CRUISE REPORT RC2512

ITINERARY:	St. Georges, Bermuda - San Juan, Puerto Rico Oct. 16 - Nov. 10, 1984		
WORK COMPLETED:	6900 km (3730 n. miles) of digitally recorded single channel water gun seismics and 20 sonobuoys; continuous magnetics and 3.5 kHz echosounding; 7 piston cores.		
FUNDING AGENCY:	NSF		
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OBJECTIVES:

The scientific objectives of RC2512 involved collecting data relating to both the tectonic and the paleoceanographic history of the western Bermuda Rise. For the first objective, we collected long seismic profiles oriented parallel to presumed crustal flow lines which together with underway magnetics will be used to evaluate the amount, time and distribution of basement uplift that formed the Bermuda Rise. The second objective called for a more detailed seismic survey of several peculiar lenses of sediment buried beneath Neogene strata on the southwestern Bermuda Rise. With high-resolution seismics and piston coring of presumed outcrops of equivalent strata east of the lenses, we hoped to determine the age and origin of these lenses, and assess their relationship to the seismic stratigraphy and paleoceanographic history of the region.

OPERATIONS:

We were prepared to depart St. Georges at 1200 (all times cited here are GMT) on Tuesday, October 16 and were scheduled to take on fuel at the Navy fuel dock on the north side of the island. However, adverse swells from distant hurricane Josephine forced us to remain in St. Georges harbor and take on fuel delivered by trucks. We finally cast off at 2100 of that same day.

The pilot stepped off CONRAD at 2130, the pit log was zeroed, and we began to deploy gear. We began 3.5 kHz echosounding at 2200. By 0040 hrs. of October 17 the newly purchased Teledyne streamer was deployed by hand and the two L-DGO water guns (each equipped with newly fabricated towing fins) were over the side. Immediately both guns began firing erratically and had to be retrieved to replace O-rings. At 0140 the magnetometer was streamed and recording was begun.

Lacking a working water gun, a 3.5 kHz survey of the rough topography around DSDP Site 386 was designed so that basement outcrops could be avoided when seismics resumed. One water gun was back in the water by 1400 on October 17, but it was then clear that the streamer was too shallow and too much wave and towing noise was being recorded. We decided to add weight to the streamer. Unfortunately, the old L-DGO streamer was still on the take-up drum on the 01 deck, so it had to first be removed. Faulty hydraulic work has apparently been done recently on CONRAD, because when starting up the pump to drive the streamer winch we experienced the first of several broken hydraulic lines that plagued us throughout the cruise. While hose repairs were being made, the Teledyne streamer was retrieved by hand. Two pounds of lead were added to every five feet of the dead head section for a total of 16 lbs. When redeployed at 1900 the streamer still showed a surprisingly poor signal-to-noise ratio; traces from each of the four isolated 25-m sections were examined on the shipboard oscilloscope, but none was found to be performing differently from the rest. For comparison, the old L-DGO streamer was deployed but it, too, showed disappointingly poor seismic records and we were convinced that the Teledyne streamer was not at fault. By contrast, the URI ship ENDEAVOR recorded some excellent single water gun profiles in the same general region on their cruise 91 in October of 1982. Towing speeds were comparable (7.0 knots on ENDEAVOR, 6.5 knots on CONRAD); they may have achieved their superior results because of twin streamers towed in parallel. It is hoped that if indeed experience bears this out that a second streamer can be purchased for CONRAD. Alternatively, we are not convinced that the recording system aboard CONRAD is not partially at fault. A tremendous noise level was traced to Ithaco amplifer #3 at one point in the cruise and taken off the circuit; weak 10Hz noise synchronized to the gun firing times showed up late in the cruise and its origin was never determined. However, if system noise is present, it must be external to the digitizer because both the analog recorders AND the digital monitor suffered from excessive noise. This leaves only the streamer, tow cable, deck connector, deck cable and "SCS I/O panel" in the main lab as suspect, because from this point on, the analog and digital circuits are separate.

With few other choices remaining in an effort to reduce noise, we added another five pounds to each of the two active sections on the Teledyne streamer for a total of 26 lbs, and re-deployed it at 2100 with several feet of shock cord at the tow point to minimize surge on the cable. Our noise problem may have been that the streamer was STILL too shallow. Although there are depth transducers on this new streamer, we have no shipboard monitors; those for the MCS streamer are incompatible. It is hoped this can be rectified before long.

