROGUE
PHASE 2 TOW TEST RESULTS

MANSHIPS QUARRY
GLOUCESTER, MASS.
MAY 13-15, 1974

AVE. VELOCITY		WIDTH REYNOLDS NO.	AVE. $(F_D)^2$ (POUNDS)	AVE. DRAG COEF., $(C_D)_i$	$\theta = \sin^{-1}(F_D/W)$	EQUIV. $(C_D)_o$	COMMENTS AND OBSERVATIONS (*)
KNOTS	FT/SEC	(VW/ν)					
.085	.143	1.29×10^5	16.8	2.73	12.7°	2.78	Total drogue st.=82 lbs. in water (76 lbs at bot) 355 - second run
.097	.164	1.48×10^5	20.2	2.52	15.3°	2.59	290 - second run
.099	.167	1.5×10^5	22.4	2.63	17.1°	2.72	235 - second run
.099	.167	1.5×10^5	23.4	2.81	17.8°	2.91	220 - second run
.134	.226	2.03×10^5	33.8	2.20	26.3°	2.38	80 - second run
.154	.261	2.35×10^5	44.5	2.15	35.6°	2.49	95 - second run
.187	.316	2.84×10^5	57.6	1.92	49.0°	2.55	225 - second run
.189	.32	2.88×10^5	59.8	1.94	51.5°	2.66	50 - second run

(*) DROGUE DIMENSIONS: 25.8' ℓ. x 12.0' w., Herculite Marine DR material.

Figure 20 SHALLOW WATER WINDOW SHADE DROGUE TOW TEST RESULTS - PHASE 2

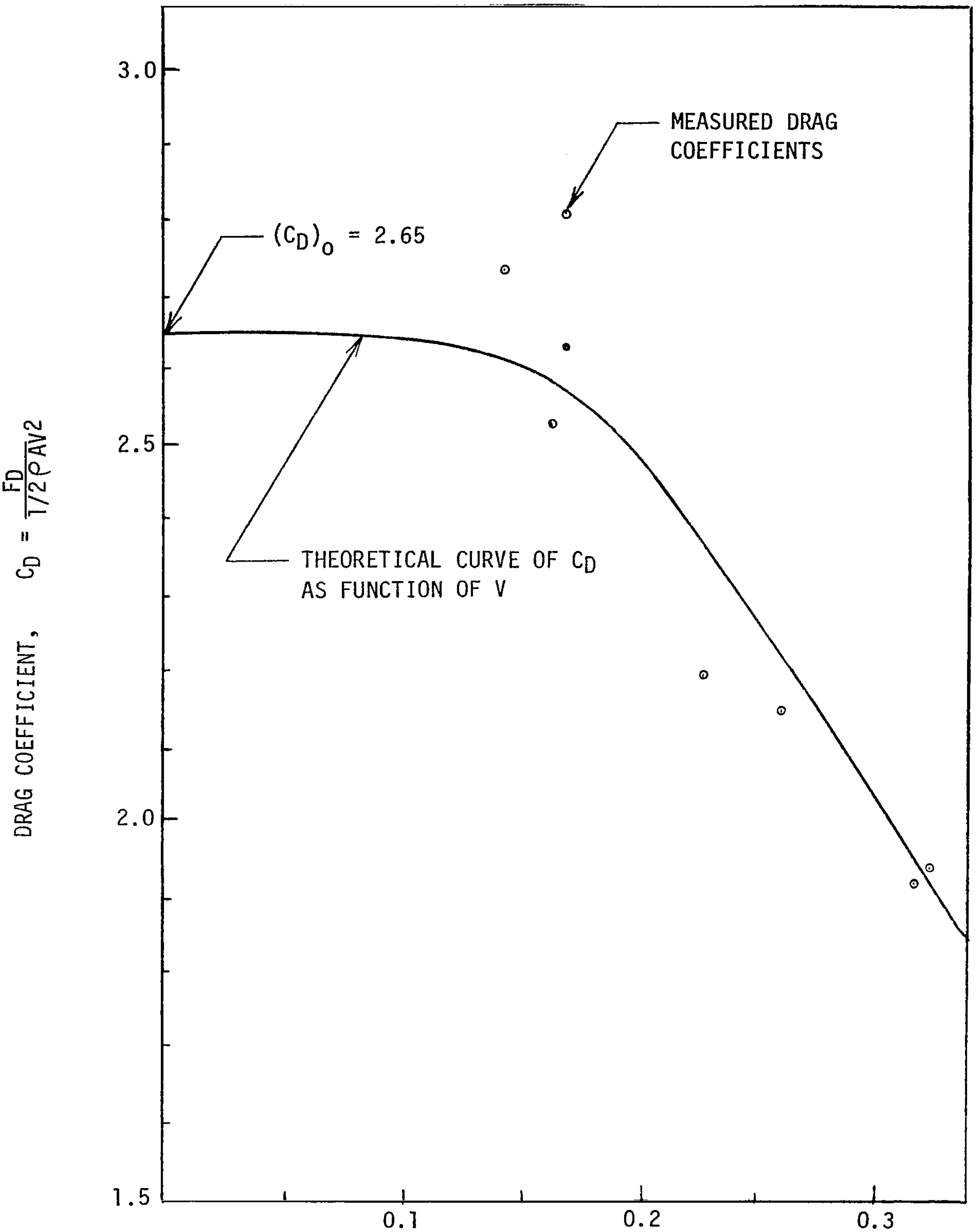


Figure 21 WINDOW SHADE DROGUE, FULL SCALE TEST RESULTS FROM QUARRY TESTS (PRELIMINARY)

DROGUE TEST SUMMARY

BEST TYPE(S) OF DROGUES

1. "WINDOW SHADE"

PROS:

(a) Simple

(b) Inexpensive

(c) Easily Stored & Deployed

(d) Very High Drag Coefficient

CONS:

(a) Question Angular Response in Low Currents

(b) Question Dynamic Behavior as it Effects Survivability of Drogue & Buoy

(c) Weight Balance Critical

2. 3-AXIS CROSSED VANE

PROS:

(a) C_D Relatively Independent of Angle

(b) Performance Simpler to Understand

CONS:

(a) More Complicated to Build, Store and Maybe Deploy

(b) Moderate Drag Coefficient

(c) Question Dynamic Behavior

Figure 22

OCEAN CIRCULATION COMPUTER MODELING

by
Tom Rees
Langley Research Center

The Langley Research Center (LaRC) has been named the lead center within the National Aeronautics and Space Administraiton (NASA) for environmental monitoring with respect to pollution, both in the air and the water. We feel that numerical models are necessary to enhance ocean monitoring, and to provide capabilities for predicting pollution dispersal.

The objectives of Langley's circulation and pollution dispersal modeling efforts are therefore two-fold. Firstly, the modeling is intended to provide inputs to sensor definition studies and monitoring strategies by helping to decide; 1) what data are needed, 2) how accurate the data must be, and 3) how the data can be utilized. Secondly, the modeling will provide analytical techniques to be combined with data gathering systems for prediction of pollution transport and dispersal.

The Author: Mr. Rees received his Bachelors Degree in Aerospace Engineering from North Carolina State in 1973. He worked at the NASA Langley Research Center from 1969 as a cooperative education student until 1973 when he became a permanent employee. He has been associated with the environmental modeling group for the past 3 years.

Circulation modeling at Langley began about four years ago with modification of a mathematical formulation of a global atmospheric circulation model developed at the National Center for Atmospheric Research. The math model was reformulated in tensor notation and generalized to a form suitable for simulation of atmospheric or oceanic circulation on various scales. A computer program was then set up to solve the dynamic equations for the global atmosphere by finite differences on a 10°-grid. (Several finer scale atmospheric models have subsequently been developed.) The 10° global atmospheric model was modified to simulate the global ocean to the same resolution. From the global ocean model have evolved the two models which are the subject of this presentation--a limited region model, presently set up to simulate the flow of the East Coast, and a non-global validation model, presently set up to model the North Atlantic.

Figure 1 shows the relative scales and regions of the two models. The North Atlantic model covers the region on the left--from just south of the equator to about 80° North - with a 2 1/2° longitude-latitude grid. The Continental Shelf model encompasses a 10° by 10° area on the Mid-Atlantic Coast from the latitude of Jacksonville, FL. to that of Philadelphia. The grid spacing is 5/9°.

The LaRC general ocean circulation formulation is a primitive-variable model based upon conservation of mass, momentum, salinity and internal energy. The fundamental assumptions are common to most large-scale ocean models. The ocean is assumed to be in hydrostatic equilibrium, and an empirical equation of state is used to obtain density from salinity temperature, and pressure. Turbulent transfer coefficients are assumed. The assumption is made that,to the resolution of the grids, the deviations of the upper and lower surfaces are small (in other words, that the earth is nearly sperical). Finally, the rigid-lid approximation is usually employed to filter external gravity waves; a significant time step increase results. The model has the ability to function without the rigid-lid approximation, however.

The basic equations are simply the two horizontal momentum equations, the continuity equation (from which, because of the hydrostatic approximation, vertical velocities can be computed), and conservation equations for internal energy and salinity.

The boundary conditions for the equations are required at the upper and lower surface of the ocean and at land mass boundaries. At the upper surface, the air-sea interaction terms, which must be specified or computed in conjunction with at atmospheric model, are wind stress, net mass transport due to evaporation minus precipitation, and heat transport. When the rigid-lid approximation is used, an additional boundary condition at the upper surface is that the vertical velocity is zero. At the bottom of the ocean, the topography is parameterized and the velocity normal to the bottom vanishes. At land mass boundaries, the geometry of the coastline is represented to the resolution of the grid, and the free slip condition (no transport normal to the beach) is imposed for a water column. The initial conditions are principally the velocity field, and the density, temperature,and salinity distributions.

Turning specifically to the North Atlantic model, figure 2 summarizes the initial validation case with which we are trying to check out the basic formulation and computer program. It is a simple case since it does not exercise most of the options in the model. The region is from approximately 80° north to 10° south, with a horizontal resolution of 2 1/2° by 2 1/2°. The case is restricted to upper 100 m with 5 layers in the vertical. (Obviously, the bottom topography is not modeled here.) The rigid lid approximation is applied, and the model open boundaries have been artifically closed to simplify the numerics. The wind stress is taken from the literature and held constant, but the other surface interaction terms are set to zero. Also, the ocean is assumed to be homogeneous in this case (density, temperature,and salinity gradients are not modeled.)

In summary, this case demonstrates the response of the model to constant wind stress acting upon the near-surface layers of the ocean.

Figure 3 illustrates the region of the model and the land mass geometry as the model sees it. The artificial boundaries at the northern and southern extremes of the region (shown by hased lines on the figure) were chosen so that the real-world currents at the boundaries were approximately zonal so that the mass transport normal to the boundaries were small. These artificial boundaries are treated in the model as if they were land boundaries.

Figure 4 shows the applied wind stress pattern. These data are interpolated averages for the summer months taken from estimates published by Dr. Hellerman of the Geophysical Fluid Dynamics Laboratory. The dominant feature of the flow is a large clockwise gyre made up of the westerlies and trades.

The model was started from rest and integrated on a computer for 15 days with the wind stress being the only driving force. Most of the major currents of the North Atlantic are fairly well depicted, as can be seen in figure 5. The flow pattern is for the most part, made up of three counter-rotating gyres. The plot is for the surface layer (0 - 20m). The other four layers (not shown) were very similar, except that in the bottom layer, a subsurface equatorial counter-current developed. The current patterns, again, agree qualitatively with observed currents. However, the magnitudes of the velocities are typically somewhat lower than those of the real world, and the gradients less sharp due to the averaging caused by the finite difference grid.

Now turning to the Continental Shelf model, figure 6 illustrates the structure of the limited region model. Again, five layers are used, but here the layer thicknesses change to accommodate depth variations. The 5/9° grid spacing implies 19 x 19 grid points in the 10° x 10° region.

The main purpose of this model is to provide the capability for pollution transport experiments. Also, we are developing a two-body three-degree-of-freedom computer program to predict trajectories of free-drifting buoys in conjunction with this model.

The heavy lines in figure 7 locate the region of the model.

Figure 8 is a vertically exaggerated three-dimensional projection of the depth field of the model. The data were taken from depth charts. The view is from the northeast (looking southwest). Note the sharp increase in depth in the southwest corner where the shelf drops off.

The model has been initialized with seasonally averaged data and propagated for 7 days on the computer. For this case, the initial temperature and salinity fields were held constant.

The open boundary treatment is the most difficult aspect of the model. Presently, artificial boundaries are treated as follows:

1) on a boundary, flow parallel to the boundary is integrated normally;
2) flow normal to an outflow boundary is extrapolated from local interior flow so that mass is conserved, and;
3) flow normal to an inflow boundary is maintained at the initial value to drive the model.

Figure 9 shows the initial flow field in the surface layer - monthly averages for May - taken from a U. S. Naval Oceanographic Offic Publication. Although the salinity and temperature distributions were taken from a similar publication, it should be noted that the initial salinity - temperature - velocity data are somewhat inconsistent. No attempt was made to balance these data *a priori*. In the initial flow field, the strong current entering at the lower left and flowing to the upper right, is, of course, the Gulf Stream. Slope water is entering at the top center and flowing southward. Note that velocity gradients are not expecially sharp due to the averaging.

Figure 10 shows the computer surface currents after 7 days. The velocities in the Gulf Stream have picked up considerably and the velocity gradients have increased in magnitude. These two features of the flow are realistic, representing an instantaneous condition as opposed to the averaged initial data in which the currents and gradients were smeared somewhat. However, the figure does not represent an equilibrium solution. Due to the inconsistencies in the initial conditions and the boundary conditions, the significance of the computed currents is difficult to determine.

Since the plot is for the surface layer only, the current flowing away from Cape Hatteras indicate strong upwelling there. The apparent discontinuity in the Gulf Stream (the circled area) is not completely understood at this time. However, vertically integrated plots show that there is no discontinuity in mass transport here, and it is thought that the phenomenon is connected with the sharp increase in depth indicated on figure 8.

We plan to continue validation experiments with the model - studying the response to various driving conditions - and to improve the treatment of artificial boundaries and the physics of the model. At some stage, pollution transport equations will be added to provide predictive capability. Plans are also forming to tie the work to a data gathering project such as MESA. And as I mentioned earlier, a two-body, three-degree-of-freedom buoy trajectory program is being developed which will enable us to take actual data and compare with computed data.

OCEAN MODELS

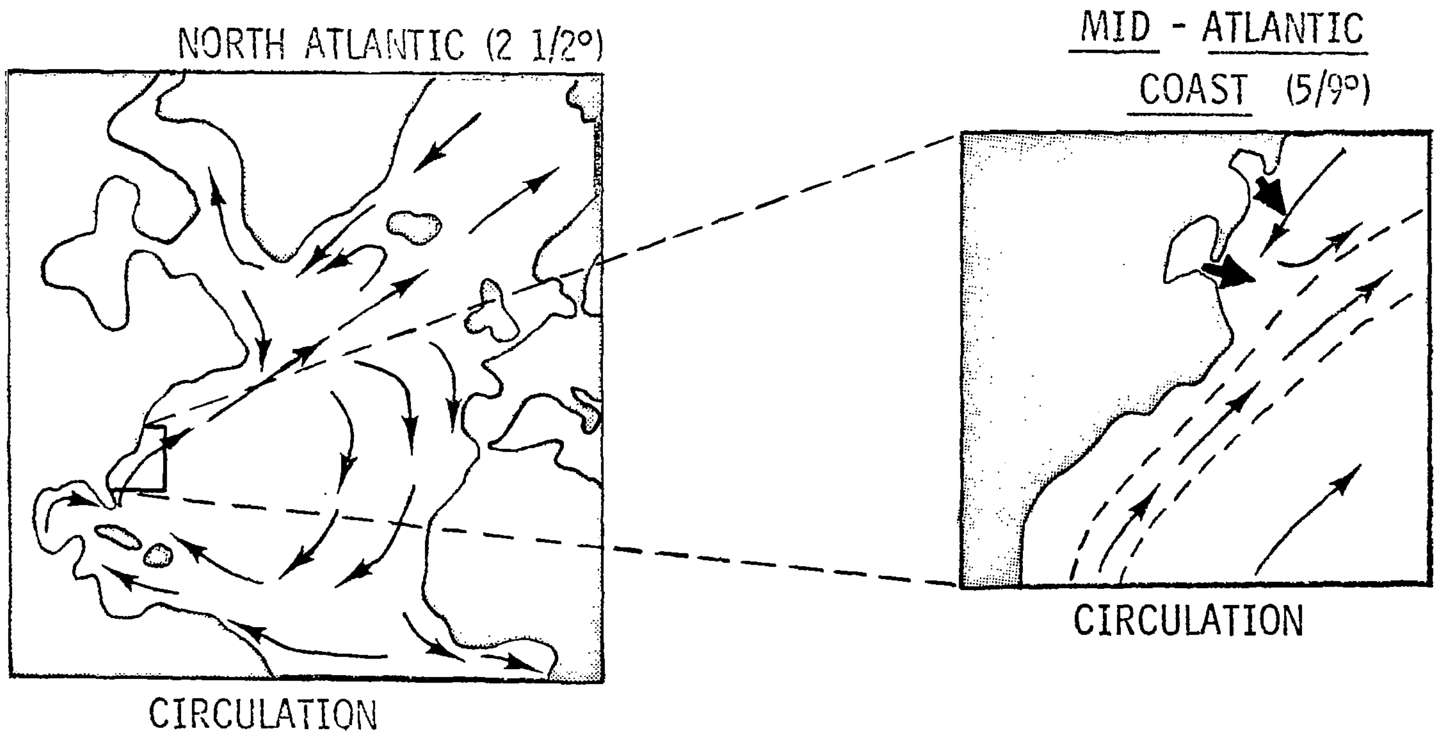


Figure 1. RELATIVE SCALES AND REGIONS OF THE TWO OCEAN MODELS

NORTH ATLANTIC OCEAN MODEL
(INITIAL VALIDATION CASE)

- REGION: 82.5°N - 10°S, 100°W - 20°E
- HORIZONTAL RESOLUTION - 2.5° x 2.5°
- SURFACE LAYER (100 M) UNIFORM DEPTH
- 5 VERTICAL LAYERS (20 M)
- RIGID LID
- CLOSED BOUNDARIES
- OBSERVED WIND STRESS
- OTHER SURFACE INTERACTIONS ZERO (E-P,Q)
- HOMOGENEOUS IN ρ, T, S

Figure 2.

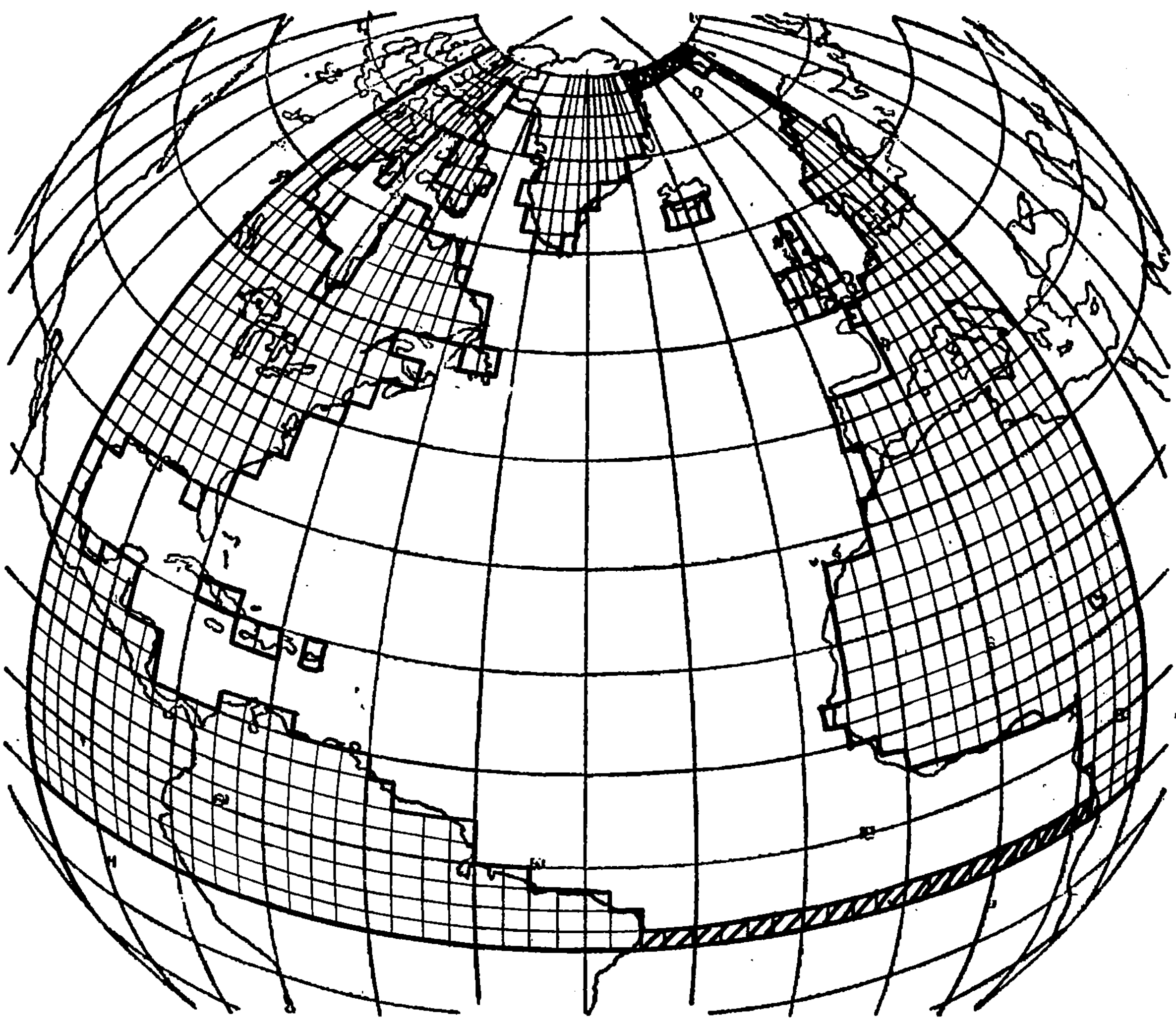


Figure 3. NORTH ATLANTIC MODEL REGION AND RESOLUTION

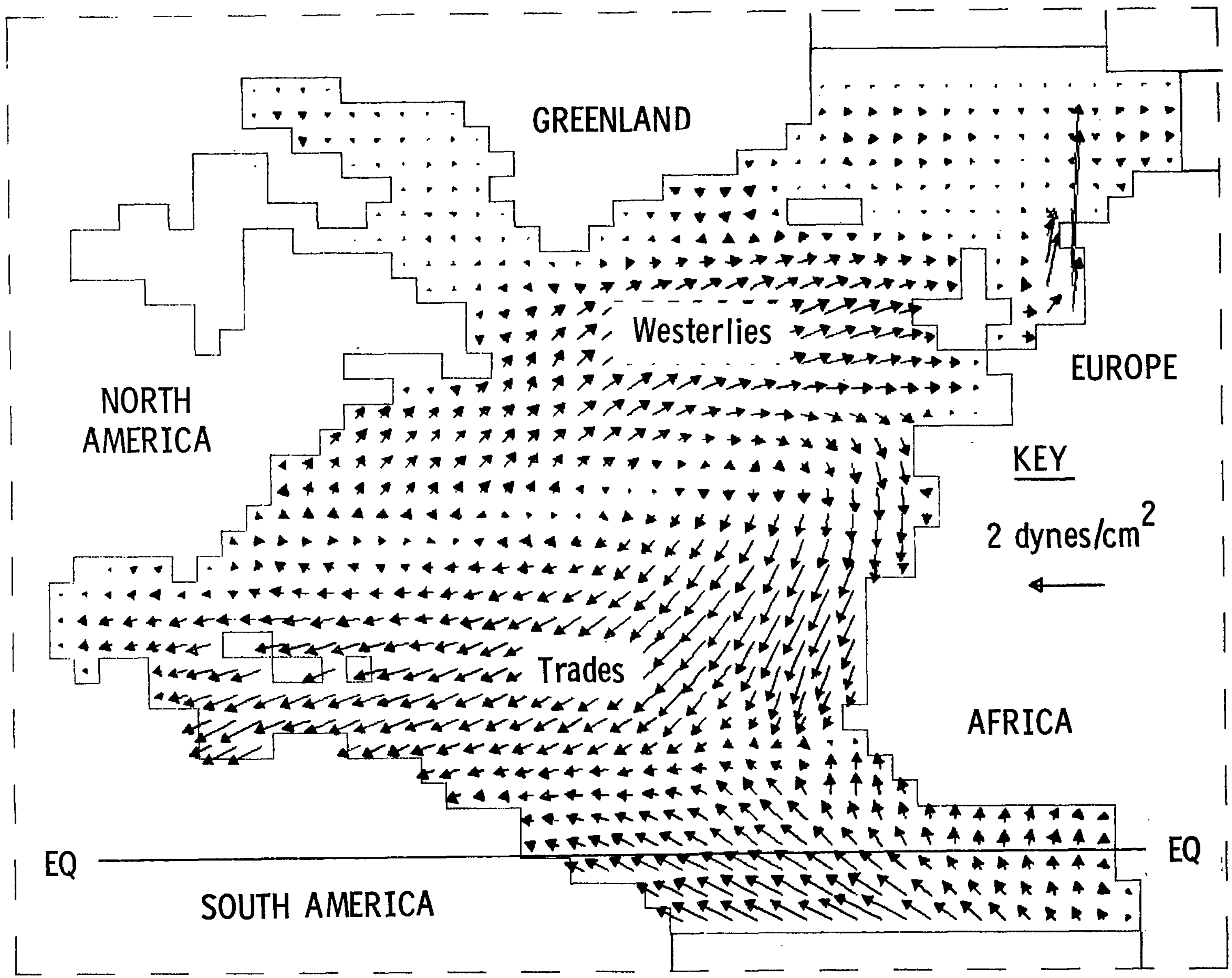


Figure 4. SEASONALLY AVERAGED (SUMMER) WIND STRESS

SURFACE LAYER (0 - 20 m)

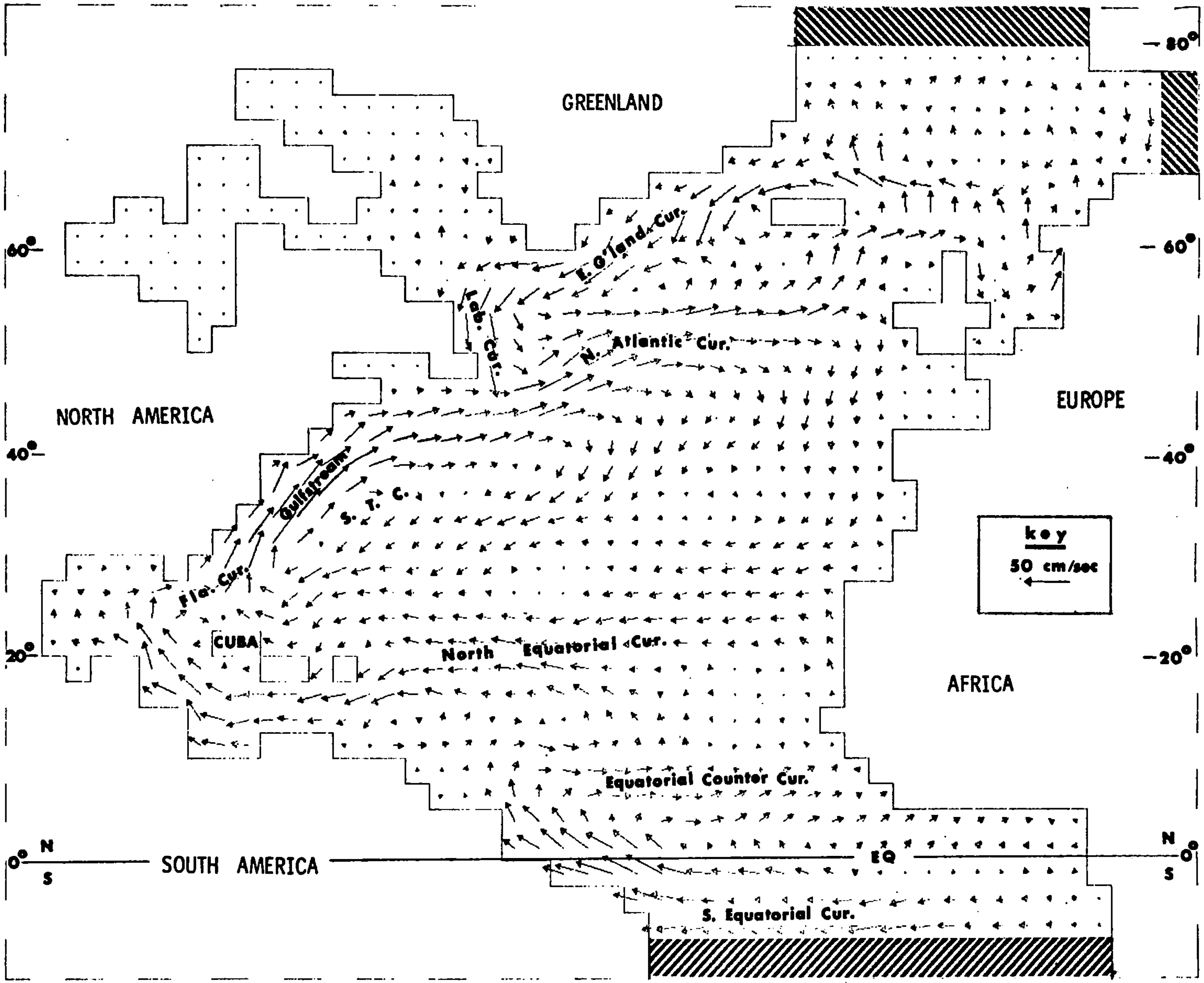


Figure 5. NEAR-EQUILIBRIUM COMPUTED CURRENTS (SMOOTHED)

