Aug. 18, 1986

TO:

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Gerard, S.	-	LDGO
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Leyden, R.	-	LDGO
Lotti, R.	-	LDGO
Raleigh, B.	-	LDGO
Ruddiman, W.	-	LDGO
Ryan, W.F.B.	-	LDGO
Sykes, L.R.	-	LDGO
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RESEARCH CRUISE REPORT

R/V ROBERT D. CONRAD 27-04

Attached is a copy of a cruise report for the above CONRAD cruise.

Ann Burns
Ann Burns
Marine Office

Enc.

C2704 CRUISE REPORT

PORTS: COLOMBO - COLOMBO
DATES: MAY 14 - JUNE 16, 1986
WORK COMPLETED: 10,360 km (5600 n.miles) of SEABEAM swath-mapping; 6600 km (3600 n.miles) of digitally recorded single channel seismics; 7 sonobuoys; continuous gravity, magnetics and 3.5 KHz echosounding; 3 hydrocasts; 30 piston cores, 22 giant gravity cores, 6 box cores, 3 trigger weight cores; 4 plankton tows
FUNDING AGENCY: NSF / JOI, Inc.
PERSONNEL:

DR. WARREN PRELL..........Brown.....Chief Scientist
DR. GREGORY MOUNTAIN.......L-DGO.....Co-Chief Scientist
DR. SAM GERARD............L-DGO.....Hydraulic Engineer
DR. JON OVERPECK..........L-DGO.....Watchstander
DR. LARRY PETERSON........Miami.....Watchstander
MR. WILL HOWARD...........WHOI......Watchstander
MR. BOB BREYNAERT.........Brown.....Watchstander
MR. JOHN FARRELL..........Brown.....Watchstander
MR. STEVE CLEMENS.........Brown.....Watchstander
MR. DAVE ANDERSON.........Brown.....Watchstander
MS. LISA DUBOIS...........Brown.....Watchstander
MR. NEAL DRISCOLL.........URI.......Seismic Technician
MR. HARRY VAN SANTFORD.....L-DGO.....Electronics Technician
MR. STEVE LABRECQUE........L-DGO.....Electronics Technician
MR. KURT FEIGL............L-DGO.....Navigational Technician
MR. FRANK ROBINSON........L-DGO.....Winch Operator
MR. JOHN FREITAG..........URI.......SEABEAM Technician
MR. PETER LEMMOND.........URI.......SEABEAM Technician
MR. MARTIN ILTSZCHE........L-DGO.....Water Gun Technician
MR. ROPATE QUALI..........L-DGO.....Coring Bos'n

OBJECTIVES:

The objectives of C2704 were: 1) to investigate the oceanographic effects of seasonal monsoon upwelling in the western Arabian Sea; 2) to survey a small portion of the western Indus Fan; and 3) to evaluate processes affecting the stability of sediments on the Oman margin and the Owen Ridge. Each objective was intended to provide information essential to future ODP sites to be drilled in the area.

NARRATIVE:

Upon arrival in Colombo, we went to the offices of the agent to ascertain the status of the air and sea shipments to the Conrad. The sea freight was in hand but some of the air shipment was still at the airport. We asked that it all be dockside when the ship arrived on Friday the 9th. We also inquired about the status of dry ice for preserving box core samples. This had been the subject of several telexes between Overpeck and the agent. By telex, the agent had quoted a price of $100.US/200 kilos of dry ice. On the basis of this quote, we decided to purchase dry ice in Colombo rather than to bring CO2 in cylinders and make dry ice on board. This decision had to be made a month in advance of the cruise. We planned to purchase 400 kilos of dry ice. On our arrival the agent informed us that the price was 450R ($16.67) per kilo! Our original amount of dry ice (400 kilos) would now cost more than $7000!! At this point we had no alternatives, we didn't bring equipment to make dry ice. We finally decided to get 225 kilos at a cost of about $3700. A real budget buster: half the amount for about twenty times the quote.

Initial testing during the Pt. Hedland-Colombo transit had revealed problems with the newly installed core winch. Consequently, work during the Colombo port stop focused on this equipment. One of the problems had been overheating of the hydraulic fluid; Ken Caillouet stayed on in port to install a cooling circuit. Because Jay Ardye was in the Arctic and unavailable, and Bernie Gallagher had flown home for personal reasons, Sam Gerard flew to Colombo to assist Ken, arriving on the afternoon of May 10. At the outset it was unclear whether Ken would be asked to sail with us to be on hand for any potential hydraulic work; it was clear, however, that he really did not want to sail out of Colombo.

Progress with the hydraulic work was slowed by the absence of shipboard engineers on their one port day, Sunday May 11. By 1500 hrs. on May 11 (this and all times for in port and throughout the hydrualic testing are in local time), Sam determined that the scheduled departure of 0900 on the following day was unrealistic, and postponed it to 1500 hrs. This was met with agreement by the chief scientists, because it better insured that proper hoses, fittings, etc. could be located from shore supplies, and that the work done would be performed under optimum conditions, not at sea. Furthermore, it was agreed that a deep-water load test of the hydraulics (both the core winch and the hydro winch) should be performed before Ken left the ship. Bathymetric charts showed that we could reach 2000 meters of water roughly 30 miles from Colombo. This plan was additionally attractive because it would allow us to easily return to Colombo to offload Ken; the only other option at all feasible was for him to sail with us to the Oman margin and get off there. We had been in contact with the U.S. Embassy in Muscat re: an Omani observer joining our cruise, and we were advised that offloading anyone for passage out of Oman would be extremely difficult. We were informed by telex on May 12 that the Omanis had withdrawn their request for an observer, so we continued with our plan to load test the hydraulics near Colombo, offload Ken, and then begin our scientific cruise. With this plan agreed upon, Sam informed us that he would be joining our voyage to assist in any work with the new hydraulic systems.

Despite the long hours that Ken was working on the hydraulics (usually alone in the evenings) Sam evaluated the progress in the morning of May 12 and decided to postpone the departure for this test until the next morning, May 13. We had been tied up at pier QE4, and at 1400 we moved out to anchorage in the harbor. By dinnertime, the departure time had slipped to early afternoon, due in part to the promise of a noon-time delivery of much-needed hydraulic oil.

By 0900 on May 13 Steve LaBrecque began reconstructing the winch control board that had also proven faulty during the previous transit leg. Pressure testing and non-load conditions tested out during the remainder of the daylight hours. At 1630 the work was completed and the pilot boat was called. We weighed anchor, made for the 2000m isobath, and arrived on station at 2030. We had intended to fully test the winches by actually lowering cores and retrieving samples, but the 3.5 KHz recorder suggested a steep and highly reflective bottom. Rather than risk damaging or losing coring gear, we decided that merely lowering the piston core head to within a few hundred meters of the bottom would be a satisfactory test, and would also allow Ropate and Frank a chance to get the feel of the winch controls with Ken present to advise. The winch and the operators performed without any problems. Unfortunately, we have only a mechanical meter wheel that is made for 1/2 in wire, not the 9/16 in. now installed, and however inaccurate due to slippage, it can be seen only by a person standing right next to the cable, not by the winch operator.

The core winch test was completed by 2230; our attention then turned to the hydro winch, from which we had planned to lower 20 or more giant gravity cores (GGC's.) The winch performed well. The only malfunction detected was with the jib extension on the Slattery crane; it could be lengthened, but would immediately and without command retract fully. This was overcome by attaching a steel angle brace that fixed the crane at a roughly 3 foot extension, but obviously this hindered use of the crane and made coring more difficult.

We had been unable to use the crane before this time (hoses had been removed for use in the core winch), so this was our first opportunity to experiment with ways to deploy and recover the GGC. A core was rigged, lowered to nearly 2000 m and retrieved (without attempting to core the highly reflective bottom.) Gear was secured on deck by 0230 of May 14, and by 0530 we hove to at the pilot station. Ken and Sam discussed present and future aspects of the winch system; Sam will have the critical job of trouble-shooting hydraulic problems we may encounter during our leg. At 0630, Ken stepped off onto the pilot boat outside Colombo, and we departed for the start of our cruise.