At 0238 of October 18 we were near DSDP Site 386 on a course across the thickest part of the sediment pond that contains the drill site and, despite having only one water gun in the water at the time, we launched the first of 20 sonobuoys to be used on the cruise. At this point we were beginning the first of several NW-SE profiles across the western Bermuda Rise. Each was designed to cross DSDP drill sites and stay as far as possible from known fracture zones and accompanying basement highs.

The second water gun (with towing fins) was repaired and in the water by 1220 of October 18. Only a modest improvement in data quality was observed. Although we were not totally satisfied with the S/N ratios even at 6.5 knots, we realized that we were behind schedule already, and to meet our objectives farther down the line we chose to tolerate the disappointing records and increase our speed to 7.5 knots at 1445. The low cut filters were increased on all recorders to minimize the increased towing noise. As a further step towards noise reduction, the streamer was moved over to tow from the starboard boom at 1600 on October 19. It was then out of most of the ship's wake, and a modest improvement was noted.

Suspecting that perhaps the water guns themselves were being towed too deep we decided to shorten their tow cables. As mentioned, new tow fins were on the guns for the first time, designed to stabilize each gun at towing speeds higher than we've used in the past (ca. 7.5 knots vs. 6). We thought this stabilization especially important because our past experience has shown synchronization of firing times of two guns requires that both guns be at the same depth, give or take a couple of feet. Because of the uncertainty of how the guns with their new fins would tow, the cables had been lengthened to 96 feet before the cruise; we shortened each back to its former length of 63 feet at 1300 on October 20, but found no improvement in S/N ratio. Nonetheless, we left the guns at this tow length for the remainder of the cruise.

While the port gun was aboard for the tow cable replacement it was found that one of the two 3/4" bolts that join the gun to the tow fins was missing. It was replaced and redeployed when the 63 foot cable was made up.

At 0912 on October 21 we entered the region of aftershocks that followed the Bermuda Rise earthquake of 1978. We slowed to 6.0 knots to improve seismic data quality and conducted a grid survey looking for faults, slumps and structural discontinuities that may be related to the seismicity of the region. At 1715 we resumed 7.5 knots and returned to the long NW-SE profile plan along presumed crustal flow lines.

A nearly catastrophic problem with the port water gun occurred the next day. The gun stopped firing at 1008 on October 22 and began slewing severely towards starboard. It was brought along side to where it was determined that the main towing bridle had parted and the gun was hanging only by the airhose. All other gear was retrieved and the ship was brought to dead halt, engines stopped. G. Mountain went over the side, attached a line to the stainless bracket at the rear of the gun, and it was hoisted aboard. It was found that the main shackle attaching the chain-plate bridle to the tow cable had parted; rust on the broken shank suggested that it was a faulty shackle that had long ago developed a hairline crack. It was decided that because of the added weight, drag and possible rattling of the fin/gun couplings, the towing fins were not worth the risk of losing a water gun, even if we would not be able to synchronize both guns as well without them. While the ship circled, the fins were removed from each gun. At 1330 one gun was back in the water and firing and we were back on line; by 1630 the second gun was in as well. To our relief, the two guns remained synchronized to fire within 1 or 2 msec of one another at all times. Clearly the fins had not been worth the added strain on the towing harnesses.

While re-deploying the magnetometer at 1345 the rubber boot covering the coupling that joins the cable to the maggie bottle pulled loose and wiring in the bottle was damaged. It is possible that the whole device had been strained to nearly the breaking point by a recent shark bite; fresh gorges were visible on the metal housing. The bottle was opened, the wiring repaired, and we were recording magnetics again by 1512.

This close synchronization was determined by monitoring the blast phones on a dual beam oscilloscope. Oddly, the shot times on the Data Logger monitor screen occasionally showed that gun #1 (starboard) was firing ca. 50 msecs before the other. This continued throughout the entire cruise, and we believe that the Data Logger was picking up some unknown precursor pulse. The graphic recorders, by contrast, worked identically while tripping off either blast phone. The precursor is probably somewhere in the Data Logger circuitry. For processing we will use blast phone 4 (port gun) exclusively.