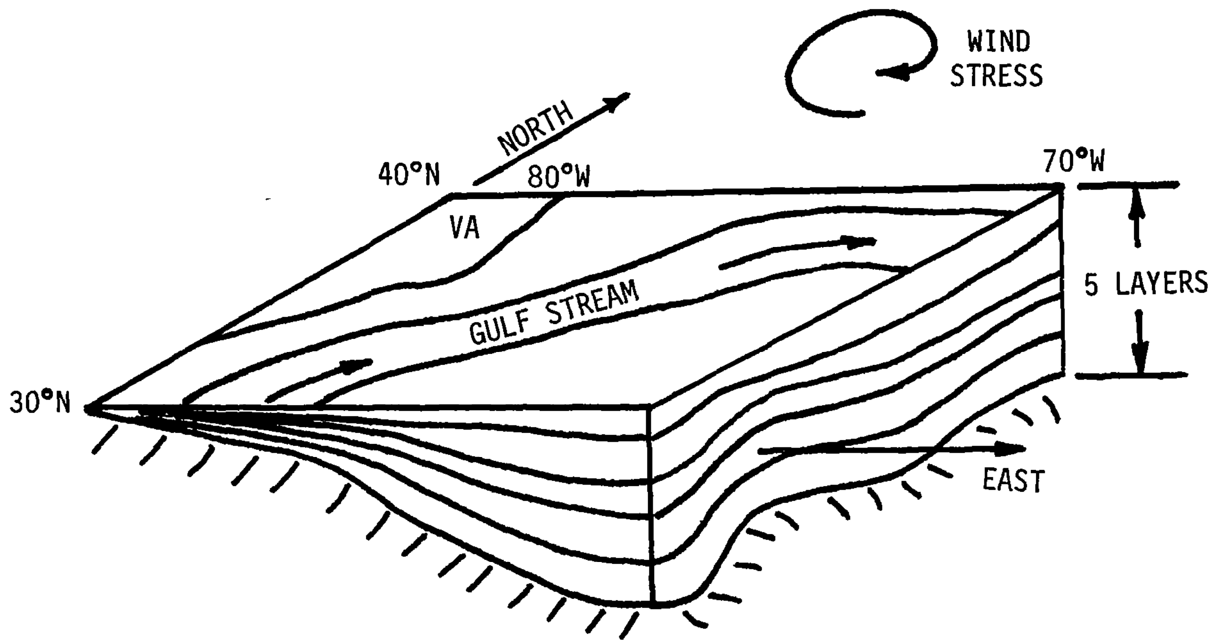


Figure 6. STRUCTURE OF THE LIMITED REGION CONTINENTAL SHELF MODEL

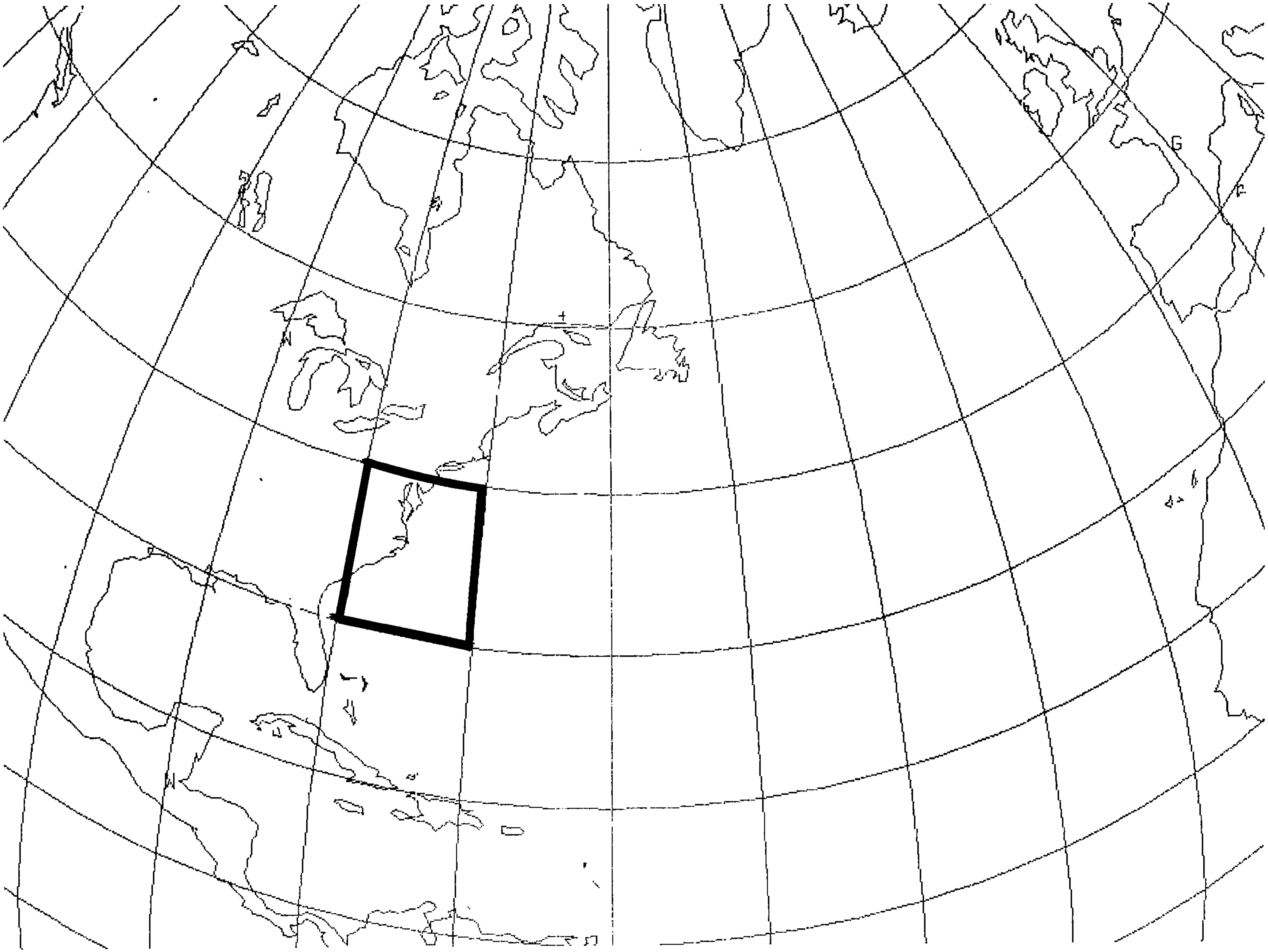


Figure 7. LOCATION OF THE LIMITED REGION CONTINENTAL SHELF MODEL

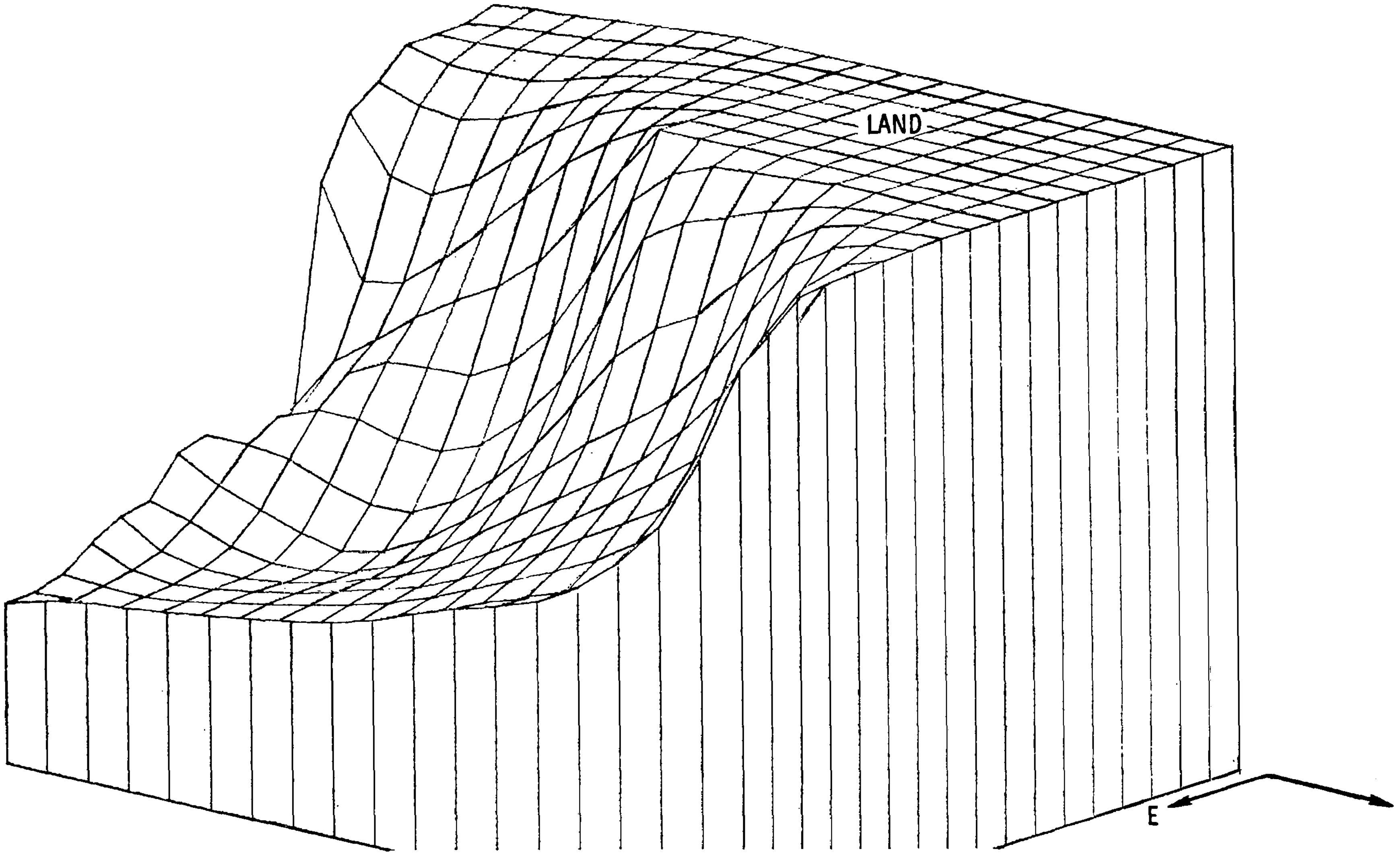


Figure 8. CONTINENTAL SHELF MODEL BOTTOM TOPOGRAPHY (VERTICAL EXAGGERATION 125:1)

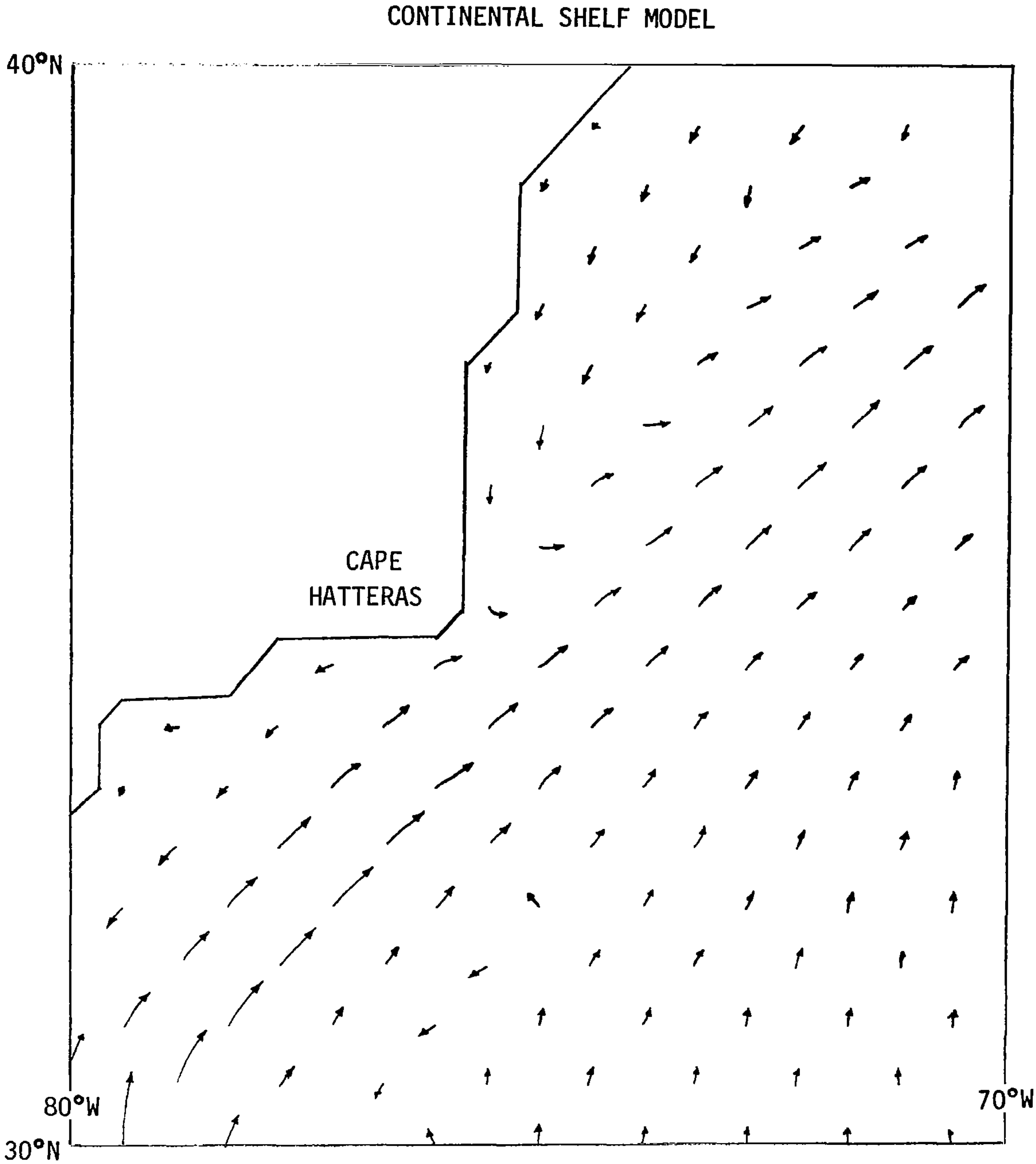


Figure 9. INITIAL SURFACE CURRENTS (OBSERVED AVERAGES FOR MAY)

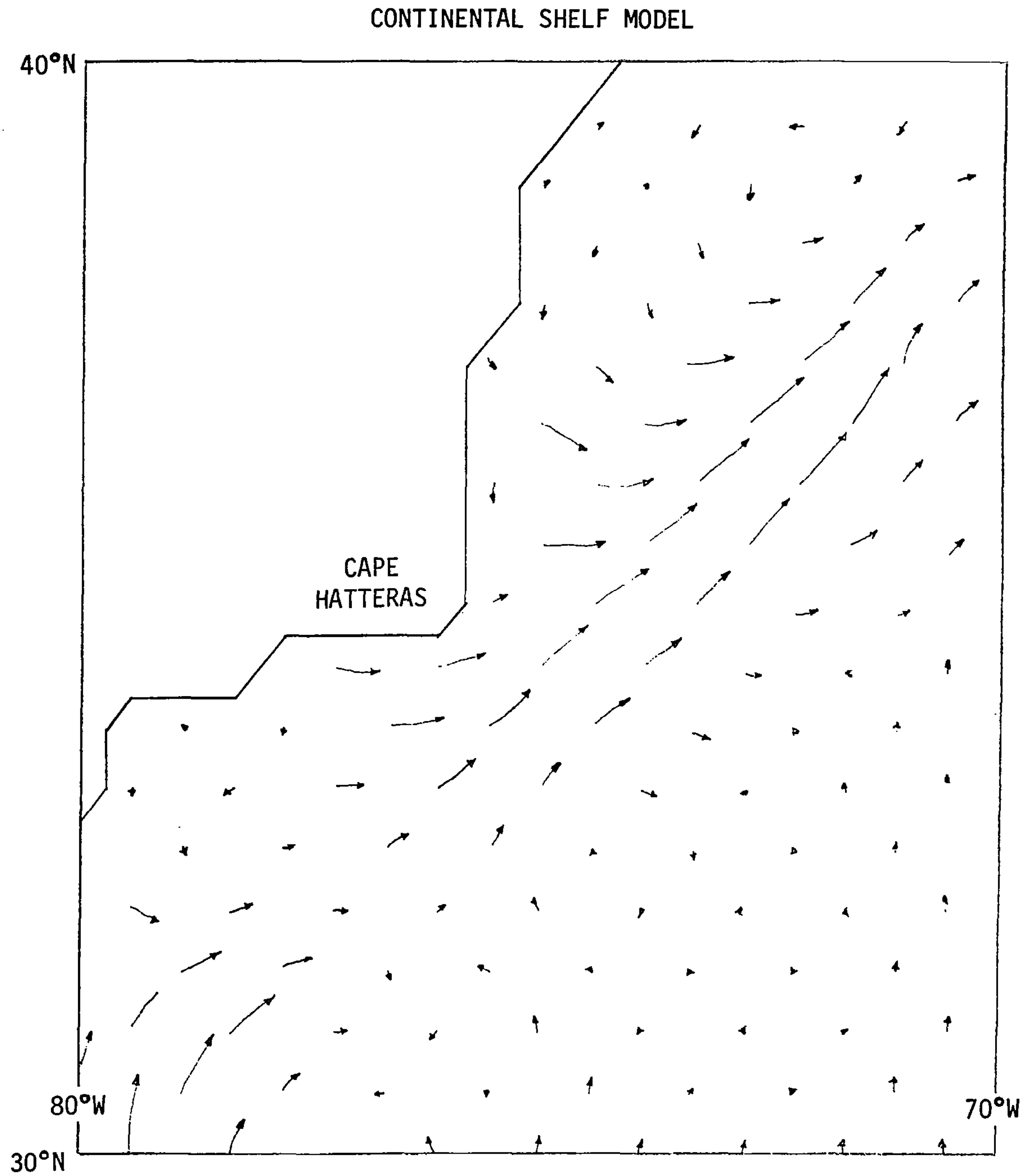


Figure 10. COMPUTED SURFACE CURRENTS (7 DAYS)

METHODS OF ANALYZING LAGRANGIAN TIME SERIES

by
T. P. Barnett
Scripps Institution of Oceanography

Introduction

There are two major purposes in making this presentation. First, I wanted to talk about different ways to display and interpret buoy data. By doing that I hope to convince you that the only way we are really going to get anywhere with the buoy problem is to put more of them out in the ocean. I'll show you some displays made from real data but not attempt a complete scientific interpretation of them.

The sources of the drift buoy data are Ron Johnson, John McFall and, as I found out yesterday, 18 others who tracked some buoys in Chesapeake Bay. The second source of data is from the NORPAX POLE Experiment which was completed in February. The location of that experiment was 900 miles north of Hawaii. The buoys, 30 in number, were built by Chip Cox; deployed by Bob Knox and tracked from an aircraft by myself. The buoys carried a radio beacon which allowed us to locate them with aircraft overflights. Accuracy of position was better than half a nautical mile. The buoys were tracked for 3 weeks.

The Author: Dr. Barnett received his Bachelor of Arts degree in Physics from Pomona College and later his Ph.D. in Oceanography from the University of California's Scripps Institution of Oceanography. From 1963 to 1966 he worked with the Naval Oceanographic Office on Wind Wave prediction and generation; from 1966 to 1971 he was with the Westinghouse Ocean Research Laboratory working on wind wave generation, Lagrangian deep Ocean measurements and brain wave studies. In 1971 until now he has been with the Scripps Institution of Oceanography doing research with drift buoys and climatic dynamics. He is now Scientific Coordinator of the North Pacific Experiment.

Chesapeake Bay Data

Let's start out simply with the standard methods of display. In presenting these displays I hope to answer the following questions. What type of technique should we use to analyze the data? What can we find out about the physics of the flow from the display of the data? And, finally, have we really got a representative sample of what is happening in the ocean?

The first set of illustrations will be the Chesapeake Bay data. Figure 1 is a commonly used display. Given an X-Y coordinate system, we plot the various buoy positions as a function of time. It really doesn't make any difference on this graph where the buoys were put in, but you look at these things and say, "Wow, that motion sure is complicated." In this case, a rarity, you have the hope that there is some kind of periodicity in the motion. It's perhaps not as bad, or should I say, as confusing as some of the "MODE worm tracks" that we have seen.

A slightly more sophisticated approach (fig. 2) is to say, "Well, I'll take out the motion of the center of mass and then see what the buoys did." Same type of display, X and Y coordinates upon which we note the sequential positions of the buoys relative to the center of mass. Join up the positions and, well, what more can we say? The buoys move relative to the center of mass and there is the hint of some longer period variability here; certainly not the short period stuff that we saw in the first presentation.

Figure 3 goes one step further. We used the sequential position of the buoys to compute the U component of velocity which is shown in the top graph. Right away you look at the plot and say, "A-ha! That's tidal motion." The lower panel shows the motion of the individual buoys - three of them in this case - relative to the center of mass. There is not much tidal motion in there. What you do see is a periodicity that looks like the inertial period, but it is not very clear. One thing, however, both of these displays bring out right away is that the absolute motion is in the order of 35 cm/sec ±; in the lower panel the relative motions are more like 2 to 5 centimeters a second. The mean drift, which is there, is smaller than both of those combined. To first order, this field is essentially tidal motion.

Fortunately, Don Hansen just gave a little tutorial on correlation functions and structure functions so I can introduce the next techniques directly. The auto- and cross-correlation coefficients for three buoys are shown in figure 4. The top panel shows the U component of buoy no. 1 correlated with itself and with the other two buoys. Notice that the curves are virtually identical. The U component of the second buoy with itself and the other buoys is shown in the middle panel, and so on. The first zero crossing of the correlation function is at 3 hours, which implies a 12-hour oscillation of some type. From the shape of the correlation curve it is a very pure oscillation. And, of course, this looks like the semi-diurnal tide. This is the situation for the absolute motion of the buoys. You can do the same thing for the relative motion and find a time scale--it is a little more confused--of 16 to 20 hours, suggestive of inertial motion.

If you are an engineer working in estuaries, or making a dynamic model of an estuary, you might like to represent the expected diffusivity. Figure 5 shows the turbulent diffusivity tensor (ε) computed as

$$\varepsilon_{ij} = \frac{d}{dt} \langle X_i X_j \rangle$$

I don't want to belabor this figure but I do want to point out that these curves, when laid on the tidal height curve, clearly indicate that the diffusivity, at least in this one example, is very tightly tied to the tidal cycle. Extrapolating this results suggest that any dynamic time dependent model of an estuary that did not include tidal mixing effects might give misleading results.

Motion in the Central North Pacific

Let me now switch to the data from the central Pacific. The results here are new and in fact some came off the computer last week. We were 900 miles north of Hawaii (35°N, 155°W) where the mean flow 'according to National Geographic' is supposed to be to the east. For the 3 week period that we were out there, the mean flow was to the northwest--although I think Jerry McNally will tell us later that the National Geographic folks may be correct on longer time scales. Remember, we had 25 buoys drogued at 30 meters. The surface elements of drogues were extremely small and a detailed analysis suggests that the effects of wind drag and surface current drag were negligible.

Figure 6 shows what I will call an X-T plot of the current field. Pick an arbitrary coordinate system in the ocean with origin X_0, Y_0 and some arbitrary start time t_0. Let's look at the velocity field, in this case the U component for every buoy position. For every estimate of U, I have entered on the display the sign of U. This kind of display, when used in a strongly periodic flow field, would give contours of U = o, that ran vertically up and down the figure. In fact, no such periodicity is apparent. On the other hand, a very definite contour of U = 0 exists along the X axis. In other words, there is a 'division' of the U-flow field in X-space. On one side of the region, in this case to the east of it, all of the flow is to east. To the west of the 'division' all of the flow is to the west. There is a region of divergence and because it apparently maintains its integrity through the whole length of the experiment, we feel that it is a predominent feature of the flow.

I would like to go one step further and address the isotrophy of the flow field. Figure 7 is a plot of sequential buoy positions relative to the initial point in $\underset{\sim}{X}$-dimension where each buoy was put into the water. I've taken the sequential positions, subtracting from them for each buoy, its initial insertion location. That gives a coordinate that looks like $\underset{\sim}{X}$ and again a time. These data make up figure 7.

If the data were distributed uniformly in X_0, Y_0 space, we would infer that the buoy motion was isotropic over the time of the experiment, i.e., there was no perferred direction for buoy drift. A look at figure 7, however, clearly shows that there is predominant tendency for the buoys to move to the northwest. We infer from this that the flow field observed during the POLE experiment was not isotropic. One might extend the results of this illustration further to talk in terms of the elipticity of the distribution of data points, thereby quantifying the degree is isotropy. For instance, if an elliptic fit to the data resulted in an elipticity of 0, i.e., a circle, we would infer isotropy. Values of elipticity greater than zero then provide a quantitative measure of the degree of an isotropy.

The data imply that the turbulent intensity, $\underset{\sim}{U}'/\bar{\underset{\sim}{U}}$ is large. The flow is basically turbulent and highly variable on the scales with which we are working.

With the questionable assumption that the flow is at least quasi-isotropic, I would like to go to the next figure (8). This is a space-time correlation function for the ensemble of buoys. 'τ' is a time lag in hours, $|R|$ is an absolute radial separation distance between buoys. By the time we reach τ = 72 hours, and $|R|$ = 40 kilometers, the buoy motions are essentially not correlated with themselves. In other words, the typical scales that we associate with the flow field are 160 kilometers and 12 days. These are the characteristic scales of motion in the ocean, at least as we observed them during POLE.

Concluding Remarks

The story I get from these results, and others reported here, suggest it is a waste of time to do simulations of buoy motions in the ocean. These simulations require knowledge of the basic driving force - the three-dimensional spectrum of ocean currents. But that is what we want to measure. We don't know very much about this spectrum, so it appears that the only way that we are going to get the necessary information is to start making measurements. People have asked, "Can you tell us how accurately you want to make your measurements?" "How many buoys do you really need?" "What is your deployment strategy?" Those are the questions that the engineers rightfully ask. From the scientist's (my) point of view, I can't answer these questions without a basic knowledge of the flow field. Let's get on with the job of deploying buoys and start making measurements; <u>not</u> spending lots of time and dollars 'fantasizing' about the ocean's structure. It may turn out that certain problems can be addressed with modest deployments of say, perhaps, 10 buoys. In this case, I could care less of they are $1500 or $5000 a copy; 10 is all I need. Why spend time designing the super-cheap buoy? Unfortunately, there may be a large number of problems that we simply cannot solve with a few buoys. This leads to the questions of accuracy, cost and feasibility. Again, it is going to be basic data from the field that lets us determine the necessary answers and, ultimately, what problems are soluable and which ones aren't.

QUESTIONS

Dr. Kirwan, Texas A & M

I have two quick comments and a question. One is, I did carry out an analysis of the errors associated with the drifters that were deployed during POLE. For the wind velocities which we observed there, which were quite low, the errors associated with drifter motion were of the order of 1 percent for your mean drift. This means that for the typical absolute velocity values that you are reporting, say 16 centimeters per second, the error associated was about 50 percent of the value that you were getting.

Dr. Barnett, Scripps

What was the 1 percent error?

Dr. Kirwan, Texas A & M

The 1 percent errors are the 16 centimeters per second, ± As I understood one of your graphs, your U' values were t 4 or 5 centimeters per second which means the error that i with the wind is about half of what you are getting of U' use to calculate the dispersion.

Dr. Barnett

I'm still not with you. You said that the error was 1 per a second. Is that correct?

Dr. Kirwan

I am assuming that the error was 1 percent and I'm assumin absolute velocity that you are getting was 16 centimeters what I picked off from one of your slides.

Dr. Barnett

Those are some of the extremes I had

Dr. Kirwan

O.K. If you take 1 percent of that--that's about 1-1/2 to second which is about 1/2 of the U' values that you were us

Dr. Barnett

One percent of 16 cm/sec is o.16 cm/sec . . . You seem to h decimal place.

Dr. Kirwan

All right, you are right. In any event, it is 1 percent. during the calculation you seem to have realistic values.

Dr. Barnett

But then we're basically in agreement--at least over the PC we do not have a serious windage problem.

Dr. Kirwan

The other comments that I have is that you quickly passed over the point that you had a divergent flow field there and that may be true, but the analysis of the divergence in a field of drifters like that is a good deal more involved and, apparently--I don't think you have addressed that problem yet--at least you haven't discussed it.

Dr. Barnett

I'm well aware of that and, of course, there are a number of other computations that I could show here. I think the best thing to say is that there was a region in our area that was perhaps unique and I'm glad that we have some temperature/salinity sections through it so we can do a dynamic interpretation. Please remember I promised at the outset of this talk that I wouldn't try to give a full scientific interpretation of the results today.

Unknown

The question I have is: What significance did you attach to the negative diffusivity that you were getting with the Chesapeake Bay data?

Dr. Barnett

I don't have a good explanation

Unknown

How did you do the estimate of diffusivity?

Dr. Barnett

Basically, it is written up here on the board and is essentially the time derivative of what looks like the correlation function.

Unknown

That's how you computed it?

Dr. Kirwan, Texas A & M

The 1 percent errors are the 16 centimeters per second, ± percent each error. As I understood one of your graphs, your U' values were the order of 4 or 5 centimeters per second which means the error that is associated with the wind is about half of what you are getting of U' which you use to calculate the dispersion.

Dr. Barnett

I'm still not with you. You said that the error was 1 percent of 16 centimeters a second. Is that correct?

Dr. Kirwan

I am assuming that the error was 1 percent and I'm assuming that typical value absolute velocity that you are getting was 16 centimeters a second. That's what I picked off from one of your slides.

Dr. Barnett

Those are some of the extremes I had

Dr. Kirwan

O.K. If you take 1 percent of that--that's about 1-1/2 to 2 centimeters a second which is about 1/2 of the U' values that you were using.

Dr. Barnett

One percent of 16 cm/sec is o.16 cm/sec . . . You seem to have lost a decimal place.

Dr. Kirwan

All right, you are right. In any event, it is 1 percent. I just say that during the calculation you seem to have realistic values.

Dr. Barnett

But then we're basically in agreement--at least over the POLE Experiment we do not have a serious windage problem.

Dr. Kirwan

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Basically, it is written up here on the board and is essentially the time derivative of what looks like the correlation function.

Unknown

That's how you computed it?

Dr. Barnett

Yes.

Unknown

There is no need for the diffusivity to be positive but the physical interpretation of it, or course, is something else again. This approach is subject to a certain degree of noise because you are working with a derivative of the observation.

Dr. Kirwan

As far as I'm concerned, any approach that you use in calculating diffusivities will frequently or usually result in negative numbers and I just was wondering if you had words of wisdom that would provide a little more edification on that problem.

Dr. Barnett

The two-dimensional expansion and contraction of the float cluster with time will obviously give the computed result. Beyond that I have only words of ignorance on the problem. Sorry.

Dr. Hansen, NOAA, AMOL

The room is probably half filled with aerodynamicists but I think this particular approach is the one that has been used in smaller scale turbulence problems for a number of years and it is generally recognized that, because you are working with a derivative of observation, you do tend to get a higher degree of noise and so on, than if you could actually make the observations, determinations of the autocorrelation and then integrate. Integration being a smoothing operation as opposed to differentiation, which is an inverse operation.

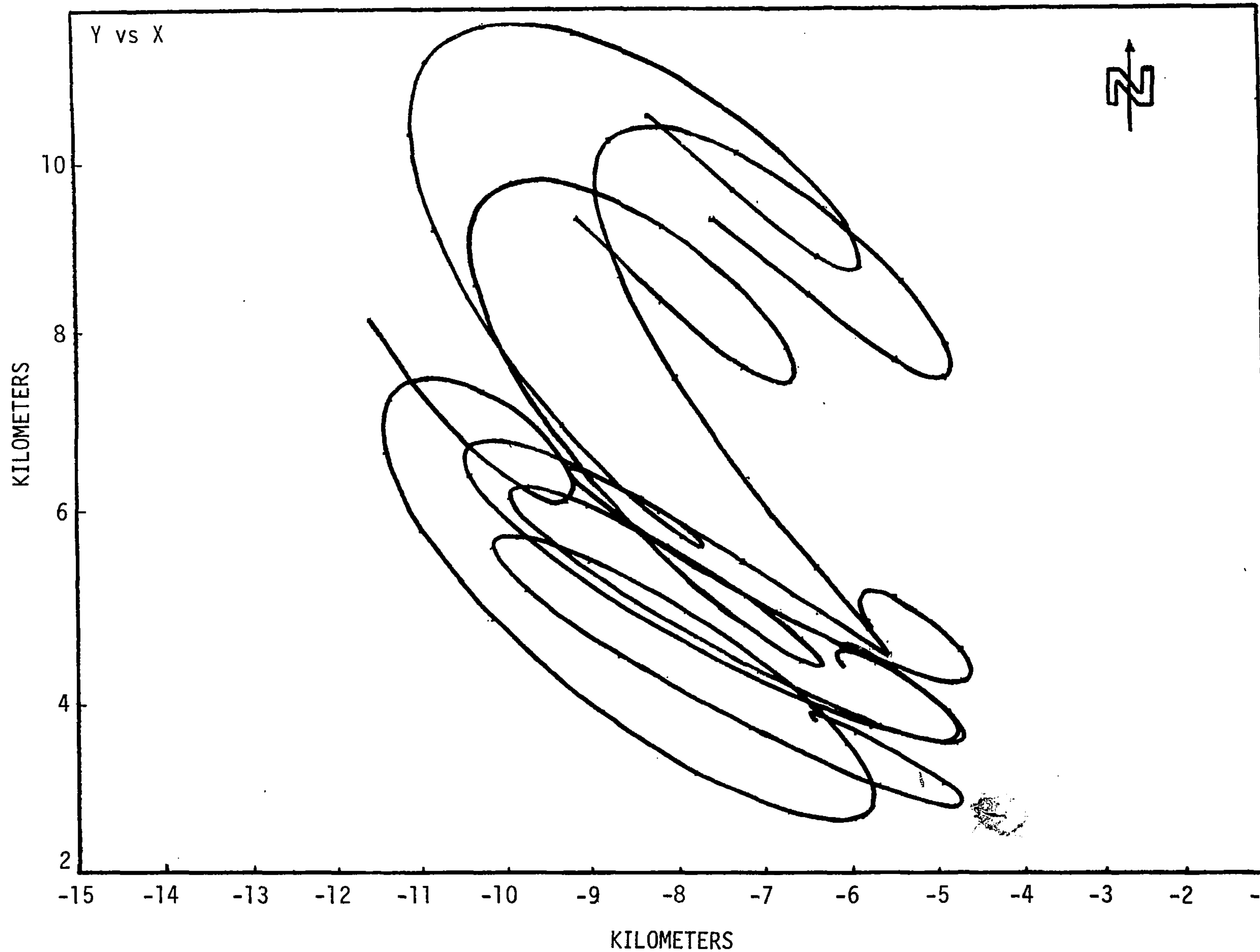


Figure 1. Sequential positions of freely drifting buoys in X-Y space. This is a standard technique of presentation which normally displays a rather complicated flow field.