The pit log was zeroed at 0100 GMT on May 14. We began operating the 3.5 KHz echosounder and SEABEAM at 0127 GMT. In addition, the center beam of the SEABEAM array was displayed as a 12 KHz record on a Raytheon recorder beginning at 0211. Once beyond the shelf edge and into deep water south of India, the magnetometer was streamed at 0435. The Indian continental shelf was crossed later in the day, so for safety the maggie was retrieved from 1354 to 2036. We experienced a total power failure at 0414 on May 15. Electronic equipment in the main lab was turned off immediately, but within 3 minutes power was restored. We began losing way and decided to retrieve the magnetometer. This proved prudent a short while later when at 0436 the bridge reported a loss of steerage that was related to powering up on the gyros. When steerage resumed and we returned to 10+ knots, the maggie was redeployed at 0546.

We had few problems while turning equipment back on in the main lab. Fortunately the SEABEAM acquisition computer has a 6 minute memory backup. The only difficulty we encountered was with the data logger and its time base. The true time clock, normally getting absolute time from the GOES satellite, had been linked to the 5 MHz oscillator in the new rubidium clock, and had been providing us with relative time, but not precise absolute time. During the power failure, this link was lost. When power returned, the instrument was searching for its satellite signal; at our location in the Indian Ocean we are out of range. No one knew quite how to return to using the rubidium oscillator, so we decided to run the data logger off the chronolog clock instead. It took until 0516 to determine how to recalibrate this clock to the "absolute" time provided by our navigational transit satellites. Furthermore, we experienced read/write errors with the first two data logger tapes (both brand new) that were installed after the data logger was re-booted. We feel that this problem was unrelated to the power failure, and was merely bad luck with bad tapes.

It was soon discovered that a power supply for the Bell gravimeter had been damaged during the power down/up event. This was repaired and by 0650 we were once again collecting gravity data. At 0720 the SEABEAM acquisition computer crashed for no apparent reason. It was up and running again within five minutes. The bridge reported our passing several long lines (fishing gear) at 1257. No difficulties with the magnetometer or the streamer were encountered.

For several hours during this day we were passing over an abyssal plain perched between the southwestern tip of India and the Laccadive Islands. We needed to find a particularly level region for a SEABEAM roll-bias calibration test, and had been planning to conduct this on the Indus Fan. We contemplated performing the test instead on this plain, but before we could complete all of the preparations, the seafloor shoaled and became too rugged for a reasonable test.

The remainder of the day of May 15 was uneventful. A squall accompanied by heavy rain, very limited visibility, thunder and lightning moved through at 0600 on May 16. At 1615 we experienced a 20 minute failure of the 12 KHz recorder (the SEABEAM center beam.)

At 1715 we stepped down off the Chagos-Laccadive Ridge and onto the Indus Fan at about 4400 m. The magnetometer was retrieved at 1844 in preparation for the abrupt Williamson turns that would be necessary during the subsequent SEABEAM roll-bias test. The first turn was initiated at 1846, and after 9 passes of the same 3.4 nautical miles of level fan, a roll-bias error of about 3/4 of a degree was measured and installed as a correction factor. This number was derived by averaging the values from both the Bell gravimeter and the Pyro vertical reference indicator. The exact Bell gravimeter figure was within 1/200th of a degree of the correction factor calculated a year ago, indicating that despite having been removed and re-installed, the gravimeter was placed surprisingly close to its former position. At 2300 the test was complete, the maggie was redeployed, and we continued on towards the study area along the Oman margin.

Digital acquisition of single channel water gun seismics began at 0435 on May 17. No record was kept previously of the length of all elements of the new AMG streamer; during deployment some of the following were estimated while others were read from the few specs delivered with the streamer:

tow cable	500 ft
weighted head section	50 ft
stretch section	100 ft
4 active sections	468 ft
42 phones, 20 cm spacing, 17.2 m total	
42 phones, 40 cm spacing, 25.4 m total	
42 phones. 1 m spacing, 50.0 m total	
42 phones. 1 m spacing, 50.0 m total	
rope tail	300 ft
total length	1418 ft

Recording on profiler "C" (the digital monitor) began at 0445. Because some of the wiring had not been set up in advance, we took a little more than an hour getting the other two profilers on line. At 0600 the individual sections of the streamer were sampled one at a time, and it was observed that channel 1, the near trace, was far more noisey than the others. However, when all four were summed, this noise was largely canceled, and it was decided to record this four channel sum during our deep-water surveying. Over the next several hours we increased our speed from 6 to 10 knots; from 6 to 8 there was little visible deterioration in records with a low cut of >40 Hz. A noticeable increase in noise occurred on all records at 9 knots, and by 10 knots all of the records showed significantly increased towing noise. We chose the following parameters:

profiler A	2 1/2	sec sweep	65-200 Hz
profiler B	5	sec sweep	30-120 Hz
profiler C	4	sec sweep	40-160 Hz

We do not have depth indicators on either the streamer or the water guns. Judging from the rise time of expelled air bubbles, it appears that the guns with their new stabilizing fins tow much deeper than before. Whatever the absolute depth, at 10 knots the guns tow close to or even a few feet deeper than they used to tow without the fins at 6 knots. The lead-in to the new AMG streamer is slender and highly visible, both good attributes. However, it experiences considerable strumming at 10 knots; perhaps this is a major source of the noise we observe.

At 0816 it was discovered that no header information was being intercepted from the data logger by Kurt's "Acquire" program. After experimenting with various combinations of digitizing parameters it was decided that without the True Time clock (see above), "Controlrto" could not tolerate a recording delay; in an effort to conserve tape, a 5 second delay had been used since beginning seismic recording. After re-booting with 0 delay we had no problem. At least we think so; all we can be sure of is that when read on the SEABEAM Vax computer, appropriately sized headers and seismic files can be detected.

At 1318 the water gun was hauled aboard for a quick visual check; all looked OK.

We began having problems with the aft (B) data logger tape drive. We had experienced similar read/write errors earlier, attributing them to bad tape. It now appears that the drive itself was at fault. Harry worked on it a bit, and although we noticed a large improvement, starting a new tape often encountered numerous write attempts. Evidently the maximum number of attempts was never exceeded, however, because no further error messsages showed up on the data logger terminal. Later in the day, Harry replaced several write and controller cards in the tape drive with ones taken from the third, unused drive in the main lab. We had few problems from then on.

Sixty cycle noise, especially apparent on profiler A, plagued us as it had on earlier SCS cruises. Harry thought at one point that he had tracked it down to the streamer patch board that sums the incomming active sections. However, he grounded it, and though altered, the noise continued. We looked further, and found that several of the Ithaco filters on the data logger rack are extremely noisey. We switched to the quietest one of the eight. We planned to shut down the seismics during an upcoming hydrocast station, so Harry planned to do more extensive testing for this problem at that time.

Acoustic penetration gradually decreased to about one second as we crossed the more sandy portions of the channelled lower-middle Indus Fan. Penetration had been as much as 1 3/4 seconds earlier in the day. Slightly increased seas may also have been responsible for this decrease in data quality, although they were still slight - perhaps 3 to 5 feet. Since the power failure we had been unable to resurrect the True Time clock. After reading the operation manual, Kurt learned the start-up technique, and we returned to this time base at 1906.

Soon after including the True Time, however, we were unable to get Furuno information on the data logger terminal. Oddly, neither SEABEAM nor Acquire had any problems with this data. At 0600 on May 18 we discovered that the Furono lead in to the back of the data logger rack had come loose. Once reconnected, of course, we had no further loss of data.