At 2226 on October 23 we experienced the first of several crashes of the Data Logger. In this instance, it was caused by operator error. The tolerance of errors in changing recording parameters is dangerously narrow; it is hoped that software changes can be made so that typo errors do not so easily crash the program. With this and all other crashes no more than 8 minutes of data were lost; often it was closer to 3 minutes.

By 0800 on October 24 we entered the region on the southwestern

Bermuda Rise characterized by buried lenses of sediment that were the focus of our paleoceanographic objectives. To optimize the quality of the seismic data we slowed to 6.0 knots and decreased the time between shots from 15 sec to 12 sec. Resolution and signal-to-noise ratios increased dramatically. We suspect that perhaps there is a certain "break-in" time for a new streamer after which original stiffness is gone and the plastic sleeve and enclosed wires have become more supple and less prone to towing vibrations. Our profiles never again degraded to the disappointingly poor quality of the first couple of days.

The second Data Logger crash occurred at 1645 on October 24, and began a curious pattern. For the next four days, without apparent cause, the system crashed every 23 hours and 30 to 50 minutes. As yet we don't know why these occurred or why they stopped after October 28.

Dan Chayes had been working on the ONLINE2 program to provide LORAN (either Time Delay mode or Range-Range) or GPS navigation to the bridge by way of the VAX computer, and at 1319 on October 25 the program was up and working. For the duration of the cruise GPS was available in two sessions per day for a daily total of 5 to 6 hours. LORAN TD's were generally reliable during the day; the latitude and longitude readouts calculated by the main lab INTERNAV set were routinely calibrated to transit satellite positions as the latter arrived on the two-channel Magnavox receiver on the bridge and were relayed to the main lab. LORAN Range-Range data was far more difficult to translate into lat/long, was far less stable, and was rarely used. Nightime LORAN positons were often very unreliable due to low S/N ratios and the inability on our part to force the INTERNAV to track on anything above the third peak. Local squalls appear to have also contributed to low S/N ratios at all times of day.

At 1620 on October 25 we broke off from the seismic survey of the buried sediment lenses and headed east to begin a search for an outcrop to be cored. The motive was to gather a little reconnaissance data of potential coring targets and then return west to be in position for the MCS deployment early the next morning to make use of a full day of sunlight if needed. One outcrop zone was located for later intensive surveying and coring.

The starboard water gun was pulled because of worn O-rings at 0000 on October 27; by 0800 of the same day the port gun was occasionally misfiring, indicating it, too, was wearing out O-rings. Martin has noted that water guns are reliable for about 50,000 shots per set of seals and O-rings. This translates to 9 full days at a 15 sec repetition rate, exactly what we got from both guns. At 1200 all gear was retrieved and we began the MCS deployment. We attached 10 birds, calibrated 6 DT's and counted roughly 250 1-lb. weights already on the streamer, and it was totally deployed by 1700. The first shot with four 466 cu. in. airguns was at 1743. The firing cable on gun #2 was replaced at 1800. At 1846 we began recording MCS channels 43 through 46 on the Data Logger, digitizing at 1 msec (vs. 4 msec on the DFS-IV) for later comparison. We began recording on the DFS-IV at 2100 near SP 10250 of the two-ship seismic line NAT-15, and continued NW along this line. At 0506 on October 28 the tail buoy crossed CDP 8850 of NAT-15 and we shut the guns off, re-fitted each with new chambers, turned and resumed firing along the reciprocal course along NAT-15 at 0826. The object was to test the performance of an array of 4 "tapered" guns. This combination of 750, 500, 300 and 235 cu. in . airguns will hopefully provide a less reverberent source while still providing good acoustic penetration. Gun #4 (the 500 cu. in gun) had to be pulled twice to replace the firing cable. At 1553 the head of the streamer was towing too shallow (25 feet vs. 35-42 feet for the rest) and we slowed the ship in hopes of lowering it. Unfortunately, the center sections then rose rapidly to the surface and would not dive again, despite our resuming 5.0 knots. Evidently the streamer was not sufficiently weighted; it had been necessary all along to keep the birds dialed for 55 feet just to keep the streamer at 40 feet. Fearing that we may have broken off bird wings, the streamer was retrieved beginning at 1647; at 2205 the tail buoy was aboard and no damage to any birds was found. Nonetheless, considering (1) the time required for re-deployment, (2) the fact that one of the two MCS tape drives had just broken down (it was later found to be a failed transistor that was repaired), (3) the fact that if we continued MCS it would mean a maximum of 5.0 knots and (4) the acceptably good quality of the SCS data, we ended MCS at this point and redeployed one water gun at 2355. Over the next several days two MCS active sections were drained, stripped and packaged for shipment back to Houston for repairs. One was AS#9 which we took right off the streamer as it was reeled in. The other was a section wound on a spool and tied down on the Ø1 deck. Onto the spool freed up by removing the latter we wound the old L-DGO streamer, and secured it on the Ø1 deck on the port side below the stack.