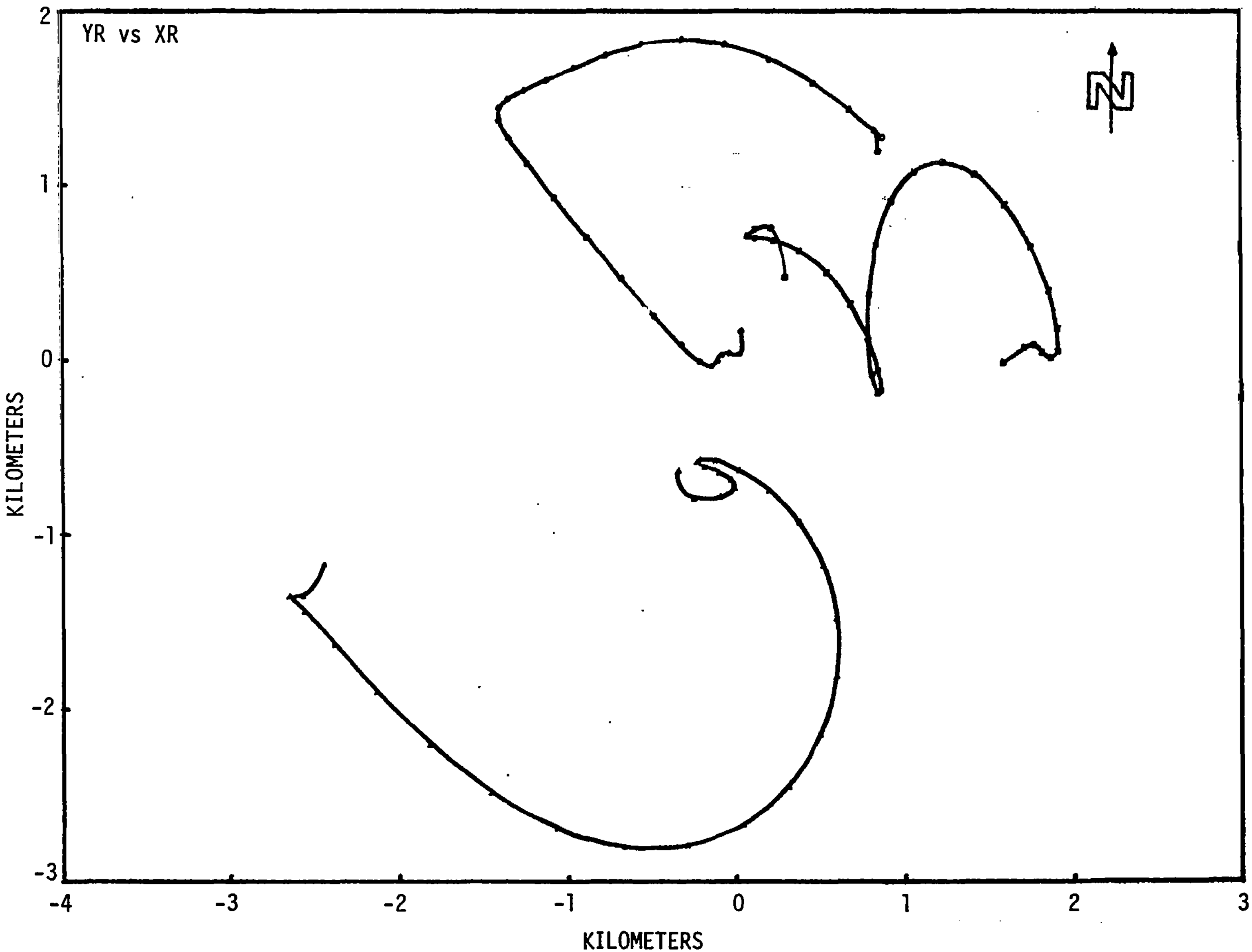


Figure 2. Sequential positions of buoys in X-Y space relative to the center of mass of the buoy cluster

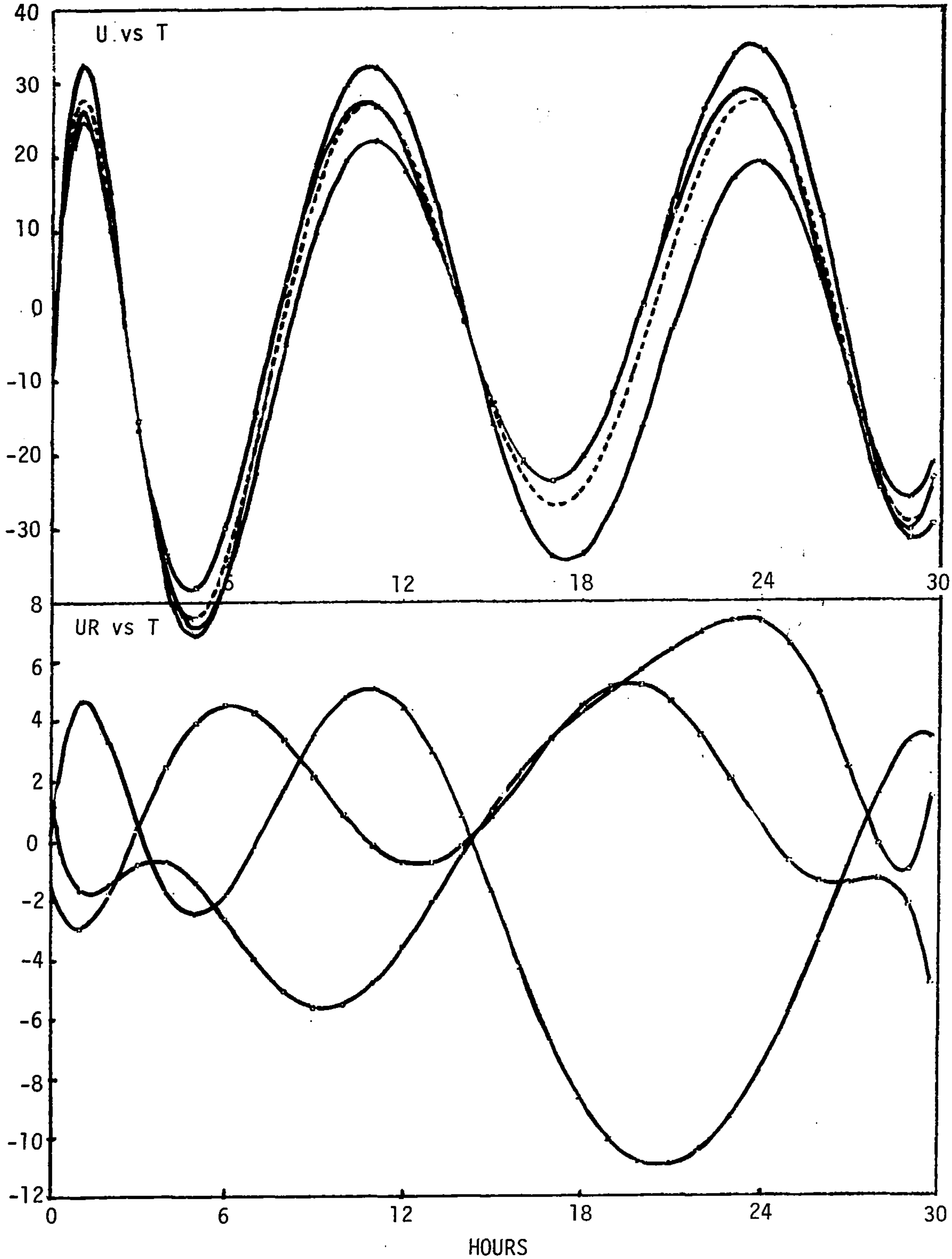


Figure 3. Time history of absolute value of U-Component of buoy motion for each of the three buoys, and the U-Component of motion relative to the center of mass (UR). The dash line in the upper panel represents the motion (U-Component) of the center of mass.

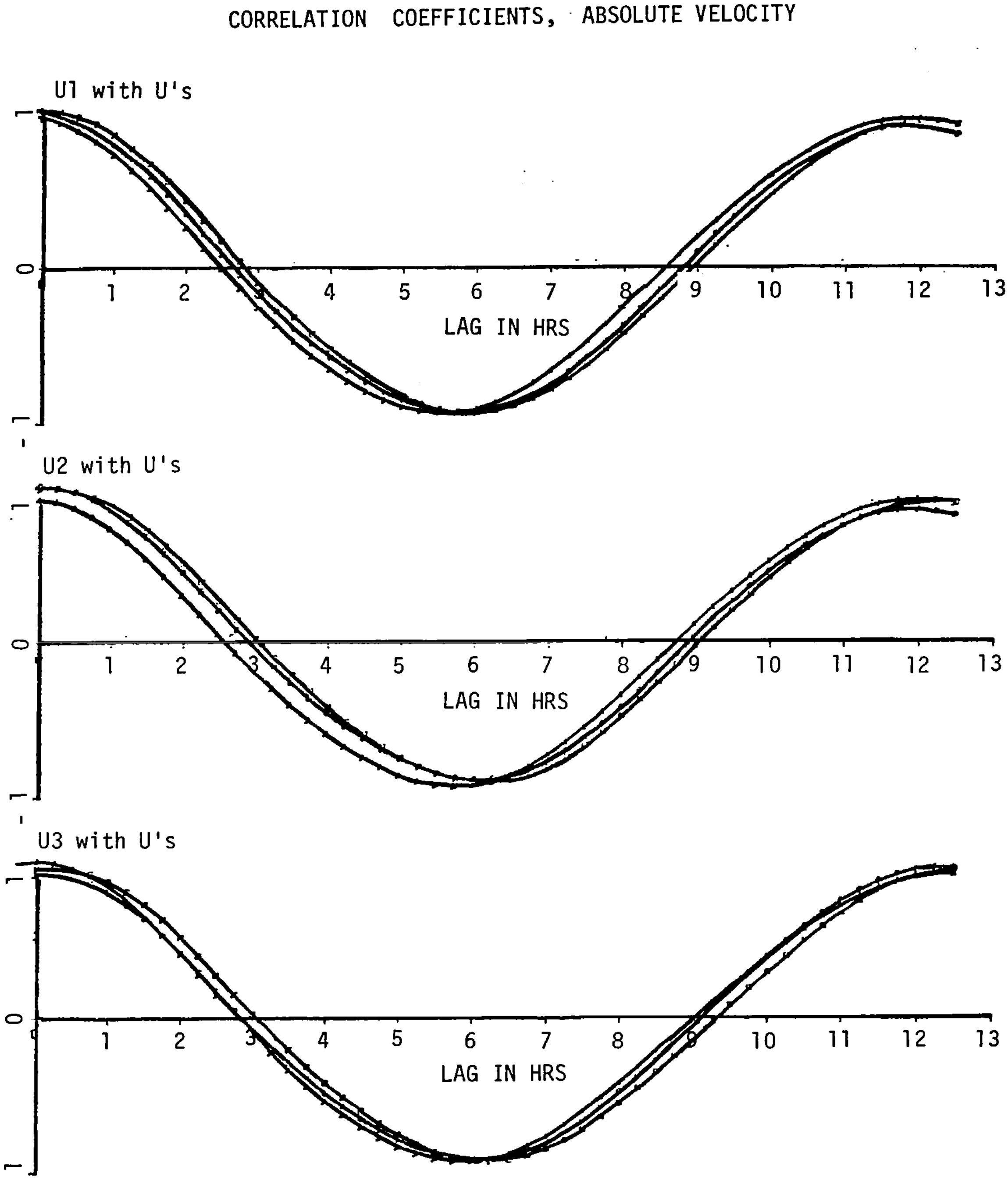


Figure 4. Cross correlation functions between U-Components for each of three buoys. The upper panel represents the U-Component of buoy 'one' cross correlated with itself and the other two buoys. Note the characteritic zero crossing of the correlation function at 3 hours, and imploying a characteristic 12-hour time scale.

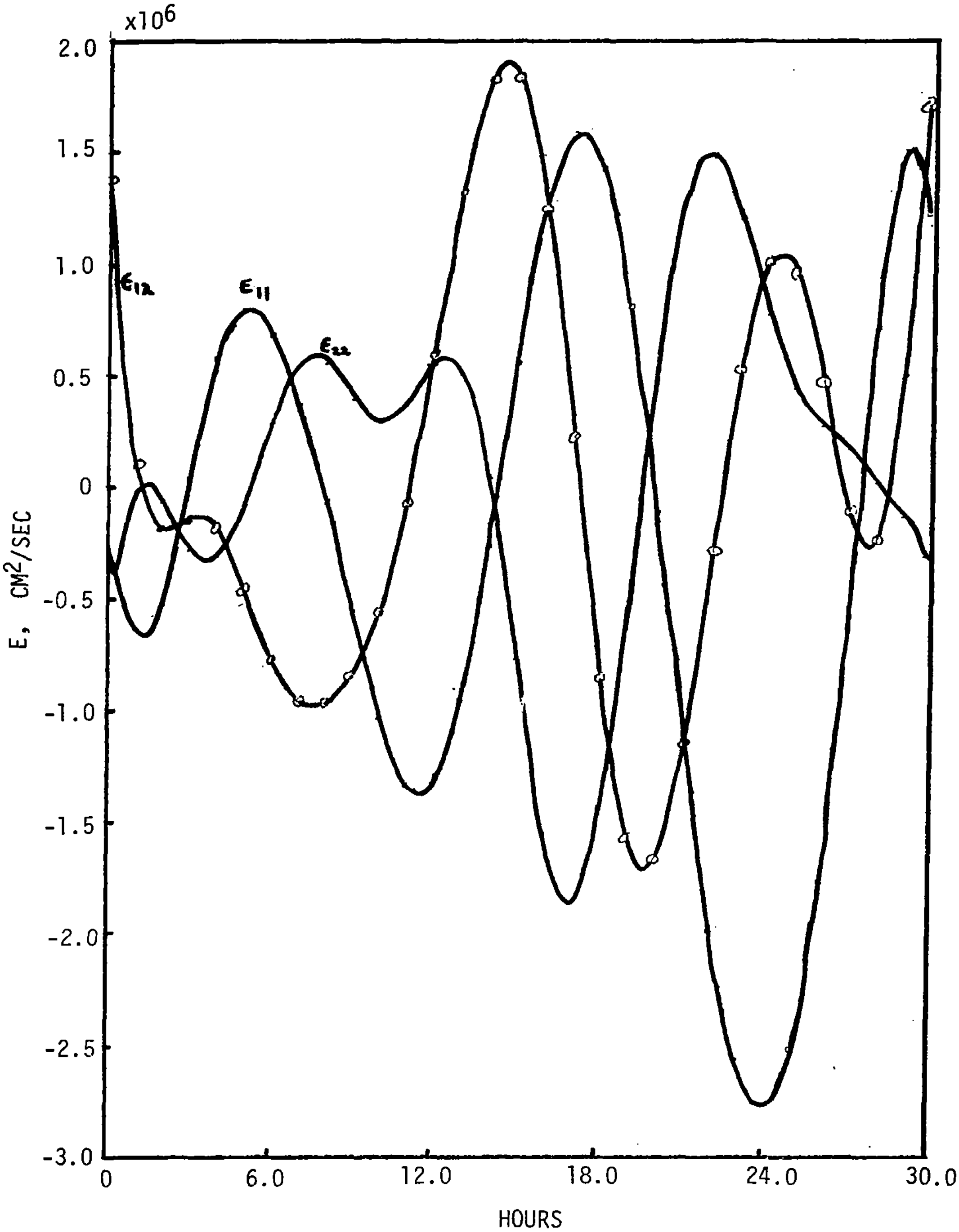


Figure 5. Turbulent Diffusivity vs. Time. The various components of the tensor field bear a striking phase relation to the phase of the tide.

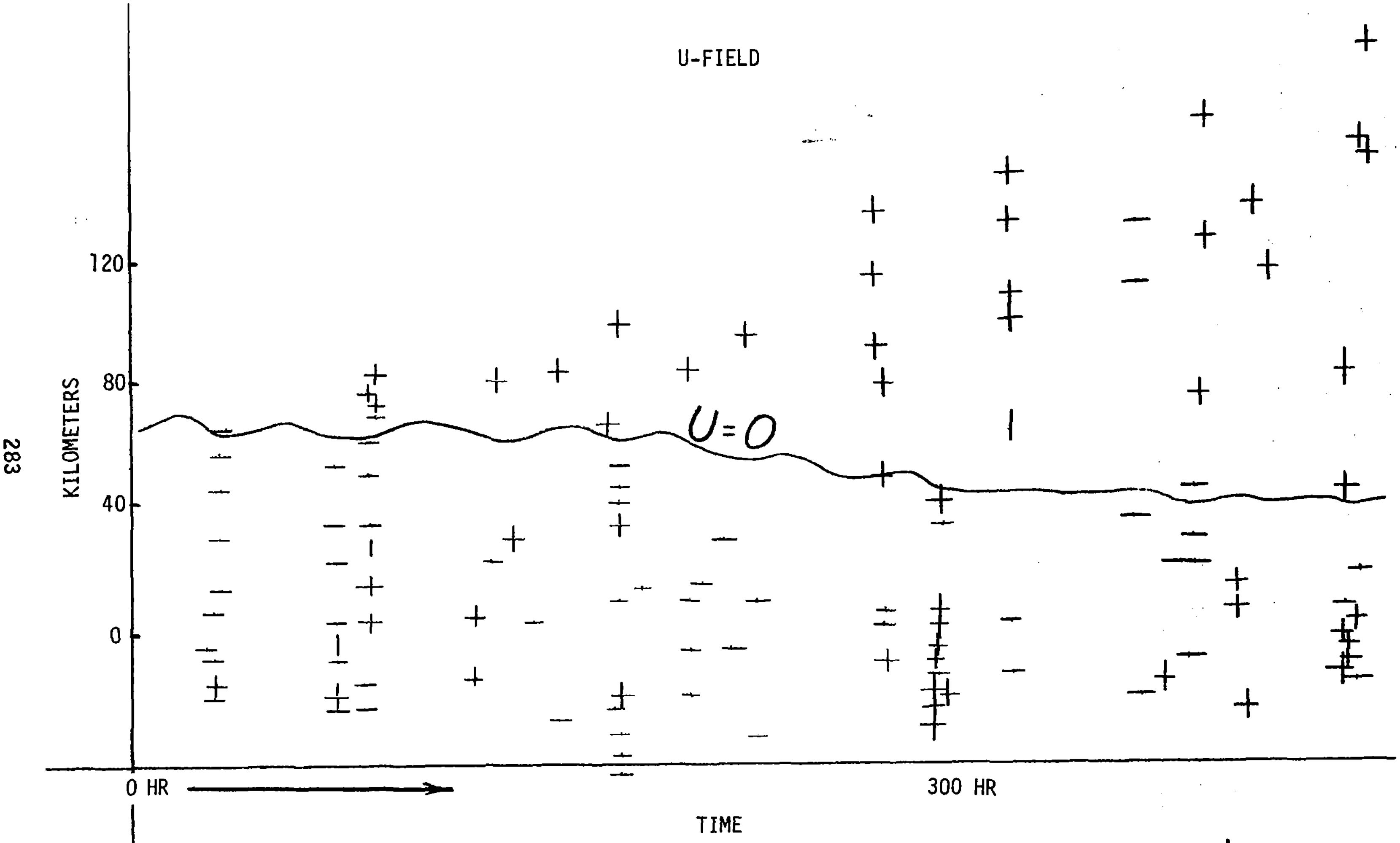


Figure 6. Part of the U-Component velocity field at X-T space; pluses indicate easterly flow, while minuses indicate westerly flow. Plots of this type can be used to determine if the flow field is spatially homogeneous or temporaily stationary. In this case, a spatial inhomogenity appears to exist at approximately X=60 kilometers.

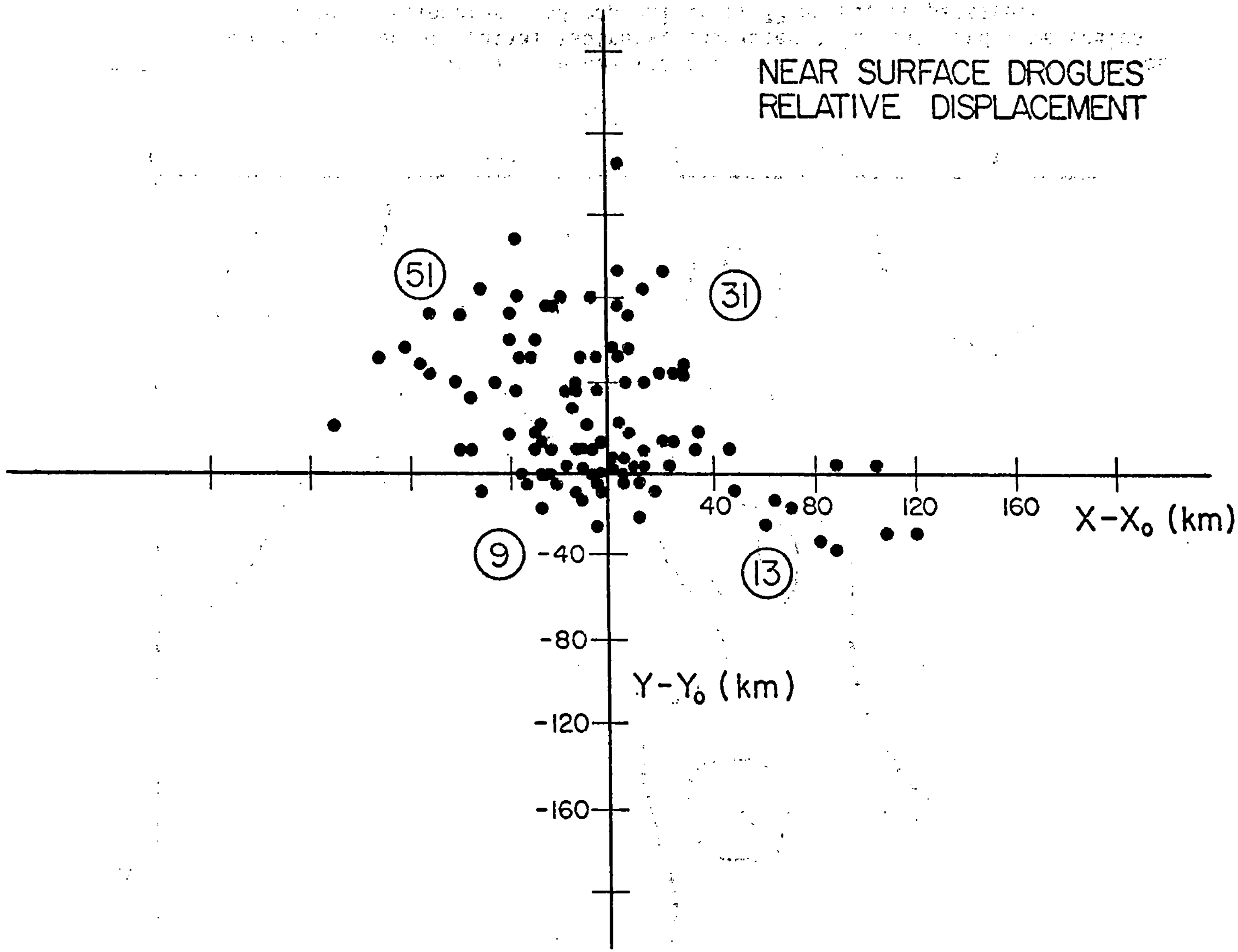


Figure 7. Plot of buoy dispersion in 'Relative Displacement Space'. The grouping of data in the upper left quadrant indicates a slight anisotropy in the flow field.

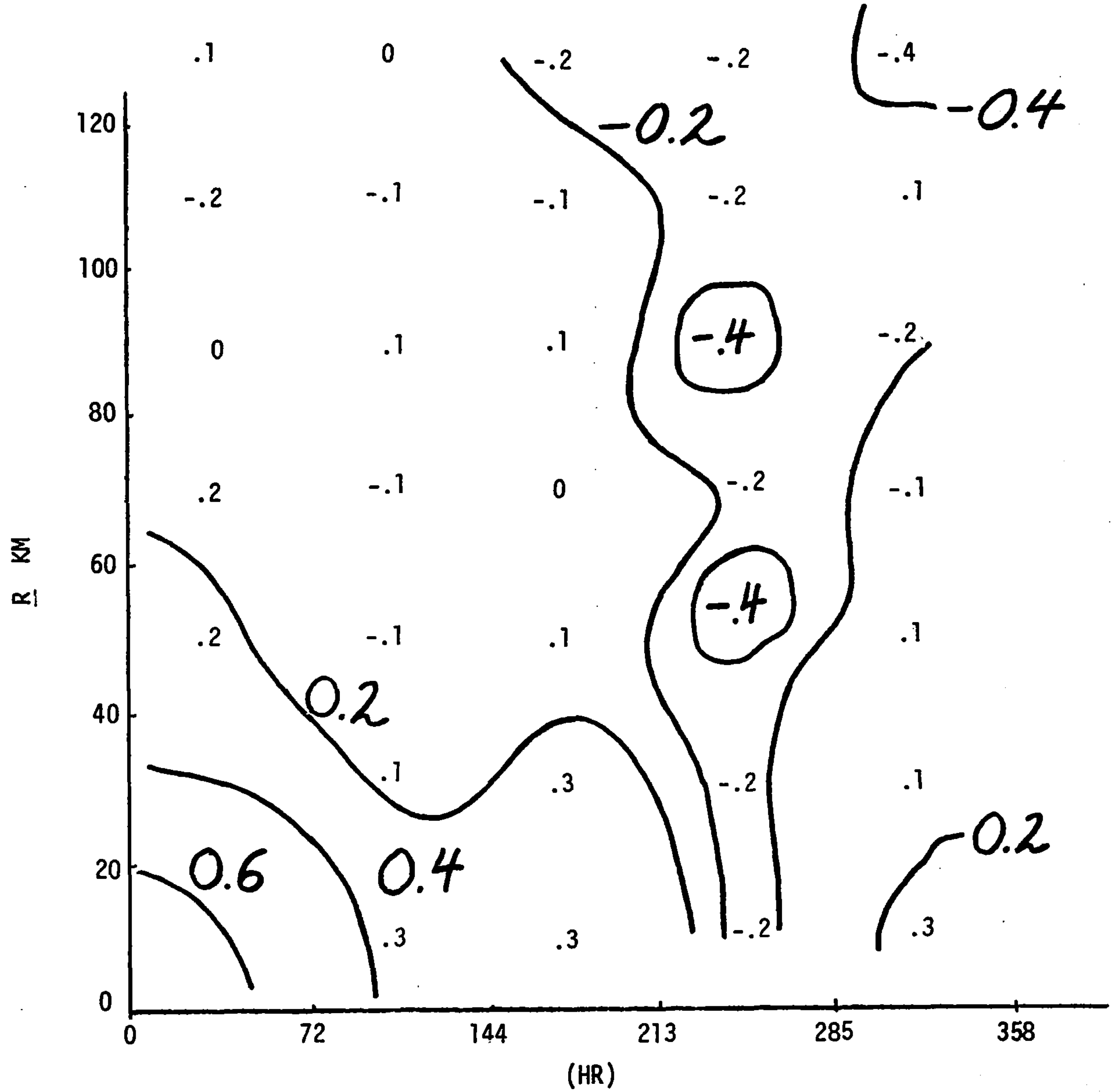


Figure 8. Two dimensional correlation coefficient for the U-Component of flow. Under the assumption of spatial isotropy, characteristic space and time scales are 20 to 30 kilometers and approximately 72 hours, respectively.

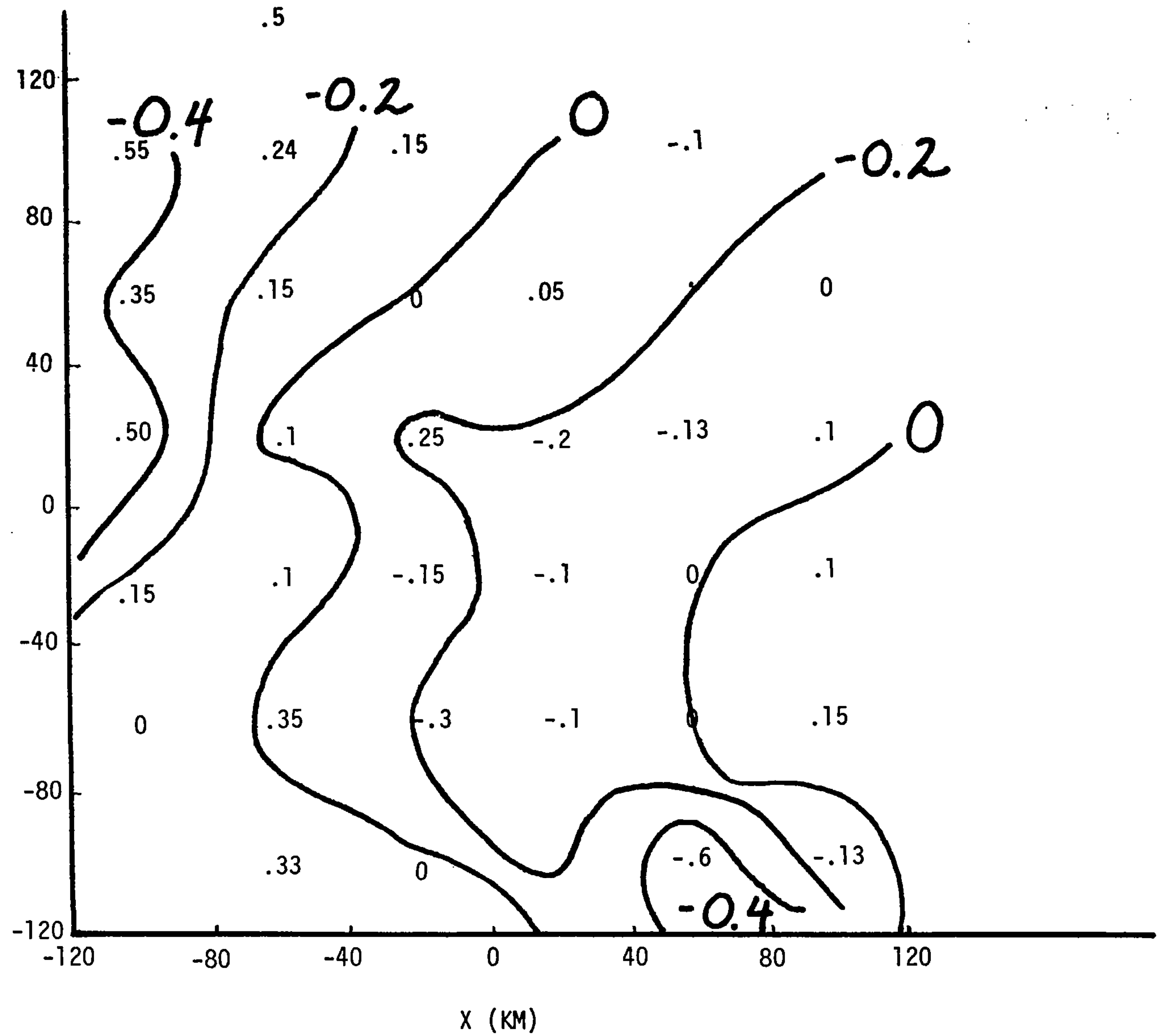


Figure 9. Cross correlation in X-Y Lag space for U-V components. Time Lag is approximately zero. No spatial coherence seems to exist between the U and V component.

SESSION D

Recent Experience and Plans

Chairman: Dr. Mike Hall, NOAA Data Buoy Office

PROGRESS REPORT ON THE NOAA DATA BUOY OFFICE DRIFTING BUOY PROGRAMS

by

Dr. Mike Hall

NOAA Data Buoy Office

I would like to note before I start that Bill Richardson of Nova University has done an awful lot of the work that our office has funded. He couldn't be here today but he sent a letter asking that one or two of the plans and developments that he's working on be mentioned. I'll attempt to mention those as I go through my own presentation and if time permits perhaps come back to one or two that I might omit. I think it's fairly important that I go through a description of our overall drifting buoy program. I'll try to raise enough new points that those of you who've seen it might get something out of it. As you know our development money and the justification for spending it comes primarily from large programs such as NORPAX and GATE, the upcoming global experiment. Those kind of programs do two things: 1) they give you a focus for a development effort by stating fairly narrowly the requirements that must be met, and 2) in many cases those large programs are putting a specific requirement on the kind of lifetimes that have to be achieved by the systems we're developing. The global experiment that

John Masterson described to you for example. If lifetimes on the order of two or three hundred days can't be achieved for these buoys, then implementing the experiment becomes much more difficult. We think that's a fairly ambitious goal for systems of this type, particularly in an area like the Southern ocean where the environment is known to be rather severe. The system then that I'll describe in a little bit of detail is the Nova Buoy. There's one on display out here. I'll assume that most of you have reasonable experience with it. I'd like to tell you a little bit about the buoy transmit terminal development status, which is the transponder for the Rams System and will be used with the Nova Buoy. I will discuss our plans for procuring that system and how interested parties might be able to get a terminal. Finally, if I have time I'll highlight some of the new drifting buoy development efforts we'll be getting into. Things that most of you don't know about or haven't heard about, and indicate some of the implications of the design goals of those systems.