The relatively noisey records continued to be a source of annoyance. We had been advised by several people that profiles on this new streamer were as good at 10 knots as earlier records had been at 6. This appears to be a little exaggeration. Because we wanted to get to the Oman margin as soon as possible, we had been running with one gun at ten knots. The options were: 1) tolerate the records; 2) keep increasing the low cut; 3) slow down; 4) put the streamer on a boom, out of the wake; and/or 5) attach shock cord to the streamer to mechanically isolate it from the ship. This was still part of the "break-in" time for both equipment an personnel, so we were not going to settle for option 1) and tolerate poor data. For 2) we did raise the low cut on profilers B and C, but only slightly. We were committed to getting to the study area soon, so we were not going to slow down. We considered swapping the maggie and the streamer, and placing the latter on the port boom. However, we examined the lead-in tow cable on the streamer and learned that all of the conducting wires are wrapped around the outside of the stress member - a 3/8 inch wire cable. We decided that these wires could not long tolerate a small radius of curvature, and should not be passed over a block at the end of the boom. Furthermore, if wrapped by "Chinese fingers" to attach a shock cord member, we decided that great care ought to be taken to distribute this wrapping tension as evenly as possible. The Bos'n made up the fingers that terminated in a thimble, and these were wrapped around a tape/Scotch-Kote base. Three one-inch shock cords were then attached to a chain come-along and secured to the overhead rail on the fantail. No change in towing noise could be detected on the records.

At 0630 the "real time" contour mapping on the Calcomp plotter quit. Peter traced the problem to a software bug that had allowed the Vax to jump ahead to JD 140. Evidently the system was sitting idle, waiting for this day to arrive. The glicth was solved and the system running again at 0750, and it is assumed that during post-processing the lost data will be recovered without much difficulty.

The wind off our port bow began increasing at about 1100. It continued to build through the night, and by 0300 on May 19 was blowing 20+ knots with short 4-6 ft. seas. This led to an uncomfortable pitching motion that took its toll among our scientific crew. Our speed over the ground was reduced about 1 1/2 knots to roughly 9, though we kept a constant 180 RPM on the main engines. The quality of the seismic records showed only a slight deterioration due to the weather. By the end of this same day the winds had returned to 10-15 knots, and we were once again making nearly 10 1/2 knots over the ground.

We reached Owen Ridge at 2000, and due to the moderated seas and the interest in collecting good profiles across the western flank of this Ridge, we slowed to 160 RPM and settled down at about 9.3 knots. Furthermore, the low cut filter on the 2 1/2 sec sweep profiler A was increased to 75 Hz. Though this might yield less penetration, the reduced noise would give better resolution of fine structures in these ridge-top sediments. SEABEAM provided excellent coverage of slumps and possible channels off to the side of our track. Clearly a single beam record along our track would have provided a much less informative - if not deceptive - picture of the importance of mass wasting on the Owen Ridge.

We crossed the basin between Owen Ridge and climbed the rugged portion of the Oman Margin near 17.5 degrees North. Our destination was a point just seaward of the 2000 m isobath where a pair of hydrocasts would be run to profile dissolved oxygen. These data would then be used to target our coring efforts on the center of the oxygen minimum zone. The guns were shut off at 0834 on May 20, the maggie was retrieved, and all bottles of the first cast were over the side at 1047. We had eight Niskin bottles aboard; to get samples from the surface to 100 m off the bottom at 200 m spacing we had to make two lowerings. These were completed at 1542.

At 1559 we began to stream gear again, and by 1703 we were underway towards the Oman margin with both water guns in the water. At 1827 both guns stopped firing, and the cause was never determined with certainty. Three possibilities were discussed: 1) loose wires connecting the data logger and the gun fire control box; 2) a software bug in the data logger related to the timing and amplitude of the fire command pulse that goes to the new fire control box; or 3) some totally unexplained quirk in the oscilloscope used to monitor the detect signal from the blast phones. Though quite odd, the latter is considered possible because once the trip wires were disconnected from the 'scope, the guns resumed firing. A problem with the data logger seems unlikely; the command pulse was monitored for days after this event, and never showed any unusual features; furthermore, the system was re-booted at the time of the gun failure on the 20th, and it made no difference.

As we crossed the margin and reached water as shallow as 300 m, the aft two sections of the streamer were turned off; soon one of these was turned off also, leaving only the near trace. This is a 17.2 m section with hydrophones spaced 20 cm apart. Our speed was reduced from about 10 knots to 8. The quality of the records was very good in water depths as great as 1000 m. Beyond this point, where seismic returns weaken because of deeper water, we added 1 to 3 more sections.

At 2255 we turned NE to follow near the 300 m isobath along the Oman margin. This began our seismic and SEABEAM survey that was to identify coring targets and map out structures of the continental slope. The general plan was to run reconaissance strike and dip lines at spacings of 3 to 6 miles; this was to take about 3 1/2 days. Once a suitable coring area was identified, more detailed coverage yielding 50 to 100% SEABEAM coverage was to be collected for another 1 to 1 1/2 days.

We deployed our first sonobuoy at 0033 on May 21. Although in less than 1 second of water and with less than 1/2 second of sediment above apparent basement, no strong refractions were detected. Water guns yield good detail on oblique reflections, but refracting arrivals appear to be hard to detect.

At 1341 of that day we received a distress call from a sinking Indian freighter 90 miles to the southeast. Other traffic was in the vicinity, but it was not clear which ship - if any - was responding. We retrieved our gear at 1349, broke off from our survey, and headed for the stricken vessel's position. However, at 1836 we received word that another ship was on the scene and aiding the freighter; we turned, offset from the track we had just taken out from the margin, and returned along a parallel course that provided unique SEABEAM coverage. At 2123 the two water guns were firing once again, and at 0145 on May 22 we picked up where we had left off on the margin survey, for a total interruption of 12 hours.

At 0630 we crossed the first of many deep, V-shaped canyons that we soon learned characterize the slope north of 18.5 N. After several more crossings in this region it was decided that no coring targets would be found in this area, so we altered our plans to concentrate in the south.

At 1559 the starboard water gun began misfiring, and by 1622 it was aboard and we were continuing on the port gun alone. The solenoid from the water gun we had borrowed from URI was swapped in and the rebuilt gun was re-deployed at 1830. Despite a successful deck test of the solenoid and low pressure seals, the gun failed immediately after it was lowered into the water. A completely overhauled L-DGO gun was re-deployed at 2059, and we had no further problems with it.

Continuous single channel seismics, magnetics, gravity, echosounding and SEABEAM acquisition continued without interruption for the next two days. Sonobuoy #2 was launched at 0223 on May 24. Like the previous one, it was centered over the uppermost slope where water depths and sediment thicknesses were both minimal. Results were much the same as the previous buoy, i.e., good oblique reflections, but probably no refractions.

The starboard water gun appeared to begin misfiring at 0655, and it was at first suspected that the gun was at fault. However, it was determined that one of the boards in the new fire control box was malfunctioning. It was swapped with the card for the adjacent (unused) gun controller, and we had no further problem.

We had one of the very few bouts with bad SEABEAM data beginning at 1522. A board failed in the Bell gravimeter, producing incorrect but at first believable topographic information. When discovered at 1713, the problem was easily solved by switching to the Pyro vertical reference beam. Meanwhile, the board was replaced and the collection of useful gravity data resumed at 1830. Vertical reference information continued to be supplied by the Pyro device.

The starboard water gun began leaking near the end of the 24th, and by 2320 the low pressure alarm in the main lab alerted us to the problem. Because we were very nearly at the end of our survey, we decided to simply keep 500 psi in the gun to prevent flooding, and complete the work on the port gun alone. Consequently, the seismic quality of the last several hours was slightly below that of the previous part of our survey.

At 0259 on May 25 all gear was brought aboard, and by 0448 we hove to at station 4, the first coring effort of our program. Station work continued until 1024 on May 29. Guided by the dissolved oxygen measurements from our first two hydrocasts, our coring strategy was to collect samples at 100 m intervals from 1400 m water depth up to 300 m. The lowest oxygen levels appeared to be in the 500 m to 700 m range, and it was here that we planned to look especially hard for laminated sediments in both giant gravity cores and Soutar box cores. Locations, recovery and water depths of all cores are summarized in Table 1.