With one gun we conducted a seismic tie line from the buried sediment lenses SE to DSDP Sites 417D/418A. Both sites were crossed on a single S-N line, and then 417D was crossed again going W-E. It had been hoped that these lines could have been done with MCS.

By 1730 of October 30 a second water gun was in the water. This time it was the 80 cu. in . gun that had been borrowed from URI to be used as a back-up spare. Despite a badly pitted distributor chamber and the need to include several rings and seals from the L-DGO supplies to make up a complete gun, it performed well.

We next embarked on the piston coring portion of our program, the objective being to recover samples from outcrops that would determine the age and origin of the seismic reflectors bracketing the sediment lenses buried farther to the west. We devised a plan that would restrict seismic and 3.5 kHz surveying to the nighttime and reserve the daylight hours for rigging the cores and especially ensure coring during the hours of 1500-2000 when GPS positioning was available. As a further restriction, GPS is not generally reliable at any time on weekends because of modifications being done by the manufacturer back in California.

All seismic records collected across potential core sites were quite good because of (1) dual water gun coverage, (2) ship speed was kept near 6 knots, and (3) shot cycle was reduced to 12 sec from 15. The 2.5 sec sweep displayed on profiler "A" was especially useful because its resolution overlapped that of the 3.5 kHz recorder, and consequently relatively deep seismic reflectors could be followed on the seismic recorders to where they shoaled enough to then be identified on the 3.5. To our dismay, we never found a clear OUTCROP of our two target reflectors. At best, the shallower of the two came to within 8 or 10 meters of the seafloor. With full penetration, a two- or three-pipe core should have been adequate in all cases; preliminary shipboard analysis of core samples were not very promising. We anticipated that the upper reflector was middle Eocene and this level would be rich in siliceous fossils as was the section recovered at the nearby DSDP Site 417D. Fish teeth of early Miocene to Oligocene age provided the oldest dates yet determined at the bottom of the cores collected on RC2512.