The hard work capabilities that we believe have to exist in the drifting buoy area in order to satisfy the requirements of these programs in the next, let's say, 4 or 5 or 6 years are as follows: In communications we primarily have relied on the polar-orbiting satellite RAMS System. Right now we have the transmit terminal pretty well developed. It's undergoing test and evaluation right now. The RAMS System John Masterson and Chuck Cote described to you will be the system we'll rely on. This, in other words, will do the development in support of these large operational long term reliability programs that we've got to have such as GATE and others. As I go through this there will be one or two more communication schemes that will arise, but the point is this is the primary one.

I want to briefly mention the ice experiments area, we include that in the category of drifting buoys. In fact, what we mean by drifting buoys is anything that is unmoored and necessarily has a position fixing capability. Right now we've got a development underway at the polar research laboratory on the west coast to build a system for tracking ice motions in the Arctic for the Arctic Ice Dynamics Joint Experiment, which will be held starting in the spring 1975. We've been working with NDBO and the Polar Research Lab to formulate the plans for this system and that development is well under way. That is a system which utilizes high frequency communications so that the communication is satellite independent. It uses Navy navigation satellite position fixing. This position fixing scheme is a little different. The plan here is to utilize an array -- a circular array of buoys in which remote ice stations are controlled and operated by a centrally located manned station. These stations will make observations of position, hopefully, to an accuracy of a few hundred meters and they will observe atmospheric pressure and temperature measurements and not much more. It's possible some underwater current meter measurements will be made but we are not in on that development. That system then is an expensive one, it's a non-expendible buoy, it will probably run around $50,000 a copy, and will never be used in large numbers.

In the area of expendable drifting buoys, our main history has been in the Lagrangian buoy, the Nova Buoy that was described to you. We've had this system under development for about 2 1/2 years. It is designed primarily for a moderate environment. In fact, it was originally conceived to be used in the tropics. When the global experiment plans came along this buoy received a lot of attention, but we were quick to point out that it was never designed for use in the southern ocean, and in order to get from where we are now to a suitable design is going to take a little work. You heard we had plans to use it in GATE for example. Don Hansen was going to deploy a number of them, but that has fallen through. Nevertheless, there's perhaps on the order of 100 of the moderate environment versions that will be used in the next year or two. That seems to be the indication. We are starting an effort to achieve the same Lagrangian capability in severe environments. That's the ambitious one -- to be able to put buoys in places like the southern ocean or the North Pacific, and make long-term observations with a drogue device that provides data adequate for oceanographic purposes. Right now we've asked Bill Richardson to build a strengthened version of the moderate environment buoy-- has more reserve buoyancy--it's stronger--it's approximately the same size. We hope that a number of those will be used in approximately January or February in the southern ocean, in fact we're fairly sure that a number of them will be used. That's another point I will come back to momentarily.

If the evaluation of this improved Nova Buoy proves to us that it is not up to that environment, we anticipate initiating a completely new development for a hull--specifically for that use. We had intended to start at the beginning of 1974 but some recent tests with the Nova Buoy have been rather promising. We've put off a decision on that for a while. So, our main thrust for a severe capability buoy for the next 6 months or so will be in this modified Nova Buoy.

You've heard from John Masterson the requirements on a GARP Meteorological Drifting Buoy-- they're rather simple in that they measure atmospheric pressure, key surface temperature and report their position. That's a fairly simple requirement and it's probably possible to build a buoy for that application that is much simplier than the Lagrangian Buoy. It might have a drogue whose only purpose was to slow down the wind drift, for example, but there is no coupling requirement whatsoever.

We have not initiated a separate and distinct effort to achieve that capability. We are saying now that if we achieve this one with something like a Nova hull, this is covered. We can take that design and simplify it and put out a large number of those buoys. Probably more cheaply than if we initiated a new effort. I would mention, however, John McFall yesterday raised a point about air deployability. I think this is the area where air deployability might be a possibility and again it would not have to have any of the Langrangian capability. An air deployable Lagrangian Buoy is probably an order of magnitude more difficult. The final area that I will mention if time permits is what I am calling increased capability met buoys. The reason I call them met Buoys is their primary mission is related to making atmospheric measurements, perhaps in support of a program like NORPAX, but, nevertheless, the measurements are atmospheric and again there is no Lagrangian capability required in these buoys. These buoys are ambitious systems - that measure much more than this simple suite of variables.

This is the Nova Buoy (figure 1). The hull and some sensor payload is being developed by Bill Richardson at Nova. The buoy transmit terminal is being developed by American Electronics under contract to the buoy office. We have some sensor electronics and some interfacing going on at a couple of places and we have sensor developments going on at a couple of other places.

This essentially describes the buoy system that Bill Richardson has been trying to put together. I mention it briefly because I'd like to point out a couple of the characteristics of the buoy transmit terminal. It's possible with this terminal to transmit 8 variables without doing any multiplexing, if no more than four of the inputs are digital. If any more than four are digital you must multiplex, or if you attempt to get more than 8 channels of data from this you must also multiplex. Also under development at both AEL and RF sources in Boulder, we have a couple of different types of test sets for use with this system. The bench test set is simply the digital logic and display necessary for a hard wired output from the BTT. It could be used during integration of this system or perhaps checkout of an operational system if some kind of an umbilical were available. The other test set is identical to the bench test set except it has an RF portion so that there's an air link capability. It has two purposes: it's primarily to take on shipboard in operations where a number of buoys will be launched from the same vessel. We'd like to be able to put a buoy in the water, talk to the buoy and find out if it is working or not, because in an experiment such as the Global experiment, there is probably a day of steaming time between locations for a vessel and if they put one in that's not working, it is probably going to be necessary to immediately launch another.

The field test set will also allow us this fall to take some fully integrated systems and put them in the water and operate them for long periods of time overflying them with this test set to see if they're still reporting data. Naturally, we'll get no position fixes from that but we will get some indication to the long-term reliability of the system.

We are also trying to undertake a number of tests to check out the various parts of the Nova Buoy. The hull and drogue tests that have been going on have not all been conducted by NDBO. The POLE experiment was described to you in a previous presentation. We got some indication of the lifetime of the thing in the North Pacific during the POLE experiment but, as Jerry pointed out, we don't know too much about what actually happened to the buoy in the engineering sense.

The Draper Laboratory Drogue Study has been described to you by Bill Vachon. I'll only mention here that we're interested in working more with these people to see what further progress we can make in the performance of these window shade drogues. For example, an instrumented drogue which would give some indication of the slippage in the water and the effectiveness of coupling. There was one quick drogue evaluation that Bill Richardson did down at Nova in which he placed a parachute drogue and a window shade drogue in a dye blob and tracked them with aircraft. That test was highly positive; he found in comparison between the dye and the window shade a very small threshold of velocity difference. Perhaps that's just fortunate.

There are three other buoy test projects I would just like to mention briefly. Some time ago, in I believe December, we shipped three buoys to Southern Australia, to be launched there. We had two things in mind. Back then we had no indication of how this thing might behave in a severe environment.

We were invited to participate in Australia and it looked like a good chance to find out. Also we wanted to find out some of the launching and logistic considerations that Denny and Jerry have raised. How difficult is it to launch buoys of this type from a vessel of opportunity? We shipped three to Australia--no one went with them--only a little typewritten description of how to shakle them together and launch them. And they were placed in the water from a vessel we didn't know too much about. I'm told the sea state there was approximately 15 ft. maximum. And sure enough, during the launch one of them was damaged badly. It was recovered and repaired and put back in the water. The other two were successfully launched. In general, the comments from the people who launched them were favorably but I think the indication is that we've got some work to do in launching. The technique here was to lower them from a boom using a rope sling. So the problem again was impact with the hull of the vessel. These two are probably the more significant tests--they both have been documented but neither has been fully worked up yet. One of the things we wanted to know about the buoy that we considered to be a prime importance were it's motion characteristics in high sea states, such as the southern ocean. Two things about its motion characteristics: 1) with a window shade drogue with its inherent high vertical drag, what will the buoy motion look like? Will the buoy, for example, be pulled under during high sea state? We had some fairly definite evidence from John Garrett in Canada, that the buoy would, in fact, be pulled under by a window shade drogue; in any kind of a big sea state. The other thing we wanted to know from this was some indication of what the motion characteristics of the antenna are. The antenna is essentially just a vertical cylinder which sits on top of the buoy. And we wanted to be able to state something about what the motion characteristics of that thing looked like in a sea state, so we'll know a little better whether or not the antenna we've decided to use will be adequate for these purposes.

We performed that test by taking two instrumented buoys, one with a drogue and one without a drogue - I shoudn't say instrumented, the only thing that they each had was a VHF transmitter that would output a continuous carrier from an antenna that looked something like the one we would use. The one had a drogue at approximately 30 meters. The other one had no drogue, just a ballast weight. The idea was to put them out near a fixed platform which was made available to us by Shell Development Co. in the Gulf of Mexico, and keep them on the platform and wait for weather. That's precisely what we did and we got the weather event we were waiting for. The wave condition during this period was approximately a significant wave height of 12 ft. It seems that the maximum wave heights during the investigation were something on the order of 20 or 22 ft. The buoys were launched and the carrier was continually monitored to see if the antenna quenched out at any point from either having been pulled under water or from wave activity. During the entire operation, the antenna on the buoy with the drogue never did quench out. In fact, the buoy hull, the top of the cone on the buoy, never did go under water. We were rather encouraged by the whole thing. The sea was approximately 40 to 45 knots, steady wind, good seas and the buoy stayed rather rigid, rather erect--it did experience a lot of vertical motion but it was confined to a fairly narrow range. In fact, we expect to be able to extrapolate somewhat, what another buoy design might do under similar sea states, or what this one might do under a greater sea state. The results of that test were encouraging in terms of the nature of the hull design. If that antenna can sit there and output a signal continually in those sea states, we take that as a fairly positive indication. Obviously this test was short-term--by the way, we made motion picture observations in order to get the angular behavior of the antenna during this period and those have turned out very well. We're just beginning to work up the data. There was no attempt to get long-term indications of any kind from this experiment. It lasted 1 day.

In the area of long-term testing, Bill Richardson recently put out two of these buoys for us in the Gulf of Mexico. He put them out off the west coast of Florida with the hopes of tracking them around the southern tip of Florida and picking them up in Ft. Lauderdale. One of them was recovered after a month and was inspected, and was still operating. It seems to have been fairly successful. The other one did sweep around the southern tip of Florida and, I believe, both of them meandered a little bit but in general they followed the current around the tip of Florida. The second one did run aground in Ft. Lauderdale and he was able to recover it. With that particular one the drogue was virtually destroyed. We also just heard an indication from Don Hansen that some of the drogues we've been using with these buoys in tests, are having problems as far as coming apart under the stresses. So it looks like we've got a little bit of work to do in drogue design. We've been concerned primarily with the shape of the drogue so far, and it looks like we've got to do some work in making one that will last long enough.

I would like to next mention three areas where Lagrangian information is an important input to the design of the system but is not an end in itself. One of the raging controversies right now that hasn't fully been brought out at this meeting is whether or not, first of all you can technically achieve anything you might call a reasonable Lagrangian trajectory with a buoy; and secondly given something that is a true Lagrangian trajectory what is it's significance dynamically. Those are the two questions to be answered. A number of operational systems coming up down the road will call for some information on Lagrangian considerations, but where the dynamics of the flow is not the primary concern. The first of these is the global experiment that John Masterson described to you: 300 buoys will be put out on that experiment. We don't want the plan today for that amount of investment without having some indication of what's going to happen to these things. You saw some results yesterday worked up from a southern ocean experiment and you noticed that there was a strong northward component in the paths of some of those buoys. That's going to be a significant problem to the implementation of an array if it comes to fruit. So there are cases where we feel like it's necessary to put out buoys and learn what we can about the fate of a drogue buoy, independent of the dynamic consideration. Secondly there's a possibility that in January or February of next year an investigation will be conducted in the Drake passage region, under the U.S. Ice Program. This oceanographic investigation would include a number of drifting buoys. That's not definite but it looks fairly promising for that experiment. If that happens then, for a period of a few months in the Drake passage here, there would be on the order of 15 or so buoys with RAMS Communications. A number of us then are working on an attempt to approach the South Africans, to put out a number of buoys in this region, the same kind of buoys--the strengthened version of the Nova Buoy. South Africans are planning to put out three themselves in this region from 40 to 50 South Latitude, and we would provide them an additional four or so to put on down on their line toward the continent. We also are trying to work with the Coast Guard icebreakers that operate out of Christs Church to put out a third line of buoys. I don't mean to imply too much that we know what these arrays would look like, that remains to be seen. But the funding is now available for a certain number of buoys in this area in addition to the probability of there being a Drake passage experiment. What we hope is, that independent of the Drake passage investigation, we can conduct a pilot investigation of the GARP buoy array here to get some idea of the long-term reliability of the system we're designing, some idea of the difficulty in launching the buoys, and in particular some idea of the trajectories of buoys placed in these regions. One more thing I wanted to mention before I go on. The scheme to launch from ships of opportunity leaves one notable gap; in this region approximately 80 - 120 or 130 degrees longitude here, the ships of opportunity launch does not adequately cover the area. I would suppose then that if air deployed buoys, with meteorological payload were available, this is where they might be utilized.

The other two systems that we're looking into right now that might call for Lagrangian information, we are presently reviewing a proposal from Bill Richardson at Nova, for a buoy that would be used in NORPAX's heat budget experiment. Essentially this is just a meteorological drifting buoy that makes radiation and humidity measurements at 10 meters elevation, along with wind speed and direction, barometric pressure and a few temperatures in the air and through the upper couple hundred meters. That would be a RAMS System, but again I think it's primary purpose would not be Lagrangian, but rather to get air-sea interaction data for this large scale NORPAX's investigation. On the other hand, the Lagrangian information in question then is what happens to an array of these things that might be put out--where will they go? You've heard a number of people tell you they've planned an array and put everything on the right hand side and sure enough everything went to the right so the experiment wasn't really born out. But it's necessary to have some indication of what's happening before you plan the experiment. Finally, we have one more system we're looking into that calls for rather good Lagrangian information from an area. Off the east coast of the United States, in support of the national weather service we're putting in a number of large moored buoys that measure primarily wind speed and barometric pressure and temperature, which the weather service uses in forcasting purposes. As all of us know by now, those buoys are inherently rather expensive. The very fact that you have to moor them and board them, make them rather expensive. We're looking now, into just a feasability study of, can expendable drifting buoys be deployed off the east coast of the United States, whose mission would be this same weather service mission--to take wind, speed and direction, atmospheric pressure and temperature, as a minimum, off the east coast. These buoys have some requirements that make it tough. They have to have a low cost aneomometer with long term reliability for example. But more importantly they can't report data on the frequency of the envisioned RAMS systems. They've got to report at least 6 hourly, if not 3 hourly. We think 6 hourly would be acceptable, but unless we hear something very promising about a multiple satellite system with some kind of real time capability, we're obviously going to have to go to some other communications scheme. Position fixing is not a stringent requirement for a system like this, high accuracy position is not required. In fact, if you got it's position within 10 miles twice a week, you'd probably be okay. But the major question in our mind now, we've got a hypothetical design down, but what we don't know again is, what happens in the flow regime off the east coast if you implant some of these buoys? Should you drogue them? If so, what depth should you drogue them to? Given an array of these buoys what will be their fate? The idea of a system like this is, you continually replace the buoys in it. Perhaps if the buoys will couple to some flow off the east coast, for example the Gulf Stream, you can use that to at least help you in dispersing the buoys. So that system is under investigation; whether or not we do it will depend on what the economics look like. Whether these expendable buoys are actually competitive with the large version. If we do that, if it even looks promising, I'm sure you'll find us, together with any oceanographic support we can find performing some experiments off the east coast, looking for Lagrangian information. Again as an end in itself, independent of the dynamical consideration. Questions or comments?

Speaker Unidentified:

One question Mike, or one comment rather, particularly for the benefit of Chuck Cote. These are in regard to your remarks and John's about the 32 or 34 GATE buoys having gone by the board. I would say it slightly differently. The department has gone by the board, but the buoy hardware has been in production for some time, as it had to be, as we're going to see next month. We're re-thinking the deployment plans, but there will be a deployment of those buoys.

Mike Hall, NDBO:

Right. In fact, I want to say just a word about the BTT procurement. The slip of the satellite really has allowed a lot of the oceanographic programs that John described to you, to be much more prepared now than they were originally. We were being pushed very hard by the GATE schedule. It looks like now approximately a total of 200 to 250 buoy transmit terminals will be bought in the next year or so. We made one buy of 75 ourselves is order to drive the price down and we're king of coordinating a second buy. We don't intend to make the procurement but we intend to get enough individuals together that it will be a large procurement and come with the price break that is inherent in large procurements. Right now that second go around looks like it'll be between 100 and 150 buoy transmit terminals. I might add, it's not too late to get in on that procurement for anybody who might want to. The actual exchange of money we will not take part in. We prefer that the individual experimenter deal directly with the contractor.

NOVA FREE DRIFTING BUOY

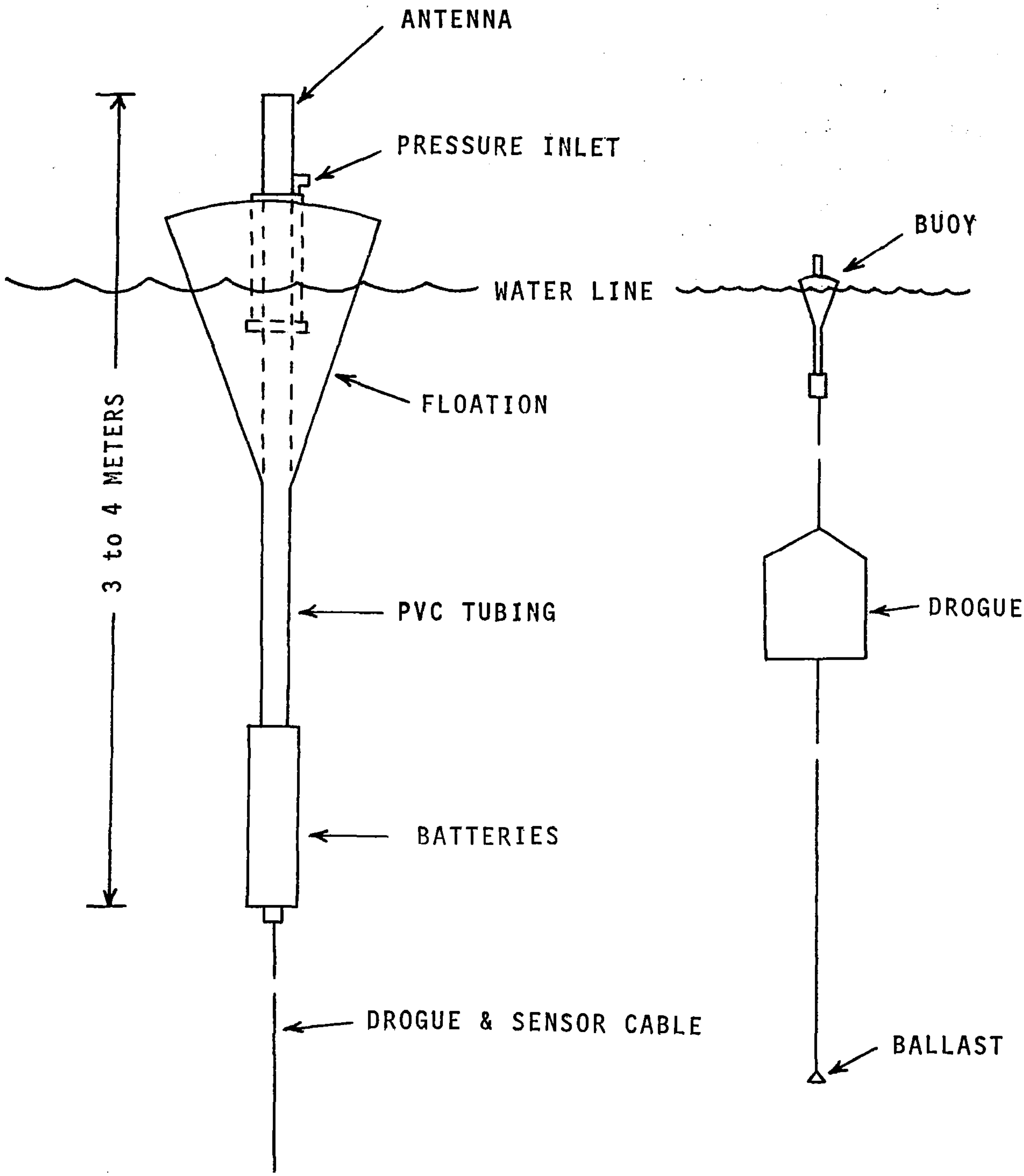


Figure 1

SOLAR CELL DEVELOPMENT AND TESTING FOR TERRESTRIAL AND OCEANOGRAPHIC APPLICATIONS

by
Tony Ratajczak
National Aeronautics and Space Administration
Lewis Research Center

I think it's obvious that Lewis is neither in the oceanography nor the buoy business, but we are the lead center for solar cell development within NASA and as such we've developed a power supply technology that we think is of interest to a user community such as yours. What I'd like to do is to review some of our objectives and programs and describe some of the projects we are involved in.

Figure 1 describes our major objective and the tasks in progress to achieve that objective. Since we are NASA, our effort pertains mainly to space applications although we are giving increasing attention to terrestrial applications.

The Author: Mr. Ratajczak is presently in the Solar Cell Application Section of the Energy Conversion and Materials Division of NASA's Lewis Research Center. He has had 10 years experience at NASA with solar cell technology and prior to that worked on solar cell related problems. He received his Bachelor's degree in Electrical Engineering from the University of Detroit in 1958.

There are two ways to reduce solar cell and array costs: one is to increase the power output from individual cells; the second method is to reduce the cost of fabricating individual cells and array systems.

The Advanced Silicon Cell Development program is intended to advance the state of solar cell technology. It consists of contracts with the two principal solar cell manufacturers in this country -- Controlab and Heliotech -- and an in-house effort, and is directed to such areas as solar cell contacts, anti-reflection coatings, resistivity of the bulk material, and junction depth.

The FEP-Teflon Covered Solar Cell Module development contract with TRW is an outgrowth of the FEP-Teflon encapsulation technology that was developed at Lewis. FEP, which bonds to the cell, replaces the fused quartz cover slide that is applied to solar cells to protect them from radiation damage in space. The cover slides are expensive, costly to install, and the adhesive that holds them to the cell is subject to optical degradation from ultraviolet radiation. FEP is about the same density as quartz, so about the same thickness gives equivalent protection. Since FEP is also an excellent adhesive, a second layer is used to bond solar cells to a thin plastic substrate. Conventional arrays consist of solar cells bonded to aluminum honeycomb substrates using conventional adhesives.

The FEP-encapsulated technology results in substantial costs savings in the material alone. As an example, a typical cover slide for a four-square centimeter solar cell costs between \$2 and \$3 -- the FEP costs about \$2 a square foot. Where the individual cover sides have to be individually applied to the cells, we have demonstrated that one piece of FEP can be laminated to a 480-cell module during a single lamination. A 480-cell module is roughly 17" x 20" and forms a 27 Watt, 28 Volt module.

The FEP Module Improvements Program is an upgrading of the FEP technology to interconnect the solar cells rather than welding preformed metal interconnects to cells. It is also directed to what we hope is the first step in an automated lamination process to bond the FEP to solar cells.

The Vapor Deposited Silicon Solar Cell program is an attempt to develop a less expensive way of making cells by vapor depositing silicon onto an inexpensive metal substrate.

Primitive Solar Cell Fabrication probably conjures up images of the Flintstones, and that's pretty close to what we are trying to do. In this program, we are trying to use the cheapest materials and the most primitive technology in terms of having the fewest number of steps and the least degree of sophistication to make a solar cell. We want the least expensive device that works well. The Low Cost Silicon Solar Cell Array program is basically an attempt to further automate conventional solar cell manufacturing processes.

An equally important area that isn't covered in figure 1, is our terrestrial applications program. That program started in 1970 when the NASA-Flight Research Center asked us to build a solar cell power supply for a radar beacon to be located on mountain tops and which would be used in conjunction with some space shuttle work they were anticipating. The solar cell modules we designed and built for them use conventional space type technology, but with changes to accommodate them to a terrestrial environment.

About the same time, an Environmental Research Office was organized within our division. It's purpose was to identify air pollutants and to model the air pollutant flow in the greater Cleveland area. Wind speed and direction was essential for such a study. Small weather stations were placed at various high schools in the area, and we attempted to put a solar cell powered weather station on the City of Cleveland water intake crib 3 miles out in Lake Erie. As it turned out, the problems involved in getting the data back to shore outweighed the value of the data from that location and so the station was located instead on a Coast Guard pier at the mouth of the Cuyahoga River. Although there was 110 volt power available at that pier, we decided to use the solar cell powered station as a demonstration of a terrestrial application of a solar cell powered system. Since program changes at the Flight Research Center had precluded their need for the modules we had built for them, we used them to power the weather station (fig. 2).

The power supply consists of the three solar cell modules, each generating approximately 10 watts peak power and a 70 ampere-hour battery. The anemometer is on top of the light tower. The temperature sensor and the dew point sensor are mounted on the super structure and the storage batteries and electronics are inside the enclosure. Data is sent to the Lewis Research Center continuously on leased telephone lines. Figure 3 is a close-up of the solar cell array.

The cells are bonded to a rather heavy aluminum substrate using conventional space type adhesives. The entire module is covered with a piece of plexiglass. The cavity formed by the plexiglass and the substrate containing the cells was evacuated and backfilled with dry nitrogen. This system was installed December 1, 1972. The power supply has been in operation since then and has experienced no difficulties other than a power cable being cut by ice.

The pitch of the array, along with rain and especially snow sliding off the array, has precluded any extensive dirt build-up on the plexiglass. This is noteworthy in light of the fact that the system is located in an area of very high air pollution from nearby heavy industry. This is one way to build a terrestrial solar cell module. We don't recommend it, however, because it is costly and heavy, but it does work.

As we were working with the solar powered weather station project, we were receiving encouraging results from FEP-encapsulated modules on test on our laboratory roof. The FEP-encapsulated module offered the prospect of significant cost savings for both space and terrestrial applications and also the degree of durability needed for terrestrial applications. We, therefore, embarked on a two-pronged program to encourage the use of solar cells for terrestrial electrical power generation.

One branch of the program was a contractual effort to analyze the market for the terrestrial applications of solar cell powered systems. The final report from that contract is now available from the NASA-Lewis Research Project Manager, Robert Masters.

The second part of the program was to seek agreements with other government agencies whereby we would supply a solar cell power supply for some remote device application. Such an approach would test FEP-encapsulated modules under a variety of environmental conditions and would demonstrate to a potential user, in conjunction with this equipment, that solar cells are a viable power source.

Our roof-top tests had shown that some changes had to be made to the FEP-encapsulated module developed for space to make it suitable for terrestrial application. Figure 4 shows our aluminum substrate terrestrial module. It is designed for the most rugged applications, generates approximately 1 watt peak power, and is of such a size as to be easily adaptable to various system power and voltage requirements.

The module substrate is a 1/16 inch thick piece of anodized aluminum. The solar cells are interconnected three in parallel by eight in series by parallel gap resistance welding preformed interconnects to the cells. To fabricate the modules, the interconnected cells are placed on the aluminum substrate between two pieces of FEP. A 0.005-inch thick piece of FEP acts as the adhesive layer bonding the cells to the substrate and a 0.010-inch thick piece acts as the cover layer. The assembled components are placed in a laminating fixture and under the action of heat (300°C) and pressure 15 to 30 psi for 5 minutes, the FEP flows into the cell interstices and bonds to the cells and substrate. The result is a solar cell module in which all the electrically active components are completely encapsulated in FEP. A less expensive and somewhat more fragile terrestrial module shown in figure 5 substitutes fiberglass cloth for the aluminum substrate.

Since most of the applications we encountered were for 12 or 24 volt systems, five of the 1 watt modules were connected in series and assembled into a 5 watt 12 volt module (figure 6). The operating voltage of this module is high enough to charge a 12 volt battery under the highest operating temperature we presently anticipate, 75°C. The aluminum and fiberglass modules can be used interchangably in this 12 volt module.

A necessary part of our terrestrial applications development effort was to develop a technique to determine the size and orientation of a terrestrial solar cell power supply. Designing for terrestrial applications is a good deal more complicated than designing a system for space. In space the sun shines steadily and at predicted levels. On the earth, of course, it doesn't. Sky cover and atmospheric turbulence vary and usually are not well defined for remote locations. The data used to design our systems comes from the Climatic Atlas and is in terms of mean daily insolation and mean sky cover. The system sizing technique uses these values of insolation and sky cover to generate monthly solar cell output. That data, the monthly night time loads, peak loads, and the montly daytime loads, are used to calculate the number of paralleled solar cells (array current generating capability) and the battery ampere-hour storage capacity. The number of series solar cells is an independent function of system voltage and maximum array operating temperature. The intent of the design is to achieve an annual energy balance. Generally, there are energy input deficits during the winter months. The batteries supply the deficit power during these periods.

During the spring and summer months, the batteries are completely recharged. To design a system whereby the solar cell array would generate the total power required each month would result in an uneconomical and excessively large array and large power surpluses during the summer months.

The first agency we entered into an agreement with was the NOAA. The Equipment Development Laboratory at the NOAA was just to the point of field testing their RAMOS (Remote Automatic Meteorological Observation System) weather station. Their original plans were to use propane fueled thermoelectric generators for power, but since the RAMOS systems by definition would end up in remote locations in Alaska and on tops of mountains, etc., NOAA was anxious to develop a power supply which would not require expensive refueling. They had, down-line at least, intended to look into solar cells, so our marriage was a perfect one. Thus, their first two experimental RAMOS systems were installed using solar cell power supplies.

Figure 7 shows an experimental RAMOS and its 40 watt solar array at the NOAA test facility in Sterling, Virginia. In all fairness, RAMOS isn't here as it was undergoing environmental tests at the time this was installed last October (1973). The white box contains the batteries, an array voltage regulator, and a simulated RAMOS load.

Figure 8 shows the solar cell powered RAMOS installed on 11,053-foot high Mammoth Mountain, California, November 1973. The NOAA chose Mammoth Mountain, located southeast of Yosemite National Park and in the Inyo National Forest, as a test site because of its severe winter environment. The 60 watt solar cell array provides enough power to operate the RAMOS in its normal operational mode with allowances for random additional interrogations by the Snow Ranger at the Mammoth Mountain Ski Resort.

Figure 9 shows the installation following a not-too-severe December 1973 storm. Rime ice accumulations, such as this, damaged some of the RAMOS weather measuring equipment and the solar array. Winds during storms exceeded 92 mph. During periods of clear weather between storms, large pieces of ice falling from the tower inflicted substantial mechanical damage to the array (fig. 10). Although eight of the individual 1 watt modules sustained bent substrates and cracked cells, the total array current loss was less than 2 percent. Subsequent inspection of the damaged modules showed that although individual cells were severely cracked and the module substrates bent, the FEP encapsulant had prevented the current grid lines on the cells from breaking. Thus, electrical output was not appreciably affected.

Figure 10 shows an interesting feature of the FEP encapsulant as observed by Forest Service personnel at Mammoth. Typically, rime ice accumulates on all exposed surfaces. It did not, however, appear to accumulate on the FEP. Rather, the ice would build up between the tower and the array and then gradually emerge in the spaces between the modules and build up over the FEP surface. During periods of clear weather, the array, with its high absorptivity, would quickly clear itself of this type of ice accumulation.

The array was originally to have been mounted near the top of the tower to preclude damage from falling ice. High winds and a lack of sufficient personnel during installation, however, forced its location as shown. The array will be raised to the top of the tower before the 1974-75 winter season.

Figure 12 shows an FEP-encapsulated module on test on a Coast Guard buoy in Boston Harbor. The Coast Guard is evaluating different types of solar cell arrays in anticipation of using them to power such buoys. The large blocks adjacent to the FEP-encapsulated module are solar cell modules manufactured by Controlab. Our test program consists of one module mounted permanently and two modules mounted for alternate

These are the projects we have in effects now. We have other FEP-encapsulated solar cell application projects planned, however. One is to power a simulated RAMOS transmitter load on a NOAA buoy moored about 60 miles east of New Orleans in the Gulf in Mexico. This particular buoy will have 110 volt power onboard, so we will be able to fully instrument our package. Another program is to build a solar cell power supply for the Inyo National Forest (California) to power a voice repeater station. This system will go on top of 14,242-foot high White Mountain Peak which is about 60 miles east of Mammoth Mountain. We're also building, for that forest, a small backpack solar array which their back-country guards will use to charge the batteries in their walky-talkies. These people go back into the woods for 2 to 3 weeks at a time to patrol the trails during the summer season. The radio batteries are too low a capacity to allow the guards to monitor continuously, thus limiting their mission effectiveness. The small solar array will allow continuous monitoring plus unlimited transmit power. We are building these arrays as a favor to the Forest Service and as another means of testing the durability of an FEP-encapsulated array.