The ability to select desirable coring targets, re-locate them after completing the survey, and hold station while lowering the core were made possible by equipment and personnel. For the former, proper coring targets were easily identified on the excellent 3.5KHz records that we obtained during the survey. Furthermore, the SEABEAM swath showed conclusively how the single trace echosounder profile related to the local topography. Twelve or so hours of GPS positioning each day provided us the opportunity to re-occupy hand-picked coring targets. But none of this would have been of much benefit if station keeping ability had not been as good as it was. We were fortunate to have relatively calm weather for all of the coring work of this cruise, but because we often were trying to take paired piston and giant gravity cores at exactly the same location, station keeping was no small effort. The bridge did a consistently superb job in this regard.

Our deck operations were aided considerably by Jim O'Loughlin, the Chief Mate. While on coring stations the Captain stood Jim's watch on the bridge, and Jim directed much of the over-the-side work. In the beginning we tried to take alternate piston cores on the coring winch and giant gravity cores on the hydro winch. This ambitious effort soon became unreasonable, however, owing to both the rapid turn around time of shallow water cores and the frustrating difficulties we had with the gravity corer.

The latter problem was three-fold. First, the arrangement for deployment was awkward. We considered it crucial that the gravity cores be kept vertical until drained and sealed, so we tried to work out a way of rigging and retrieving the corer without ever laying it on its side. The Slattery crane on the 01 deck was used to hoist the core head to a workable height where it could be secured against the angle frame that also serves as its cradle. There an 8 to 10 foot PVC pipe was fastened on; lengths were limited by the span between that cradle and the main waist deck below. We had hoped to be able to extend the crane out away from the ship, the weight transferred to the hydro wire, and lowered away. Due to a malfunctioning jib, the crane had to be a fixed length, and this procedure was not possible. As a result, deployment and recovery required five people handling tag lines plus a crane and a winch operator to move the core in and out. Furthermore. the safety of securing the core against the crane cradle left a lot to be desired. After the second day, a steel bracket was welded against the cradle, and made us feel much better while working beneath the core on the waist deck.

A second problem with the gravity coring involved the adequacy of the catchers. Several of the earliest attempts came up completely empty, with a couple of them losing their sediment right before our eyes during recovery. Numerous explanations were discussed, but the general concensus laid blame on the catchers. We found that double catchers with a sock or some sort of added fabric sleeve between them led to better results.

A third problem probably involved the competency of the sediments themselves. We consistently got very short cores, despite evidence of several feet of penetration. None of the sediment turned out to be the soft, very watery laminated material we had expected; in contrast, it was surprisingly firm, occassionally coarse-grained and rather homogeneous. Suspecting this, we drove the corer into the bottom faster and faster, on one attempt reaching what we estimate to be 170 m/min. This resulted in only marginal improvement, and increased the occurrences of the wire becoming wrapped around the fins of the core head. We eventually switched from the 585 lb. WHOI corer to the longer and heavier (875 lb.) L-DGO corer. This seemed to improve recovery, but still the longest core was 2.68 m (see Table 1).

The piston coring went very smoothly. The new coring winch performed without any problems, although it leaked hydraulic fluid and its drag brake remained partially engaged during use. The first two cores were single pipers. With only one exception, all the rest of the efforts throughout the cruise were two pipers; one was three. Core 19-PC at 459m water depth bent the upper pipe; the next core was this other 1-piper. We had two collapsed upper liners in the first several deployments; the cause was most likely too long a scope; this was shortened a few feet and we had only one additional collapsed liner in core 27.

One special frustration, however, involved the number and condition of the available core pipes. Liners were occassionally very difficult to extract from the pipes, requiring a capstain and considerable manpower. One was so tight that it was set aside to work on when time allowed after the coring session was over. Another pipe was out of commission for several days until it could be straightened. A third was accidently dropped over the side. Consequently, we had to drag out some pipe that hadn't been used for a long time, and on one of these we couldn't even get the reeming device through during the cleaning operation that had to precede putting a liner in it. Several hours of ship time were wasted while we prepared these rusty pipes.

Once we settled on a deployment and recovery system for the Soutar box corer, this operation, too, went smoothly. We had hoped to be able to fit the device through the A-frame and out the door in the starboard rail, but this proved too difficult. Thanks to the Captain's intervention and order to swing the starboard air gun boom out over the rail, the box corer was deployed directly from the fantail deck and over the rail using the main ship's crane. Although dependent on relatively calm weather and requiring as many as 6 deck hands plus a crane and winch operator, this technique was satisfactory. All but one box core returned with a meter or so of good core. During the lengthy sampling task, we tried to make good use of the time by either gravity coring or conducting short SEABEAM and echosounding surveys.

Although we had both 3.5 KHz and 12 KHz pingers available for use on all of our cores, we found them not to be worth the trouble in most instances. It was thought that the 3.5 attached above the piston corer would yield especially clear sub-bottom echo records as well as provide precise information on the depth of the corer and its height off the bottom. Given the fact that the mechanical meter wheel on the new winch was somewhat inaccurate, we thought this latter attribute of a wire mounted pinger especially valuable. However, our shipboard 3.5 records were of excellent quality, and the wire mounted pinger records were only slightly better. Furthermore, because of the relatively shallow water, piston core trips generated obvious jumps on the wire Consequently, we stopped using the piston core pinger because any derived benefit was more than offset by the time it took to put it on and off the wire. Similarly, the 12 KHz pinger we had intended for the GGC was discontinued after several deployments.

The water depths were known accurately before lowering (thanks to SEABEAM and to the relatively smooth regions we cored in), and the hydro winch had a very reliable metering system.

Added to this was the inherent risk to a pinger when we found it necessary to drive the GGC into the bottom at ever higher speeds, and we soon concluded that with the GGC, too, pingers were not worth it. The gentle placement of the box corer on the seafloor, by contrast, required that a pinger be mounted on the frame. In the very shallowest deployments Ropate was able to see the touch down on the bottom.

The last several coring stations were to be box cores in the zone of the most pervasively laminated sediments. Hydrocasts 1 and 2 had predicted these would most likely be found between 700 and 400 m water depth. Surprisingly, all the box cores came up with worms and bottom dwelling organisms that exclude the likelihood of anoxic bottom waters and resultant undisturbed laminae. We conducted another hydrocast at station 36 for comparison with the previous measurments. Low oxygen levels were found again throughout most of the water column down to the bottom at 688 m. Thinking that the truly anoxic levels could be farther landward, we moved upslope and attempted an exploratory GGC at station 37 in 495 m of water. We had been noticing that cores tops were becoming increasingly coarse-grained with decreasing water depth; at station 37 the GGC broke off at the top of the PVC pipe, and it was suspected that sand-rich sediments were to blame. Rather than risk losing any more GGC catchers (two had pulled loose at station 34 and we had only two more), we chose to try trigger weight cores on the hydrowire in search of fine-grained muds to box core. In three attempts above 250 m we were almost totally unsuccessful (see Table 1). We decided to terminate the coring effort, and begin heading out towards the Owen Ridge.

We planned to split some of the cores onboard so we could see if we were recovering laminations and to sample some cores for work over the summer. We devised a core splitting rig (after many false starts) by fixing a circular saw on a stand fastened to a table. The saw was kept stationary and the core was passed under it on a tray made from a D-tray and the core was stabilized by foam tubing on the sides. The blade was adjusted to barely cut through the liner. The system worked well and we split five full cores (2 pipes) and several individual sections (includes both Oman Margin and Owen Ridge cores). We sampled split cores using 10cc syringes and 10cc DSDP sampling tubes. All sample holes were filled with foam tubing.

Before leaving the margin, however, we deployed the 700 cu. in. airgun at 1332 on May 29, and conducted three more sonobuoy experiments. Two intersecting lines were chosen that cross a thick sedimentary basin near the 800 m isobath. It is bounded on the landward side by a basement hinge zone where sediments appear to be less than 300 m thick. To seaward, structures that could be either continental rift-stage blocks, basaltic intrusions or a fringing carbonate margin are covered by an equally thin blanket. Between these features is a basin that was not fully penetrated by the water gun profiler. We used the largest gun available that could be run on one compressor and still be fired as often as once every 20 seconds. A dip line and two reversed strike line sonobuoys runs were acquired. Several refractions were detected, and it is likely that total sediment thickness will be measureable. If the outer fringing structure had been sufficiently linear we would have tried to measure velocities within it as well.