Seven cores were attempted between October 31 and November 3; six were recovered with varying degrees of success. All were accompanied by a 12 kHz pinger clamped to the wire 50 m above the tripping arm. Core 1 was rigged with 3 pipes; we got 2 pipes of penetration and a badly bent second pipe. Though the cutter showed no signs of damage, it is possible that penetration was halted by a hard sub-surface layer, possibly the target horizon. Initial shipboard examination showed this core is mostly unfossiliferrous. Core 2 was taken in virtually the same location, but was only 2 pipes in length. Full penetration was achieved. Fish teeth biostratigraphy indicated Oligocene to lower Miocene strata at the bottom of this core. The value of a pinger was demonstrated on Core 3 when it was used to re-position the core. With 5300 m of wire out in 5400 m of water, the bottom return of the wire-mounted pinger showed that the core had drifted away from the target area and that its sub-bottom objective was too deep for the 2 pipe core we had rigged. Likely drift was calculated and the bridge was asked to steam on the wire. After about 10 minutes the ship hove to once again, and the core wire was allowed to catch up to the ship and settle down. When it did a few minutes later, the sub-bottom reflector we were trying to sample had risen to about 10 m below the seafloor. Unfortunately we got only 1 1/2 pipes of penetration and recovery (ca. 10m). Like Core 2, the basal strata were Oligocene to lower Miocene. Perhaps once again the target reflector was a hard surface that halted any additional penetration. This seems even more likely in light of the fact that the cutter returned empty. We considered swapping core heads and installing the 2,000 lb. one that was stored below decks, but chose instead to continue using the 1,500 lb. device and increase the free fall distance. For the next core we rigged 2 pipes again but allowed for free fall equivalent to a 3 pipe core. Unfortunately, this core pre-tripped at 1400 m (as recognized LATER on the 12 kHz monitor), and in effect we took a long gravity core that despite an apparent penetration (mud-line) of 36 feet recovered only 4 feet of core. No shipboard age determinations were possible on Core 4. This deep penetration lent further support to our suspicion that the surficial sediments were very soft, and that anything substantially less than full recovery on a properly functioning piston core was the result of a very hard subsurface layer. We rigged 3 pipes for core 5. Unfortunately, because of an inoperable pipe straightener and because of the accidental loss of an empty pipe over the side during a severe ship's roll, we were down to few choices of pipes. For some reason two or more good core pipes had been off-loaded in St. John's. Hence we rigged a few oddly-sized pipes for a total of 62 feet; 61 feet of core liner was inserted, and in hindsight that one-foot gap proved disastrous. The hit was clearly visible on the pinger record for core 5, and full penetration appeared to have been achieved. But on recovery we learned that the core was very badly bent at the coupling between pipes 2 and 3. After a difficult disassembly we discovered that the piston was wedged in the one-foot space between the top two core liners. This prevented the core from free-falling any farther and explains way it toppled over and bent ABOVE the point of the seized piston. This analysis only became apparent long after core 5; the Co-Chiefs were not kept informed of all these pertinent facts at the time. Hence we were convinced at the time that it was simply that 3 pipes were overly ambitious, and we rigged for 2 pipes from then on. Core 6 was recovered without mishap, despite the discouraging likelihood that it never actually hit an outcrop, and recovered only upper Neogene strata. Core 7 was attempted under extremely adverse conditions: at night with no GPS, only scattered transit satellite coverage, local squalls that invalidated LORAN TD's, strong surface drift, and variable, strong winds. On top of that, the target was an extremely narrow outcrop zone. Several attempts were made to heave to and maintain position, but it was the advice of the bridge that without the experience of Captain Jorgensen at the helm it ought not be attempted. Because of the slim chance of successfully coring the suspected outcrop against these odds, the decision was made to cancel the attempt entirely.

Hence at 0500 on November 3 the seismic gear and magnetometer were once again streamed, and we headed off north to begin the final N-S seismic tie line along the western Bermuda Rise. To optimize data quality we maintained 6 knots and fired the guns every 12 secs, slowing to 15 secs only during sonobuoy deployment. We crossed a portion of the ENDEAVOR-91 track that indicated a possible outcrop of mid-Tertiary turbidites. Closer inspection without 3.5 kHz echosounder as we steamed over the site showed that the target was covered by a prohibitively thick cover of younger sediment. However, a very promising, true outcrop was passed at 1612 of November 4, and we halted for a coring attempt. Much was in our favor: the core was still rigged along the rail, GPS was up, the weather was clear and calm, and the outcrop appeared to be adequately large (roughly 1.5 n.mi. along the direction of our track). Unfortunately it was probably quite narrow in the other direction; we saw substantial side echoes on the seismic profilers. The core hit bottom 200 m short of the expected depth, and reconstructing the event on the 12 kHz record shows clearly that the core drifted off to the flank of an adjacent high despite the fact that the ship maintained excellent position over the target. 10 cm of very solid manganese nodules/pavement was all that was recovered. It is apparent that very swift bottom currents exist at present at this location. Because of the high probability of repeating this result with another core and the fact that it would have to be done WITHOUT GPS, we decided to resume the seismic tie line.

After 102 hours of operation (roughly 31,000 shots) the URI water gun developed an air leak at 1900 on November 5. The second L-DGO gun was ready, and redeployment was completed by 2130.

At 2100 on November 5 we reached the northern limit of any reasonable survey that could get us back south towards San Juan at less than 7.5 knots. We turned, increased speed to 7.0 knots and profiled south along the western edge of the Bermuda Rise. The final sonobuoy (#20) was deployed at 0210 on November 7. At 1215 on November 7, the L-DGO water gun on starboard began double firing. It had about 144 hours on it (ca. 43,000 shots, close to the typical 50,000 shot maximum). The URI gun was repaired by this time and back in the water along starboard by 1430. We crossed DSDP Site 417D at 1552 on November 8, and hauled in the seismic gear at 1609. From there to San Juan we steamed at 10.5 knots with only the magnetometer deployed; we did not think it wise to risk losing a gun or damaging the streamer at this necessarily high speed. The magnetometer was retrieved at 0000 November 10 on the inner wall of the Puerto Rico Trench, and scientific watches in the main lab were ended. We docked in San Juan at 1051 that day.