Another project is to build solar cell power supplies for three free-drifting buoys for Langley. One will be for the EOLE buoy that was described earlier, another will be for a radar beacon buoy, and the third will be for a radio beacon buoy. These, hopefully, will be installed sometime in the middle or late summer.

In summary, that is the present extent of our terrestrial applications demonstration program. Based on our results thus far, we see no reason why the FEP-encapsulated module shouldn't meet the requirements for any terrestrial solar cell system.

QUESTIONS

Tom McKerr - Coast Guard

In addition to the solar cell array and the buoys, which seem to be working quite well, we are also working with a wave actuated generator, which is off the Chesapeake Light. It's working quite well. I'd like to point out that those funny looking things sticking up off the (Boston Harbor) buoy were to keep Jonathan Livingston's brothers away.

Tony Ratajczak - NASA/Lewis

I should have pointed that out. Interestingly, we have a lot of gulls at the lake front in Cleveland, but we've never observed the evidence of any gulls having sat on the solar cell array we have there.

Bill Vachon - Draper Lab

Do you have any rules of common cost and power per square inch or per unit that you can work on? For the oceanographic community to plan on using them, they should have some handle on what they could plan on.

Tony Ratajczak - NASA/Lewis

That's right. I neglected to mention that, and I'm glad you brought it up. The 1-watt module yields about 7.6 watts per square foot based on it total area. But you have to be careful--that is on the basis of peak power from a module aimed at the sun. Buoy installation will, in all probability, not use oriented arrays, but rather, horizontally mounted arrays. The output of a horizontal array is more sensitive to seasonal changes in the sun angle. As an example of horizontal array output, we calculate that the solar array for our experiment on the NOAA buoy in the Gulf of Mexico will generate about 11.3 KWH per square foot per year.

On costs, the people who make terrestrial systems are quoting $25 to $50 a watt. I don't know if that's what they're actually delivering them for or not. It's hard for us at Lewis to know what the costs will be on these FEP modules because we make ours in an experimental laboratory, so our costs are not realistic, and no one else is making them yet. We only have one man making modules - he works on it for a bit and then he does something else. But my guess is, if someone were making the FEP modules now in small quantities, the prices would be more like $40 to $70 a watt. Quantity orders would do a lot to bring the price down.

Quantity orders, in fact, are what the whole solar cell industry needs. Typically, NASA or the Air Force order solar cells for a spacecraft, and either of the two principle manufacturers have to, over a very short period of time, manufacture several thousand cells. So they bring in people, train them, get their production lines going and manufacture the solar cells, and then, it's several weeks or even months before they receive another major order. Well, they can't keep all their manufacturing people on overhead, so they send them home. And that's pretty much the story of their existence - up and down. They never really get a chance to run their assembly line continuously over a long period of time to achieve the economies that long-term quantity production will yield. We hope that by stimulating a terrestrial market for solar cells, the manufacturers will be able to keep their lines running continuously so that the costs to both the space and terrestrial user come down. A second benefit derives from the differing requirements of users. Space people are very fussy. Not only do their solar cells have to work properly, but they have to look perfect. For terrestrial applications that's not quite the case. Here we can use cells with small corner chips and cosmetic imperfections. Generally speaking, the terrestrial market can use a lot of cells that space community cannot use. So, by having a terrestrial market, you generate a need for the so-called second class cells that space community doesn't use. Given these conditions, the $25 to $50 a watt figure is realistic and possibly even conservative.

NASA-LeRC SOLAR CELL AND ARRAY TECHNOLOGY

OBJECTIVE: REDUCE THE COST OF SOLAR CELL ARRAYS FOR SPACE AND TERRESTRIAL APPLICATIONS.

APPROACH: DEVELOP HIGH EFFICIENCY CELLS.
DEVELOP LOW COST CELL FABRICATION METHODS.
DEVELOP LOW COST ARRAY FABRICATION METHODS.

TASKS IN PROGRESS:

ADVANCED Si CELL DEVELOPMENT	- CENTRALAB	- \$81K
ADVANCED Si CELL DEVELOPMENT	- HELIOTEK	- \$86K
ADVANCED Si CELL DEVELOPMENT	- IH	
FEP-COVERED SOLAR CELL MODULE	- TRW	- \$250K
FEP MODULE IMPROVEMENTS	- IH	
VAPOR DEPOSITED Si SOLAR CELLS	- IH	
PRIMITIVE SOLAR CELL FABRICATION	- IH	
LOW COST SILICON SOLAR CELL ARRAYS	- SPECTROLAB	- \$37K
LOW COST SILICON SOLAR CELL ARRAYS	- CENTRALAB	- \$32K

Figure 1.

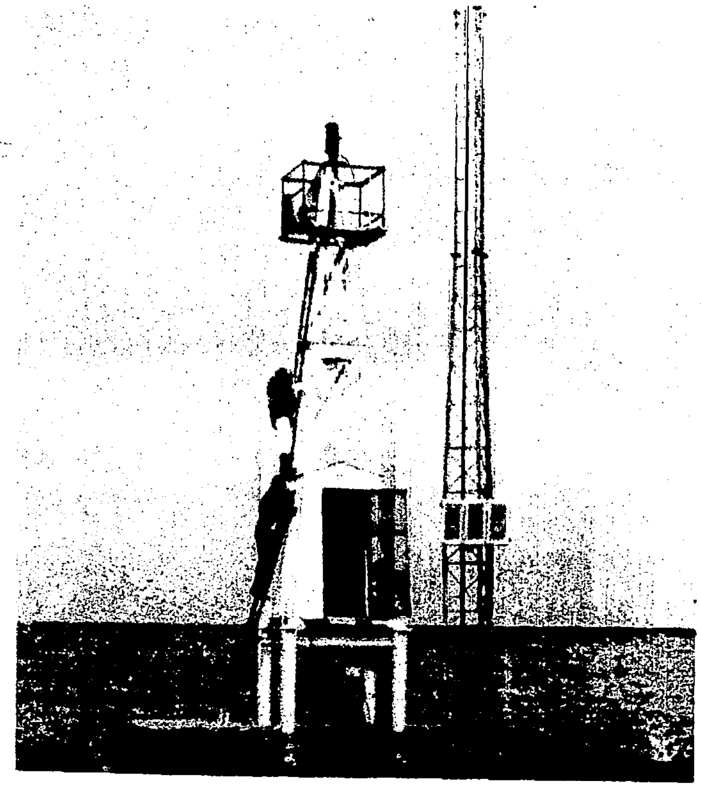


Figure 2. SOLAR CELL POWERED WEATHER STATION AT CLEVELAND, OHIO LAKEFRONT

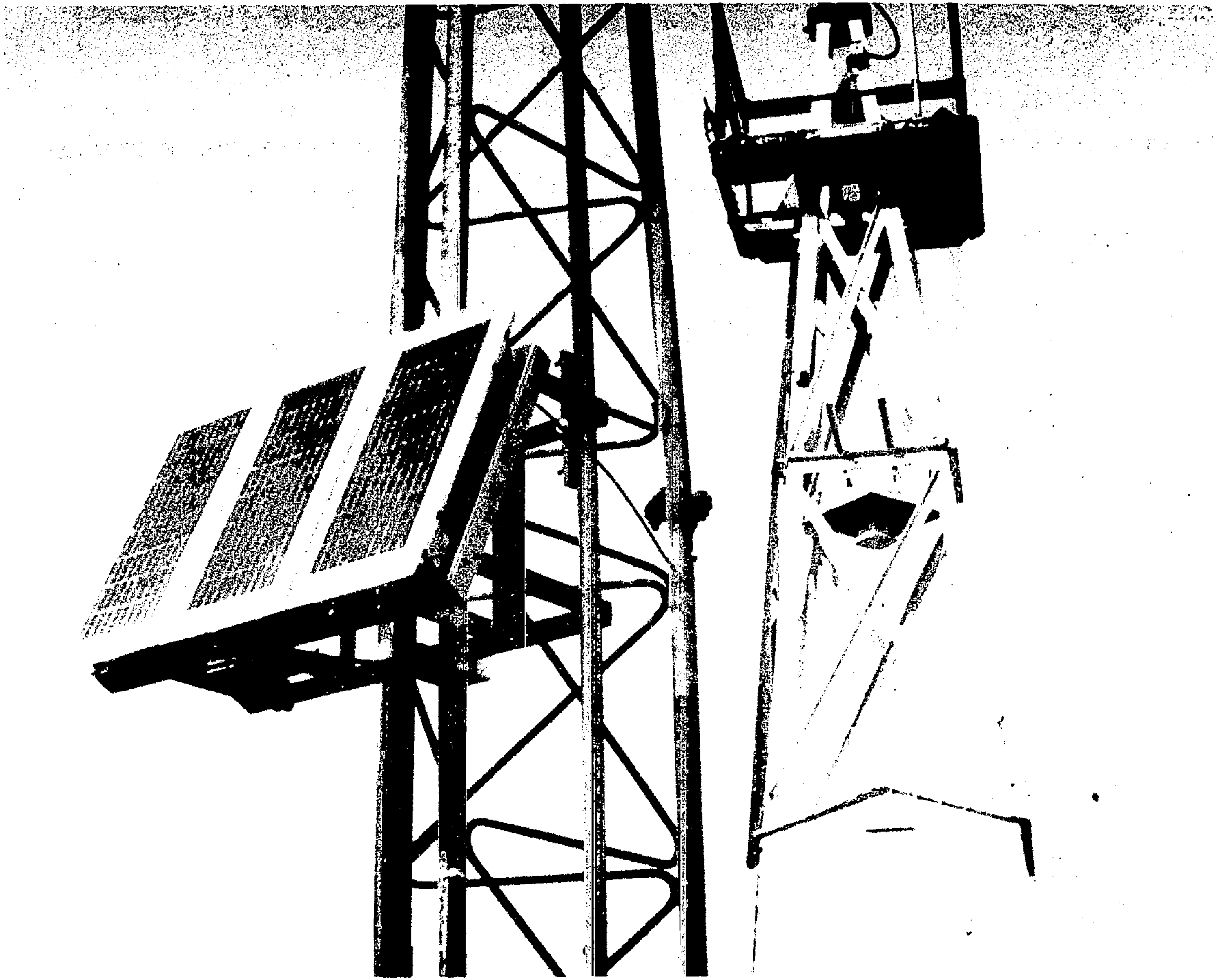


Figure 3. LUCITE-COVERED SOLAR CELL ARRAY AT CLEVELAND LAKEFRONT

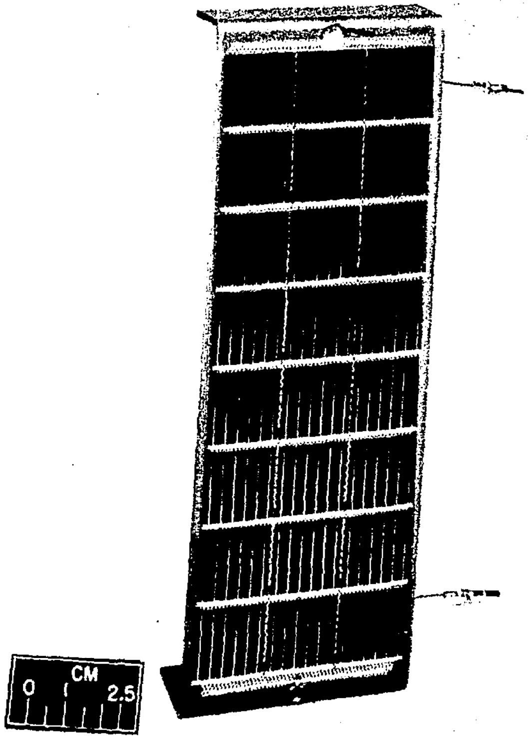


Figure 4. A 1-WATT ALUMINUM SUBSTRATE FEP-ENCAPSULATED MODULF

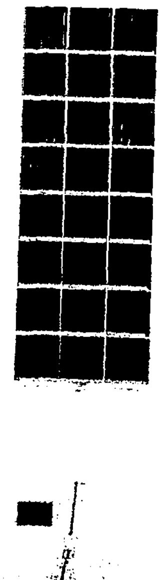


Figure 5. A 1-WATT FIBERGLASS CLOTH SUBSTRATE FEP-ENCAPSULATED MODULE

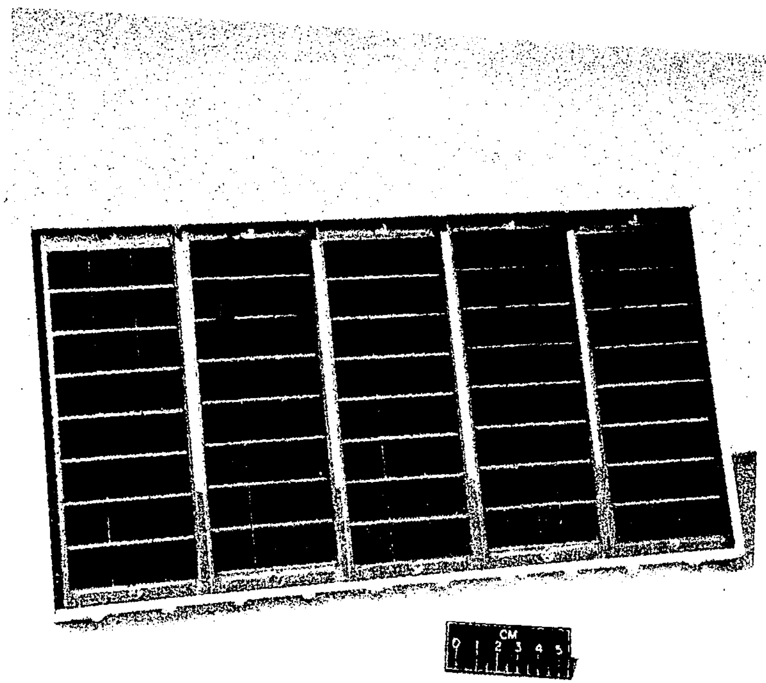


Figure 6. A 12-VOLT MODULE OF FIVE 1-WATT FEP-ENCAPSULATED MODULES

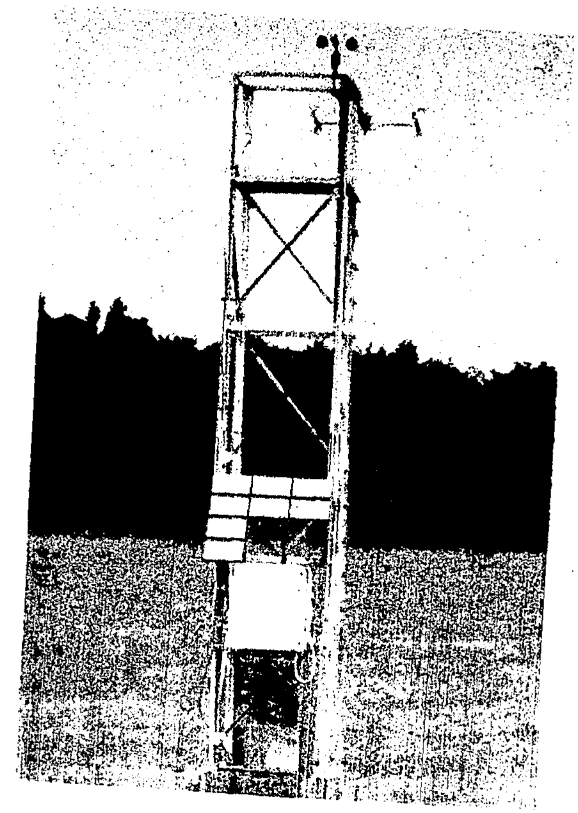


Figure 7. SOLAR CELL POWERED RAMOS WEATHER STATION, STERLING, VIRGINIA

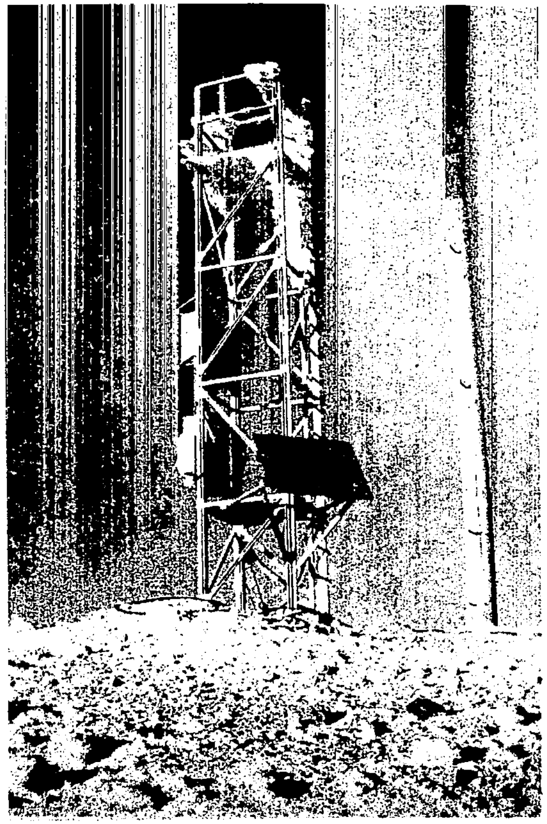


Figure 8. OLAR CELL POWERED RAMOS WEATHER STATION, MAMMOTH MOUNTAIN, CALIFORNIA

Figure 9. MAMMOTH MOUNTAIN INSTALLATION FOLLOWING WINTER STORM

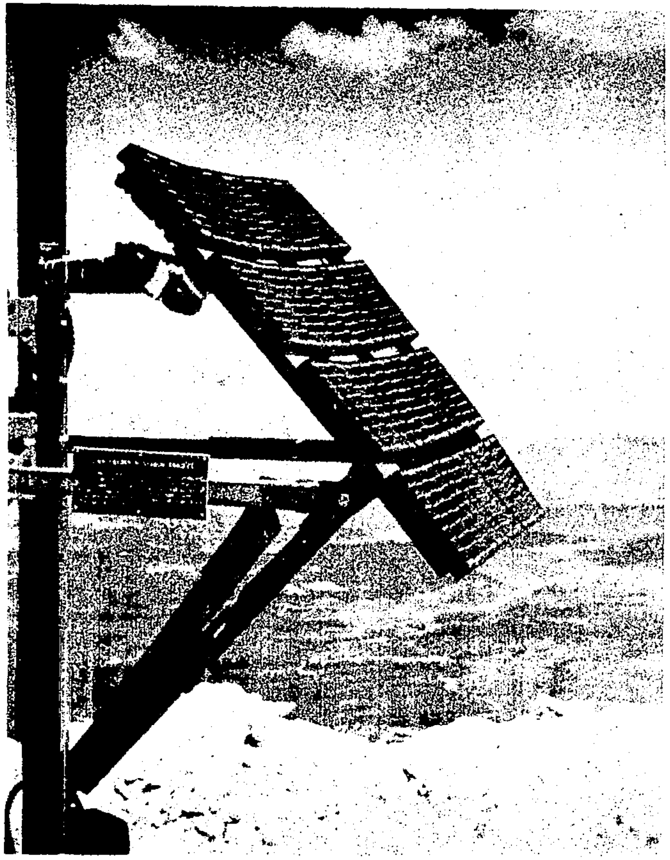


Figure 10. RIME ICE STRUCTURAL DAMAGE TO MAMMOTH MOUNTAIN ARRAY

Figure 11. DETAIL OF RIME ICE BUILD-UP, MAMMOTH MOUNTAIN ARRAY

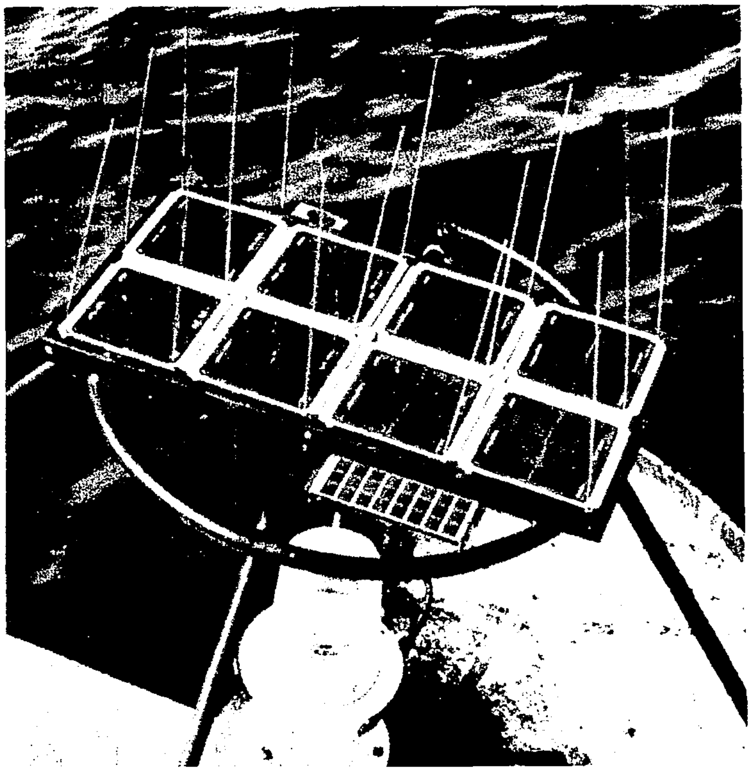


Figure 12. A 1-WATT FEP-ENCAPSULATED SOLAR CELL MODULE MOUNTED ON A COAST GUARD NAVIGATION BUOY, BOSTON HARBOR, MASS.

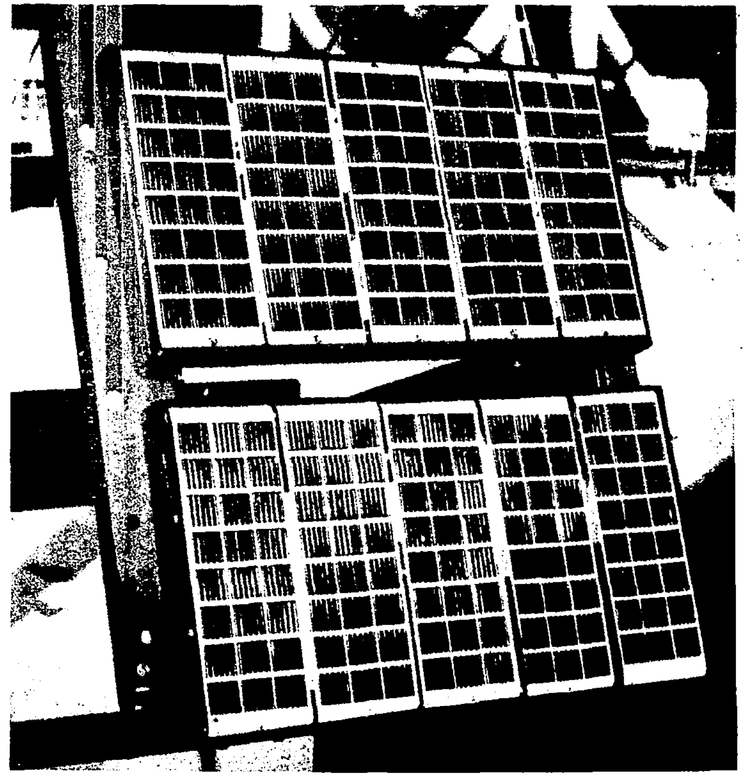


Figure 13. SOLAR CELL POWER SYSTEM EXPERIMENT OF NASA-LeRC LABORATORY ROOF. UPPER 12-VOLT MODULE CONTAINS ALUMINUM SUBSTRATE MODULES. LOWER 12-VOLT MODULE CONTAINS FIBERGLASS CLOTH SUBSTRATE MODULES.

VIRGINIA INSTITUTE OF MARINE SCIENCE (VIMS) -
NASA LANGLEY RESEARCH CENTER (LaRC)
EOLE BUOY PROGRAM

by

Christopher S. Welch
Virginia Institute of Marine Science

In late 1972, John McFall of NASA/LaRC, called us and suggested an EOLE buoy program to John Ruzecki, of VIMS. When John told me the proposal to follow buoys off the Virginia coast using a French satellite (EOLE) with our data routed through a tracking station in Africa, Paris, and NASA Goddard, I told him that was the silliest idea I had ever heard. I wish to take this opportunity to apoligize to both gentlemen and say that I am glad you prevailed.

The program has been opportunistic in nature because its end, caused by the failure of the EOLE satellite which has not yet occurred, has been uncertain. Under this constraint, we have devised a program which will be presented in three parts: the available equipment, the area of our specific interest, and results of partial analyses to date.

The Author: Dr. Welch received his Bachelors Degree from Stanford University in 1966. He received his Ph.D. under the MIT-Woods Hole Oceanographic Institution Joint Program in 1972. He is presently an Associate Marine Scientist at the Virginia Institute of Marine Science and an Assistant Professor at the University of Virginia and the College of William and Mary.

Figure 1 shows the EOLE buoy with its drogue and adjustable length chain linking the two. The buoy contains the primary floatation, instrument and battery case, three antennas, handling bail, marker light, and a solar switch. The electronics package contains the primary EOLE responding circuitry and sensor, converter, and multiplexer circuitry for telemetering data from four thermistors to the satellite on command. Also, there is a command decoder which can accept a single command relayed from the base station via the EOLE satellite. In the LaRC configured buoy, this signal is used to activate a back-up recovery beacon. The main recovery beacon is controlled by the solar switch to operate only during daylight hours and so conserve battery power.

The EOLE buoys were launched and recovered from a variety of small ships using the mechanical design of Leon Williams of LaRC. The job from VIMS's 55-foot R/V Pathfinder was marginal, but possible in calm weather. On the 90 foot R/V Annandale of the Delaware Research Consortium, made available to us through NASA Wallops Island Station, the job was feasible in nearly all weather.

Data from this system are received in two to five sets of bursts during a 12-hour period, the same format as described by Hanson (1974). Accuracy was specified at about 1 kilometer and depended strongly on the relative positions of the buoy and the satellite orbit.

We were offered the opportunity to use this drogued buoy remote navigation system for an undetermined length of time with up to five buoys over the local continental shelf.

The local shelf region is somewhat different from others we have heard described today. Figure 2 is a chart of the area with several hydrographic features illustrated on it.

With an abrupt southern boundary at Cape Hatteras, the shelf circulation extends as a hydrodynamic entity north to Cape Cod. We confine our studies to about the southern 150 kilometers between Cape Hatteras and Delaware Bay, where the width is about 100 km. The Gulf Stream forms a definite eastern boundary to this area trending about northeastward from Cape Hatteras. A typical depth over the shelf is only about 40 meters. The hydrography is dominated by two primary water types, shelf water and slope water, with the mouth of Chesapeake Bay as a large fresh water source in the center of the area. Over half of the fresh water entering the northeastern shelf systems flows through the mouth of Chesapeake Bay. Figure 3, prepared from a report by Norcross and Stanley (1967), depicts a seasonal cycle of temperature in a transverse section across the shelf near the mouth of Chesapeake Bay. The station locations are shown in figure 2. The four sections shown in the figure illustrate the seasonal cycle in this region. There are two primary hydrographic states over a year's time, a vertically straitified one during the spring and summer and a horizontally straitified one during autumn and winter. The former state is characterized by warming, while the latter is characterized by cooling. The transitions between these two states occur quite rapidly, certainly in less than a month's time. As might be expected, the circulation in response to a given stress differs between the hydrographic states.

In order to study this area using the system which was made available to us, we designed and ran seven separate deployments of the buoys between September 1972 and February 1974. The schedule for these is shown in figure 4 as an event diagram. The individual deployments of EOLE buoys lasted between 3 days and a month using from one to four buoys. Those not recovered are represented in figure 4 by dotted lines.

The deployments were arranged in three separate experiments. The first deployment was devoted to an engineering test of a single buoy. Deployments II through VI were devoted to a statistical description of drogued buoy trajectories originating near the mouth of Chesapeake Bay with drogues set at a depth of 5 meters. Deployment number VII was devoted to following an initial line of buoys drogued at mid-depth with a Langrangian hydrographic survey. For this last experiment, the EOLE buoys were augmented with several radio location buoys.

The data are not all in from these deployments yet, and those that are in are not all analyzed yet. There are some analyzed results which are worth reporting. Figure 5 shows pathlines with dates of launch and recovery for the buoys from deployment IV, part of the statistical description experiment. The buoys, drogued at 5 meters, were deployed on February 19 in a small cluster near the Chesapeake Light Tower. They stayed close to that position for about a week, suddenly headed to the south, and turned an exceptionally sharp corner to the northeast as they got caught in the Gulf Stream. This behavior seems to be characteristic of the winter hydrographic state.

It is important to note that the buoys actually did pass from one water type to another when they entered the Gulf Stream. This was shown dramatically on temperature recorders attached to the drogues, which were later recovered. It points out that a drogued buoy is Eulerian in the vertical even if Lagrangian in the horizontal, and that vertical motion cannot always be neglected.

The motion of the water at 5 meters leaving Chesapeake Light Tower during the winter hydrographic state, then, seems to be rapid and intermittent in nature and to the south in what may be a narrow coastal jet. That water passes beneath the Gulf Stream at Cape Hatteras, and the time it takes to travel from Chesapeake Light to Cape Hatteras is about 2 weeks, from our preliminary analysis During the only summer state experiment for which we have data (V), the buoy never left the vicinity of Chesapeake Bay mouth. The batteries finally ran down after about 60 days with the buoy never having gone more than than a few tens of kilometers from its original position.

A more detailed analysis has been performed on the data from experiment III, due in large part to the efforts of John Wallace, of LaRC. These data will be shown to illustrate both the character of the flow field and some of the characteristics of the EOLE navigation package. The general motion was similar to that in experiment IV, just examined. Because the greatest displacement was parallel to the coastline, only the north-south component of displacement is shown as a function of time in figure 6. The irregular nature of the sampling is apparent in this figure, locations from successive orbits being connected by straight line segments with intermediate estimates of position represented by dashed lines. The plotted jogs to the north which occur on single orbits in the positions of all four buoys are almost certainly artifacts of the measurement rather than actual location changes. The vertical arrows in the figure indicate satellite passes nearly overhead, as indicated by a coincidence of the actual and ambiguous positions calculated by the EOLE data center.

Turning our attention to the interpretation of the data rather than its quality, we note the intermittency of the southward motion in figure 6. Roughly 90 percent of the total displacement occurs during about 30 percent of the time. The steepest slopes not associated with the position jumps correspond to a velocity of about 2 knots. We have not yet correlated these Lagrangian data with local and shelf-wide weather conditions, but expect to do so when the data are all in.

The data from the same experiment are presented in figure 7 as a single track of the centroid of the four buoys with orbit number as the time parameter for the curve. The motion exhibits an initial trend towards the northeast, lasting for 29 orbits and then seems to stagnate for 44 orbits in a position directly off the mouth of Chesapeake Bay. It then heads south on its way to Cape Hatteras. The motion seems to change character at orbit 6843 from random to directed.

A dispersion index of the four buoys was constructed by calculating the mean square displacement of the four buoys from the centroid. This index is plotted as a function of time in figure 8. The lines through the points are drawn by eye. A scale change was made at orbit 6843 in order to accommodate the sudden and rapid change in the dispersion rate, the initial line being represented at the expanded scale by a dashed line. The first point of note is that a single such line fits the points moderately well initially. The slope of such a line is related to a theoretical dispersion coefficient, the number obtained from these data being 2.5 x 10^6 cm^2-sec^{-1}. This range of other dispersion coefficients quoted for small enclosed seas, about an order or magnitude layer than those usually found in estuaries, and an order of magnitude smaller than open ocean values. After orbit 6843, the dispersion is associated with a strong straining deformation, the dispersion in the direction of the flow increasing greatly and the cross-stream part actually decreasing. At the end of the experiment when all buoys are on board the retrieval vessel, the calculated dispersion is o.4 km^2, an indication of a relative position accuracy of 0.6 km.

Experiment VIII was our first attempt to follow a line of buoys and observe the changes of the hydrography along that line. In the end, we were not able satisfactorily to distinguish between the radio beacons of the various buoys, and so follow the deformed line. From the sightings we did obtain, it was apparent that the initial line soon became greatly deformed, making rapid hydrography along it impossible in any case. We did run two standard east-west hydrographic sections from the shelf edge to Virginia Beach. They indicated that the region into which we placed the buoys was, at the time, hydrographically complex and by inference dynamically energetic, so we may have stumbled on a difficult time to attempt the experiment. Two of the buoys, which were drogued at 20 meters, have recently been reported in the vicinity of Bermuda, where attempts are underway to retrieve them

The VIMS-LaRC EOLE drogued buoy program has, to date, been a productive experience in several regards. We have obtained a fair amount of experience at relatively low cost in handling buoy systems. The capability of satellite-linked systems in this field has been demonstrated. Finally, we have come to appreciate the unique capacity of drogued buoy systems to gather a large amount of information for each data point. We have learned things about the shelf circulation which would have required a formidable effort to learn using any other approach.