At first we attempted to record three of the streamer sections on individual channels in the hopes that back at L-DGO we could treat these as multichannel recordings, measure move-out, and actually stack the traces. The water was so shallow that this may have been possible despite the small offset between active sections. However, it becomes very difficult to monitor either the sonobouy or individual traces when more than two channels are being recorded on the data logger. Rather than chance doing something wrong and not being able to detect and correct it, we took the cautious route, summed all of the streamer sections, and together with the sonobuoy input, treated it all as two channel recording. Sonobuoy number 6 was terminated at 2300, and two water guns were redeployed for the transit to the Owen Ridge.

At 0132 on May 30 the port water gun began misfiring. When pulled and inspected, no serious problem could be found. Martin replaced a seal, however, put it together, and placed it in reserve. Meanwhile, the spare URI water gun was put in the water in its place at 0414.

Once we were down off the Oman margin and once again into deep water, the seismic reflections became weaker and the annoying 60 cycle noise experienced earlier in the cruise returned. It had been suspected that the lack of shielding on the deck cable could be partially responsible. It was thought that the extra 40+ feet of cable coiled behind the data logger rack was particularly careless. We passed this excess cable back into the aft MCS lab, away from the majority of computers and other RF sources, and the records cleared up dramatically.

At 1900 we passed the first way point of our Owen Ridge survey and began a series of long reconnaissance dip lines that crossed from the westernmost Indus Fan, over the Ridge, and out onto the debris flow plain to the west. Along the way we crossed 1/2 mile north of DSDP Site 224 at 2345. At 1746 on May 31 we ended the dip line survey and began a series of 60 mile north-south strike lines that provided from 50 to 75% SEABEAM coverage of the region.

A persistent 10-15 knot wind out of the west southwest prevailed throughout most of this Owen Ridge survey. As a result, we often noticed a clear improvement in upwind (north to south) seismic lines over the reciprocal headings that had a following sea.

The starboard water gun began leaking air at 0301 on June 2. By 0407 the gun was aboard and it was learned that the problem was with a failed air hose fitting at the towing bridle. A new hose was made up, and by 0737 we had two guns firing once again. In the interim, however, pressure built up in the single port gun, and at 1900 psi it began to double fire. It would not appear that this is a designed safety for the gun - it is advertized to operate at 2000 psi. We bled a little air out of the system, and as long as pressure was kept below this value we had no further problems. For most of the seismic work throughout the cruise we operated at 1600 to 1750 psi.

Problems with the water guns cropped up again towards the end of that same day. The port gun resumed misfiring despite standard pressures, and at 2002 it was turned off for a short time. What followed was a frustrating several hours of theorizing, testing, trouble shooting, and little resolution of the fundamental problem. At the end, the problem seemed to fix itself, and we were not sure what had solved it. The symptoms were weak, jittery returns that appeared to indicate either badly synchronized guns, erratic fire command pulses,a bad deck fire line, and/or malfunctioning guns.

For a while the first explanation seemed likely. The fire/detect control display had been showing no problem whatsoever; this instrument told us that the guns were firing within .25 msecs from one another, but the profiles told us differently. Because of both bad CRT's and bad controlling knobs, neither Tektronix oscilloscopes in the data logger rack could be used to monitor the blast phones. Kurt then installed the new digital 'scope that had not been made available to us, despite our requests that a better 'scope be located and used. With this he was able to determine that indeed the troubled gun was firing 4 msec or so late. With a 4 msec delay added to the port gun at 2255, the records returned to their former appearance of strong, continuous returns until about 0130 on June 3. Erratic double firing on this same port gun resumed, and even when we turned the fire pulse off this gun continued to fire. So we kept this gun off, isolated it and reduced its pressure to 1000 psi, and continued on the starboard gun alone.

At 0352 the solitary starboard gun began to misfire. We slowed to 5 knots, retrieved the dead port gun while continuing to use the ailing one on starboard. At 0518 the spare URI water gun was made up and in the water, replacing the port gun. At the same time, the starboard gun was pulled. The seismic data looked awful. We still had the weak, jittery returns that had plagued us the night before. The possible explanations were reduced at this point. The problem affected different guns, different firing cables, was not solved by re-synchronizing by a fixed offset, and occurred regardless of which detect module was used in the fire control box. At 0616 we slowed to 4.3 knots to begin seeing what effect towing speed might have on the record quality. We incremented speed by about 1.5 knots, holding each speed for about 10 minutes. As expected the data quality was best at low speeds, but to our relief we found that when we finally returned to 9.3 knots the jittery records were cleared up. Hoping that the problem was now behind us, we prepared and deployed the second starboard gun at 1019.

Although not a completely satisfying explanation, we are left with the possibility that the problem was with a fouled tow cable on port. We had been making some rather abrupt 90 degree turns over the last several days of our grid survey, and when the port gun had been retrieved at 0352 we found that the wire rope tow cable was badly chafed. It could have been that this gun was being pulled in some unstable, fouled configuration, and that this led to its misfiring, perhaps even to its spontaneous firing. These problems never occurred again for the remainder of the cruise.

Over the previous several days John Freitag had been designing and wiring a new card for the SEABEAM acquisition computer. Its purpose was to provide more accurate gating so that interference from the 3.5 KHz echosounder could be eliminated, or at least reduced. It proved to work well, although it occassionally allowed minor interference to creep in during some especially rapid ascents up the steep eastern face of Owen Ridge. One drawback, however, was that the digital vertical beam read-out on the SEABEAM panel was incorrect as much as 90% of the time. Though this number had never been used as the depth beneath the keel (we always consulted the 3.5 KHz recorder for this) it was useful for quick, ball-park numbers when setting delays on the profilers, estimating time for a core to reach bottom, etc.

At 0521 on June 5 we completed the closely-spaced grid survey of Owen Ridge and retrieved the seismic gear and the magnetometer. At 0713 the first giant gravity core of this phase of our program was over the side. Throughout the next 3 days and 3 hours we were in a coring mode, the results of which are summarized in Table 1. The strategy was to collect a transect of cores from the top of the Ridge at about 1900m down to the lower reaches of the western flank at nearly 3500m. At each station we first collected a GGC; after retrieving the core but before breaking it down for the next deployment we then rigged and launched a two-pipe piston core. While this was over the side we easily had time to prepare the next GGC. Often we were breaking down the PC while enroute to the next station. At four locations a plankton net was hung off the boat deck on the port side. Samples were fixed in formalin and packed for shipment back to Miami.

During the period of June 5 to June 6 the weather started to change. We saw high cirrus and coumulo-cirris clouds move in the area and large, long period swells came in from the Southwest. This interval probably marked the onset of the monsoon in the western Arabian Sea.

The detailed topographic and seismic survey of the Ridge revealed a complex western flank riddled with slump scars. This seriously restricted the coring opportunities. The very crest of the Ridge appeared to be intact, and site location was rather easy. But as little as 200m below the crest are large and numerous erosional outcrops or debris flow deposits that we detected on the 3.5 KHz records and/or on the seismic profiles.

As opposed to those of the Oman margin , the sediments of the Owen Ridge are highly calcareous and apparently this contributed to the excellent recovery by the GGC. Because of the disappointing results earlier, this time we used only the heavier L-DGO core head with its full complement of weights (total about 875 lbs.) We also eased back a bit on the entry speeds, generally hitting the bottom at 75 to 100 m/min, far less than the flat-out 175 or so that was attempted on the Oman margin. We had none of the problems of poor penetration, failed catchers or broken PVC pipes that had plagued us earlier. All deployments returned at least 2m of good mud. Because of the difficulty of rigging anything longer, we stuck with 8 or 10 foot core liners. We placed a 12 KHz pinger 40m above the core in all cases, and with this being recorded on the entire SEABEAM swath of transducers we were able to see the core head, the single glass float 10m above (used to minimize the chance of wrapping the wire around the core), and, of course, the pinger itself another 30m above the float. Despite the precautions of the float, the wire came back wrapped on the core head fins more than once .