SCIENTIFIC RESULTS:

Only the most cursory of observations hold any validity at this point; the seismic reflection/refraction data await digital processing, and the regional field must be removed before magnetic anomalies can be displayed along track and analyzed. One fact not noticed on earlier seismic profiles is that the high-amplitude reflectors indicative of turbidites in the Horizon-A complex of the Bermuda Rise actually THICKEN towards elevated basement highs. This raises the possibility that these topographic features shed debris ("pelagic turbidites") during mid-Tertiary uplift, and that not all of the turbidites of the Horizon-A complex were derived from the N. American continental margin. Core 7 was to test this, but as described above, was blown off outcrop by bottom currents.

No faults were clearly identified in any of the profiles, but abrupt changes in depth to particular reflectors was noted on either side of many basement features. This was especially apparent in the aftershock survey area.

Water guns do not appear to be rich enough in low frequencies to generate refracting waves; only a few refracting arrivals were observed in 20 sonobuoy experiments. Oblique reflections, however, promise to yield excellent velocity information in the 1+ second of sediment.

The sediment lenses on the southwestern Bermuda Rise are most definitely NOT buried sediment waves. Only careful processing will tell for sure, but it appears that their shape is the result of a mid-Tertiary erosional event that planed off the top of a blanket of pelagic sediment draped over undulating topography. This would suggest that the "lenses" are Late Cretaceous to early Tertiary in age and that the "A^u event" well documented along the margin extended out to the western Bermuda Rise as well. Alternatively, erosion may have occurred earlier, at the time that sediments comprising reflector A^c were deposited (early/middle Eocene).

RECOMMENDATIONS:

1. Improve the SCS signal-to-noise ratio: The single most frustrating problem of the cruise was the uncertainty of WHY our records were so noisy at the beginning. A thorough, systematic check of all components of the acquisition system ought to be conducted to ensure we aren't doing it to ourselves. If that turns up nothing, decreasing the hydrophone spacing on the streamer should be considered, or perhaps a second, identical streamer ought to be purchased and towed off the opposite side of the ship. Some means of monitoring and then controlling the towing depth of both the guns AND the streamer should be devised.

2. Purchase depth monitors for the SCS streamer: The cable contains two DT's; we need the means to READ the depths aboard the ship.

3. Optimum seismic set-up:

- 2 water guns, monitored and synchronized by the watchstanders
- no towing fins on guns
- 6.0 knots is excellent, 7.0 knots good, 8.0 knots too fast
- fire guns as often as possible (8 sec cycle is fastest possible)
- tow the streamer from a boom with shock cord
- sum all four streamer sections to one in water depth > 3000 m
- sample at 1 msec

4. Organize personnel better for core deployment/recovery: Despite the experience and talents of both Martin and Rapaté, neither one is the right person for overseeing coring operations. For efficient and safe work in a potentially hazardous situation, a calm and assertive supervisor is required. In most cases the Chief Scientists and other members of the scientific party are not experienced enough with the ship's equipment to fill this role. In our estimation one of the ship's officers ought to be present when the core is going over the rail, and more importantly, when it is coming back in. Any one of them will have the knowledge of available ship's equipment (blocks, cables, lines, capstains, etc.), the necessary rigging experience, and the authority needed to command the respect of the crew doing the work. We discussed this with Capt. Jorgensen, and in general terms he concurred that this would be a wise change. In

addition, someone besides Rapaté ought to be a qualified core winch operator. As it now stands he is the ONLY one, and this is obviously hard on him during numerous back-to-back cores.

5. Move the streamer take-up reel to port: At present the SCS streamer tows off starboard, after being snaked through the guard rail on the Ø1 deck and over the A-frame. Deployment and retrieval would be very much easier and safer for the eel if it went off what is now the magnetometer winch.

Gregory Mountain

Kenneth Miller

November 16, 1984