EOLE BUOY

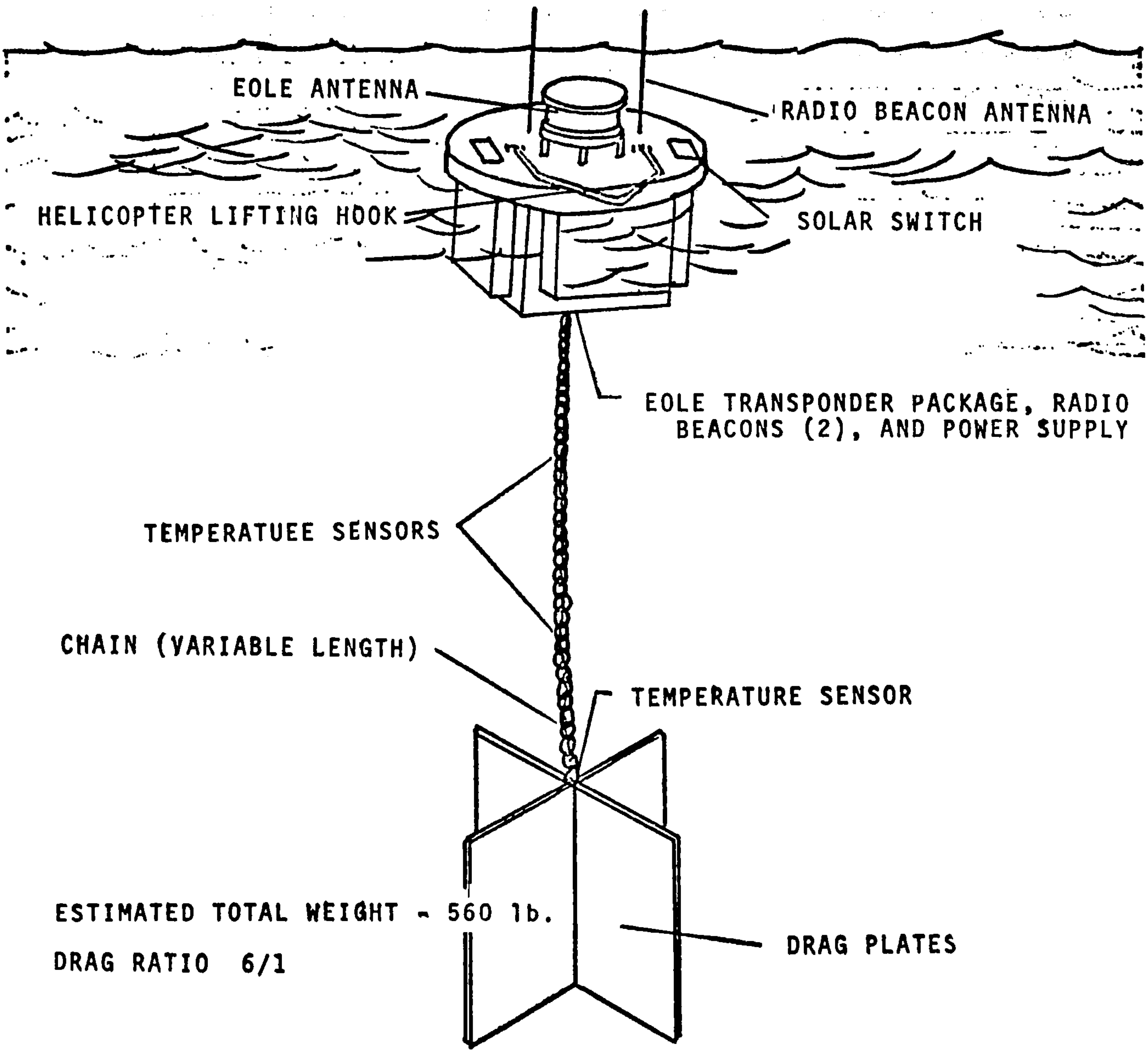


Figure 1. FREE-DRIFTING EOLE SATELLITE INTERROGATED BUOY

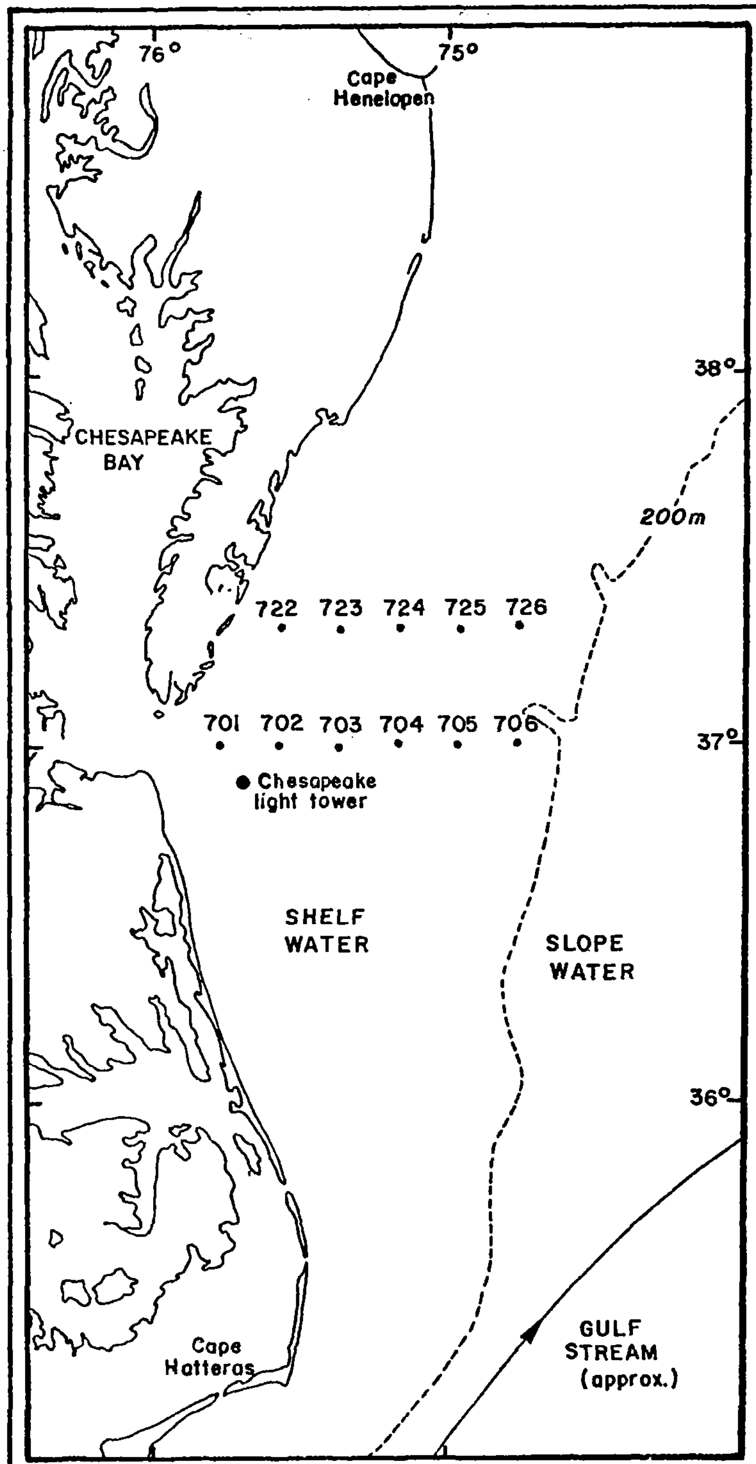


FIGURE 2. BATHYMETRY OF THE CONTINENTAL SHELF, CAPE HATTERAS, NORTH CAROLINE TO CAPE HENELOPEN, DELAWARE. (Station positions refer to hydrographic sections in the following figure.)

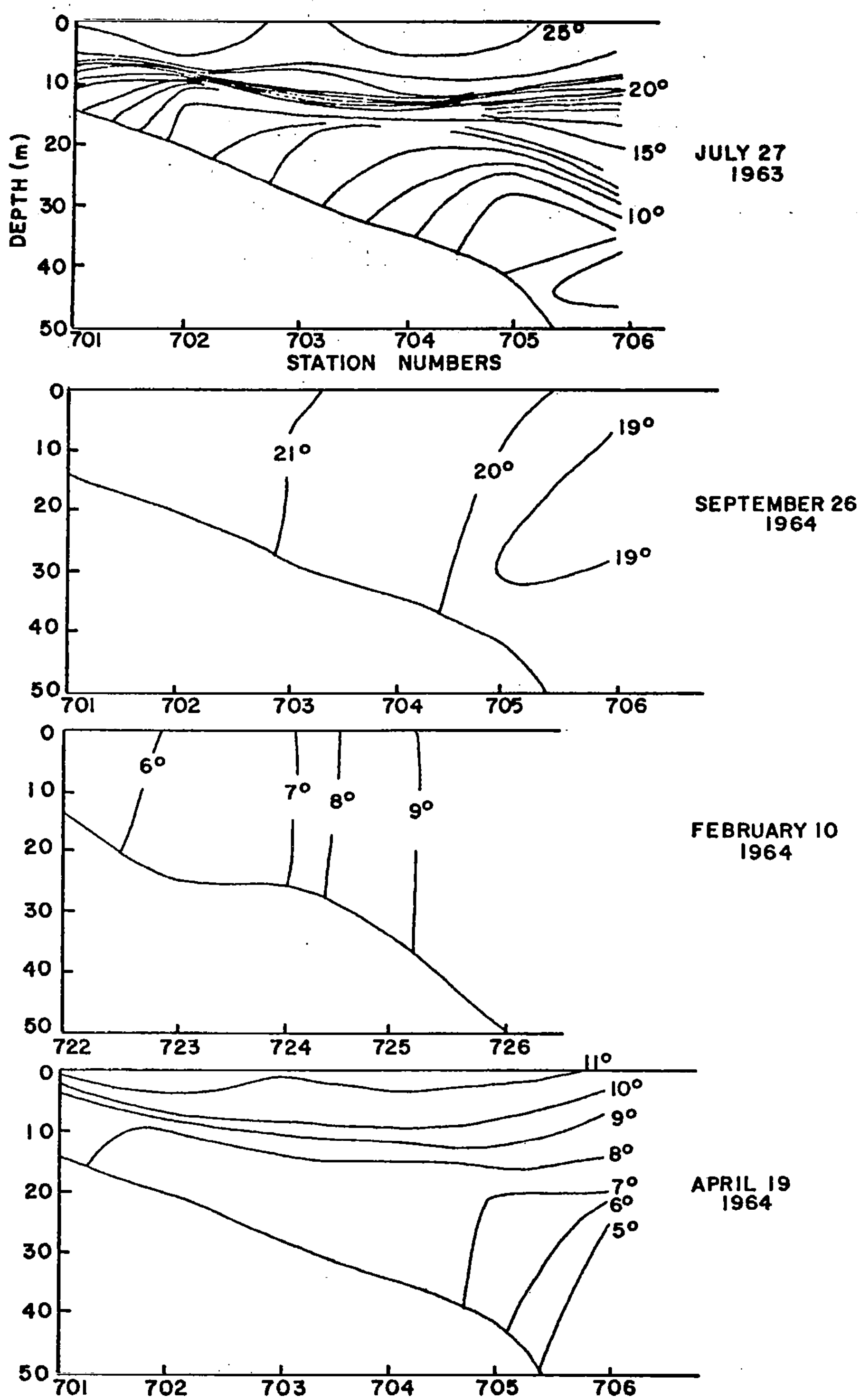
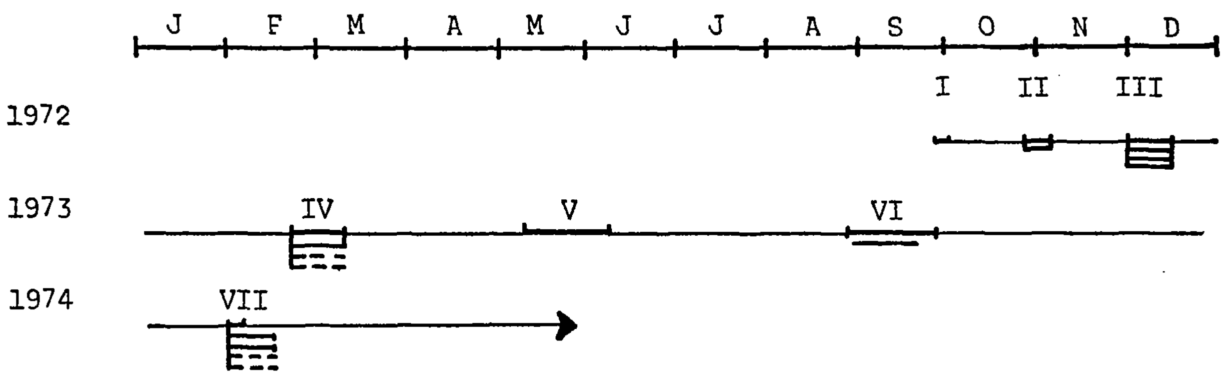


FIGURE 3. TEMPERATURE SECTIONS ACROSS SHELF ILLUSTRATING SEASONAL TEMPERATURE CYCLE (after Norcross and Stanley, 1967).

EOLE BUOY DEPLOYMENTS



Each line represents a deployed buoy.

Dashed line correspond to buoys not retrieved.

FIGURE 4. EVENT DIAGRAM OF EOLE BUOY DEPLOYMENTS TO DATE

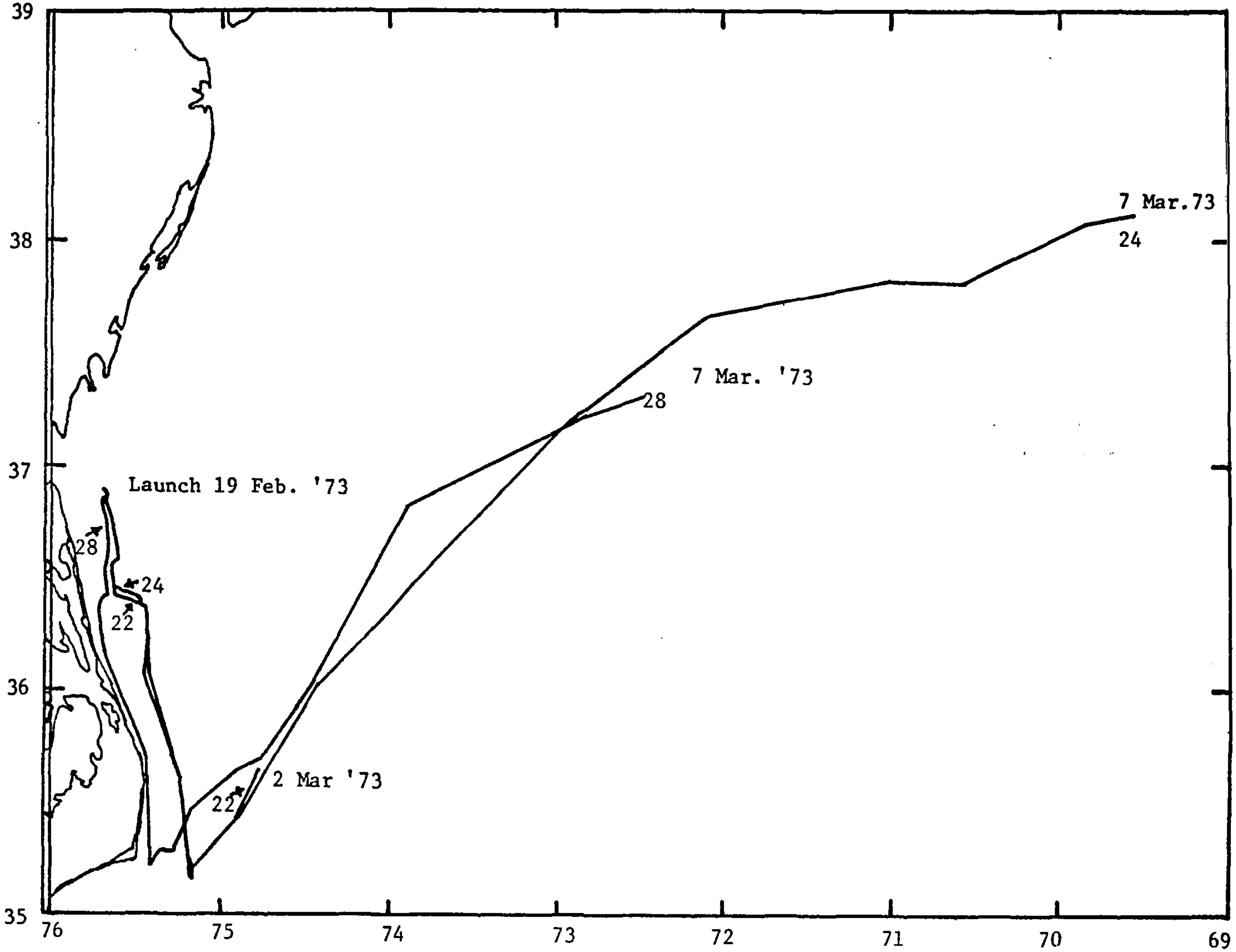


FIGURE 5. PATHLINES OF BUOYS LAUNCHED NEAR CHESAPEAKE LIGHT TOWER DURING DEPLOYMENT IV

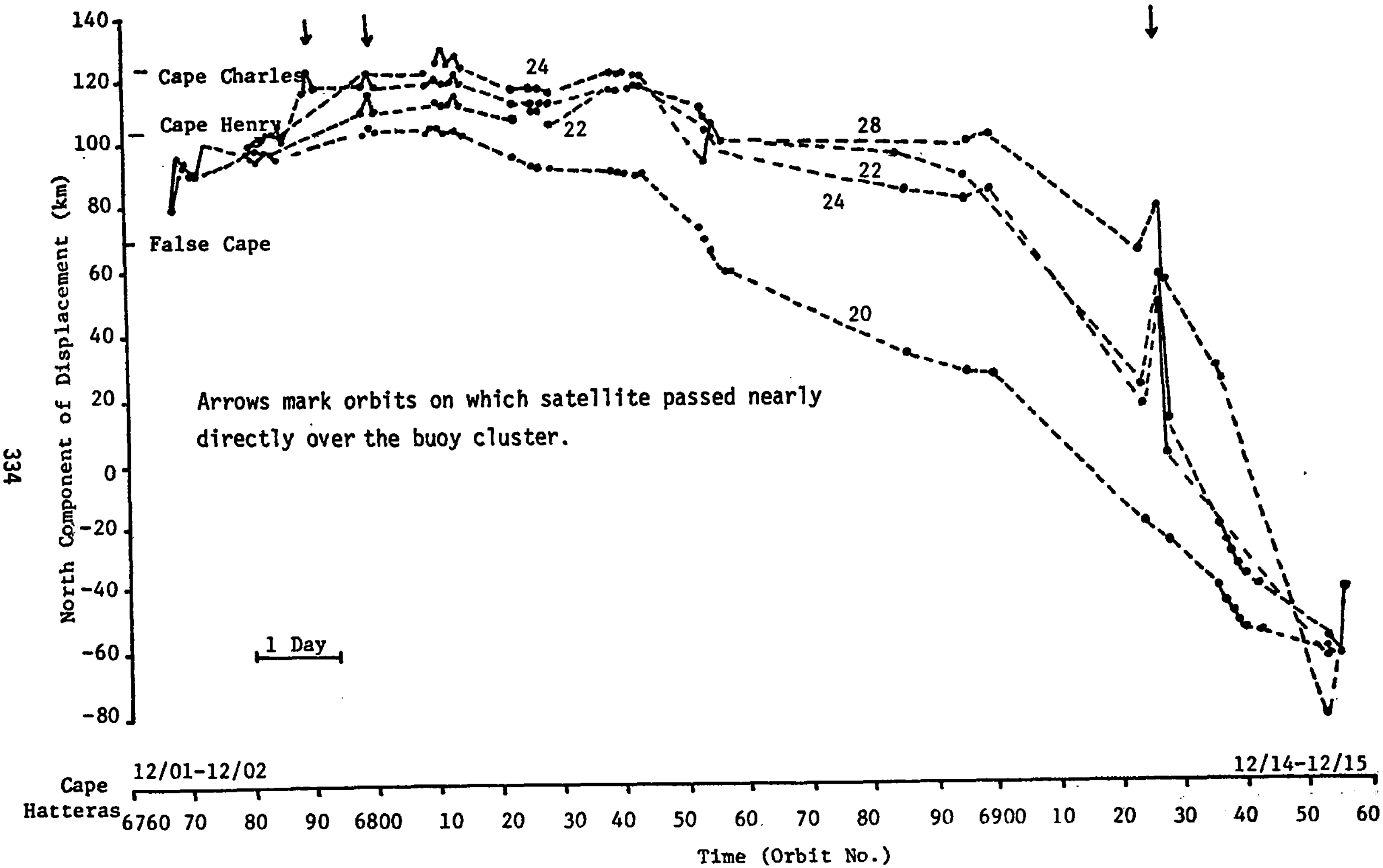


FIGURE 6. NORTH-SOUTH DISPLACEMENT OF FOUR EOLE BUOYS FROM DEPLOYMENT III AS A FUNCTION OF TIME

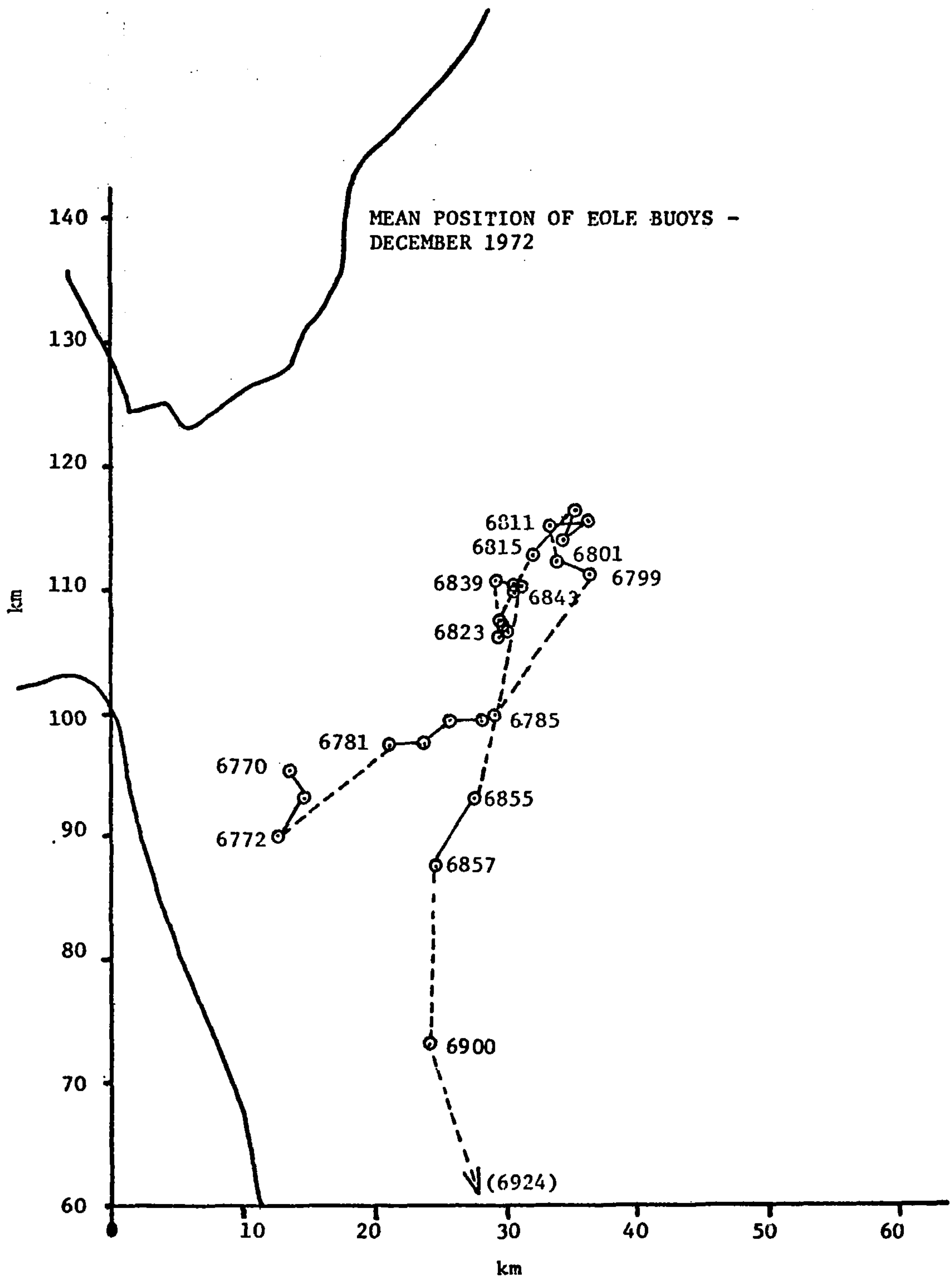


FIGURE 7. CENTROID TRACK OF BUOY CLUSTER IN DEPLOYMENT III. (Orbit number is parameter of track)

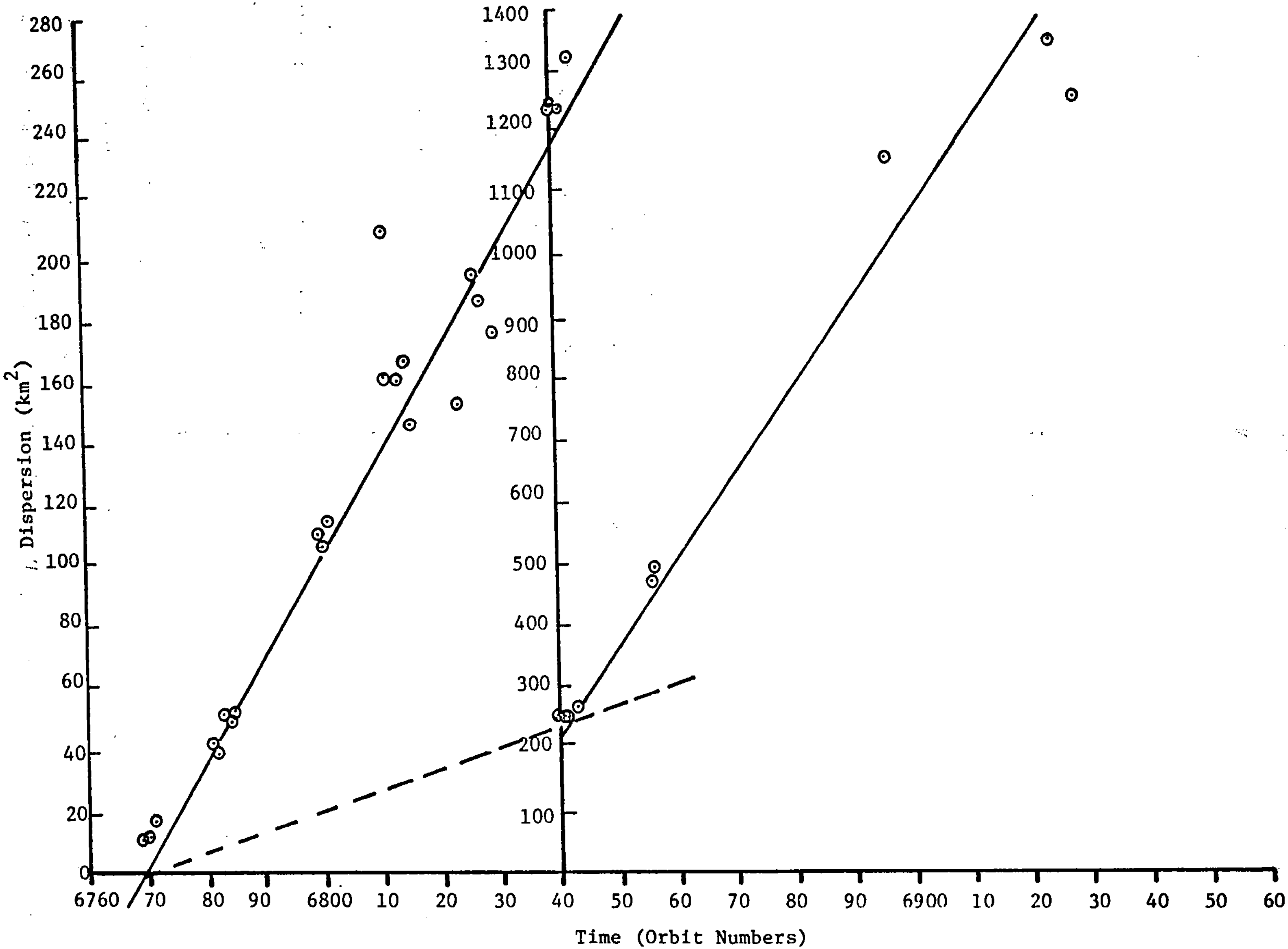


FIGURE 8. MEAN SQUARE DISPLACEMENT OF BUOY CLUSTER FROM CENTROID IN DEPLOYMENT III AS A FUNCTION OF TIME

DRIFT BUOY EXPERIENCE IN THE POLE EXPERIMENT

by

Gerald McNally
Scripps Institute of Oceanography

Denny Kirwan and I, in an effort to enhance the NORPAX POLE Experiment, undertook to put out some longer term drifters than those that Jim Barnett described to us this morning. Since we were locked into the POLE Experiment and the time frame of the POLE Experiment, we had to look for some way of remotely tracking the buoys other than the elegant system that will fly hopefully in November. Through the Office of Navel Research we came upon the over-the-horizon radar which at the time we were not aware of. We deployed six buoys and three of which were coupled to the surface, and three of which were the NOVA buoys of Bill Richardson's design which we drogued at 30 meters. The idea was that we would try to get some idea of the shear in the very early going in the experiment where the buoys would be fairly close together and in the long term some idea of how representative a surface drifter vs. a drifter at 30 meters would be. Each drifter was equipped with an over-the-horizon radar repeater and the only other sensor we chose to put on was a device to monitor the battery voltage because there was some question about the power consumption. It turns out the harder you hit the repeater the more it tries to talk back to you. You can limit the power of say 3 watts, but what's the average drain over a long period of time? It becomes a function of how hard its hit. The buoy also doesn't know the difference between other RF transmissions and the correct one. Somebody nearby starts to talk and the buoy tries to answer. We also inhibited the buoys so they only operated for 6 hours every day. This in part was necessitated by the power and the other part was that there were other experiments being conducted simultaneously within the POLE Experiment. One was a Back Scatter Experiment also using the over-the-horizon radar. The two unfortunately interferred with one another so that we were limited to operating 6 hours a day. We picked what we thought would be the optimum time for the transmissions that we needed for the propigations.

The over-the-horizon radar as I said was pretty new to everybody. We had some estimates of the accuracy that we could expect, which was approximately 10 miles by 10 miles. That was their best estimate. We decided that if we're going to do the experiment we ought to check the accuracy. We were fortunate because we did have a fixed position: it was Flip, the research vessel which Scripps operates. It has a satellite navigation receiver on it and we installed a radar repeater on it also. We also installed a repeater on the Washington, which was the support vessel during the experiment. Now the Washington and Flip would separate by as much as 60 miles. We thought we could get some idea of what the differential accuracy was, what the absolute accuracy was and what the differential accuracy as a function of separation between drifters was. This is another reason that we chose to use a cluster experiment, because absolute accuracy of 10 miles is just about on the fringe of usability. However, we thought perhaps we could get differential accuracy, at least after speaking to the people at SRI, that's Stanford Research Institute, who operate it. They said we might expect differential accuracies of 2 1/2 miles, which is far more appealing. The result of the check of absolute accuracy, which was to take simultaneous fixes with the over-the-horizon radar and satellite navigation verified their 10 miles. And that was over the course of 3 weeks and about 15 fixes. The differential accuracy never could be determined. It turned out the two repeaters that were on the vessels were so bloody strong that they interferred with one another and the radar couldn't handle both of them simultaneously.

It became academic because of the six buoys deployed only two were actually tracked. We managed to almost destroy one during deployment. The antenna was pretty badly beaten against the side of the ship. The quick release hook hung up and as the ship rolled the buoy crashed into the side. The surface drifters which were built at Scripps were tracked for 1 day, and they were never seen again. It was difficult to tell what happened. There was some confusion about what SRI was tracking. They were reporting that they were tracking one of the surface drifters and it turned out it was an interference pattern between two of the other drifters. When we instituted the search to look for the missing buoys, we told SRI to look for them to the east of the deployment location because everybody knows that the North Pacific in that part of the world moves to the east. We also told them to look for the one that most likely was the one they'd been tracking. Well, we were wrong on both counts. They never did see the number 4 buoy; it was a mutual interference pattern, and all the indications were that in that first 3 weeks in the POLE experiment the flow was to the north--slightly to the west in our case, although Tim, I think, found it slightly to the east. Also, over-the-horizon radar operates on ionospheric propogation. It just so happened the location of the POLE experiment was right at the break point between one hop and two hop propogation. They also had considerable propogation losses at the time of the experiment because of magnetic storms. These two things combined gave us a lot of difficulty in the early going. It's also another possibility that we just plain lost the surface drifters and we don't even know when the event took place.