The piston coring, too, was very successful on the Owen Ridge. The Chief Mate continued to direct the efforts on the fantail, and his help was greatly appreciated, but gradually our own team was able to work smoothly without much guidance. In all instances we were aided by both calm seas and no bent pipes. Ropate's experience at winch operations proved very valuable. In the few swells and seas that we did encounter, hits at several thousand meters were sometimes quite subtle. We had pingers aboard to place on the wire if needed, but putting one on and off the wire each time would have added up to a fair bit of time. Ropate's keen eye never missed a trip, and we dispensed with the pinger.

We were aided by as many as 13 or 14 hours of GPS positioning during each day of these coring operations. Station keeping was greatly facilitated by this convenience. Few coring stations were more than 10 miles apart. We generally turned SEABEAM on during these short transits both for added coverage and for locating the coring stations. In most cases we were able to find seafloor that appeared to be free of slump deposits and with numerous parallel sub-bottom reflectors on the 3.5 KHz records. Often, however, these desirable targets were on local highs a few miles from steep-sided slump scars.

Only one box core was attempted on the Owen Ridge, and it met with difficulties. It was deployed in nearly 3500m of water, far deeper than either of the successful recoveries along the Oman margin. Perhaps this was partly responsible for our problems. The corer itself at 3500m is far outweighed by the wire. Although we had a pinger mounted on the frame and could see the device settle onto the bottom, it appears that too much wire settled down on top of it - there were clearly visible wire rope scrapes along the painted edges of the frame when retrieved. Furthermore, there was mud on the top of the frame, indicating that the device either tipped over or was dragged sideways and spilled while on the bottom. Lastly, the spade closures at the base of the corer were seriously bent, either from being dragged along the bottom or from getting entangled in the cable.

We had additional problems with the core winch during recovery of this same box core. Perhaps again because of the very light load, perhaps further complicated by kinks put in the wire during the awkward lift-off from the bottom, several bad wraps developed on the core winch drum. Once a few bad wraps were wound on, the level-wind was unable to properly lay on the subsequent wire. We ended up having to yo-yo the corer up and down numerous times, trying repeatedly to nudge the level-wind across the bad spots on the drum. This operation ended up taking several extra hours, and still left us with a poorly wound wire. We weren't sure that any subsequent use of the the core wire would go well at all until the bad wraps were corrected, and this could only be done by streaming the wire out beyond 3500m.

Because of the above problems, no additional box core deployments were attempted.

We wanted one last sample from the top of the Ridge, so we turned to 1880m water depth and took the last GGC of the cruise at 2104 on June 7. Unfortunately, this was the only attempt of the Ridge portion of the cruise that was less than 100% successful. For some unknown reason the PVC pipe bent and nearly broke about half-way up its 10 foot length. It is possible that too much wire was paid out after the hit, although the pinger record clearly showed that the distance between the float and the bottom remained a constant 10m between the time of hit and the pull-out. Perhaps we simply failed to penetrate very far, and the core partially fell over under its own weight. Nonetheless, we recovered nearly a meter of good core.

In an effort to retrieve one slightly longer climate record, we attempted a three-pipe core at this last station on Owen Ridge. Because of the risk of bending one or more of the last few good pipes, we had saved this to last. Once over the side we had a successful 16m recovery. Getting there, however, was another story. The first problem involved the launch. The ship took a roll just as the core head was being raised out of the cradle at 2315. As the head swung out unexpectedly, all but the very bottom of the core (60 feet forward along the rail) prematurely slipped off the gunwale brackets. Suspended thus at the cutter and at the head, the middle pipe was slightly bent before we could pry the core off the last remaining bracket.

Once over the side and hanging vertically, it was discovered that the core wire had somehow jumped the shieve, and was wedged in the A-frame totally outside the cheeks. Ropate and Jim together agreed that we had little choice other than to land the core, dismantle it, terminate the wire and feed it through the shieve once again. Sam Gerard, however, saw it differently and insisted that we secure the core against the rail and, still in the vertical position and suspended from the chain fall, attempt to wind out enough slack from the core winch to feed the wire back over the shieve. This was done, but after some time inspecting the situation up on a ladder it was determined that only a wire under considerable load would be straight enough to fit back in the narrow confines around the shieve. We ended up having to land the core, dismantle it, terminate the wire to slip it back out without a feige fitting, re-thread it, and make up an entirely new core. This operation took several hours, and rather than merely sit on station, we designed a short SEABEAM/3.5 survey of a zone of deformed Indus Fan sediments at the eastern foot of the Ridge. Back at the site at 0605 on June 8, we took a successful 3-pipe core.

Explaining the mishap involves one or more of the following. First, during the initial winch test on the Pt. Hedland-Colombo leg the winch controls were found to be difficult to operate smoothly. With a core head on the wire, the winch apparantly ran away and two-blocked the core against the A-frame shieve. It took a large gouge out of one of the cheeks on the shieve and broke a guide that restrains the wire from jumping the shieve as happened to us. Second, because of the kinks that developed in the core wire during the box core attempt on Owen Ridge, there may have been a strong tendency for the wire to not lay properly over the shieve, especially during the initial core deployment en the majority of the tension is taken up by the chain fall and not by the core wire itself. Lastly, the clumsy deployment of the three-piper described above may have contributed to the situation.

The bad wraps on the core drum inherited from the ill-fated box core attempt made for a slow recovery of this piston core. It became apparent that in addition to having problems laying wire across the bad wraps, the level wind simply was not properly adjusted. This became even more apparent in the next - and last - coring effort on the deep Indus Fan.

All coring gear was secured, we got under way for the Indus Fan, and the maggie and seismic gear were deployed by 0841 on June 8. Kurt had been working on the most recent problems with the fire control box, and discovered that whenever one of the guns fired spontaneously, all subsequent fire commands are adversely affected. In essence, if a gun fires off-cue, then the software in the fire control box tries to compensate on the next shot. If the gun continues to fire erratically, the mis-timing simply gets into a feedback loop where it is unlikely that regular shot spacing can be re-established. Kurt worked on a new algorithm that essentially narrowed the time during which the detect box looked for a shot. This new software was installed at 0952 on June 8, and was accompanied by a necessary delay on all shots of 50 msecs relative to the previous fire control system.

Our strategy with the short time remaining in our cruise was .o cross back over the Indus Fan, offset from our outgoing track by about one to two SEABEAM swath widths. In particular, we were interested in learning more about the frequency and morphology of channel systems. Several had been observed in all three acoustic systems on the outgoing track. One particularly good example of a channel/levee system was identified near 16 10 N by 60 37 W. We detoured from our track at 1347 to intersect this feature, and then followed it upstream for the maximum amount of time available. We were able to see both levees on the swath plotter, and keeping them in view we directed the bridge through a series of turns that allowed us to follow a tightly meandering system for a few tens of miles. The Captain had calculated a break-off time of 2200 in order to be on schedule for Colombo. We wanted to take a core on the levee, so this meant we had to haul in the seismic gear at 1842. We hove to over a particularly thick and moderately reflective levee. It was in marked contrast to the highly reflective, presumably coarse-grained sediment of the inter-levee regions. At 2252 a successful 2-pipe piston core was back on deck, and by 2328 the seismic gear was re-deployed and we were underway for Colombo.

To aid in the estimation of thickness and character of sediments in this survey area, sonobuoy #7 was deployed as we departed this core site. Surprisingly clear refractions were observed, and promise to provide helpful information should this ea be considered for a future drilling target.

The weather had been building since the last core on the Owen Ridge the day before, and by early on June 9 the winds had

increased to a steady 20 knots (gusting to 40+) and we had a
quartering swell of 10 to 15 feet. Fortunately all of this was pushing us towards our destination. The seismic records continued to be of excellent quality, despite our speed of 9.5 to 10 knots, a heavy sea and a rolling ship (up to 35 degrees.)

Both engines were shut down momentarily at 0150 while the ailing port engine was taken off line for inspection; the starboard engine was up again five minutes later. A problem of poor piston seals had long been known about in the port engine, but it was rapidly becoming a serious problem. At 0347, both engines were back up and running.