I should point out something about the searching for these buoys. The over-the-horizon radar has about a 10 X 10 mile footprint. Contrary to what you've already heard, if you look at a buoy and you knew exactly where it was and you pointed the radar at it, there's no guarantee you're going to see it, because of atmospheric fading. So if you did find a buoy it would come and go periodically, which means if you institute a search you've got to look at each 10 X 10 square mile for something in the order of several minutes to be absolutely sure. If you start looking in an ocean for a little tiny buoy, it is quite a problem.

The two drifters that did work are the two Nova Drifters. One lasted for exactly 1 month, and the other we're still tracking. The general results were that as Tim pointed out, in the early going of the first 3 weeks, these things went north at about 10 centimeters per second and then they headed east and west, but the east-west movement almost cancelled out in the 3 weeks. This means the track was almost due north. When they got to 36 1/2° latitude they suddenly broke to the east and we discovered the North Pacific drift or something like that. The one that is still being tracked drifted that way for 2 months at a speed of something like 18 or 19 centimeters per second. In the last month it has just about stopped. It has not moved more than 40 miles in 1 month and the last fix I got from it showed a slight westerly drift. I thought it was interesting that both drifters did not turn to the east until they got to 36 1/2° latitude. It took one drifter more than 3 weeks to get there and it took the other almost a month to get to the same point.

The battery voltage that we're getting back from the drifter would indicate that we could track it for another several months. The one Nova Buoy that did disappear had a very healthy battery voltage so we can't attribute its loss to that. I think it really points up a need for some other indication of the health of the buoy. I think the one that comes to mind is the one that comes out of the result of what Dennis was talking about this morning with the drogue. You have a drifting buoy out there and if you don't know if the drogue is on or off you have two entirely different animals. You must know whether that drogue is on or off. There has to be a sensor on there that gives you this information, or you're really in for a hard time trying to reduce your data.

We manage to get three fixes a week, and it's still out there working. It wasn't exactly a total disaster although it came very close.

I apologize for not having slides so I could show you the data but I think you might enjoy a film because it does show the buoys. I think if you don't have any questions I'm just going to turn this over to Dennis Kirwan and he will show the film.

Dennis Kirwan:

As Jerry mentioned we had two types of drifters which were launched; three of the Nova drifters and three of the surface drifters, which were coupled to the surface. The idea was that we could use each of these three types to go through this sort of analysis of the kinematics that Bob Molinari described. In addition, by noting any differential velocities between the two sets of drifters, we could get some idea of the vertical shear. As Gerry indicated these two goals of the program were not met due to technical difficulties. I guess we might as well go ahead and start the film and I'll just sort of comment as we go through this. (A film was shown on the deployment phase of the six buoys.)

PLANS FOR EXPENDABLE BUOYS IN THE GLOBAL ATMOSPHERIC RESEARCH PROGRAM

by

John E. Masterson

National Center for Atmospheric Research

It is indeed a pleasure for me as a meteorologist to show marine scientists and oceanographers some of our plans, problems, and hopefully some solutions that are generated in the Global Atmospheric Research Program (GARP) and its First GARP Global Experiment (FGGE) which will be conducted in the later 70's. My discussion will cover those requirements for the Global Experiment that can be met by expendable drifting buoys and a brief description of the satellite system, and also a word on the system that's proposed for the Global Experiment again in the late 70's.

The Author: Mr. Masterson is presently on the staff of the Global Atmospheric Research Program (GARP) group at the National Center for Atmospheric Research (NCAR). While assigned by NCAR to the Goddard Space Flight Center, he was concerned with the development of the Random Access Measurement System (RAMS) as a measurement tool for the First GARP Global Experiment.

Before joining NCAR in 1966 he was Head of the Environmental Sciences Division of the Range Development Department at the Pacific Missile Range, Pt. Mugu, California, where he was responsible for the development of techniques, instrumentation, and systems for environmental measurements. As chairman (1960-1962) of the Meteorological Working Group of the Inter-Range Instrumentation Group (IRIG), he was one of the initiators of the National Meteorological Rocket Network.

Since 1960 development and testing of instrumentation through in situ measurements of the atmosphere and ocean have progressed well beyond the experimental stage, and we are now on the threshold of using a large number of inexpensive platforms immersed in the atmosphere and the ocean to measure variables necessary for experiments ranging from small to large to global. Today we've heard of some of the smaller experiments and spatial scales described.

The combined system of the polar orbiting satellite and the expendable drifting buoy offers opportunities to oceanographers and marine scientists of many disciplines to augment aircraft, ship, balloons, buoy, and satellite observations over the broad areas of the ocean. During the global experiment in the later 70's, perhaps starting as early as mid-1977, polar satellites will locate and collect data from constant level balloons and drifting buoys as well as provide remote soundings including temperature profiles. With respect to the requirements for the global experiment, the most crucial variables at the surface of the ocean for GARP are the measurement of atmospheric pressure to + or ± 1 millibar and the measurement of the sea surface temperature, which is an immersion temperature not radiative temperature. The reasons for these measurements are as follows:

A determination of the state of the atmosphere depends on the measurements of temperatures by satellites in combination with surface based observational networks. Land stations - especially in cloudy areas - provides an adequate coverage to map the meteorological variables in the northern hemisphere; however, in the southern hemisphere there are insufficient land based stations, few ships and fewer aircraft observations to provide comparable overlap with radiometric temperature soundings. For example, as you know the land area between 35 and 65° south is less than 6 percent of the total area; therefore, additional surface observations of atmospheric pressure and sea surface temperatures are required in the southern hemisphere. These will be obtained from constant altitude balloons at about 14 kilometers measuring pressure, temperature and altitude and complementing these measurements will be drifting buoys in the region between 50 and 65° S.

Figure 1 indicates the increase in the RMS wind error with respect to latitude, and you see that we have an error here of about 3 meters per second in the lower latitudes, and it improves a little bit in the mid-latitudes, but the wind error rapidly becomes intolerable for the global model from 40 to 65°. The reason for this is that the satellite Tiros provides an infrared (IR) sounding down to the clouds but not through them in this region of persistent cloudiness. In order to construct a satellite temperature sounding here, we need a reference point--called the pressure reference level. It can be obtained from a constant level balloon which is randomly dispersed throughout the hemisphere or from drifting buoys on the surface of the ocean, measuring pressure and water temperature. Air temperature is desirable but is not required in the scheme of the modeling as much as air pressure and water temperature.

In addition to providing measurements of atmospheric pressure and temperature, in situ measurements from buoys will contribute substantially to specific oceanographic investigations of much longer time scales than the atmospheric, i.e., months and weeks versus days--and spatial scales being much smaller than the atmosphere--10's versus 1000's kilometers. The system will also provide synoptic observations for National Weather Services where there is a paucity of data over the broad ocean areas--for example, southwest of Australia--for a better understanding of the physical basis of climate--the second objective of GARP. The first objective of GARP is to understand, study and to find the larger fluctuations in the atmosphere which control the water in order to extend the range of forecast to 10-14 days. The second objective is a better understanding of climate. One of the major elements influencing climate is the exchange of energy between the oceans and the atmosphere. The ocean storage and transport of heat are critical to the atmospheric heat budget. Temperatures at the surface of the ocean and at depths of the mixed layer are critical variables. The study and understanding of the climate is, and I can't overemphasize this, a vital and important part of GARP. These measurements in the ocean cannot be obtained by remote measurements from satellites. Therefore, you must have an in situ platform.

Figure 2 illustrates the use of the expendable drifting buoy that is to measure temperature and currents in the mixed layer to meet the needs of the global experiment. This curve is taken from al Eltann STD trace. What we have here is the pressure and the temperature measurement to meet the first GARP objective of pressure and temperature measurements at the surface of the ocean; secondly, these measurements through the mixed layer will assist the numerical modelers in establishing a data base for understanding climate.

With respect to the satellite buoy system which Chuck Cote described yesterday, I'll touch on some of the highlights of the system. It involves the polar-orbiting satellite, expendable drifting buoys, data communications, processing, analysis, and dissemination.

Figure 3 gives a little bit of a history of how we resolved this system, The significant part is that back in 1964-69 we were under the process of developing the concept, submitting proposals for technology and feasibility and some scientific experimentation. The packages weighed a lot - 50 kilograms for the first ones we flew on balloons with the OPLE system. The cost of these in power was 25-30 watts. The cost per package as Chuck Cote mentioned yesterday was \$50 - \$35,000, but this has decreased. In November 1974 for the Nimbus-F Twerle experiment, we'll have ballons with a package cost of \$2500, which is a balloon and the complete experiment. They will be launched from four stations in the southern hemisphere. Likewise, we'd expect the buoy cost to come down. Hansen mentioned something like \$10,000, and we're looking for something better than that when we get into the Global experiment in the 1970's with the operational satellites.

I might mention here that the cost of the balloon at $2500 (flight train inclusive of sensors and electronics)--and this is the way of looking at this--the balloon lasts, for example, 250 days on an average. The longest balloon flight we've had in the southern hemisphere has been 744 days. But at $2500 the Nimbus-F launched in January of this year will average about $10 a data point. If the buoy costs $5400 and lasts for 6 months or approximately 180 days and provides data twice a day, that's only $15 a data point. That certainly eclipses the data costs that were mentioned when a ship sent out.

The position of the buoy platform, am important variable, is derived from the relative motion between platform and satellite as determined by the measurement of the frequency change of the signal received from the platform. Three or more transmissions are required during the satellite overpass to provide the platform position--no platform velocity is involved. In the case of the ballons, we would like to have two consecutive orbits. The accuracy for the position of a balloon that is moving is about 5 kilometers RMS. A buoy may be more accurate in that since the velocity is not very great, it can be considered almost a stationary platform. In response to an invitation by NASA Headquarters, namely the Nimbus Program Office, a number of national and international investigators proposed programs using the Twerle/RAMS system on Nimbus-F in 1974 and 75. These investigations pertinent to research of the organizations. The proposals for drifting buoys and platforms are in the Pacific, the Antartic, the Artic, the Indian Ocean and on figure 4 are those who have shown interest and have assigned addresses. This figure is not complete, but shows you the spectrum of interested people who have responded to this invitation as instigated by Twerle Management. The circles indicate the approximate number (I know is some cases this had changed). This is

John Garret from the University of British Columbia, he will have 10 buoys in the North Pacific; here is John Knauss's experiment proposed to look at some of the ring eddys that were described this morning; Don Hansen's 36 buoys have gone by the boards for Gate I understand because the Nimbus satellite has been delayed. It will be launched in November rather than last June as officially mentioned. The meteorologists are working with GARP and the Global experiment and are concerned primarily with buoys in the southern hemisphere. A very interesting experiment is by George Presswell who has increased his number of buoys to 25, and although he will have pressure and temperature sensors aboard, he's primarily concerned with the currents; currents in this part of the ocean carry the rock lobster larva out to sea and then they seem to submerge and are returned to shore. This has positive implication on the harvesting of these lobsters which is a $30 million a year industry in Australia. Arch Dyer of CSIRD of the Atmospheric Physics Division will do some air-sea interface experiments in this particular region. The South Africans have two locations for their buoys under the experiment of Frank Anderson - three in the Aghulas Straits and three out here as weather buoys along this line.

I think Mike Hall will have more comments on the augmentation of these experiments.

Figure 5 illustrates where we're going in 1977. This is a proposal. I say it's a proposal because it's what I understand will be our system and what you can look forward to in 1977. The same random access measurement system that we put together for Nimbus F--an extremely simple system--will be on the satellite. This location and data collection system, as a cooperative part of GARP, will be paid for by France. Likewise, the stratospheric part of the infrared satellite instrumentation will be provided by John Houghton's organization at Oxford in the UK. Microwave is now going to be aboard the Tiros-N because of the analysis of the Nimbus 5 and the hopeful good results of an analysis from the Nimbus F that is coming up. These are the sensors aboard Tiros N, which Chuck Cote mentioned is a series of satellites to become NOAA satellites after they are in orbit. After they are turned over by NASA, they become operational satellites. The platforms with which the oceanographers and meteorologists are concerned are the balloons and buoys. You've heard of the vehicles and transmitters, the antennas, the power, and the essential parts of it. The satellite has a receiver, processor, memory, and transmitter. The output is the identity, time, position and the sensor variables. The data distribution for the meteorologists in this system in 1977 will be by the Global Telecommunication System of the World Weather Watch. Our requirements are for data every 12 hours; however, if there are two satellites--one on the descending node and one at the ascending node--the data could be available day and night every 6 hours. Those details must be worked out.

The minimum requirement for grid and approximate spacing of 400-500 kilometers calls for about 150 data points as shown in figure 6. These data points were arrived at in a number of studies conducted by the Joint Planning Staff for GARP in Geneva.

Figure 7 is simply a hypothetical case of what 150 buoys would look like if you could put them out on a grid. Under this belt of persistent cloudiness is the region through which we cannot get good temperature profiles from the satellite.

This is not limited to 50° latitude; however, we'd like to have data between 30 and 40° (fig. 8). That's also a data scare area. John Garrett of the University of British Columbia hypothetically deployed these buoys from supply ships going to the Antarctic. This wasn't necessarily in a random basis because the Scientific Committee for Antarctic Research (SCAR) of International Council & Scientific Uricus (ICSU) contacted 12 nations who have bases in Antarctica and said, "Would you be willing to deploy buoys--simple, expendable drifting buoys that won't deter you in time--from your supply route to Antarctica in say December through March?" This is a dispersal commencing in about January. These buoys have been out for 4 months and some of them have been deployed from island stations such as Campbell and MacQuarrie. These buoys are out less than 3 months, and again this is just a very simple ocean current. The best data that were available from the Navy Hydrographic Office.

Figure 9 shows you the dispersal of these buoys after the ships have returned with some buoys being deployed on the return trips in February and March. Here is the dispersal of buoys in the month of May. This is with the idea that buoys will last 6 months and that there is no death among these buoys - the mean time between failure is still 6 months. In spite of this seemingly good distribution of platforms for measurement of temperature, pressure, and ocean temperatures, we have in this region between 80 to 140° W a lack of buoys, thus no data. The solution seems to be as follows:

1. We are looking at the void at NCAR with respect to the model - what does it mean to have no data in a large area like this?
2. Could research ships be diverted to deploy buoys in this area? The Russian research vessels indicated that they would be willing to deploy buoys from their research vessels. We might have U.S. ships go through this region and deploy buoys or we could consider the use of air deployed buoys from the west coast of South America.

With respect to programs that are underway to oceanographers, Professor Stommel of MIT is a consultant on the Joint Planning Staff for GARP, and he has been charged with stimulating interest and activity among the oceanographers to develop those oceanographic programs that my inter-relate with the first GARP global experiment in 1977-79.

In conclusion, I maintain that the expendable drifting buoy, satellite system presents an opportunity for use of new tools for both scientific and operational groups. I suggest that by the time of the global experiment that the expendable drifting buoy will be as common a tool as the radiosonde and the XBT and HYBT. With respect to some of the future, it is not likely that we're going to have a buoy that will meet all requirements. There'll be a meteorological buoy and an oceanographic buoy because of the reasons I suggested earlier--the difference in temporal and spacial scales. This was pointed out very early in these investigations and is being looked at by Henry Stommel.

I'd say that this technique holds great promise and hopefully the systems with the Nimbus F will carry on for a couple of years. The follow on, Tiros N, will lead a continuity to experiments, although the systems will not be identical. There will still be a Random Access Measurement System (RAMS) but the electronics will be a little different. The Nimbus F should provide us with experience so that we can define and describe the hardware we need for the global experiment. This brings to mind the one item that presents the greatest problem to the meteorologists and that is simply the pressure sensor aboard the buoy. We had sufficient problems with the pressure sensor aboard the balloon. We lock it at 150 millibars on the surface--float it up to 150-200 millibars and then expose it to the atmosphere so it doesn't go through a long range of hystersis. The southern oceans are the areas in which we are most concerned about data from buoys, but data from any of the oceans will be certainly welcomed and especially pressure measurements. If you use small clusters of buoys as Tim Barnett has described, even if there is only one or two pressure sensors among that cluster during the Global experiment or in the Nimbus F experiment, that will be extremely valuable to the meteorologists.

THE DEVELOPMENT OF THE SATELLITE LOCATION AND DATA COLLECTION SYSTEM WITHIN THE NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

by

Chuck Cote
Goddard Space Flight Center

At Goddard we began our work in Data Collection Satellite Systems in the early 60's, and have since devised and developed a number of systems and techniques. By the end of the 70's, NASA in cooperation with other agencies, will have demonstrated on the order of seven different techniques. Basically, all these attempt to achieve the same goals: collection of in situ data related to the Earth and its environs. There has been substantial progress in the interest of users, particularly in reducing cost and complexity of platform equipment. Of the seven systems, five are experimental while two systems are flown on operational satellites. SMS-1 (Synchronous Meteorological Satellite) was launched last Friday. I'll try to cover the major characteristics of these systems and will be interested in any comments or questions you may have.

The Author: Mr. Cote received his Bachelors Degree in Electrical Engineering from the University of Detroit in 1961. He began working for the Goddard Space Flight Center in 1962 as a design engineer for scientific satellites. In 1968 he was appointed principal investigator for the IRLS system in Nimbus III and IV. In 1972 he assumed the responsibility for the development of the Nimbus F Random Access Measurement Systems (RAMS).

Satellite techniques have been used for numerous applications, many of which are shown in figure 1. These have involved moving and stationary applications for both data collection and position location. In concept all of these techniques use telecommunications to relay information from a buoy, an aircraft, or whatever to a satellite and subsequently to a ground center for data processing and distribution. Doppler, ranging, and Omega, are the principle techniques used to locate and track various objects.

Figure 2 indicates the wide variety of systems in the form of a decision tree. The main objective is points-to-point communication; systems are separated on the basis of satellite orbit and capabilities. Close participation with experimenters/investigators and the user community in general has been invaluable in the evaluation of techniques responsive to user needs.

Figure 3 shows the concept used in the first system developed which turned out to be the second one launched. The IRLS, which has been mentioned by a number of speakers already, is the Interrogation, Recording, and Location System. It is a low orbit system and was flown on two satellites -- NIMBUS 3 and 4. In the general mode of operation, platform ID codes, or addresses, and the time of anticipated overpass of the platform are programmed from a central ground facility. As time elapses through the orbit, interrogations were executed automatically over the platforms. As revolutions are completed, the data is read out, and transmitted to the center for distribution.

This location technique used a ranging system where the slant range distance between the platform and the satellite was measured. Two such measurements provide the geometry information necessary to determine the platforms position on the Earth. The position accuracy obtained with this system was in the order of 5-7 kilometers. The ranging accuracy was 0.5 kilometers which should have lended itself to 1 1/2 kilometer variance in location. However, knowledge of the satellite orbit was very critical (the system was sensitive to the orbital accuracy).

Stan Turner's concept is utilized in the Navy Transient System, where precision orbits are maintained. The IRLS platforms were quite expensive; our first purchases were in the order of $50,000 in small quantities. On a standard learning curve costs would reduce to maybe $15,000 in large quantities.

If the name on figure 3 was changed to EOLE this figure would hold for the French system. The EOLE used a similar technique, except that a Doppler measurement was added to the ranging. This improved the accuracy, and through calibration of the orbit, the 5 to 7 kilometers was improved to the order of 1 to 2. EOLE, as with the IRLS, has a relatively expensive transponder which also required a substantial amount of power. Solar cells have been used, mainly on balloons, to alleviate this problem. There are many cooperative developments particularly in the area of antennas: UHF has been the choice of frequency.

Figure 4 shows the configuration of another system developed in that time frame. The system is called OPLE, which stands for the Omega Position Location Equipment; it utilizes the Navy Omega Navigation System as the locating technique. The concept consists of receiving and relaying the Omega frequencies through a synchronous satellite to a ground station. The platform contains a VHF receiver-transmitter combination plus an Omega receiver. The early versions of this equipment were in the $50,000 area, were quite heavy and contained a very high power requirement. The accuracy of the OPLE is in the order of 1 or 2 nautical miles, which is consistent with the Omega system. The experiment simply proved that the relay of Omega signals through a space link does not degrade the Omega. Through differential Omega techniques, accuracies in the order of a tenth of a mile have been achieved. OPLE is considered a very viable concept for the future, and a lot of work has been completed in terms of miniaturizing and reducing cost of equipment. However, OPLE has a disadvantage which deals with position ambiguity. Every 72 miles across the Earth, an ambiguous position exists unless counting techniques are used. Studies and experimentation are being carried out to resolve this problem.

Figure 5 shows an early satellite experiment involving a drifting buoy. The buoy was implanted near Puerto Rico, position 1, in 1970. In this application the IRLS system was used to collect data on temperature, pressure, wind and wave parameters; the buoy was installed by the Naval Oceanographic Office. The buoy broke loose due to a ship cutting the anchor line, and became free drifting. As you can see, it encountered eddies near the southwest tip of the island and began to drift north toward open oceans. Through satellite tracking a Coast Guard ship was directed to the recovery point. This illustrated the application of satellites to recovery of derelict buoys as well as to free drifting.

At this point in time we at GSFC began to think about the future of satellite systems in terms of cost and power -- particularly cost, and to give serious thought to developing techniques to enable reduction in these areas. We considered the approach which Stan Turner used which is to miniaturize the equipment and retain your receiver, transmitter, etc. However, an alternative is to apply the principle of removing components to reduce cost and complexity, and thus come up with a minimum platform package. Work began in random access systems. In such systems a 'transmitter only' type package which is positioned by Doppler is utilized. It became quite clear that large quantities of randomly transmitting packages could be deployed, positioned, and that this concept lended itself to low cost (fig. 6). The concept was first applied to the system flown on the ERTS-1 (Earth Resources Satellite).

The system was developed to serve government users basically in the United States or in the western hemisphere. Platforms were installed at fixed sites; there was no location or tracking requirement. The messages were transmitted from the platform to the satellite in real-time; there was no storage aboard the satellite; this system, by the way, is still flying and another one will be launched next year. The results of this were very encouraging. It's interesting to note that the power requirements on the platform were such that you could expect 6 to 9 months, from battery primary sources (70 ampere hours). The data quality has been excellent, and there were over 200 platforms deployed. The statistics that were developed here were applied to the NIMBUS-F/RAMS which added a position-location capability.

Figure 7 illustrates the SMS system concept. The system was not designed for location, but could be used if an auxiliary technique were integrated into the link. The satellite will be deployed at 100°W over the equator. The heart of the system is at Wallops Island, Virginia, where a command data and acquisition site is located. In operation, platforms are interrogated directly from the satellite. The viewing circle shown is formed by a 7° elevation angle. In order to get global coverage three satellites placed around the world would be required.

Figure 8 illustrates the NIMBUS-F concept. It also shows another system, which uses SMS and NIMBUS, called the Carrier Balloon System which will be duscussed later. The major elements of the system are the Random Access Measurement System (RAMS) carried by NIMBUS, remote platforms and a ground processing facility. As part of a Tropical Wind, Energy Conversion, and Reference Level Experiment (TWERLE), the system will be used to track 400 balloons launched near the equator for determination of meteorological parameters. In operation the balloons transmit randomly at 1:1/60 duty cycle, which is to say that a transmission occurs 1 second of every minute. As a satellite comes in view these signals are received, processed, and stored for readout to a central ground station. Data is then transmitted to Goddard, and subsequently to the users. The system location accuracies are estimated in the order of ±5 kilometers. The system will also provide a measurement of velocity to ±1 meter per second. Our decision to utilize this type of system has resulted in some interesting trends. The electronic packages presently being produced in small quantities can be purchased for approximately $1300, depending on the company. When you consider that in mass quantities, a learning curve could decrease costs even further.

The TWERLE Experiment is being conducted by NCAR (National Center for Atmospheric Research), the University of Wisconsin, and GSFC.

With each of the transmissions to the NIMBUS/RAMS, the data message shown in figure 9 is received. The total length is 1 second, and as shown 32 bits are available for sensor data. Typically up to 20 such messages could be received per day. Four to five are required for the location process. Many of our investigators are multiplexing data in order to provide additional sensors capability. Up to 16 sensors can be handled with the format shown under multiplex conditions.

Figure 10 shows an early model of a platform that was developed for the random access system. This wad designed in the early 70's under a Goddard Research Contract. It was a single unit basically intended to demonstrate that standard assembly techniques could be used to develop low cost, low power hardware. The package is in the order of 20 inches in length. With a moderate gain antenna, a 600 milliwatt transmitter will work very reliably with NIMBUS-F.

Figure 11 summarizes some of the differences in the systems just described. As shown with systems like NIMBUS-F and TIROS-N we can handle 200 platforms in view, where position is required on each. This could be increased if position is not required on each platform. Up to 1000 could be handled as is done with the ERTS. SMS can handle 10,000 in a 6-hour period, a very high capacity. We've sort of standardized on the UHF band since it is a very convenient frequency in terms of hardware. Each of these systems is or will be available. The TIROS-N and the SMS are operational spacecraft, NIMBUS and ERTS are experimental. These systems will be used between now and the 1980's and maybe beyond. Our present goals, within Goddard, are to encourage the use of these and to think beyond this time frame. We can forsee needs for increased growth, and our never ending goal to reduce cost and complexity will remain.

Figure 12 lists just some of the activities that are now on-going at Goddard. We're always interested in long-range requirements and plans for applications and user needs. Platform miniaturization is of particular interest in applications such as wildlife tracking. This is compatible with some of the types of systems discussed. Improved antenna designs are always of interest and we're looking toward increased system capacities with maybe 20,000 platforms (fixed or combinations of moving) in the future. It's becoming obvious there's increased importance on position accuracy. Work is being done with orbital models to improve accuracies in low cost systems. This summarizes our thinking in the next few years. In addition our activities supporting these existing systems will continue.

QUESTIONS

Speaker Unknown

When you have many buoys located close together on this random transmission, aren't you liable to get some garbled information because of being transmitted simultaneously?

Chuck Cote - GSFC

You must know something. There are limitations. Let's take the NIMBUS system. We see 200 in view and there is a possibility that as they are clustered, we no longer have frequency separation between incoming signals. That is, all arrive at the same frequency at the satellite, therefore, we have mutual interference. In the current system, we've conservatively saying 100 to 200 kilometers separation would be in order. For small clusters we could reduce this. That is a key distinction between the ordered or interrogated systems, such as the EOLE and the IRLS versus the Random. There is a mutually interference parameter that we must be sensitive to.

Speaker Unknown

Is the number in figure 11 for power a tested or theoretical number?

Chuck Cote - GSFC

Right now it's theoretical; we have not launched the system.

Speaker Unknown

The reason I asked is, now if I can refer to Stan Turner; does that figure coincide with the kind of power you would need on a surface unit in order to have reliable communication?

Stan Turner - NUSC

Well, for one thing Chuck is talking about a satellite which is not in synchronous orbit, orbiting about the Earth at a low altitude, like around 600 miles up, and then the propagation loss between ground and satellite is considerably less than it would be to synchronous satellite. Secondly, the application for which I built this transmitter is one to locate submarines in distress. I have to consider the fact that this transmitter may be used in the worst ocean conditions where it's going to be at a low grazing angle up near the Arctic region, this wipes out about 3 dB of your signal from the fact that it's no longer polarized. The antenna, the transmission isn't the same as the satellite would like to see it. And also at the Arctic regions there are scintillation losses which may be as much as 12 dB, so I've had to include a lot of power. Now this power that I'm using, as Chuck has, it just bursts out in a matter of seconds, a couple of seconds. So in answer to one of my questions to my talk, the salt water battery that I use for this application, costs $80 and it lasts for approximately 40 hours and it's about the size of a couple of cigarette packages, which works very well even though it has to power this transmitter of 100 watts which really requires about 200 watts from the battery. Thank you Chuck, for letting me take that much time.

Mike Hall - NDBO

If a RAMS system on TIROS-N becomes truly operational how many times daily can we expect to get position fixing from that?

Chuck Cote - GSFC

Data will be read-out daily and distributed to users from France.

Mike Hall - NDBO

I had heard talk of two satellites in a cross-orbit and it's becoming fairly important how many times daily the thing will pass overhead.

Chuck Cote - GSFC

To be conservative it's about one or two a day. There is a lower orbit currently planned that should provide more frequent coverage. But I am at a loss to give you a precise interval at this time.

Mike Hall - NDBO

You were talking earlier about the mutual interference from stations which are fairly close together. Could you give me an idea as how bad this interference would be if you had four or five drifters employed within 2 or 3 miles of each other?

Chuck Cote - GSFC

In random systems if you adjust one parameter, everything is affected. However, up to 60 should be possible in small areas.