A water leak in one of the generators required shutting the water gun compressor down for twenty minutes beginning at 1300. Meanwhile the winds and seas continued to increase and hold their position, coming from slightly aft of our starboard beam. Torrential rains blew down on us during squalls that came by every few hours. Needless to say, this weather would have totally prevented us from doing any station work.

The Captain was anxious for us to retrieve the seismic gear so that the bridge would be free to increase our speed as needed to make port on schedule. It is likely that he was also wary of the weather turning so much worse that retrieving the gear would be even more risky than it already was. We turned into the wind at 0112 on June 10 and had little trouble recovering the streamer and the two water guns, all of which were still returning excellent records despite the conditions. The maggie was left in the water, and we then resumed our heading for Colombo. Ironically, we were not able to make more than 10.3 knots even then.

The port engine continued to be a worry, and at 0753 the lab was notified that engine speed was being reduced from 166 to 40 rpm. We were concerned that the maggie would be hanging to nearly vertical at this speed, so it was hauled in and was aboard at 0812. All engines were stopped at 0835, and the port engine was shut down for the remainder of the cruise. We were back up to 155 rpm on the starboard engine at 0845, and soon doing 7.5 knots. With a following sea and winds over the next few days, this speed over the ground increased at times to 8.5 and above. Nonetheless, the Captain estimated that our ETA in Colombo would be pushed back two days to early on Monday, June 16. Favorable winds and seas continued unexpectedly, and this required our slowing at times to less than 100 rpm to arrive at the pilot station at first light on the appointed day. Lab watch was terminated when we reached the continental shelf 12 miles off Colombo at 1356 on June 15, with the pit log reading 5663.

RECCOMMENDATIONS

Deck related:

1. Gear handling off the hydro winch is very awkward due to a) failure of the crane jib, b) design of the crane A-frame support and c) inability to move the crane laterally when

spending a load. The first problem resulted simply from improper installation; presumably this will soon be corrected. For the latter two problems, we recommend that more thought be given to accomodating the varied uses for this crane: CTD, heat flow probes, hydrocasts, etc., in addition to gravity coring. It seems near-sighted for a crane to be installed that cannot be used for moving scientific gear over to the hero platform. Not only is the crane apparently incapable of this maneuver under load, but the design of the A-frame support forbids it: the hydrowire is fed between the struts. This dictates the additional constraint of not being able to raise the crane in a vertical plane more than about 4 feet. Although a makeshift cradle was devised in which to land the giant gravity cores, the A-frame should be strengthened, and additional supports and brackets should be installed to make gear handling easier and safer. With TV monitor cameras now avialable on the fantail, would it be possible to add a camera and/or talk-box near this hydro-crane?

2. The main trawl winch needs numerous revisions, fix-ups, and redesign(?). The problems are several: first, the winch is very slow, as was especially apparent on deep water deployment when wire times were excessive. Second, the hydraulic drag brake is not connected properly. For the first coring session off Oman it grabbed constantly, adding noise, heat, wear and drag; while -ing on the Owen Ridge it was re-configured, reducing the ..kelihood of our being able to halt a run-away winch. Fortunately we had no need for it, but the next PI may not be so lucky. Third, the level wind apparently needs adjusting; out beyond 2000m we had problems with laying on wraps properly. Fourth, the meter wheel does not work, but this is a result of damage on the testing leg; presumably it will be repaired soon. We used a mechanical meter designed for 1/2 inch wire on the 9/16 inch wire, and it was neither accurate nor safe for the person reading the meter. Lastly, there is a small leak of hydraulic oil around the main bearing on the winch; we merely collected the drips in a pail. Each of these last three items can probably be taken care off easily with adjustment and replacement parts. By contrast, the problems with the speed and the brake are fundamentally design problems, and though more difficult to overcome, are clearly of considerable importance.

3. Scientific storage is very limited, and this became especially apparent due to our emphasis on coring. Much of the available space in the wet lab is taken up by multichannel seismic gear. The biolab was used mostly as a storage and staging area because no other space was available. Perhaps vans could be used to either store multichannel gear or provide space for core storage and work space.

4. Core pipes need to be kept clean. We lost considerable ..me re-rigging cores when liners would not fit into pipes whose inside diameters were reduced simply because of rust.

5. The door along the starboard rail outboard of the A-frame

snould be widened. At present it is about 63 inches fore and aft, and is not centered beneath the A-frame. Widening this opening will require redesign of the piston core cradle, but we don't feel this outweighs the benefit of having a useable space for deploying gear such as box cores.

6. Water gun deployment and retrieval should be restructured. Once in the water, the two guns tow very well - they are stable and relatively safe during ship turns. However, their deployment and recovery still requires two to three people in addition to Martin, and the work is strenuous and unnecessarily hazardous. The need to keep the connecting lines under heavy tension while lowering and raising the guns can lead to people leaning out over the rail and/or risking entanglement in hoses that can be pulled out of one's grasp during even moderate seas.

7. The SCS deck cable should be shielded. Our problem with 60-cycle noise was reduced considerably by merely moving the 40 or so extra feet of cable out from behind the data logger rack, where it was obviously picking up a lot of RF noise. The most desirable design would be for a shielded cable to lead into a the summing box located in the aft lab. From there a single coaxial cable could lead to the data logger rack in the main lab.

) related:

1. There ought to be better communications between the fantail and the main lab. At present, the squawk box is unreliable, and when working, cannot address just the main lab. Similarly, an intercom with the hydrocrane operator would be a great benefit.

2. There is still no way to determine conclusively that seismic data is being written to tape properly. At the very least, a program ought to be written for the VAX that will do some rudimentary checking other than what we did on this cruise, which was to simply learn that something was on the tapes (we couldn't tell what it was) and that each tape we looked at had the same amount of something on it.

3. The two data logger tape drives are a mainstay of data recording, and it is imperative that they be maintained in excellent working condition. This could be best insured by a routine reconditioning during every few port stops. Both drives failed periodically throughout the cruise despite Harrys'constant vigilance and efforts to keep the vacuum columns adjusted and the doors sealed.

We were both very pleased with the accomplishments of this cruise, and consider that it was an enormous success. We are grateful to the crew and officers of the CONRAD. They performed their respective tasks competently and provided us with help whenever it was needed. We draw particular attention to the excellent station-keeping that the bridge maintained for all of our station work. Captain Roberge maintained clear and professional control over the many responsibilities under his command, and we look forward to sailing with him in the future.

Dr. Warren Prell and Dr. Gregory Mountain

TABLE 1

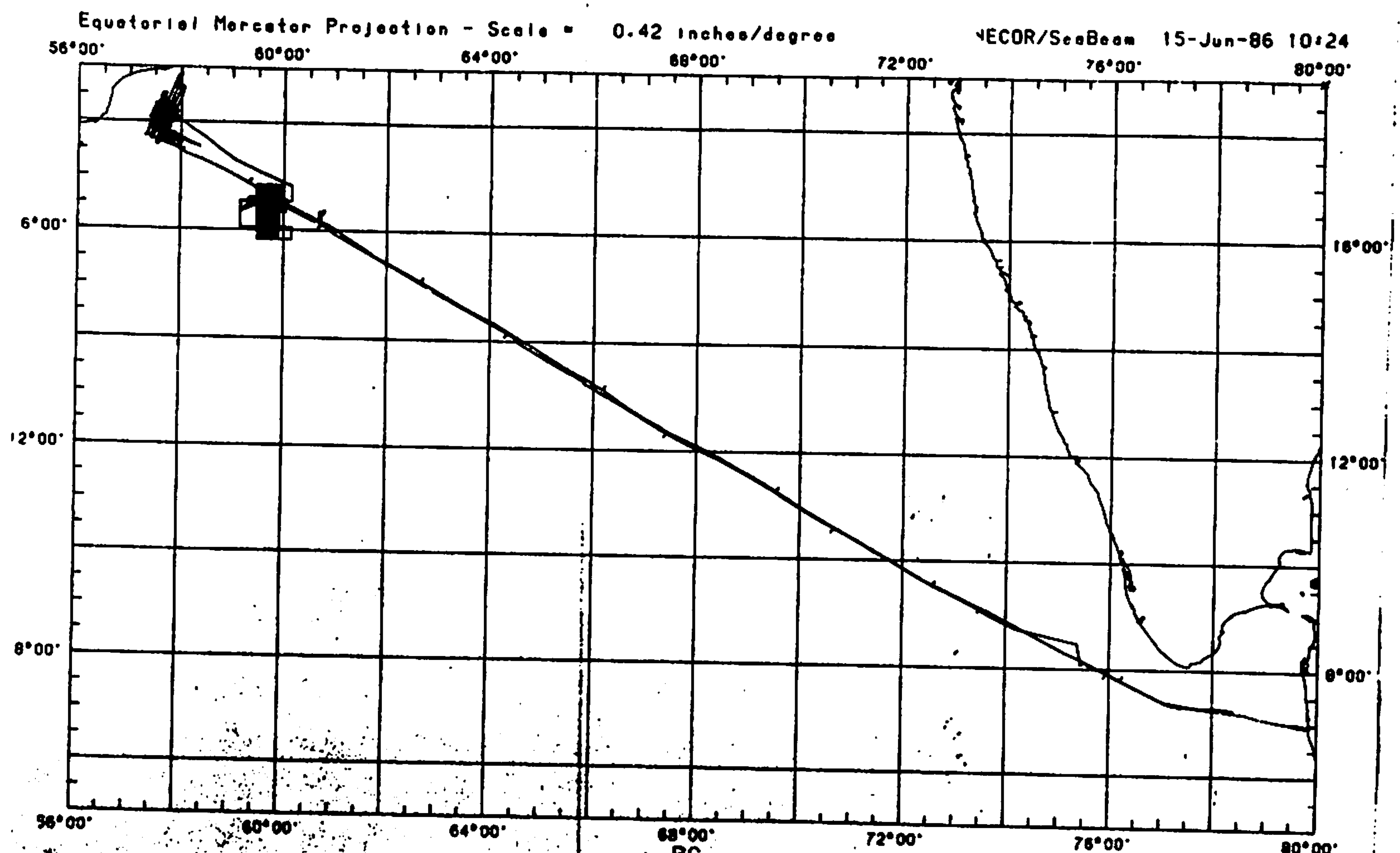
RC27-04 STATION LIST

SHIP STATION	STATION NUMBER & TYPE	TIME	LAT N	LONG E	WATER DEPTH	DATA
1	C-1PC					
2	H-1	140/10:59	17 29.5	58 02.8	2020	8BOT,0-600
3	H-2	140/14:30	17 30.3	58 03.5	2092	8 BOT,600-2000
4	C-2PC	145/06:34	17 41.6	57 52.6	1474	5.94M
5	C-3GGC	145/0817	17 40.5	57 53.0	1473	0.00
6	C-4PC	145/11:17	17 38.6	57 43.9	1276	5.93M
7	C-5GGC	145/12:43	17 38.7	57 42.9	1275	0.00
8	C-6GGC	145/15:14	17 37.7	57 43.0	1275	0.28M
9	C-7PC	145/17:43	17 42.9	57 37.6	965	10.10M
10	C-8GGC	145/19:14	17 42.6	57 37.3	957	1.02M
11	C-9PC	145/21:13	17 46.7	57 37.3	891	10.38M
12	C-10GGC	145/22:23	17 45.8	57 39.5	889	1.23M
13	C-11PC	146/00:43	17 53.7	57 34.8	861	11.04M
14	C-12PC	146/04:09	18 03.8	57 37.0	788	11.63M
15	C-13PC	146/07:25	18 14.6	57 41.0	689	10.34M
16	C-14PC	146/12:12	18 15.2	57 39.3	590	10.92M
17	C-15GGC	146/14:10	18 15.6	57 39.2	579	0.00
18	C-16PC	146/16:30	18 16.1	57 37.8	479	10.26M
19	C-17PC	146/20:10	18 16.6	57 37.6	438	9.81M
20	C-18GGC	146/21:57	18 17.8	57 38.6	452	0.59M
21	C-19PC	146/23:51	18 17.4	57 38.6	459	5.77M
22	C-20PC	147/02:31	18 16.1	57 33.4	296	5.43M
23	C-21PC	147/05:41	18 06.6	57 31.8	569	12.03M
24	C-22PC	147/11:44	18 04.2	57 33.8	749	11.25M
25	C-23PC	147/15:16	17 59.7	57 35.4	815	10.83M
26	C-24PC	147/19:40	17 43.1	57 49.2	1416	11.74M
27	C-25PC	147/22:27	17 37.6	57 39.9	1347	10.50M
28	C-26PC	148/00:52	17 40.7	57 39.9	1064	11.21M
29	C-27BOX	148/08:04	17 54.5	57 35.5	860	0.56M
30	C-28GGC	148/12:03	17 54.2	57 35.4	866	2.78M
31	C-29BOX	148/14:43	17 53.1	57 37.3	868	0.00
32	C-30BOX	148/18:47	18 13.0	57 41.0	698	0.51M
33	C-31GGC	148/20:09	18 12.8	57 41.1	707	2.68M
34	C-32GGC	148/22:30	18 14.3	57 38.7	601	0.00
35	C-33BOX	148/23:35	18 13.5	57 38.8	630	0.49M
36	H-3	149/01:10	18 12.6	57 38.9	688	8 BOT/0-600
37	C-34GGC	149/03:45	18 13.9	57 35.0	509	0.00
38	C-35BOX	149/04:48	18 14.0	57 35.9	498	0.42M
39	C-36TW	149/07:56	18 19.5	57 30.1	185	0.00
40	C-37TW	149/08:22	18 20.5	57 31.2	188	0.00
41	C-38TW	149/09:08	18 19.7	57 33.5	247	0.15M
42	C-39GGC	156/0748	16 37.2	59 51.9	1889	3.05M
43+	C-40PC	156/0923	16 37.2	59 52.1	1890	11.40M
44+	C-41GGC	156/1231	16 31.7	59 47.3	2052	2.01M
45	C-42PC	156/1355	16 31.1	59 47.0	2044	11.11M

46	C-43GGC	156/1649	16 36.2	59 46.0	2159	2.45M
47	C-44PC	156/1837	16 36.2	59 46.0	2159	11.75M
48	C-45GGC	156/2121	16 36.0	59 40.8	2406	2.88M
49	C-46PC	156/2337	16 38.8	59 42.7	2441	10.65M
50	C-47GGC	157/0213	16 38.2	59 41.5	2530	2.89M
51	C-48PC	157/0352	16 38.1	59 41.3	2545	11.79M
52	C-49GGC	157/0612	16 39.1	59 39.1	2686	2.94M
53+	C-50PC	157/0804	16 40.2	59 39.6	2707	12.07M
54	C-51GGC	157/1122	16 42.2	59 36.0	2918	2.68M
55	C-52PC	157/1322	16 40.8	59 35.9	2985	11.89M
56	C-53GGC	157/1808	16 32.3	59 27.3	3258*	2.37M
57	C-54PC	157/2031	16 32.1	59 27.3	3275	11.76M
58+	C-55GGC	158/0014	16 28.6	59 21.2	3392	2.72M
59	C-56PC	158/0247	16 29.0	59 20.7	3405	11.59M
60	C-57GGC	158/0620	16 20.9	59 17.6	3490	2.92M
61	C-58PC	158/0828	16 22.0	59 11.2	3484	11.84M
62	C-59BOX	158/1207	16 22.0	59 10.9	3488	0.00M
63	C-60GGC	158/2104	16 36.8	59 51.5	1887	0.93M
64	C-61PC	159/0608	16 39.5	59 31.4	1893	15.97M
65	C-62PC	159/2122	16 09.6	60 40.8	3997	11.78M

All core lengths are corrected for voids and short sections. An * indicates that 3.5Khz depth, wire out and pinger depths did not agree.

A + after the ship station indicates a surface plankton tow.



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