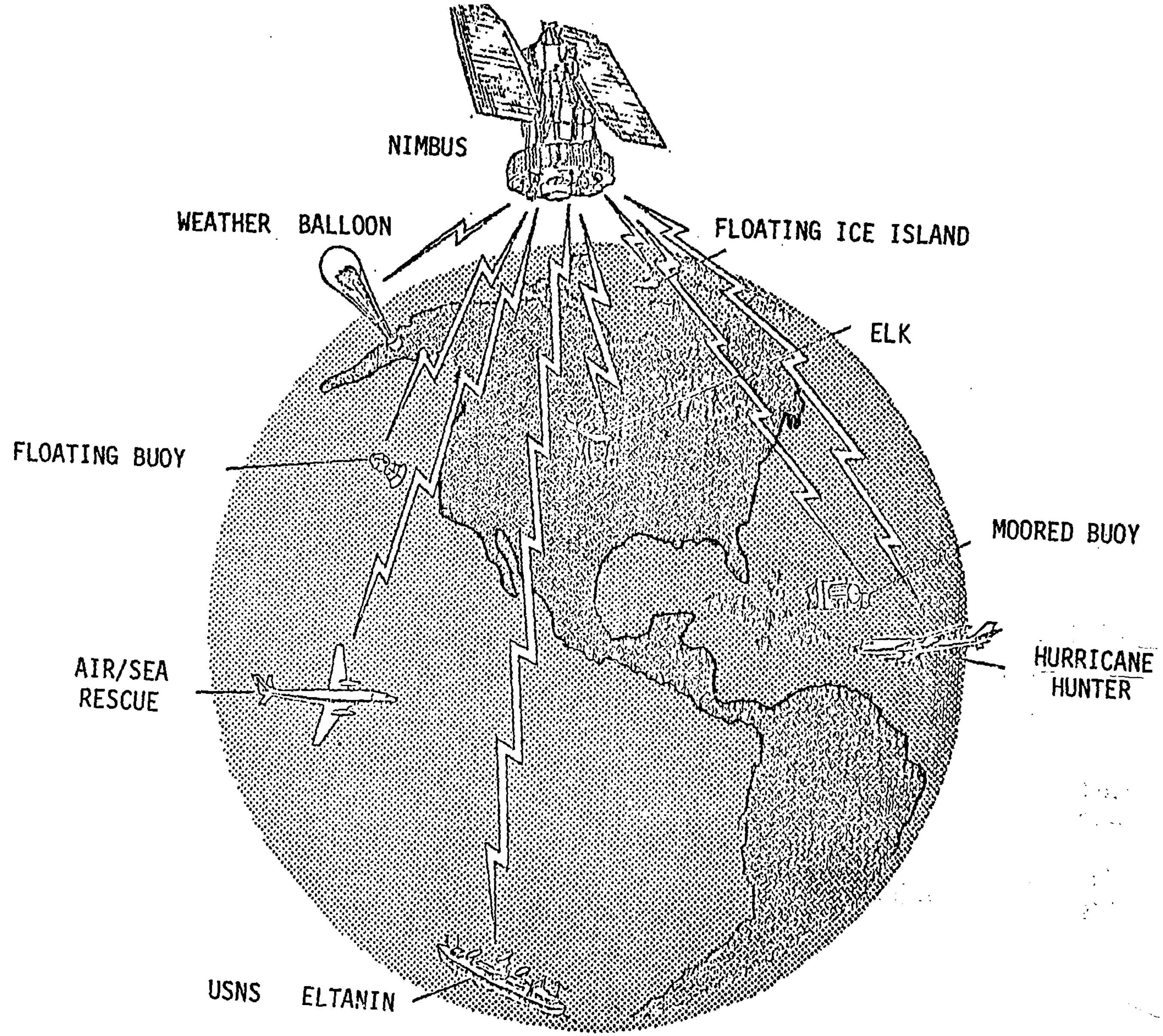


FIGURE 1. UTILIZATION OF THE NIMBUS SATELLITE

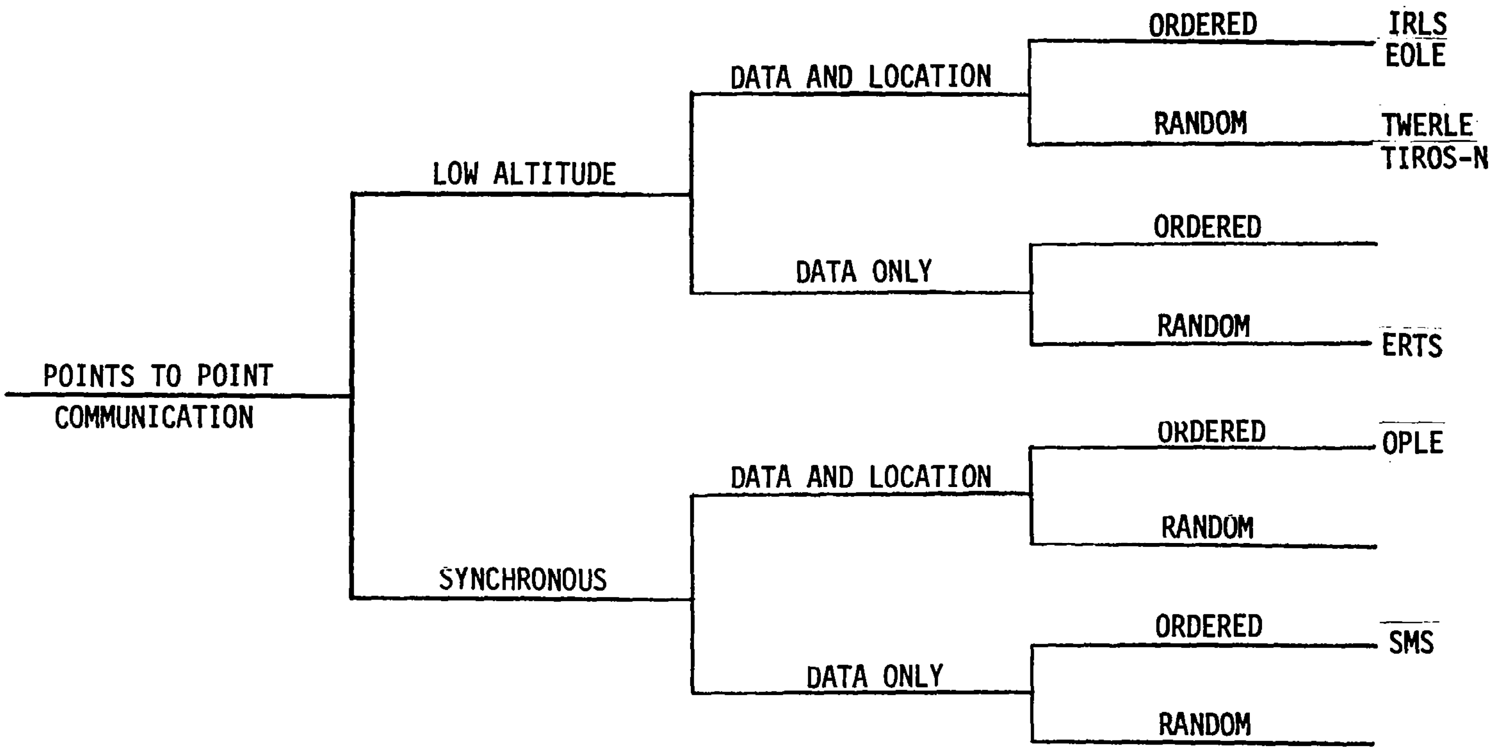


FIGURE 2. DATA COLLECTION SYSTEMS DECISION TREE

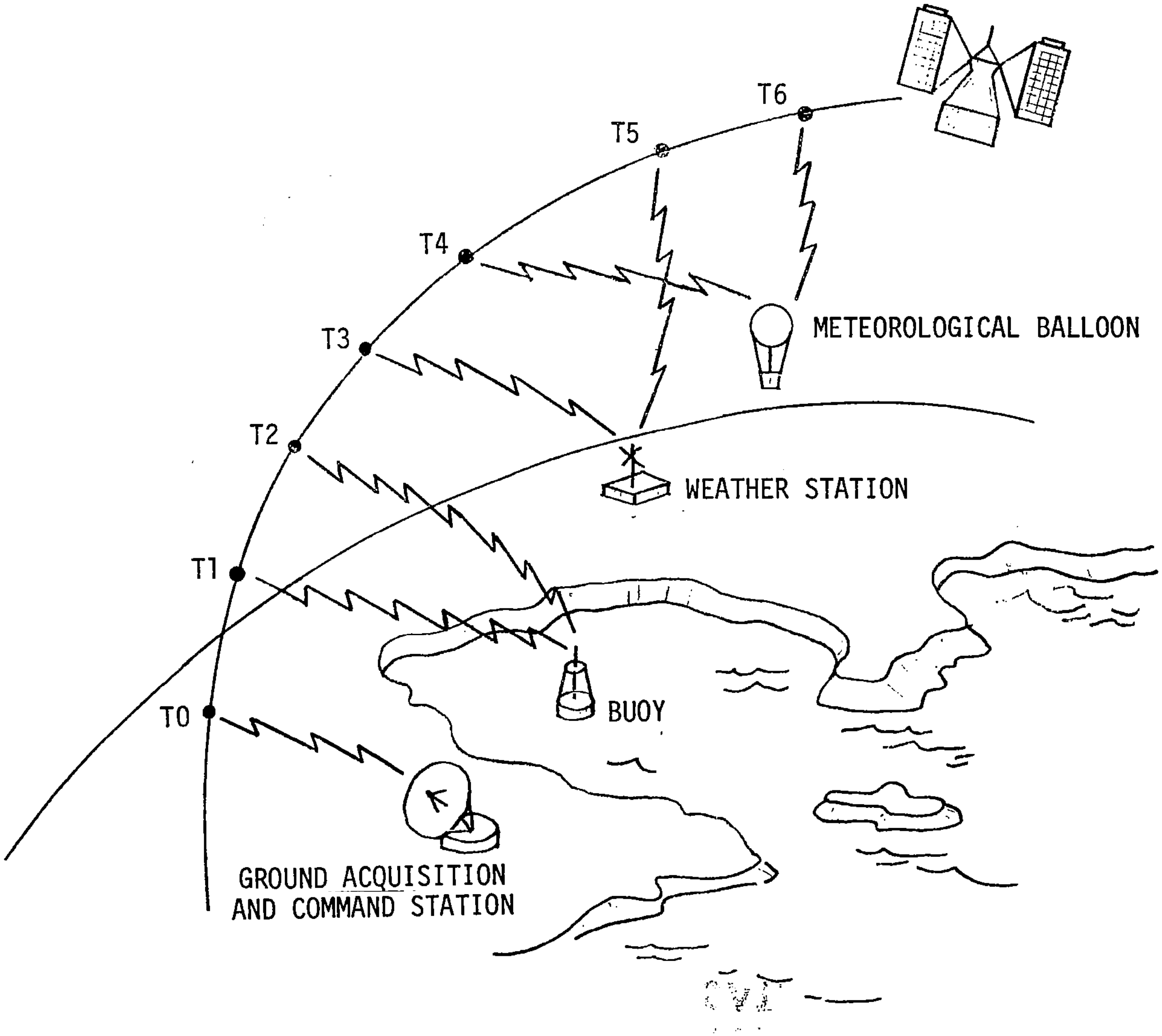


FIGURE 3. THE INTERROGATION, RECORDING, AND LOCATION SYSTEM

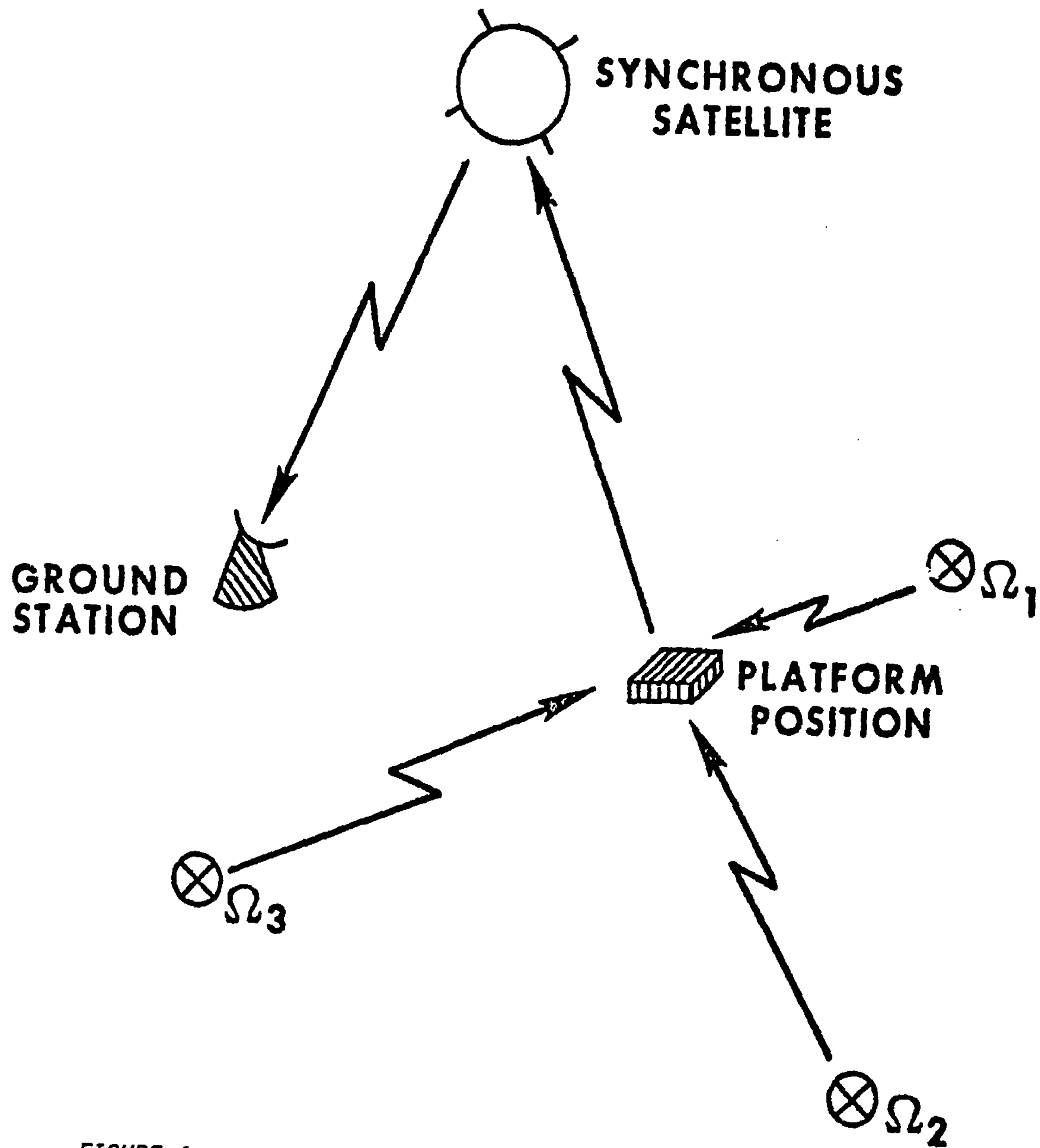


FIGURE 4. THE OMEGA POSITIONING LOCATION EQUIPMENT

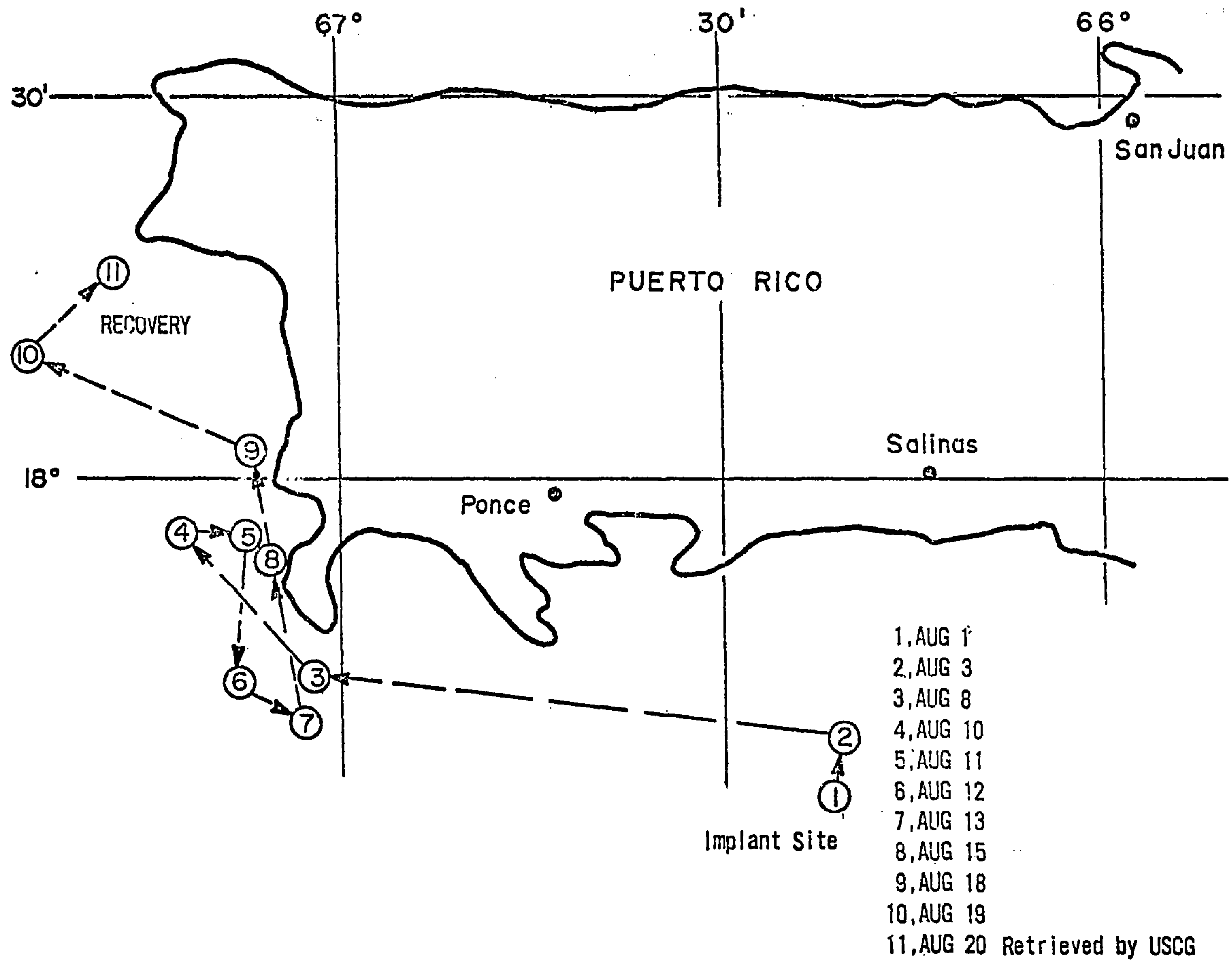


FIGURE 5. NIMBUS III IRLS TRACKING DATA ON FREEDRIFTING BUOY

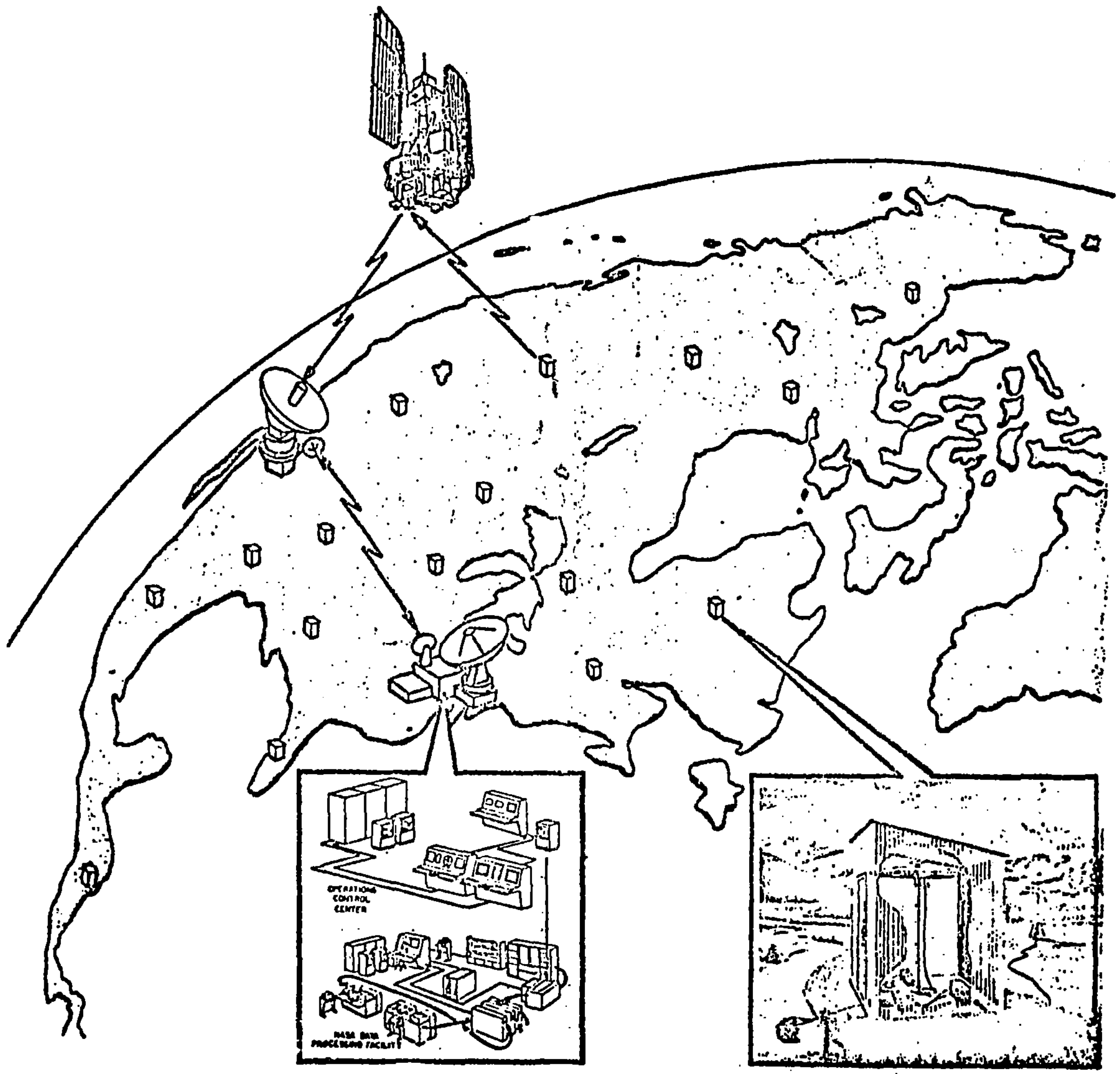


FIGURE 6. ERTS DATA COLLECTION SYSTEM

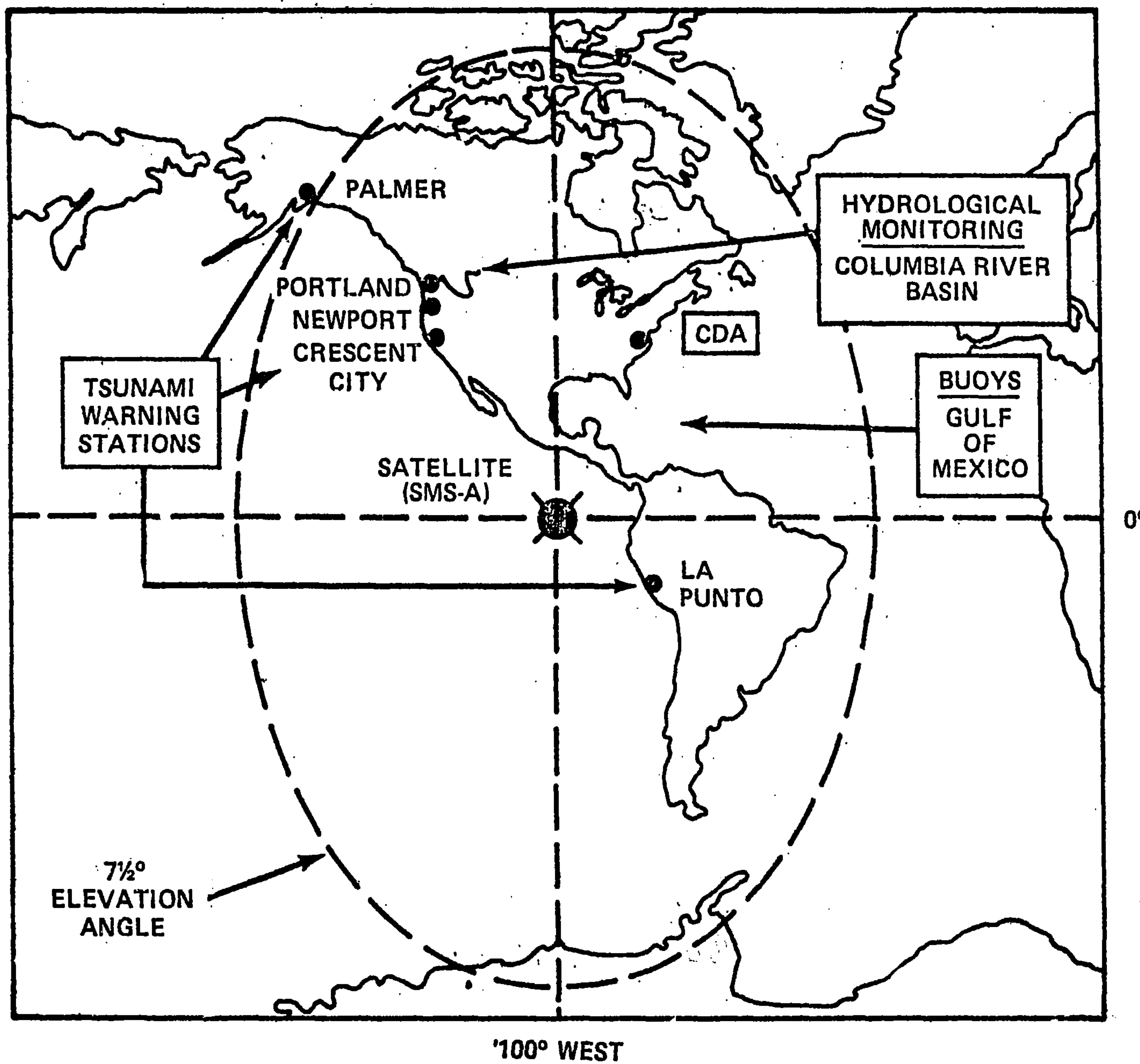
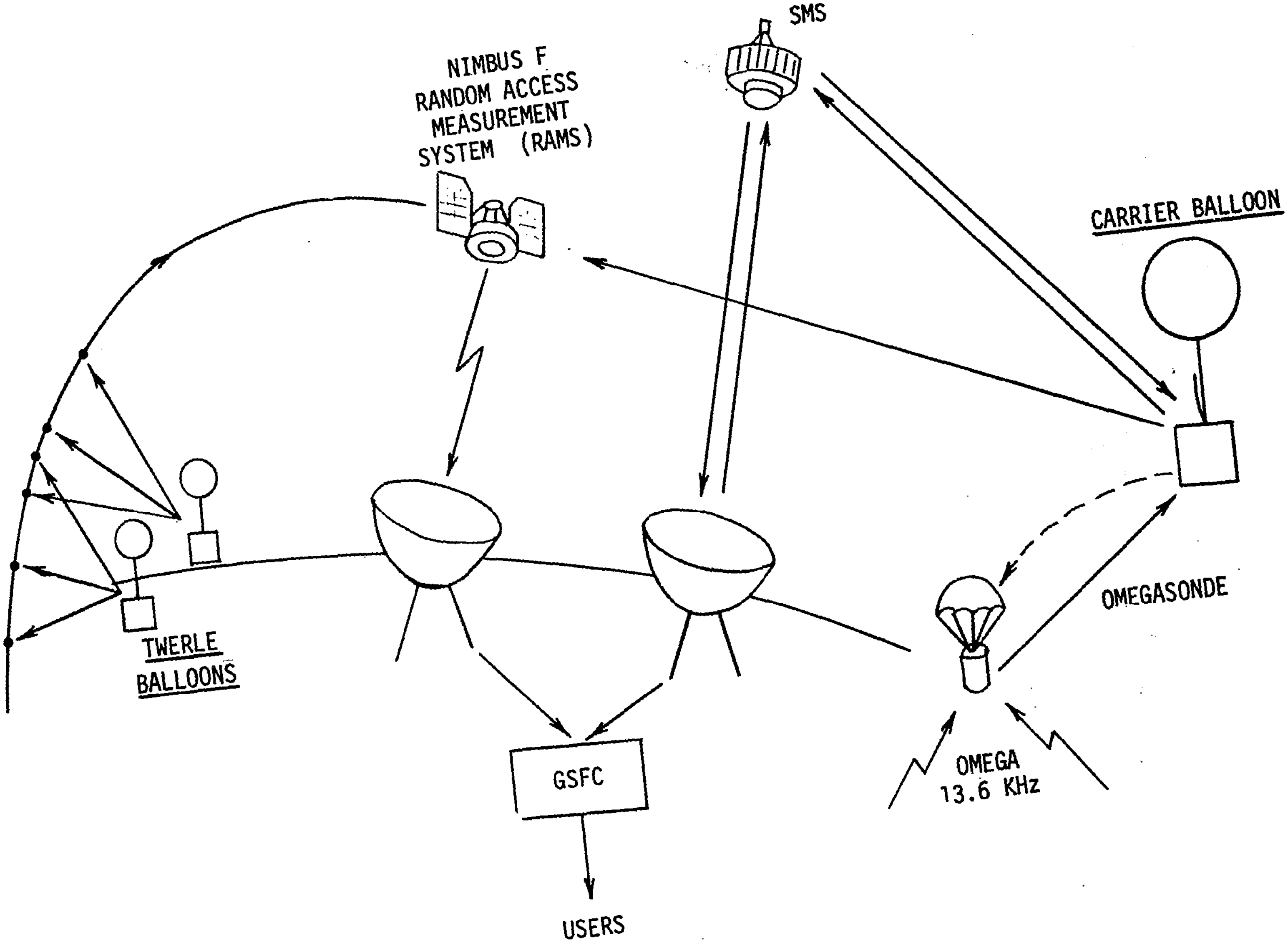


FIGURE 7. THE SMS DATA COLLECTION SYSTEM


SMS
NIMBUS F
RANDOM ACCESS
MEASUREMENT
SYSTEM (RAMS)
CARRIER BALLOON
TWERLE
BALLOONS
OMEGASONDE
GSFC
OMEGA
13.6 KHz
USERS

					CHANNEL DATA			
1st TRANSMISSION UNMODULATED CARRIER	BIT SYNC	FRAME SYNC	IDENTIFI CATION (ID)	MODE BITS	1	2	3	4
2nd TRANSMISSION UNMODULATED CARRIER	BIT SYNC	FRAME SYNC	IDENTIFI-CATION (ID)	MODE BITS	5	6	7	8
3rd TRANSMISSION UNMODULATED CARRIER	BIT SYNC	FRAME SYNC	IDENTIFI-CATION (ID)	MODE BITS	1	2	3	4
4th TRANSMISSION UNMODULATED CARRIER	BIT SYNC	FRAME SYNC	IDENTIFI-CATION (ID)	MODE BITS	5	6	7	8
320 TO 360 MSEC	8	12	10	2	32			

NUMBER OF BITS

1 SECOND (NOM)
980 MSEC ± 20 MSEC

Figure 9. BUOY TRANSMIT TERMINAL OUTPUT FORMAT

	POSITION LOCATION		NO POSITION LOCATION	
SYSTEM CHARACTERISTICS	NIMBUS F	TIROS N	ERTS	SMS/GOES
MULTIPLE ACCESS	RANDOM	RANDOM	RANDOM	• COMMANDED • SELF TIMED
COVERAGE	GLOBAL	GLOBAL	2/3 OF WESTERN HEMISPHERE	EARTH DISK
PLATFORM CAPACITY	200 IN VIEW	200 IN VIEW	1,000 IN VIEW	10,000/6 HOURS
FREQUENCY (MHz)	401.2	401.65	401.55	402
PLATFORM REQUIREMENTS				
TRANSMITTER POWER	600 MILLIWATTS	3 WATTS	5 WATTS	5 WATTS
ANTENNA GAIN	2-3 dB	2-3 dB	2-3 dB	13 dB
BIT RATE (b/s)	100	400	2,500	100
WEIGHT (ELECTRONICS)	454 GRAMS (1 POUND)	560 GRAMS (1.2 POUNDS)	4.1 KILOGRAMS (9 POUNDS)	5.5 KILOGRAMS (12 POUNDS)
DATA CHANNELS	4	4-32	8	8

FIGURE 11. DATA COLLECTION SYSTEM PARAMETERS

LOCATION AND DATA COLLECTION SYSTEM

ACTIVITIES AT GSFC

- LONG RANGE REQUIREMENTS AND PLANS
- PLATFORM HARDWARE MINIATURIZATION
- ANTENNA DEVELOPMENT AND TESTING
- INCREASE SYSTEM CAPACITIES (20,000 FIXED, 1,000 MOVING)
- PRECISION POSITION/LOCATION STUDIES
- FLIGHT PROJECTS
  - TWERLE/RAMS
  - TIROS-N
  - CARRIER BALLOON SYSTEM
  - ERTS/DCS
  - EOLE

Figure 12

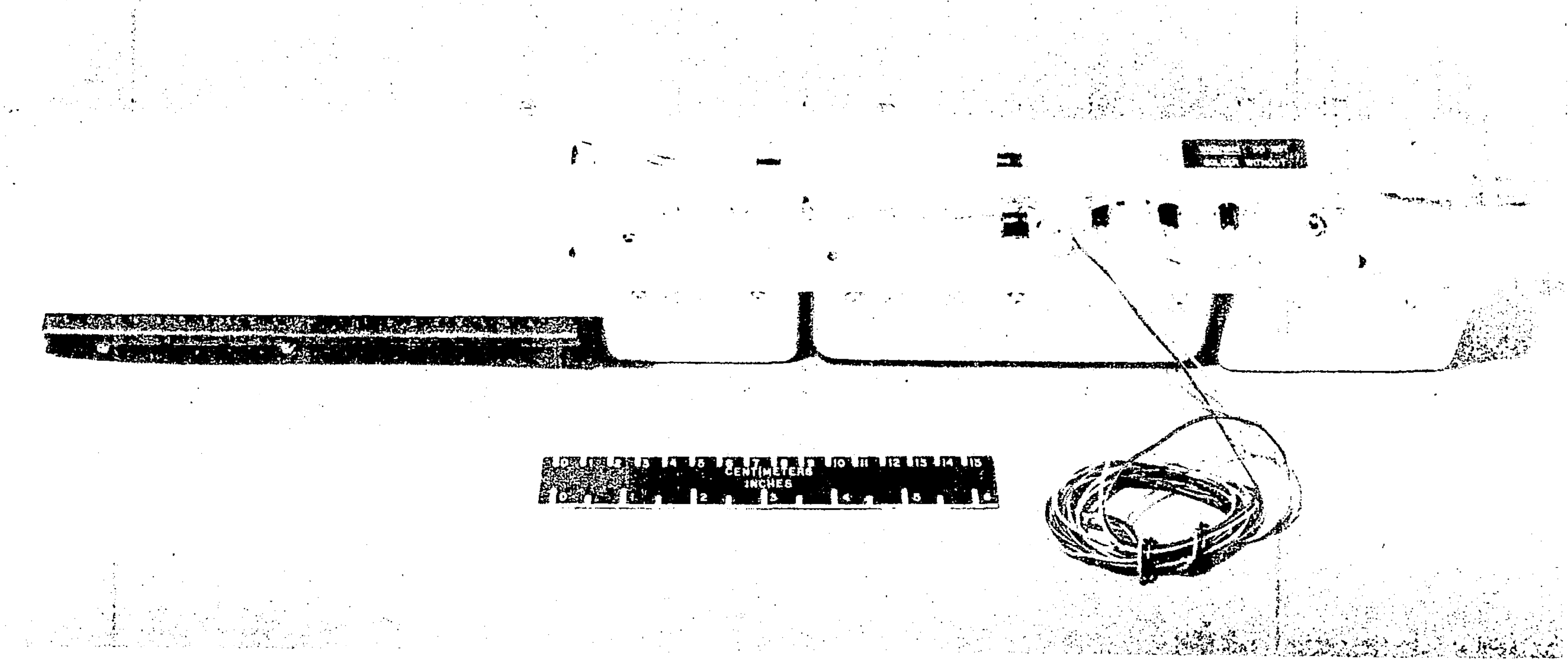


Figure 10. RANDOM ACCESS SYSTEM PLATFORM

*U.S. GOVERNMENT PRINTING OFFICE: 1976 - 735-004/6